Democracy and Capital Inflows*

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Abstract: Democratic regime change has large positive effects on income per capita, yet the underlying transmission channels of this relationship are largely unexplored. We provide novel evidence studying the effect of democratic regime change on capital inflows in a large sample of economies from 1975 to 2015. Using heterogeneous difference-in-differences estimators, we find that regime change causes an average 40-50% (60-95%) increase in gross capital (FDI) inflows within two decades. We document significant treatment effect heterogeneity by geography and colonial history (but not legal origin or culture), which we explain highlighting structural deficiencies in export concentration, trade costs, and complexity of production. We find little evidence that the geographic patterns arise from indirect effects of 'nature' shaping legal origin, culture, or colonial history.

Keywords: capital inflows, economic development, democratic regime change, deep determinants of growth, difference-in-differences, interactive fixed effects, heterogeneous treatment effects

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1 Introduction

"[Geography] tells an unpleasant truth, namely, that nature like life is unpleasant, unequal in its favours; further, that nature's unfairness is not easily remedied." (Landes, 1999, 4/5)

For a long time many academics, policymakers and development practitioners doubted the economic dividends of democratic regime change: enabling the populace to remove an incumbent government through the power of the electoral process (one of the fundamental functions of a democracy) would drive up consumption and, via tax increases to finance redistribution efforts, reduce the rate of investment, to the detriment of economic growth (e.g. Baum and Lake, 2003, 334f). Doubters would further point to the stellar growth rates in autocratic regimes such as China or Singapore to question whether democracy is *necessary* for economic prosperity. While that may not be the case, beyond cherry-picking autocratic success stories, it is widely recognised that growth outcomes vary substantially across autocracies (Persson and Tabellini, 2009; Knutsen, 2021; Imam and Temple, 2024), and the strong *average* improvement in economic development in democratising countries established in the more recent literature (Acemoglu et al., 2019; Eberhardt, 2022; Boese-Schlosser and Eberhardt, 2024) provides convincing evidence for a positive and large causal effect of democratic regime change. The most significant gaps in our understanding of the economic effects of regime change relate to tracing the effect in relevant transmission channels and probing the effect heterogeneity across countries (Acemoglu et al., 2019; Eberhardt, 2022) for any systemic patterns.

In this paper we fill this gap by studying the relationship between democratic regime change and capital inflows, which are an important driver of economic growth, thanks to increased investment, a productivity boost, and/or reduced misallocation (Prasad et al., 2007; Larrain and Stumpner, 2017; Erten et al., 2021). Democratic regime change represents the sort of reforms that "curtail the power of entrenched economic interests and liberate the economy's productive potential" (Obstfeld, 2009, 63), while at the same time making "economies safe for international asset trade" (ibid). Democratic regime change should reduce economies' objective or perceived political and economic risk factors, and hence attract higher foreign direct investment (FDI) and other financial inflows (Li and Resnick, 2003; Papaioannou, 2009; Asiedu and Lien, 2011; Fratzscher, 2012). Of course, where capital does or does not flow and why has occupied the profession for a long time — see the literature studying the 'Lucas (1990) Paradox' and the 'allocation puzzle' (Gourinchas and Jeanne, 2013). Existing work has suggested that institutions are important determinants of capital inflows (Li and Resnick, 2003; Alfaro et al., 2008; Papaioannou, 2009; Asiedu and Lien, 2011) and hence at least a partial solution to this puzzle. Linking capital flows to democracy rather than individual institutions (as in Papaioannou, 2009, among others) brings our work in line with the recent literature on democracy and growth and highlights a specific channel through which democratic regime change can lead to greater economic prosperity.

We analyse a large panel of countries (1975-2015) using a novel methodology from the heterogeneous treatment effects literature (the Chan and Kwok, 2022, Principal Component Difference-in-Differences estimator, PCDID) which employs a common factor structure to capture unobserved time-varying heterogeneity (Pesaran, 2006; Bai, 2009; Gobillon and Magnac, 2016; Xu, 2017).[1] To define democratic regime change we adopt V-Dem data (Coppedge et al., 2021) for 'liberal democracy' and also the indicator devised by Acemoglu et al. (2019) which captures similar institutional building blocks related to electoral democracy, the rule of law, and executive constraints (see Appendix Figure A-1 for a visualisation).

Our benchmark results (average treatment effects on the treated — ATET) establish that democratic regime change has a positive and sizable effect on total capital inflows over GDP on the order of 1 to 2 percentage points (for a sample mean of 3% during the autocratic period of all countries that subsequently experienced democratic regime change). For FDI inflows the effects amount to 0.6 to 1.4 percentage points (for a mean FDI/GDP of 1.4%). Based on these results we can conclude that the average country experiencing democratic regime change enjoys a substantial boost to total capital (FDI) inflows on the order of 40-50% (60-95%) over the next two decades.[2]

But these average effects may be hiding substantial heterogeneity across countries, and this issue is a major focus of our paper. We hypothesise that 'nature' (geography), related to the characteristics of climate and historical disease environment, plays an important role in this context.[3] We argue that deep-seated factors prevail in shaping the economic effects of democracy. Geography determines economic 'structure', e.g. the complexity and diversity of the export basket (Malik and Temple, 2009), or the potential for and speed of structural transformation away from agriculture (Eberhardt and Vollrath, 2018), and this, in turn, determines investment opportunities. 'Poor geography' is associated with a lack of investment opportunities, poor returns, and hence low capital inflows. We highlight that compared with their peers, countries with 'poor geography' (i) have relatively higher export concentration (in terms of goods/products) and hence are more exposed to global commodity market fluctuations, resulting in greater aggregate commodity price volatility; (ii) suffer from higher trade costs; and (iii) are characterised by a productive system of lower complexity.

A simple setup enables us to empirically isolate the differential effect of democracy *by geography*: we construct separate sets of treatment and control samples by geography ('good',

[1] Under reasonable assumptions our estimator accounts for selection into regime change and we are able to test a form of the parallel trend test required for identification.

[2] The average time spent in democracy is around 17-18 years, depending on the definition of democracy.

[3] We do not use the term 'natural endowment' employed in some of the literature since this too readily leads to association with 'natural resources' in the form of minerals, oil, etc., which are explicitly not part of our concept of geography. Our proxies for geography are, in terms of climate: (i) land area in the tropical climate zone; (ii) land area in the temperate climate zone; and (iii) absolute latitude. In terms of disease environment: (i) the share of population at risk of malaria in 1965, (ii) malaria ecology, and (iii) the historical prevalence of 7 endemic diseases. Continuous indicators are dichotomised at the full sample median.

'poor') and estimate average treatment effects on the treated for democratic regime change using the Chan and Kwok (2022) estimator. Estimating the heterogeneous PCDID for *all* countries and then computing averages by geography would undermine the clean counterfactual setup of our above strategy, which matches 'poor geography' control countries with 'poor geography' treated countries, and 'good geography' control countries with 'good geography' treated countries.[4] If the effect of geography on regime change propensity is thus taken out of the equation, we can separately identify the causal effect of democracy on capital flows in 'good' and 'poor' geography countries, respectively, and can directly compare the economic magnitudes.

Our results strongly support the notion of 'nature' being "unequal in its favours" (Landes, 1999, 4/5): countries with 'good' geography experience 1 to 4 percentage points higher capital inflows/GDP following democratic regime change for a mean of 3.8%, whereas those with 'poor' geography experience a 0 to 2 percentage point increase for a mean of 2.6%. For FDI the effects are a 0.5-2.5pp increase (mean 1.7%) and a 0-1.5pp increase (mean 1.2%) in 'good' and 'poor' geography countries, respectively.

Naturally, geography is not the only deep determinant of economic prosperity banded about in the literature (La Porta et al., 1998; Stulz and Williamson, 2003; Rajan and Zingales, 2003; Nunn, 2009; Gorodnichenko and Roland, 2017), hence we demonstrate that alternative explanations related to legal origin (French civil law origins provide lower legal protection for investors) and culture (individualism emphasizing personal freedom and achievement; linguistic similarity across countries enabling communication and exchange fostering innovation and modern growth) fail to provide manifest differences between groups of countries like the patterns in the case of geography. In contrast, colonial experience strongly correlates with meagre treatment effects for democratic regime change: ATETs for countries without colonial experience exceed those of former colonies by a factor of 2 to 3.

Revealing geographic patterns of differential democratic regime change effects can offer important insights to academics and policymakers, but cannot elucidate the reasons *why* we observe them. We deal with this in two ways: First, we highlight the strong correlation between structural deficiencies and 'poor' geography: higher export concentration, higher aggregate commodity price volatility, lower productive complexity and higher trade costs. Compared with alternative splits by deep determinants related to culture, legal origin, or history, these measures consistently put 'poor' geography countries at a relatively greater disadvantage against good geography peers. Second, we consider indirect effects of 'nature': there are many instances of geography influencing history (Nunn, 2009), most prominently so in the context of the 'extraction' of slaves in Africa (Nunn, 2008) and the patterns of different forms of colonisation (Acemoglu et al., 2001). Similarly, culture

[4] Setting a deliberately high bar for our definition of democratic regime change avoids the concern that democracy might 'mean different things' in countries with 'good' versus 'poor' geography. We also check that countries in the two geography samples spend a similar number of years in democracy and do not have differential propensities of reverting to autocracy.

can be influenced by geography, where unfavourable nature can create barriers to the diffusion of ideologies, beliefs, ideas, or the means of communication (Gorodnichenko and Roland, 2017), effectively limiting the spread of certain 'culture' or preserving cultural isolation. Our aim is to provide evidence that the strong geographical patterns we reveal may really be attributable to cultural, historical, or legal factors *shaped* by nature, and to a lesser extent due to nature itself. We answer a simple question which exploits one strength of our heterogeneous treatment effects empirical approach: what are the patterns of capital flow increases following democratic regime change between 'good' and 'poor' geography samples if we focus on different deep determinants *within* the treatment groups? For instance, among the 'poor' geography sample, how did countries with (unfavourable) French legal origin fare, and are their average treatment effects similar or very different from countries with French legal origin which are endowed with 'good' geography? Adopting this strategy for an expanded number of proxies for culture, legal origin, history and 'heritage' (genetic distance), we find no evidence that any of these characteristics is the real driving force of the strong geographic patterns we reveal.

Empirical Strategy We adopt a treatment effects framework (Papaioannou and Siourounis, 2008; Acemoglu et al., 2019), but use an implementation which adds common factors estimated from control sample regressions to the *country-specific* treatment regression model: the Chan and Kwok (2022) Principal Component Difference-in-Differences (PCDID) estimator.[5] Like any other Difference-in-Differences estimator the PCDID studies treated countries before and after treatment, but there are no control country observations included in the treatment regression. These are instead captured in the form of estimated common factors. The intuition is that our country-specific specification of capital inflows as a function of a democracy dummy, an intercept, and some control variables omits a great deal of unobserved heterogeneity: time-*varying* determinants of capital flows which are also affecting democratic regime change as well as the controls — country-specific productivity or absorptive capacity may be good examples (e.g. Eberhardt and Presbitero, 2015; Chirinko and Mallick, 2017; De Visscher et al., 2020; Madsen et al., 2021). Factor models construct proxies for these omitted factors, either by Principal Component Analysis from regression residuals (Bai, 2009) or by use of cross-section averages of all observed variables in the model (Pesaran, 2006). These proxies are then entered into the estimation equation: like a country fixed effect in a pooled panel model solves the problem of unobserved time-*invariant* determinants correlated with the outcome (capital flows) and the independent variables (democracy, controls), these 'interactive fixed effects' solve the problem of unobserved time-*varying* determinants correlated with the outcome and independent variables. This is the setup in standard heterogeneous panel models. In the difference-in-differences context, there is a tweak: the common factors are estimated from the residuals of *control country regressions* (capital flows regressed on an intercept and controls, country by country, in the sample of never-democratisers), and then included in the

[5] Existing empirical applications adopting the PCDID include Eberhardt (2022), and Boese-Schlosser and Eberhardt (2024).

country-specific treatment effects regressions as covariates with country-specific parameters. In standard DID models the parallel trend test can inform us whether treated and control countries were already on different trajectories prior to the treatment. 'Unparallel trends' constitute the single most important challenge to causal identification in the pooled DID. Chan and Kwok's (2022) paper carries the subtitle "Difference-in-Differences When Trends Are Potentially Unparallel and Stochastic", but this does not mean that the above strategy is guaranteed to work. Instead of a parallel trend test, the empirical specification has to satisfy the Alpha test for 'weak parallel trends' (Chan and Kwok, 2022), which checks whether the 'information' about unobserved heterogeneity the PCDID extracts from the control sample is equally 'relevant' in the treatment sample.

Related Literature and Contributions Our research speaks to three separate strands of literature. First, our work is related to the empirical literature on democracy and growth, which only recently established a positive and large causal relationship (Madsen et al., 2015; Acemoglu et al., 2019; Eberhardt, 2022; Boese-Schlosser and Eberhardt, 2024). Two important challenges to a better understanding of *how* democracy causes growth remain: (a) the transmission mechanisms by which democracy leads to growth have not been studied systematically; and (b) existing studies assume that the democracy-growth relationship is common across countries, which makes it difficult to derive policy implications for individual countries (Durlauf, 2020). Our paper explores capital inflows as the conduit for the effect of democratic regime change on prosperity and geography as an important factor governing the patterns of heterogeneous democracy effects.

Second, we contribute to the extensive literature on the determinants of capital inflows, studying the role of democracy as a domestic 'pull factor'. Capital inflows are widely regarded to have a positive impact on growth (Alfaro et al., 2004; Prasad et al., 2007; Erten et al., 2021) and institutions/democracy have been suggested as an important factor in determining the magnitude of capital inflows (Li and Resnick, 2003; Alfaro et al., 2008; Papaioannou, 2009; Asiedu and Lien, 2011), in addition to differences in growth and productivity, and capital market imperfections (Lucas, 1990; Gourinchas and Jeanne, 2013; Alfaro et al., 2014).[6] We contribute to this literature by quantifying the average effect of regime change on capital inflows and highlighting its differential effect *by geography*.

Third, we contribute to an older cross-country empirical literature on the deep determinants of comparative development. 2021 marked the twentieth anniversary of the publication of *'The colonial origins of comparative development'* (Acemoglu et al., 2001). Though not the first empirical contribution on the link between institutions and growth (e.g. Hall and Jones, 1999), it is arguably the paper which firmly established the quality of institutions as the most significant 'deep

[6]However, excessive capital inflows are associated with higher financial vulnerability (Kaminsky and Reinhart, 1999; Reinhart and Reinhart, 2009; Caballero, 2016; Erten et al., 2021). In separate analysis (available on request), we study whether democratic regime change has a differential effect on excessive capital inflows (bonanzas or surges) across countries with 'good' and 'poor' geography. We conclude that while democratic regime change is often associated with reduced financial vulnerability in the 'good' geography sample, the opposite is trues the 'bad' geography sample.

determinant' of long-run economic development, an important contribution recently recognised as part of the award of the Nobel Prize for its three authors. In the years after its publication empirical battles were fought over the supremacy of institutions over geography and trade openness (e.g. Dollar and Kraay, 2003; Easterly and Levine, 2003; Rodrik et al., 2004) as well as over the precise definition of institutional quality which did (or did not) cause development over the long-run (Glaeser et al., 2004). Related work has shifted attention to the study of culture (Stulz and Williamson, 2003; Gorodnichenko and Roland, 2017), history (Nunn, 2009) or legal origins (La Porta et al., 1998, 2008; Monnet and Velde, 2021).Most of this work is based on regressions in the cross-section and defines 'institutions' as time-invariant. We contribute to this literature by considering democracy as a time-varying bundle of institutions (in line with recent seminal contributions on democracy and growth) and studying the differential effects of democratic regime change across different sets of country groups defined by immutable characteristics proxying for geography, history, legal origin, or culture. We demonstrate that the strong correlation in the patterns with geography is unlikely due to an indirect effect of geography via legal origin, culture, or colonial history.

The remainder of this paper is structured as follows: in the next section, we introduce the data and empirical strategy, which is applied to demonstrate the large average causal effect of democracy on capital inflows. Section 3 provides some descriptive evidence for the effect of geography, reviews the literature on deep determinants more generally, and introduces our proxy variables for geography, legal origin, culture, and history. Section 4 details how we isolate the effect of deep determinants on the democratic dividend and presents the main results for treatment effect heterogeneity across deep determinants, alongside a range of robustness checks and extensions. We highlight the 'structural' disadvantage of countries with unfavourable geography (compared with other deep determinants) along a number of dimensions related to trade and productivity as possible explanation for the treatment effect heterogeneity uncovered. We then explore in Section 5 whether the strong correlations between capital inflow effects of democratic regime change and geography hide cultural or historical factors. Section 6 concludes.

2 Democracy and Capital Inflows

2.1 Data, Methodology and Presentation

Data and Transformations We focus on two indicators for democratic regime change which combine elements of electoral democracy and aspects related to the rule of law and executive constraints: first, we adopt the binary indicator of democratic regime change from Acemoglu et al. (2019, ANRR, ending in 2010). This represents a union, or sorts, of a positive Polity IV polity2 index and a Freedom House index (FHI) coded as 'free' or 'partially free' to "purge spurious changes in each" (50) — panel (b) of Appendix Figure A-1 provides a visualisation of

the institutions covered by these indices. ANRR further build on the practice of Papaioannou and Siourounis (2008) and consider each case of democratisation in their data against the historical narrative. Finally, in contrast to the practice in much of the earlier work, they do not retrospectively re-code short episodes of democracy. Second, we take the V-Dem definition of 'liberal democracy' combining the principle of electoral democracy (polyarchy, following the work by Dahl, 1971) with executive constraints and the rule of law (summarised as the 'liberal component' in the V-Dem data, Coppedge et al., 2021) — the latter two institutional factors are seen as the "truly distinctive" feature of liberal democracy (Mukand and Rodrik, 2020, 765). As this measure for liberal democracy[7] is an index between 0 and 1, we adopt the cross-country mean for this index as our cut-off for democracy.[8] In additional analysis, enabled by the hierarchical structure of the V-Dem indices (see panel (a) of Appendix Figure A-1 for a visualisation), we ask whether results differ according to the two building blocks of liberal democracy, adopting the sample mean of the polyarchy index and the liberal component as respective cutoffs. This distinction is of interest as political scientists have favoured electoral democracy as the minimal definition whereas economists have typically highlighted the institutional qualities of property rights and executive constraints (see Glaeser et al., 2004; Rodrik et al., 2004, for an earlier debate on whether 'institutions rule').

We study two measures of capital inflows from the Financial Flow Analysis (FFA) database (Bluedorn et al., 2013): (1) total capital inflows, excluding the official sector, and (2) FDI inflows. These measures are expressed in percent of GDP although we also employ per capita series in robustness checks. We focus on *gross* capital inflows: net capital flow dynamics may be driven by inflows or outflows and the factors underlying these may be different (Rothenberg and Warnock, 2011; Broner et al., 2013). Appendix Figure A-2 charts the median evolution of capital inflows over the past 40 years. In robustness checks, we consider net capital flows, and include additional controls for exports/total trade (constructed from IMF DOTS) as well as population growth and per capita GDP growth (from the updated 'Maddison' database, Bolt and van Zanden, 2020).

Sample Studying the details of the sample makeup in Appendix Table A-1 it is clear that our analysis here largely *excludes* advanced economies: 33 High-income economies were always democracies (Liberal Democracy definition) which are excluded in the PCDID implementation, only nine experienced democratic regime change (total of 51 treated countries) and only six are in the control sample (total of 58 control countries).[9] The time horizon is 1975-2015.

Principal Component DID We estimate country regressions for treated countries only but augment each country-regression with common factors estimated from the residuals of the same regression model *in the control sample* via Principal Component Analysis (following Chan and

[7] The nomenclature is unfortunate, but it is important to stress that we do *not* employ the Lührmann et al. (2018) ROW 'liberal democracy' definition (based only on the electoral democracy index).

[8] In robustness checks, we adopt the full sample mean plus $1/4$ or $1/2$ standard deviation of the index.

[9] The nine treated countries are Croatia, Hungary, Uruguay, Panama, South Korea, Poland, Chile, Spain, and Portugal. The six control countries are Hong Kong, Kuwait, Oman, Saudi Arabia, Singapore, and the Seychelles.

Kwok, 2022). The basic intuition of this approach is that the unobserved time-varying heterogeneity driving outcomes (capital flows) and determinants (democratic regime change, controls) in the treated sample of countries (which did democratize at one point) can be proxied by information collected in the control sample (countries which never democratized). If we ignored unobserved time-varying heterogeneity in our treatment regression, then it would suffer from omitted variable bias. Using estimated 'placeholders' for this heterogeneity, we can (under reasonable and testable assumptions) identify a causal treatment effect. The PCDID is part of a suite of empirical estimators exploiting 'interactive fixed effects' (Bai, 2009; Gobillon and Magnac, 2016; Xu, 2017): adding estimated common factors in the treatment regression and allowing each factor to have a country-specific coefficient solves the problem that treatment could be endogenous *and* that treated and control countries may be on different 'trajectories' before the treatment already (non-parallel trends). Like any DID estimator, there is some variant of a parallel trend assumption that needs to be satisfied: for the PCDID, the requirement is that the 'information' captured by the factors in the control sample is 'relevant' for the treated sample — the factor coefficients should on average be equal between treated and control sample regressions, which we can investigate using the Chan and Kwok (2022) Alpha test. We discuss our empirical strategy in more formal terms in the following.

Using potential outcomes, the observed outcome of treatment D_{it} for panel unit i at heterogeneous time T_{0i} can be written as

$$y_{it} = D_{it}y_{it}(0) + (1 - D_{it})y_{it}(1) = \Delta_{it}\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_{0i}\}} + y_{it}(0) \tag{1}$$

$$\text{with} \qquad y_{it}(0) = \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \widetilde{\epsilon}_{it}, \tag{2}$$

where the two indicator variables $\mathbf{1}_{\{\cdot\}}$ are for the treated panel unit and time period, respectively, Δ_{it} is the time-varying heterogeneous treatment effect, x is a vector of control variables with associated country-specific parameters β_i,[10] $\mu_i' f_t$ represents a set of unobserved common factors f_t (which can be nonstationary) with country-specific factor loadings μ_i, and $\widetilde{\epsilon}_{it}$ is the error term.

The treatment effect is assumed to decompose into $\Delta_{it} = \overline{\Delta}_i + \widetilde{\Delta}_{it}$, with $E(\widetilde{\Delta}_{it}|t > T_{0i}) = 0$ $\forall i \in E$ since $\widetilde{\Delta}_{it}$ is the demeaned, time-varying idiosyncratic component of Δ_{it}; we refer to $\overline{\Delta}_i$ as ITET, the treatment effect of unit i averaged over the treatment period. The reduced-form model is

$$y_{it} = \overline{\Delta}_i\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_{0i}\}} + \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \epsilon_{it}, \tag{3}$$

with $\epsilon_{it} = \widetilde{\epsilon}_{it} + \widetilde{\Delta}_{it}\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_{0i}\}}$. Given the treatment effect decomposition ϵ_{it} has zero mean but may be heteroskedastic and/or weakly dependent.

The factor structure has a long tradition in the panel time series literature to capture strong cross-section dependence (Pesaran, 2006; Bai, 2009), a form of unobserved, time-varying hetero-

[10] We assume $\beta_i = \bar{\beta} + \tilde{\beta}_i$ with $E(\tilde{\beta}_i) = 0$ (Pesaran, 2006). x can be a function of f.

geneity. Strong correlation across panel members is distinct from weaker forms of dependence, such as spatial correlation, and if ignored can lead to serious bias in the estimated coefficients on observable variables (Andrews, 2005). The combination of common factors and heterogeneous parameters also allows for non-parallel trends across panel units, most importantly between treated and control units. The above setup can further accommodate endogeneity of treatment D_{it} in the form of *inter alia* correlation between treated units and factor loadings, the timing of treatment and factor loadings, or between observed covariates and timing or units of treatment.

The estimation of the country-specific treatment effect (ITET) $\overline{\Delta}_i$ proceeds in two steps: first, using Principal Component Analysis (PCA), we estimate proxies of the unobserved common factors from data in the control group (details below); second, country-specific least squares regressions of treated countries are augmented with these factor proxies as additional covariates.

The main identifying assumptions are that all unobserved determinants of capital inflows are captured by the factors, a standard assumption in the panel time series literature (Pesaran, 2006; Bai, 2009) and related causal panel models (Athey and Imbens, 2022). Since the factors are estimated with error, there is a potential correlation between the errors of treated and control countries, which will bias the treatment estimate. This bias asymptotically disappears if we require that $\sqrt{T}/N_c \to 0$, where T is the time series dimension and N_c is the number of control countries. It is further assumed that conditional on the estimated factors the control variables x are jointly insignificant predictors for the treatment — they do not constitute 'bad controls'.[11] Treated countries further have to satisfy the 'weak parallel trend' test, which we have described above as a way of confirming that the 'information' (the space spanned by the estimated factors) from the control sample on average has the same effect in treatment and control sample — see discussion in the paragraph on Diagnostic Testing below.

The estimation equation for each treated country $i \in E$ is then:

$$y_{it} = b_{0i} + \delta_i \text{Dem}_{it} + a_i' \hat{f}_t + b_{1i}' x_{it} + u_{it}, \tag{4}$$

where $\hat{f}$ are the estimated factors obtained by PCA on the residuals $\hat{e}$ from the heterogeneous regression of $y_{it} = b_{0i} + b_{1i}' x_{it} + e_{it}$ in the control group sample, and δ_i is the country-specific parameter of interest for the democratic regime change dummy Dem_{it}. y is the capital flow measure and x are additional controls we include in robustness checks (exports/trade, population growth, per capita GDP growth). We estimate (4) augmented with two to six common factors, given that determining the 'relevant' number of factors is fraught with difficulty and ambivalence. The average treatment effect (ATET, $\hat{\delta}^{MG}$) is simply the average of the country estimates $\hat{\delta}_i$. We follow the practice in the literature and use a robust mean group estimator adopting an M-estimator (Rousseeuw, 1987) with the associated standard errors based on $\Sigma^{MG} = (N-1)^{-1} \sum_i (\hat{\delta}_i - \hat{\delta}^{MG})$ (Pesaran, 2006).

[11] We carry out Wald tests for this assumption — see discussion below and Appendix Table B-1.

All of the above is laid out for a sample of N countries. In our analysis in Section 4 we estimate separate models by deep determinant of development. We do not rule out that geography or culture or history or legal origin may have an effect on the propensity of countries becoming a democracy, but adopting high barriers on our definition of democratic regime change (following Acemoglu et al. (2019) and the V-Dem definition of liberal democracy) in each treatment sample of, say, 'good' and 'poor' geography, we in effect hold the correlation between the deep determinant and democratic regime change constant across samples. This allows us to study the effect of geography on the implications of democratic regime change in isolation between countries with 'good' and 'poor' geography (similarly for alternative deep determinants).

Diagnostic testing The validity of standard pooled Difference-in-Differences estimators crucially relies on the parallel trend assumption: treatment and control samples cannot be on different trajectories prior to the treatment. In the context of the PCDID, we can allow for non-parallel trends between treated and control samples by means of a common factor model with heterogeneous factor loadings, but we nevertheless need to confirm the assumption of 'weak parallel trends' via the Alpha test (Chan and Kwok, 2022): we conduct an auxiliary regression for the treated sample

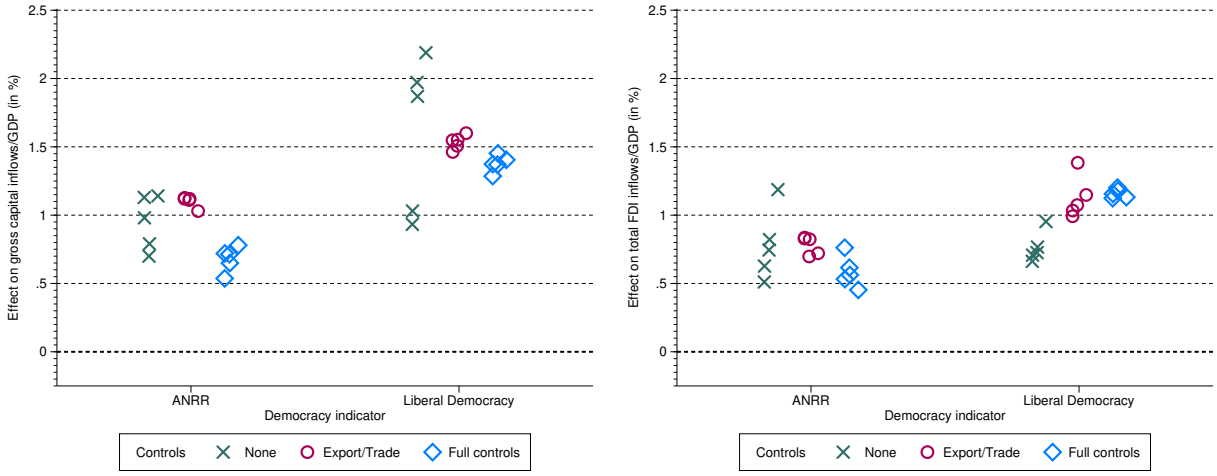
$$y_{it} = \alpha_i + \beta_i \text{Dem}_{it} + \gamma_i \bar{\hat{e}}_t + b'_{1i} x_{it} + \epsilon_{it}, \tag{5}$$

where $\bar{\hat{e}}_t$ is the cross-section average of the residual of the control sample regression $y_{it} = b_{0i} + b'_{1i} x_{it} + e_{it}$ from which in the PCDID we extract the common factors. The null hypothesis of the Alpha test is that treatment and control samples are driven by the same set of common factors and rejection of the null suggests the PCDID model is potentially misspecified. The test is in the form of a t-test for the cross-country average coefficient of γ_i in equation (5) being equal to 1, implemented via the Pesaran and Smith (1995) Mean Group estimator.

A second concern arises if we add controls to the regression model, since these may be 'bad controls' in the sense of Angrist and Pischke (2008, 64): "[V]ariables that are themselves outcome variables in the notional experiment at hand." In the present case, we assume that conditional on the estimated factors in equation (4) there is no correlation between the treatment variable Dem_{it} and the control variables x. We test this assumption by regressing the democracy dummy on estimated factors and controls in the treated sample and carrying out a Wald test for the joint insignificance of the controls. If the null is rejected we need to conclude that the controls may constitute 'bad controls'. Implementation is via the Mean Group estimator.

Presentation of results The common practice in the treatment effects literature is to report the ATET, $\hat{\delta}^{MG}$. Given the uncertainty over how many estimated factors to include (from Moon and Weidner, 2015, we know that including too many has only minimal effect on consistent estimation in OLS models, provided we have sufficient degrees of freedom), the proxies for democracy and deep determinants, and alternative specifications with additional controls the reporting of our

Figure 1: Democracy and Capital Inflows



(a) Total Non-Official Capital Inflows

(b) Total FDI Inflows

Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on capital inflows (left) and FDI inflows (right) for two definitions of democratic regime change. Each result 'cloud' (markers are randomly perturbed to aid visualisation) features PCDID augmentations with 2 to 6 estimated factors (hence: 5 markers per 'cloud'). For each democracy definition, we present results for a specification without any controls (x), with exports/trade (o), and with full controls (♢). The plots ignore statistical significance or weak parallel trend tests — see Table 1.

findings will largely be confined to visual presentation to highlight the patterns in the unequal effects of democracy on capital flows. Diagnostic test results are reported in the Appendix.

2.2 Main Results

We estimate the full sample PCDID average treatment effects of democratic regime change on capital inflows (total, FDI). Depending on the definition of democracy the treated samples amount to between 51 and 69 countries, with control samples ranging from 31 to 58 countries.

Table 1 presents the results with different panels referring to the specifications with no controls, exports/trade as additional control and, additionally, population growth and per capita GDP growth as controls. Our diagnostic tests indicate that the assumption of weak parallel trends is typically confirmed, with the notable exceptions of LibDem in model (2) of Panel A. Our test confirms that exports/trade (Panel B) is not a 'bad control' (i.e. it is not an outcome of the treatment variable, see Angrist and Pischke, 2008), while the more elaborate set of controls in Panel C does not pass this test — a pattern that will repeat itself throughout our analysis. Figure 1 visualises all ATETs for total capital inflows in panel (a) and FDI inflows in panel (b).

We find ample evidence for statistically significant and economically sizeable effects of democratic regime change: focusing on the specification with exports as additional control (pink circles in Figure 1), regime change has a causal effect of 1.1 (ANRR) to 1.5 (LibDem) percentage points

Table 1: Democratic Regime Change and Capital Inflows (1975-2015)

	Total Capital Inflows		**FDI Inflows**	
	(1) ANRR	(2) LibDem	(3) ANRR	(4) LibDem
Panel A: No control variables				
Democratic Regime Change	0.773** [0.388]	1.852*** [0.498]	0.804*** [0.178]	0.749*** [0.245]
Alpha Test (t)	-0.92	-2.08	0.23	0.31
Alternative factor augmentation				
2 factors	1.147***	2.195***	1.193***	0.959***
3 factors	0.707*	1.979***	0.752***	0.733***
4 factors	0.773**	1.852***	0.804***	0.749***
5 factors	0.989**	1.039***	0.636***	0.716***
6 factors	1.127**	0.930**	0.508***	0.659***
Panel B: Export/Trade as control variable				
Democratic Regime Change	1.102*** [0.374]	1.535*** [0.409]	0.806*** [0.196]	1.366*** [0.324]
Alpha Test (t)	-0.53	-0.48	0.25	1.21
χ^2 Test (p)	0.36	0.30	0.78	0.28
Alternative factor augmentation				
2 factors	1.036***	1.606***	0.727***	1.154***
3 factors	1.118***	1.515***	0.705***	1.081***
4 factors	1.102***	1.535***	0.806***	1.366***
5 factors	1.135***	1.471***	0.844***	1.042***
6 factors	1.117***	1.545***	0.826***	0.989***
Panel C: Export/Trade, pop. growth, GDP pc growth as controls				
Democratic Regime Change	0.632* [0.372]	1.435*** [0.483]	0.545*** [0.190]	1.162*** [0.335]
Alpha Test (t)	-0.48	-1.05	0.86	0.29
χ^2 Test (p)	0.00	0.10	0.03	0.07
Alternative factor augmentation				
2 factors	0.786**	1.411***	0.459**	1.138***
3 factors	0.721*	1.377***	0.623***	1.209***
4 factors	0.632*	1.435***	0.545***	1.162***
5 factors	0.728**	1.382***	0.540***	1.164***
6 factors	0.534	1.283**	0.759***	1.121***
Treated Countries	69	51	69	51
Treated Observations	2087	1830	2072	1830
Control Countries	31	58	31	58
Control Observations	825	1800	819	1779

Notes: We present robust mean estimates from PCDID regressions of total non-official capital inflows and FDI inflows and a democracy dummy defined as indicated in each column — these estimates can be interpreted as Average Treatment Effects on the Treated (ATET). The main results and standard errors in square brackets (estimated non-parametrically following Pesaran, 2006) are for the specification augmented with *four* common factors. In a lower part of each panel, we report the ATET estimates for specification with two to six factors. We further provide details of the Alpha test for weak parallel trends (t-ratio reported) and a χ^2 test for the control variables (p-value reported) — in both cases sound diagnostics imply we would not want to reject the null. Sample details are reported in the bottom rows of the table. We use *, **, and *** to indicate statistical significance at the 10%, 5% and 1% level.

higher gross capital inflows and 0.8 (ANRR) to 1.36 (LibDem) percentage points higher FDI inflows. These effects are economically large, given the average 3% capital flow/GDP ratio and 1.4% FDI/GDP ratio for treated countries prior to regime change. Although the existing literature primarily focused on proxies for different institutions instead of an overarching concept of democracy (Alfaro et al., 2008; Papaioannou, 2009), our findings underline the notion that a substantial improvement in institutions is associated with a substantial average increase in capital inflows.

3 Deep Determinants of Comparative Development

In this section, we offer a first glimpse of the heterogeneous effect of democratic regime change on capital flows using descriptive analysis. Our candidate explanations for these patterns derive from the literature on the deep determinants of economic prosperity — geography, culture, history, and legal origin — which we discuss first. We then describe the proxies we use to measure these deep determinants and show how some of them, especially geography, seem to mediate the effect of democracy on capital inflows.

3.1 Deep Determinants: Geography, Legal Origin, Culture, and History

Geography Arguments supporting a link between geography (climate, disease environment) and contemporary economic development are frequently centred on their impact on land, labour and production technology (Diamond, 1998; Bloom and Sachs, 1998; Gallup et al., 1998), illustrated by the suggestion that in tropical climates people are "enervated by the slightest physical or mental exertion" (a Bangladeshi diplomat cited in Landes, 1999, 15), which makes for a "slow rhythm [of work] with long and frequent pauses" (ibid.: 16);[12] or that (modern) innovations in production technology favour agriculture in temperate versus tropical countries (Diamond, 1998). Yet, these arguments are difficult to uphold given the 'reversal of fortune' (Acemoglu et al., 2002) whereby if climate had such a profound impact then countries which were rich in 1500 should still be rich today (but frequently are not). These authors further convincingly dismiss related explanations that agricultural technology reversed the early advantage of tropical over temperate agriculture. We therefore need to provide distinctly more 'modern' features of growth and development as likely reasons for a democracy-geography-growth link.

Standard gravity arguments for the flow of traded *goods* between countries find similar effects of distance and remoteness for capital flows (Head and Ries, 2008; Lane and Milesi-Ferretti, 2008; Papaioannou, 2009; Pellegrino et al., 2021), suggesting that "the geography of information is the main determinant of the pattern of international transactions" (Portes and Rey, 2005, 269). This speaks to distance as an important factor. Besides geographic predisposition to trade and capital flows, not just in terms of remoteness but also distance from the equator (Frankel and Romer,

[12] It is important to emphasise that they speak of local and non-local individuals being affected in this way: there is no suggestion that the people residing in tropical locales inherently exert a lower work effort and productivity.

1999), nature affects the structure of exports, which can leave countries prone to external (terms of trade) shocks (Malik and Temple, 2009).

Legal origin A sizeable literature has investigated the economic consequences of legal origin, in particular for financial development (e.g. La Porta et al., 1998; Beck et al., 2003). The conceptual arguments for such a link, that legal protection for outside investors is stronger in countries with origins in (British) common law than (French) civil law, are well-known (La Porta et al., 2008), though not without controversy: while post-WWII financial development seems to follow the suggested patterns, history provides many instances of a 'reversal' in the correlation (Monnet and Velde, 2021), thus undermining a structural link. We consider legal origin since arguments for investor protection seem equally relevant in the context of capital flows, with the legal system further representing a 'meta-institution' (Koyama, 2022).

Culture The origins of a proposed link between culture — typically defined as a shared set of values, beliefs and norms of behaviour — and long-term prosperity are usually found in Max Weber's *protestant work ethic*. While empirical work initially made a link to religion (Landes, 1999; Stulz and Williamson, 2003), it was the study of Gorodnichenko and Roland (2017) which systematically analysed the distinction between individualism (said to be fostering personal freedom, achievement, and hence innovation) and collectivism (emphasising embeddedness, group loyalty and discouraging 'standing out') — a distinction suggested to be *the* primary dimension of cultural differences (Heine, 2007).[13] Adopting a range of instrumentation strategies, Gorodnichenko and Roland (2017) demonstrate a causal link between individualism and income per worker.

Language "makes information operational" (Ginsburgh and Weber, 2020, 348) and provides a 'social technology' we can use to construct divisions of our sample into those with more similar and others with more dissimilar common language, as a crude proxy for culture: "The various aspects of culture are hard to describe and for the sake of simplicity, language may be, and is often, used as a proxy for culture and/or ethnicity" (ibid, 363). The specific definitions of language we focus on relate to 'intercommunication distances', which have primarily found application in the study of bilateral trade flows (e.g. Melitz and Toubal, 2014) or of lexicographical bias in firm-level exports (Cheng et al., 2020). Although the presence of a *lingua franca* enables communication, it is the notion of common ethnicity and trust captured by intercommunication distance we adopt which makes such indices attractive proxies for culture (Ginsburgh and Weber, 2020).

History One strand of the empirical literature on 'the long arm of history' relates to the 'triangular' global trade system from the 15th century onwards, which connected manufactured goods from European colonisers, raw materials from the Americas, and slave labour from Sub-Saharan Africa (Nunn, 2009). These exploitative connections have been causally linked to prosperity in

[13] Existing research, reviewed in Spolaore and Wacziarg (2013), has focused on the (intergenerational) transmission of culture and also its effect on contract enforcement, fertility choice, regulation, etc.

Europe, underdevelopment in the Americas, and African stagnation. While in our setup of democratic regime change it is prohibitive to study the slave trade corner of this triangle,[14] broader notions of colonial experience (during the 'Columbian exchange' and the 'Scramble for Africa') can provide insights into the divergent effects of historical contact with Europe (Nunn, 2020).

Proxies Throughout our analysis, we adopt a range of proxies for geography ($\times$6), culture ($\times$3), and history, as well as data on legal origin to capture these different deep determinants.

For the disease environment aspect of geography, we use two datasets related to malaria: (i) from McCord et al. (2017, malpct) the percentage share of population at risk of malaria in 1965 and (ii) from Kiszewski et al. (2004, ME) malaria ecology, an "ecologically-based variable that is predictive of malaria risk" (Sachs, 2003, 7). We further adopt (iii) data on the historical prevalence of parasitic and infectious diseases (historical pathogen prevalence) from Murray and Schaller (2010, hdp_7) — the variable considering seven diseases has the best coverage. For the climate-related aspects of geography we adopt (i) a dummy variable for zero land area in the temperate climate zone constructed from Spolaore and Wacziarg (2013, kgatemp), and (ii) a dummy variable for 'some' land area in the tropical climate zone constructed from Nunn and Puga (2012, tropical), from where we also construct (iii) absolute average latitude (using lat).

For French legal origin we use a dummy from La Porta et al. (2008, legor_fr).

For culture, we use (i) data from Gorodnichenko and Roland (2017, distE_UK) relating to a measure of distance (from the UK, one of the world's most individualist countries) in terms of frequencies of blood types. For the language aspect of culture, we use (ii) data from Gurevich et al. (2021) who compiled the domestic and international common language (DICL) database. Our proxies capture the probability that two individuals picked at random from each pair of countries speak the same native language (cnl) and a population-weighted proximity measure based on 'linguistic trees' which categorise languages (lp). These are dyadic data for country pairs, and we compute the country-specific averages for the average common native language (cnl) index and average language similarity (lp) for country i across all other countries j.

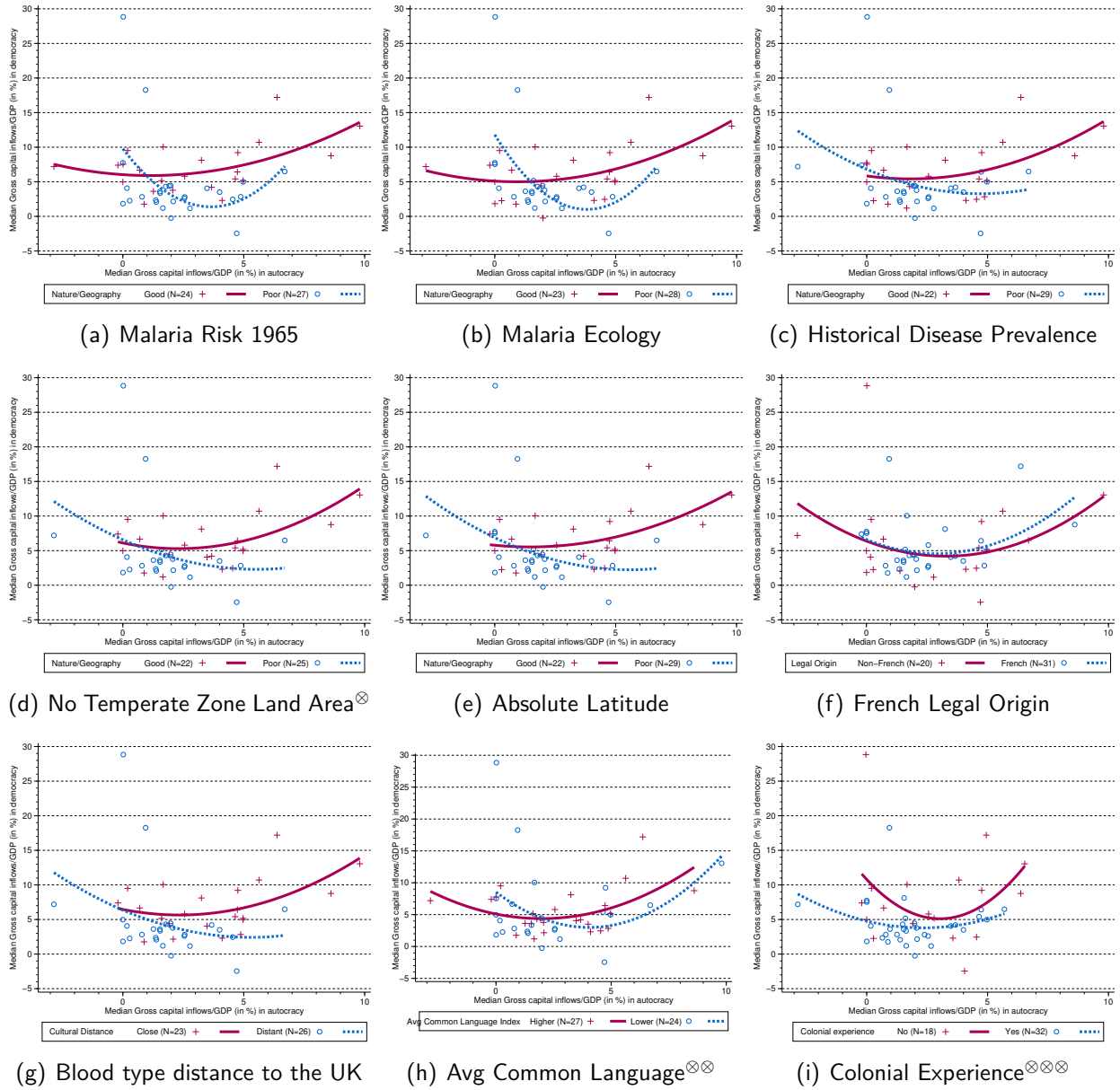
For aspects of history we adopt the Colonial Dates (COLDAT) dataset compiled by Becker (2019). Our indicator is simply whether a country experienced colonialism or not.

For all of the above: where we do not already indicate that the proxies are dummy variables we dichotomise continuous or categorical variables at the cross-country median.[15] Throughout our analysis and in the presentation of results a geography dummy value of 0 is for 'good' geography and 1 for 'poor' geography. Similarly for cultural and historical proxies. See Appendix Table A-2 for average pairwise correlations of proxies within and across deep determinants.

[14] The analysis of slavery is limited to 52 African countries, which in samples split by median slave export numbers would amount to only 8 countries with democratic regime change (definition: Liberal democracy), respectively.

[15] This is for the full sample which includes always-democracies, never-democracies and democratisers.

Figure 2: Patterns of Capital Inflows before/after regime change by Deep Determinant



(a) Malaria Risk 1965 (b) Malaria Ecology (c) Historical Disease Prevalence

(d) No Temperate Zone Land Area$^{\otimes}$ (e) Absolute Latitude (f) French Legal Origin

(g) Blood type distance to the UK (h) Avg Common Language$^{\otimes\otimes}$ (i) Colonial Experience$^{\otimes\otimes\otimes}$

Notes: We present scatters and quadratic regression lines for the relationship between median total capital inflows/GDP before (x-axis) and after democratic regime change (y-axis), distinguishing 'deep determinants' in each plot as proxies for geography, Legal Origin or culture. $\otimes$ and $\otimes\otimes$ indicate that we omit the plots for tropical land area and average language similarity, which are qualitatively identical to the temperate land zone and average common language index versions presented, respectively. $\otimes\otimes\otimes$ For ease of illustration we exclude a data marker in the sample with colonial experience.

3.2 The Uneven Effect of Democracy

In Figure 2 we provide first descriptive evidence that democracy has an uneven effect on capital inflows, and that geography appears to be one good candidate, albeit not the sole candidate, to explain the observed patterns. Each plot is for a proxy for deep determinants relating to geography, legal origin, culture, or history — the deep determinants literature and details of

the proxies we adopt are discussed in the following sections.[16] We plot the country-specific median capital inflow/GDP value during democracy (on the y-axis) against its median value during autocracy (on the x-axis) for two sets of countries: those with 'good' geography (or more individualistic/proximate culture, or non-French legal origin or no colonial experience) using dark pink markers and solid quadratic regression lines, and for countries with 'poor' geography (or more collectivist/distant culture, or French legal origin or colonial history) using navy markers and dashed quadratic regression lines.

The resulting patterns are quite similar across the geography proxies in panels (a) to (e): for similar levels of capital inflows during autocracy, regime change in 'good' geography countries on average leads to higher capital inflows than in 'poor' geography countries. Take the Malaria Risk proxy in panel (a): most navy markers are between 0% and 5% (both in terms of the x- and y-axis), whereas many dark pink markers between 0% and 5% on the x-axis (in autocracy) have post-regime change median inflows in excess of 5%. Equivalently, beyond a pre-regime change inflow of 1%, the fitted quadratic regression lines for 'good' geography countries is to the North of the 'bad' geography one and rising. The divergence between 'good' and 'bad' geography samples is particularly marked for the climate-related measures in panels (d) and (e). In contrast, using the same strategy but distinguishing countries by (French) legal origin in panel (f) yields virtually no differences between the two sets of country results. The proxy for culture based on blood type distance to the UK in panel (g), however, shows a similar deviation to the geography proxies. The measure for common language once again indicates no substantive deviation between the two country groups. Finally, the sample split by colonial experience in panel (i) is again aligned with the geographic splits, with lower regime change effects for colonised countries.

This preliminary evidence suggests that geography captures the differential effect that democracy has on capital inflows quite well, arguably better than alternative structural ('deep') determinants related to culture or legal origins.

4 Democracy and Capital Inflows: Heterogeneity

Moving from the descriptive evidence shown in Figure 2 to establishing a link between deep determinants and the differential effects of democratic regime change on capital inflows involves two distinct steps of analysis, as developed in detail in Section 4.1. In a first step, we need to provide a formal empirical approach to account for deep determinants *within our difference-in-differences framework*. We proceed as follows: for each proxy of the various deep determinants, we estimate the PCDID ATET for all countries which are deemed to have a 'poor' or 'unfavourable' deep determinant, adopting only countries with the same structural characteristic for the control sample. Similarly for the samples of countries with 'good' or 'favourable' deep determinants. Our

[16] We omit the plots for tropical land area and average language similarity, which respectively are qualitatively identical to the temperate land zone and average common language index versions presented.

results in Sections 4.2 and 4.3 demonstrate that geography (and colonial history) can explain differences in the democratic dividend, whereas legal origin and culture cannot. In Section 4.4 we provide some indication that these patterns may arise from the economic legacy of geography: compared with the alternative deep determinants, countries with 'poor' geography have limited export diversity (and hence higher commodity price volatility, lower economic complexity, and higher trade costs.

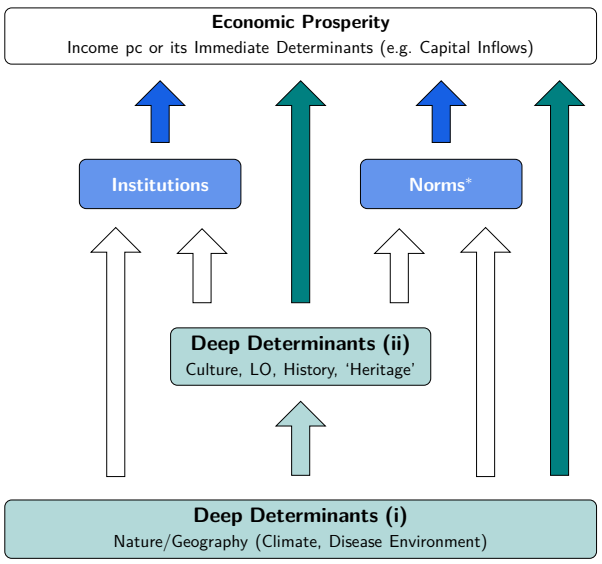
However, we cannot stop here, as we cannot just treat geography like all the other deep determinants. Geography is the *ultimate* deep determinant of comparative development, with the debate in the literature concerning whether most or even all of its effect is mediated by the differential culture, legal history or colonial history it may induce (e.g. Nunn and Wantchekon, 2011; Nunn and Puga, 2012). Thus, in a second step, we develop an approach to test if the uneven effect of democracy is the result of a 'direct' effect of geography, or whether this is the result of geography affecting legal origin, culture, and history, which then in turn create the differential patterns observed (an 'indirect' effect of geography). We do so in Section 5.

4.1 Accounting for Deep Determinants: Conceptual Development

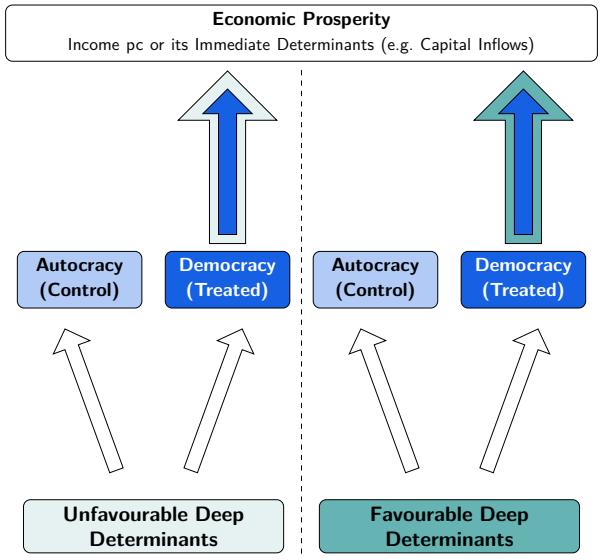
Figure 3 illustrates the endogeneities inherent in the relationship between deep determinants, institutions, and economic prosperity. Panel (a) shows the causal links between deep determinants, institutions (or norms or beliefs or trust or other factors), and economic development. The light green arrow indicates that some deep determinants may themselves be the outcome of geography, another deep determinant. The dark green arrows are for the direct effects of deep determinants on prosperity. The white arrows indicate that deep determinants affect institutions, norms, etc. The blue arrows represent the causal effects of institutions, norms, etc. on economic prosperity. The diagram highlights that accounting for deep determinants in the analysis of institutions on development is hard, because the quality of institutions itself is endogenous to deep determinants.

In panel (b) we focus on a 'bundle' of institutions (democratic regime change) and abstract from the split within deep determinants (see next paragraph), instead splitting them into favourable and unfavourable factors (note the difference in shading). We now have two spheres, those of favourable and unfavourable deep determinants, respectively (as indicated by the dashed line separating them). Deep determinants can affect whether or not countries become democracies or not, indicated by the white arrows. In our empirical strategy, the causal effect of democracy on prosperity (i) isolates the democratic dividend from the causal effect of deep determinants on the propensity of becoming a democracy, since we compare treated and control countries *with identical deep determinants*; and (ii) is (potentially) different for treated samples with favourable and unfavourable deep determinants, which we indicate by superimposing the blue democracy effect arrow on a green arrow with shading indicating favourable (darker) and unfavourable (lighter) deep determinants. Our empirical strategy hence identifies treatment heterogeneity in democratic

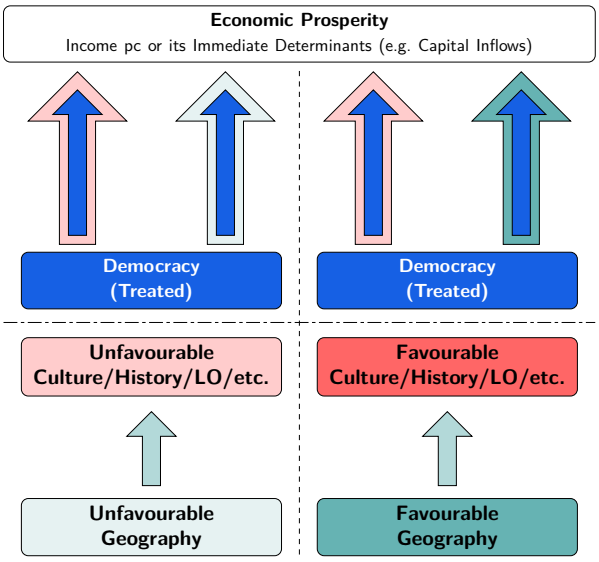
Figure 3: Problems of Econometric Identification and Empirical Strategy



(a) Deep Determinants, Institutions and Prosperity



(b) Treatment Effects by Deep Determinant



(c) Direct or Indirect Effect of Nature?

Notes: The diagrams present: the causal relations between deep determinants, institutions/norms, and economic prosperity in panel (a), * note that we merely add 'norms' as an illustration that other factors may be at play; our empirical strategy to identify the heterogeneous effect of democracy by deep determinant in panel (b); and our empirical strategy to provide evidence whether the proposed treatment heterogeneity by geography really masks treatment heterogeneity by culture, history, or legal origin which in turn are shaped by geography. See text for details.

regime change *by deep determinant*. This is the strategy we employ in Section 4.2, where we have six proxies for geography and five alternative specifications, such that we provide thirty ATET estimates for 'good' and 'poor' geography instead of a single arrow each like in the diagram (and similarly for other deep determinants).

Finally, in panel (c) we address the issue raised in panel (a), namely that culture, legal origin, and other deep determinants may themselves be the outcome of geography. Examples include instances of geography influencing history, such as in the context of the slave trade (Nunn, 2008; Nunn and Puga, 2012). In the diagram, we highlight that unfavourable (favourable) geography may cause unfavourable (favourable) alternative deep determinants (culture, history, etc.) and we use light and dark red shading for the latter. In addition to the split between favourable and unfavourable deep determinants (dashed vertical line), we now split the diagram horizontally (dash-dot line) as a shorthand to separate the conceptual problem in the bottom half from our proposed empirical solution in the top half. Our analysis can provide average treatment effect estimates for democratic regime change by geography (the blue arrows superimposed on green arrows with alternative shadings as in panel (b)), and we suggest comparing these to average estimates from the same analysis where we only consider treated countries with *unfavourable other deep determinants* (blue arrows superimposed on light-red shaded arrows).[17] This strategy exploits the unique property of the PCDID heterogeneous treatment effects estimator which yields estimates for each treated country, $\hat{\delta}_i$. Hence we can provide average treatment effects for distinct subsamples. For instance, for the 22 country estimates for regime change in the 'low absolute latitude' sample and 29 in the 'high absolute latitude' one, a subset of, respectively, 11 and 9 of these countries have French legal origin. Robust means for the 22 and 29 estimates represent the blue and green arrows in panel (c), robust means for the 11 and 9 the blue and light-red ones. The intuition for this comparison is that we would expect to see the treatment averages for the countries *with* French legal origins to be qualitatively *smaller/worse* than those for all countries (French and other legal origins) if legal origin was responsible for the observed differential patterns (indirect effect of nature), rather than geography (direct effect of nature). In Section 5 we adopt this strategy to provide evidence whether the strong observed patterns by geography are merely masking a mechanism whereby geography shaped legal origin, culture, and history.

4.2 Accounting for Deep Determinants: Main Results

We present ATET results in Appendix Table B-5 — these are only the results for the specification without any additional control variables, distinguished by geography.[18] Results for the same specification distinguished by alternative deep determinants are presented in Appendix Table B-6. Treated samples typically cover 25 to 35 countries, control samples are more modest in size. The

[17] Alternatively, we could only study those with a *favourable* deep determinant.

[18] In Appendix Table B-7 we report results for four more proxies related to geography: being landlocked, high UV radiation exposure, limited frost days and low suitability for agriculture — with the exception of the latter, the same patterns as discussed below prevail.

tables report Alpha test results for which a t-statistic in excess of 1.96 indicates the weak parallel trend assumption is violated, suggesting that the PCDID model may be misspecified. There are a very large number of estimates in these tables (120 in Table B-5 and 100 in Table B-6), even though we just present one specification (without controls) and we use the visual representation of the results in Figures 4 and 5 to highlight the general patterns.

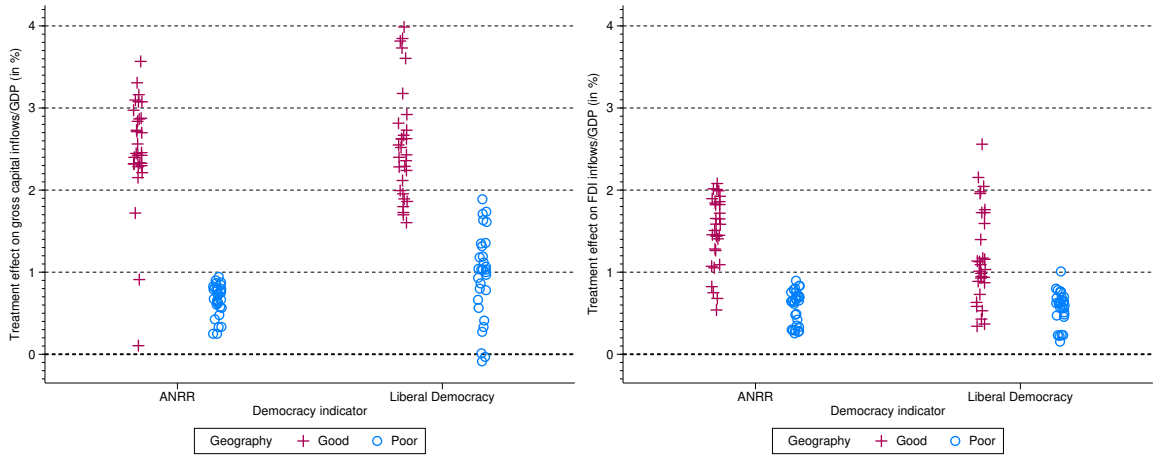
The left column of plots in each Figure is for gross capital inflows, the right column for FDI inflows; in each column the plot in panel (a) is for the specification without controls, that in (b) with exports/trade as additional control, and that in (c) with population growth, GDP growth, and exports/trade as controls. Each plot is organised by the definition of democracy (x-axis), either ANRR or liberal democracy, and markers signifying 'good' versus 'poor' deep determinants. Each marker indicates the estimated PCDID average treatment effect of democratic regime change on capital inflows: for 'good' (pink markers) vs 'poor' (blue markers) geography, or 'favourable' (navy markers) versus 'unfavourable' (orange markers) alternative deep determinants. Each result 'cloud' of markers in Figure 4 is made up of 30 estimates, since we have five alternative factor-augmentations for the PCDID and six proxies for geography. In Figure 5 we have 25 estimates in each result 'cloud'. These plots ignore the statistical significance of the ATET estimates and further do not indicate whether individual PCDID models satisfy the weak parallel trends test — we comment on these in broad brushes below (results in Appendix Tables B-1 and B-2).

Total Capital Inflows Almost all (98%) the estimates for 'good' geography (pink $+$) in the left column of Figure 4 are statistically significant at the 10% level, for 'poor' (blue o) geography just over three-quarters are. These causal effects of democracy on total capital inflows show distinct patterns which are visually striking: in the model without controls in panel (a), democratising countries with 'good' geography experience a 2-4 percentage point increase in capital inflows, whereas those with 'poor' geography see much more moderate effects, a 0-2 percentage point increase, if that. In the model with exports/trade as additional control in panel (b), these magnitudes are somewhat moderated, but the distinct pattern remains: 'good' geography results are substantially larger than those for the 'poor' geography samples. This pattern also does not change in the model with full controls. The sample mean of total capital inflows over GDP for the respective treated sample prior to democratic regime change (i.e. all years in autocracy) is between 2.5% and 3.3%, which indicates that our average treatment effects are economically large.

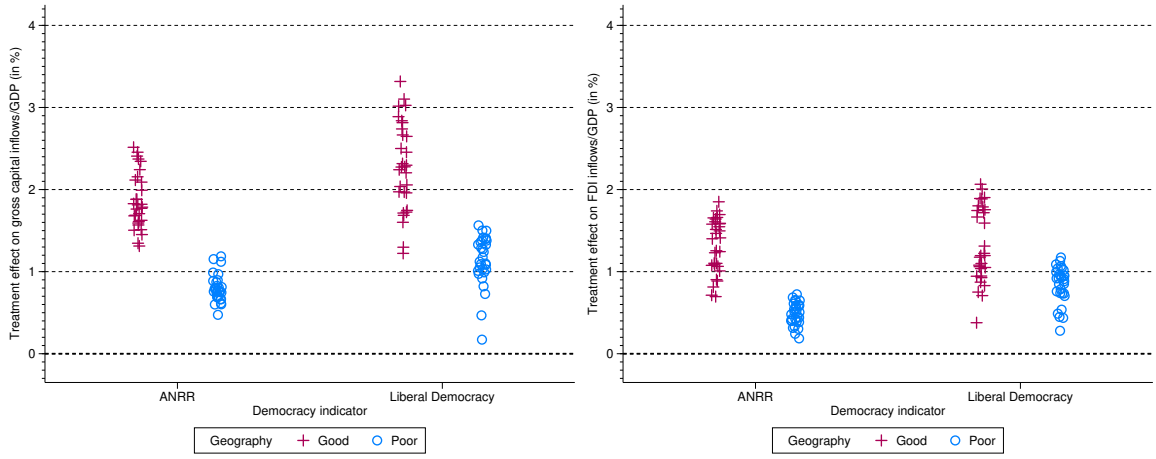
The distinction between results for 'good' (navy markers) and 'poor' (orange markers) *alternative* deep determinants (legal origin, culture, colonial history) — presented in Figure 5 — finds treatment effects much less systematically different, e.g. close vs distant culture (navy x and orange Δ) show similar 'spreads' of effect magnitudes.[19] The clear exception is the distinction by colonial experience (navy $\diamondsuit$ and orange $\square$), where ATETs for countries which did not experience

[19] One may suggest that French legal origin provides (counter-intuitive) patterns of more substantial democracy effects, yet these are much less clear than in the geography case, and furthermore undermined by the reversal of this conclusion in panel (c).

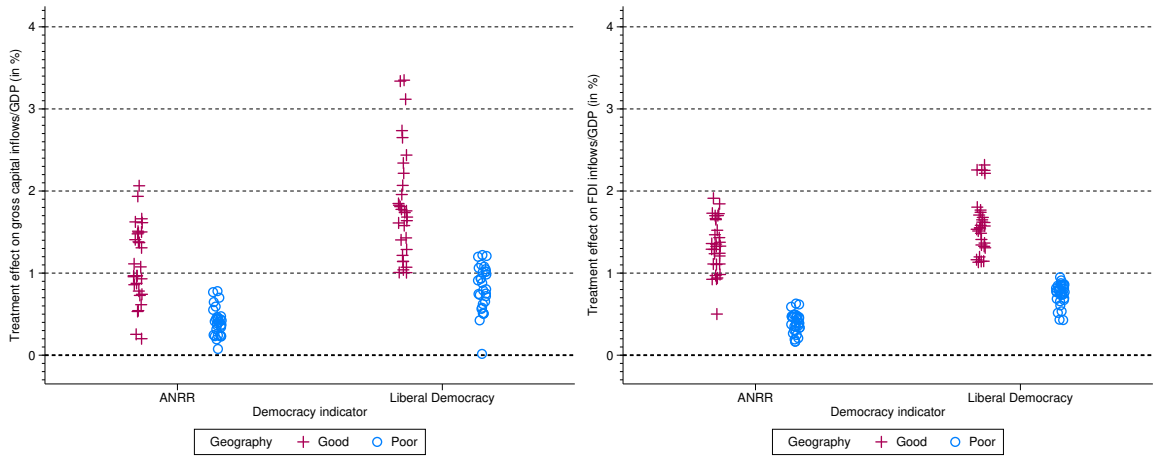
Figure 4: Democracy, Geography and Capital Inflows



(a) Total Non-Official Capital (left) and FDI (right) Inflows – no controls



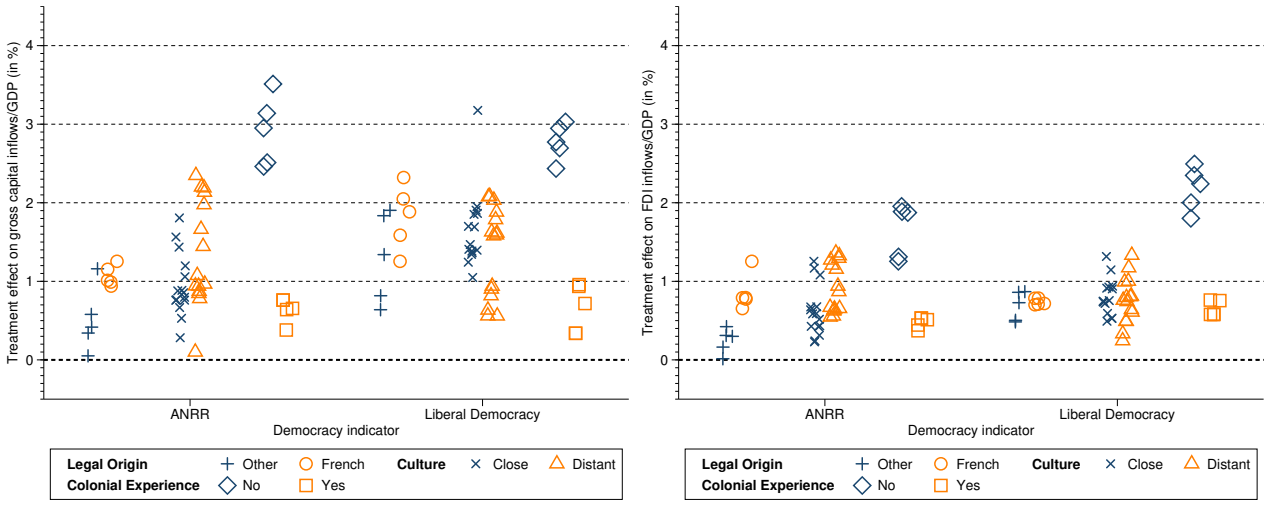
(b) Total Non-Official Capital (left) and FDI (right) Inflows – exports/trade as control



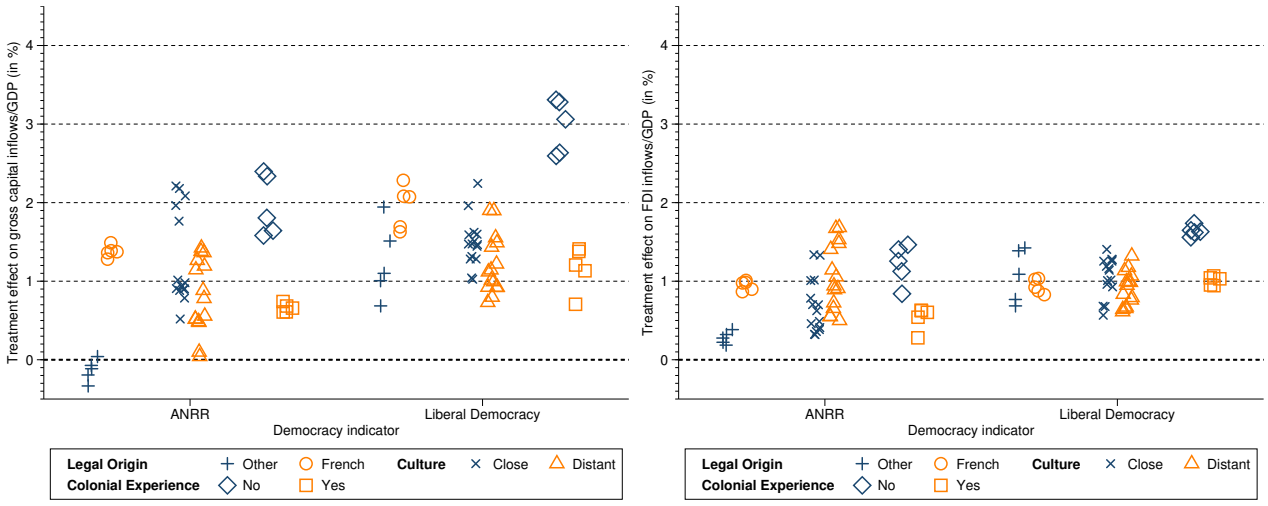
(c) Total Non-Official Capital (left) and FDI (right) Inflows – full controls (bad controls)

Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on capital inflows by geography (+ and o for good and poor geography, respectively). Each result 'cloud' (markers are randomly perturbed to aid visualisation) features 30 estimates: six proxies of good/bad geography and PCDID augmentations with 2 to 6 estimated factors. The plots ignore statistical significance or weak parallel trend tests (see Appendix Tables B-1 and B-2). Further tests (Appendix Table B-1) indicate exports/trade on its own is not a bad control, while the combination of exports/trade, population growth and GDP pc growth fails this test.

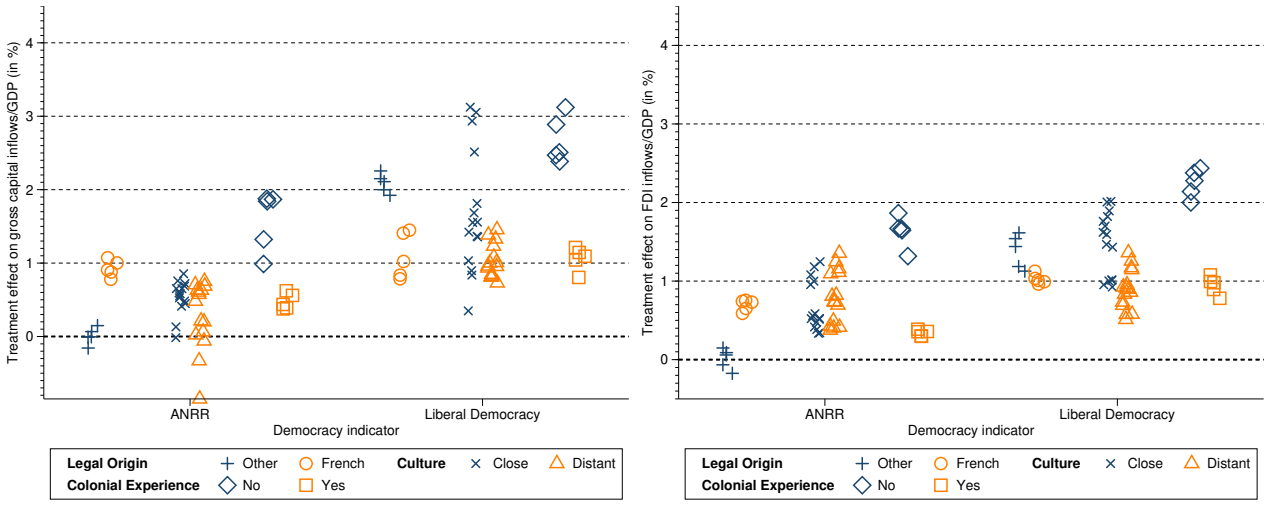
Figure 5: Democracy, Deep Determinants and Capital Inflows — Alternative Deep Determinants



(a) Total Non-Official Capital (left) and FDI (right) Inflows – no controls



(b) Total Non-Official Capital (left) and FDI (right) Inflows – exports/trade as control



(c) Total Non-Official Capital (left) and FDI (right) Inflows – full controls (bad controls)

Notes: We present ATET estimates as in Figure 4 for 'alternative deep determinants': French LO (+ and o), culture (x and △) and colonial experience (♢ and □). See notes to Figure 2 for other details.

colonialism are 2-3 times larger than those for former colonies. While there are some concerns regarding sample sizes,[20] it does not seem particularly controversial to suggest that countries with colonial experience suffer from structural weaknesses which make it difficult to realise the economic benefits from a significant improvement to institutional quality. Yet, the insight that treatment effects do vary systematically across geography and history is original to this paper.

In terms of diagnostics, using the LibDem (ANRR) democracy definition, around 19% (31%) of all models for geography and 30% (60%) of those for alternative deep determinants fail the weak parallel trend test in the analysis of gross capital flows.

FDI Inflows The right column of plots in Figure 4 visualises the ATET estimates for causal effects on FDI inflows. For this distinction by geography, the overwhelming majority of estimates are statistically significantly different from zero, and around 29% of specifications reject the weak parallel trend assumption. Compared with the results for total capital inflows the differences in the result patterns are less substantial but still marked: the ATET for countries with 'poor' geography is typically below 1 percentage point, while in countries with 'good' geography, the effect ranges from 0.25 to 2.5 percentage points. The economic effects of democratic regime change are again substantial (in relative terms even more so than for total capital inflows), given that the mean for FDI inflows over GDP is around 1.1-1.4%.

For the distinction by alternative deep determinants the difference in treatment effects is once again apparent in the results for colonial experience (see Figure 5),[21] especially in panel (a). The distinction between 'good' and 'poor' deep determinants for legal origin and culture yields virtually no differences in the distribution of the ATETs across the two samples.[22]

Overall, our results suggest that there are substantial and systemic differences in the effects of democratic regime change by geography but not by alternative deep determinants, except for colonial experience.

Ruling Out Mechanical Explanations Naturally, there would be concerns if 'treated' countries (i.e. democratisers) with one type of deep determinant (say, low absolute latitude) would have a significantly higher propensity to revert to autocracy than those of the other type (high absolute latitude): we would be comparing 'solid' and 'shaky' democracies. The average 'reversal' probability in treated samples is between 1 and 2.5 percent. Carrying out comparison-in-means tests between the treated samples of the two 'types' we find that those with 'favourable' deep deter-

[20] Among countries in the group with no colonial experience 18 or 19 experienced democratic regime change, but only 8-10 remained autocratic throughout the sample period — a limited control sample from which to construct the common factors for inclusion in the treatment regression. Having said that, all (half) of these models pass the Alpha test for weak parallel trends when we use the liberal democracy (ANRR) definition of regime change.

[21] Across all models the vast majority of effects are statistically significantly different from zero and 84% (52%) of LibDem (ANRR) models pass the weak parallel trend test.

[22] Legal origin indicates some systemic patterns for the ANRR definition of democracy, but this is not the case for the liberal democracy definition.

minants typically have lower propensity of reversal (1.3%), but the differences between these and samples for 'unfavourable' deep determinants (1.8%) are typically not statistically significant.[23]

Similarly, for the magnitudes of the treatment effects, it is important to check whether the length of time spent in democracy does not differ substantially across treatment samples of the two types, since otherwise a bigger boost to growth may simply be down to having spent many more years in democracy. The average number of sample years in democracy is between 15 and 23, depending on the definition of regime change. We find that samples for countries with 'favourable' deep determinants have an advantage, three additional years of treatment (19.1) compared with countries with 'unfavourable' deep determinants (16.1), though once again the difference is not typically statistically significant.[24]

Direct vs Indirect Effect of Democracy Our PCDID specifications allow for the addition of control variables with the econometric caveat that these may be deemed bad controls. There is an economic interpretation of this aspect: in the present context, we would be interested in understanding whether regime change *directly* affects capital inflows, or whether some or most of this effect is transmitted via improved growth performance after regime change (*indirect* effect), i.e. regime change $\Rightarrow$ economic growth $\Rightarrow$ capital inflows. Put differently, this speaks to the question whether capital inflows are a channel or a consequence of the democracy-growth nexus. We noted above that the inclusion of per capita GDP growth as an additional variable is rejected by our 'bad control' test in virtually all specifications.[25] Be that as it may, it is interesting to note that the result patterns — in panel (c) of Figures 4 and 5, respectively — are qualitatively very similar to those of the specifications without controls. With the important caveat of a misspecified PCDID regression, this would suggest regime change has a direct effect on capital inflows, rather than only an indirect effect via improved economic growth.

4.3 Robustness Checks and Extensions

Alternative Definition of Capital Flows Some researchers in the capital flow surge/bonanza literature (see discussion in Caballero, 2016) maintain that the capital flow to GDP ratio is unsuitable for analysis given potentially differential dynamics/trends of the numerator and denominator, suggesting the use of capital flow *per capita* instead. We repeat the analysis using this alternative definition of the dependent variable[26] and present the findings in Appendix Figure B-1. Results

[23] Results by deep determinant and definition of democracy (also for polyarchy and the liberal component) are presented in Appendix Table B-3. These are not the reversal propensities, but the p-values associated with 'difference-in-means' tests between 'good' and 'poor' deep determinant countries, and a low (high) p-value suggests countries do (not) have a differential propensity to revert to autocracy. The average reversal propensities reported above are for ANRR and LibDem regime change definitions only.

[24] Results by deep determinant and definition of democracy are again presented in Appendix Table B-3. These are p-values associated with 'difference-in-means' tests between 'good' and 'poor' deep determinant countries. The average years in democracy reported above are for ANRR and LibDem regime change definitions only.

[25] This is the case whether GDP pc growth is added to other controls or on its own.

[26] We adopt inverse hyperbolic sine transformations for the per capita total capital and FDI inflow measures.

are broadly consistent with those using capital flows scaled by GDP. However, the vast majority of specifications presented *fail* the weak parallel trend test (83% and 63% in the total and FDI flow models, respectively), suggesting that these PCDID models are misspecified.

We use gross capital inflows in our main analysis since net flow dynamics may be driven by inflows or outflows, which in turn may have different underlying drivers (Rothenberg and Warnock, 2011). Nevertheless, in Appendix Figure B-3 we present results for net total capital inflows and net FDI inflows. The patterns here are weaker than in our main analysis, but the diagnostic tests again indicate that the empirical equations are misspecified in the majority of models (67% of net total inflow and 58% of net FDI inflow models fail the Alpha test).

Alternative Definitions of Democracy Our dichotomised regime change indicators for liberal democracy (and in robustness checks its building blocks, polyarchy and the liberal component — see below) are constructed using the full sample mean as the threshold. The congruence of patterns of results with those based on the Acemoglu et al. (2019, ANRR) definition are an indication that we successfully capture a significant step in the institutional development of our sample countries. Nevertheless, the adopted threshold is arbitrary and to check the robustness of our findings we provide alternative versions where we take the mean plus $1/4$ or $1/2$ of the standard deviation for the V-Dem index, providing a 'tighter' definition of democracy. This substantially reduces the sample size of treated countries: reductions in the number of countries of 20% and 39% for total and FDI inflows, respectively, and of a similar magnitude in terms of treated sample *observations*.[27] Despite this caveat, results presented in Appendix Figure B-2 for geography and alternative deep determinants are qualitatively similar to those we present above using the mean index cut-off, particularly so for the FDI inflow analysis.

Building Blocks of Liberal Democracy Our analysis adopts two data proxies (ANRR, LibDem) for a concept of liberal democracy which encompasses (i) electoral democracy (polyarchy), and (ii) the rule of law combined with executive constraints. While it is difficult to generalise, political scientists frequently favour minimal definitions of democracy linked to polyarchy (following Dahl, 1971, 2000), whereas research in economics has typically focused on the rule of law (including property rights) and constraints on the judicial and legislative executive (associated with Douglass North: North, 1981; North and Weingast, 1989). The nature of the V-Dem data enables us to separate these two aspects, with the latter referred to as the 'liberal component' — results for total capital and FDI inflows are presented in Appendix Figure B-4. Across all specifications we can observe a clear pattern whereby treatment effects of regime change are similar in magnitude across the two samples of countries with 'good' and 'poor' geography when we consider polyarchy. In contrast, the treatment effects are substantially larger in good geography countries in the

[27] Using Liberal Democracy (cut-offs 0.41 for the mean, 0.48 for mean$+1/4$ SD, and 0.55 for mean$+1/2$ SD) sample size in the regressions using 'absolute latitude' as proxy for geography drops from 51 (22 and 29 'good' and 'poor' geography countries, respectively) to 41 (20 and 21) and 31 (16 and 15).

analysis of the liberal component. Distinction instead by culture, history or legal origins again offers no discernible differences in treatment effect size, with the notable exception of colonial experience (not separately highlighted). Capital flows appear to react differentially (by geography) to substantial improvements in the liberal component, whereas change in institutions related to electoral sanction have a uniform effect. It would appear that the financial flows pouring into countries experiencing regime change are differentially responsive to the economic institutions and executive constraints rather than the political institutions represented by electoral democracy.

4.4 The economic legacy of deep determinants

In this section we illustrate that economies with poor geography suffer from disadvantageous 'structural' characteristics, related to export concentration, aggregate commodity price volatility, trade costs and productive complexity. In all cases, we compare the differences based on 'good' and 'poor' geography to those when we use proxies for legal origin, culture or colonial history to split the sample. All analysis is limited to countries which experienced democratic regime change, adopting the two definitions for democracy we use throughout. Different definitions of democratic regime change affect the makeup of the treated country sample, hence the distinction.

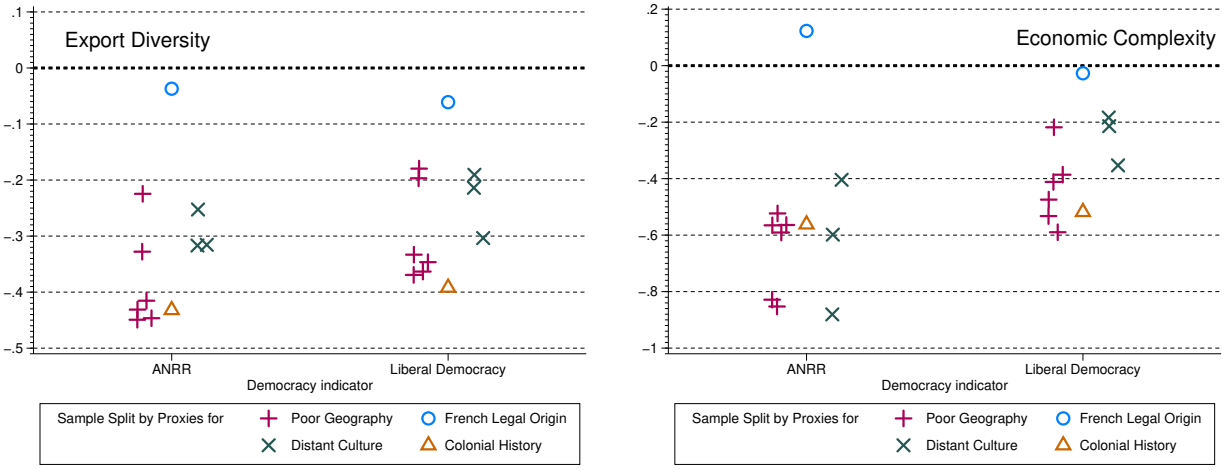
4.4.1 Diversity and Quality of Exports

The dominant paradigm for economic development in the second half of the twentieth century places significant emphasis on trade, predominantly labour-intensive manufactured goods for export (such as apparel) to (i) overcome the limits set by domestic market size, (ii) exploit low labour costs, and (iii) initiate a process of moving up the value chain and/or diversifying into higher value-added products. Studying the diversity and quality of exports can provide insights into the scope for structural transformation and the potential for countries to reap the benefits from diversification (Hausmann et al., 2007; Henn et al., 2020).

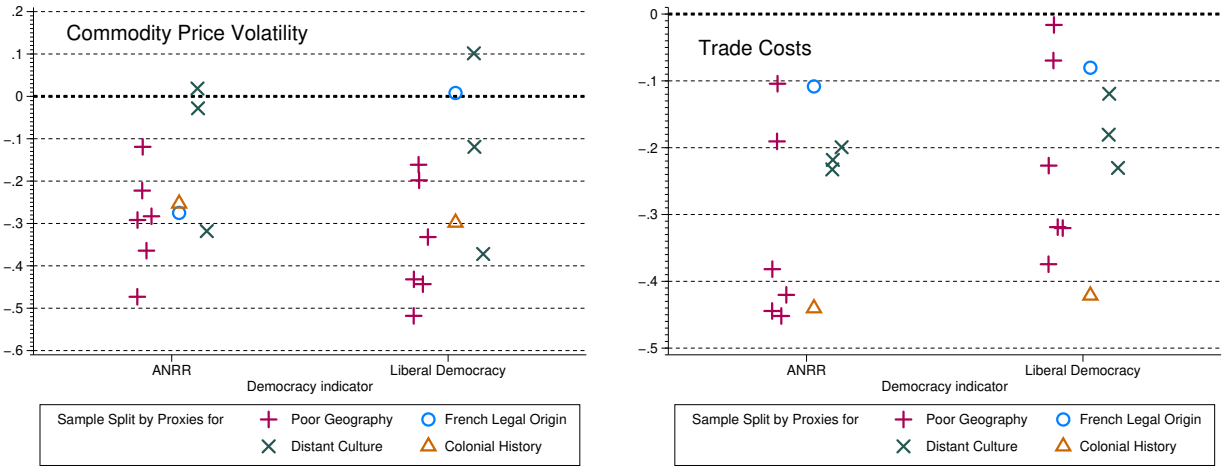
We adopt data from an IMF database on 'export diversification and quality' covering 1962-2014 (Henn et al., 2020), which is available for all our sample countries. The Theil index we use combines the concentration in the number of export products by a country and the concentration in its export volumes across products actively exported. A *higher* value for this index marks out a country with a *lower* level of diversification.

We compute the difference in the level of export diversification for countries with good and poor geography, along with a formal t-test for this difference for each of our six proxies for geography and similarly for the other proxies of deep determinants — in the vast majority of mean differences studied the t-tests and associated p-values indicate that these differences are statistically significant. Results for mean differences are presented in the left plot of panel (a), Figure 6. Across all definitions of democratic regime change, we see that democratising countries with 'poor' geography have 20-40% worse (lower) export diversity than their 'good' geography

Figure 6: How Deep Determinants Relate to Economic Structure



(a) Relative Export Diversity (left) and Economic Complexity — negative = lower/worse



(b) Relative Aggregate Commodity Price Volatility (left) and Trade Cost — negative = higher/worse

Notes: We present mean differences for export diversity, economic complexity, aggregate commodity price volatility and trade costs between the samples with 'good' versus 'poor' deep determinants of comparative development. Interpretation: a result of -0.2 indicates that the measure in question, e.g. export diversity in panel (a), is 20% worse in countries with 'poor' relative to those with 'good' deep determinants. The definition of democracy determines the sample size — we only consider countries which experienced democratic regime change (treated sample).

peers. For legal origin the differences are negligible, whereas for our cultural proxies they are largely smaller than those for geography. For the comparison based on colonial history we see substantially worse diversity.

4.4.2 Economic Complexity

Continuing with the notion of product 'sophistication' we hypothesise that the narrow(er) range of products produced for export in 'poor' geography countries is furthermore of lower complexity.

We take data on economic complexity from Hidalgo and Hausmann (2009), who provide

rankings (best to worst) across 133 economies in the level of complexity on the basis of the HS (Harmonized System, 1992) product classification. There is a caveat for these data since the series only start in 1995, and some of the sample sizes for 'treated countries' in 'good' geography locations only feature around 200 observations — these results should be taken with a grain of salt. A higher number implies a lower rank, and hence worse (lower) complexity.

Results for mean differences by deep determinant are presented in the right plot of panel (a), Figure 6. The patterns match those of the export diversity analysis above: sample splits by geography, culture or colonial history show large differences (lower complexity for 'poor' geography, 'distant' culture, or countries which experienced colonialism), while those for legal origin are comparatively small.

4.4.3 Aggregate Commodity Price Volatility

The analysis of export concentration and economic complexity focused on the basket of goods produced and exported by countries with different deep determinants. But what if, through luck or foresight, countries managed to 'pick winners' for their export baskets, goods with advantageous terms of trade and low price volatility? We investigate the economic uncertainty of the basket of primary commodities produced and exported by countries: we ask whether the primary goods exported by countries with 'poor' geography are subject to greater exogenous price movements than those with 'good' geography (and similarly for the other deep determinants).

For primary commodity price (PCP) volatility we use monthly data from Gruss and Kebhaj (2019), who employ 1962-2018 average net export/GDP weights to aggregate 44 global primary commodity prices from the *IMF Primary Commodity Price Database*: the variation captured hence relates to windfall gains and losses due to changes in exogenous world prices. Primary commodity price shocks are defined as the first difference of the monthly PCP index, $\Delta PCP_{it\tau} = PCP_{it\tau} - PCP_{it,\tau-1}$ for month τ of year t in country i. We construct a time-varying measure of PCP volatility following Bleaney and Greenaway (2001): the conditional volatility $\sigma^2_{ACP,it\tau}$ is predicted from a GARCH(1,1) model of the monthly data for 1975-2015 using a regression of the PCP shocks, $\Delta PCP_{it\tau}$, on a constant term. We convert monthly estimates to annual frequency by averaging monthly volatility within each year (see Eberhardt and Presbitero, 2021).

Results for mean volatility differences by deep determinant are presented in the left plot of Figure 6, panel (b). The patterns match those discussed earlier, although in this instance volatility is frequently substantially higher for the geography split than when we use colonial history.

4.4.4 Trade costs

Geography in the context of trade is traditionally interpreted as distance to or remoteness from large markets. We laid out above that the trade gravity literature has a close relation in the capital flow literature (Head and Ries, 2008; Lane and Milesi-Ferretti, 2008; Pellegrino et al., 2021) and

now investigate whether our geography proxies (and those for alternative deep determinants) correlate with goods trade costs.

We compute trade costs following the methodology introduced in Novy (2013), which derives bilateral trade costs from a structural gravity model, using annual bilateral goods trade data from IMF DOTS and GDP data from the World Bank WDI. We follow Milner and McGowan (2013) in creating country-specific time-varying trade cost averages from these bilateral estimates.

Results for trade cost differences by deep determinant are presented in the right plot of Figure 6, panel (b). The patterns match those discussed earlier, with sample splits by geography and, to a significantly lesser extent, culture associated with very substantial mean differences. Colonial history has the worst implications for trade costs, while French legal origin now has a uniformly negative (albeit small) association with trade costs.

4.4.5 Geography vs Alternative Channels

In summary, investigating four channels through which deep determinants can correlate with economic structure we find that across the board the geographic explanation is consistently associated with more substantial differences between country groups ('good', 'poor') than any of the alternative deep determinants offered in the literature.

5 Deconstructing Geography: Geographic Patterns for Historical or Cultural Reasons?

In our treatment analysis we have emphasised that the causal effect we identify is for democratic regime change on capital inflows using different samples, but that the striking patterns we reveal are mere correlations between countries with 'good'/'poor' geography and higher/lower inflows. *Why* do we see these patterns emerging? In this Section we carry out additional analysis to speak to this question. We investigate whether the strong geographical patterns we detect may really be due to alternative deep determinants which themselves were shaped by geography: an indirect effect of geography, rather than a direct one.

We know that geography can affect present-day economic prosperity in direct and indirect ways, the latter by establishing specific culture(s) or leading to specific historical experiences such as colonialism or slave exports. One of the most prominent examples of 'geography via history' is the study by Nunn and Puga (2012), who demonstrate the negative direct effect of one geographic feature, ruggedness, and its positive indirect effect with historically fewer slave exports from more rugged African countries. On balance, they conclude, history (indirect effect) accounts for about two thirds and their specific geographic feature (direct effect) for only one third of the income differences across countries. We cannot make such a detailed quantitative decomposition, but in

the following, we study the deep determinants for legal origin, culture, and history to get closer to an answer to the question *why* the geographic patterns we revealed above persist.

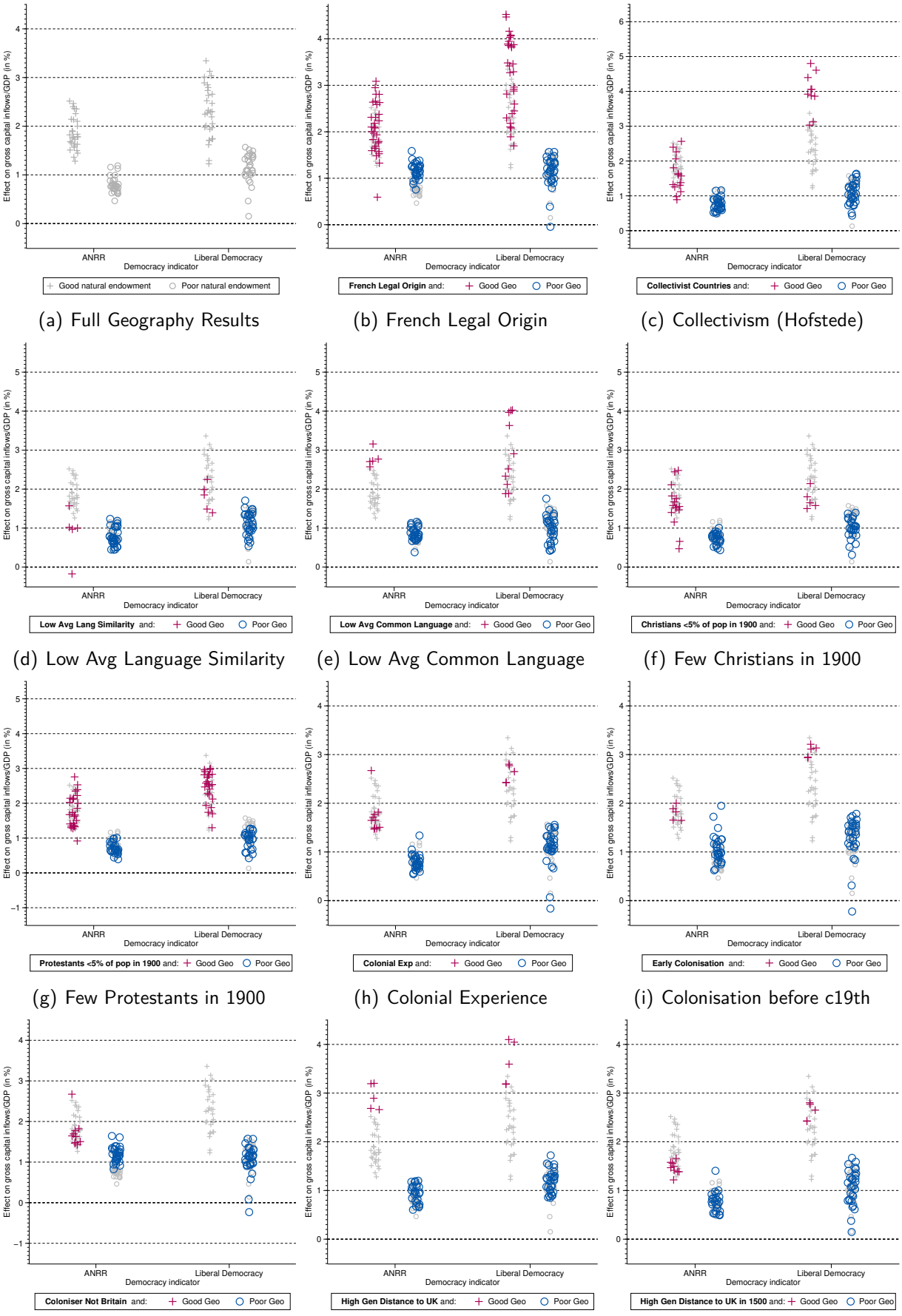
We take the country-specific treatment estimates (ITET, $\hat{\delta}_i$) underlying our main ATET results by geography presented in Figure 4 (specifically, the preferred specification including exports/trade as additional control) and compute robust mean effects for specific sub-samples of countries *within these geographical groups*. These sub-samples always represent countries with other 'unfavourable' deep determinants. Panel (a) of Figure 7 (this time in gray) is a reminder of our above results for total capital flows by definition of democracy (ANRR, liberal democracy) and geography (good, poor). In each of the other plots presented in the Figure, we superimpose (in colour) the robust mean effects of each specification (up to six proxies for geography times five different factor augmentations) for all countries *within these treated samples* which have the unfavourable deep determinant indicated. For instance, in panel (b) we compute the robust mean effects for all countries by geography which have French legal origin: of the 20 to 26 countries in the samples that make up each of the 30 ANRR 'good' geography estimates/markers, 11 to 16 are for countries with French legal origin and we compute the robust mean estimates for the latter subgroups (in dark pink +s) within each specification. We proceed analogously for the 'poor' geography sample (blue o).

The intuition for this exercise is that if unfavourable French legal origin is a strong 'drag' on capital flows, even following democratic regime change, then we would expect that the effect for treated countries that have French legal origin would be *lower* in magnitude compared with the full sample case including countries with French as well as other, more favourable, legal origins. If results are qualitatively unchanged or better in terms of magnitudes than in the full sample benchmark (in gray), then this is evidence against the hypothesis that the widely-suggested negative effect of French legal origin works *through geography* in the present context.

Since we are no longer bound by having sufficiently large country samples in the treatment *and* control groups for a 'good' and 'poor' deep determinant, we can expand our set of proxies somewhat. Note, however, that we regularly run into small sample problems in the 'good' geography subsamples with unfavourable deep determinants related to culture or history. For instance, there are no countries which experienced slave exportation but count as having 'good' geography. In case of cultural distance to the UK (from Gorodnichenko and Roland, 2017) we have at most six treated countries with high cultural distance but 'good' geography. In order to make this analysis suitably robust to outliers in small samples, we require that a sub-sample average is estimated with an M-estimator (Rousseeuw, 1987) from at least nine country estimates. Details of the number of treated countries in the reduced samples are provided in Appendix Table C-1. A robustness analysis requiring only a minimum of seven country estimates is provided in Figures C-1 and C-2.

Focusing on culture, we now also study (c) Hofstede's index for individualism (from Gorodnichenko and Roland, 2017) and label countries with below median index value as 'collectivist';

Figure 7: Alternative Deep Determinants 'within' Geography – gross capital inflows



(a) Full Geography Results (b) French Legal Origin (c) Collectivism (Hofstede)

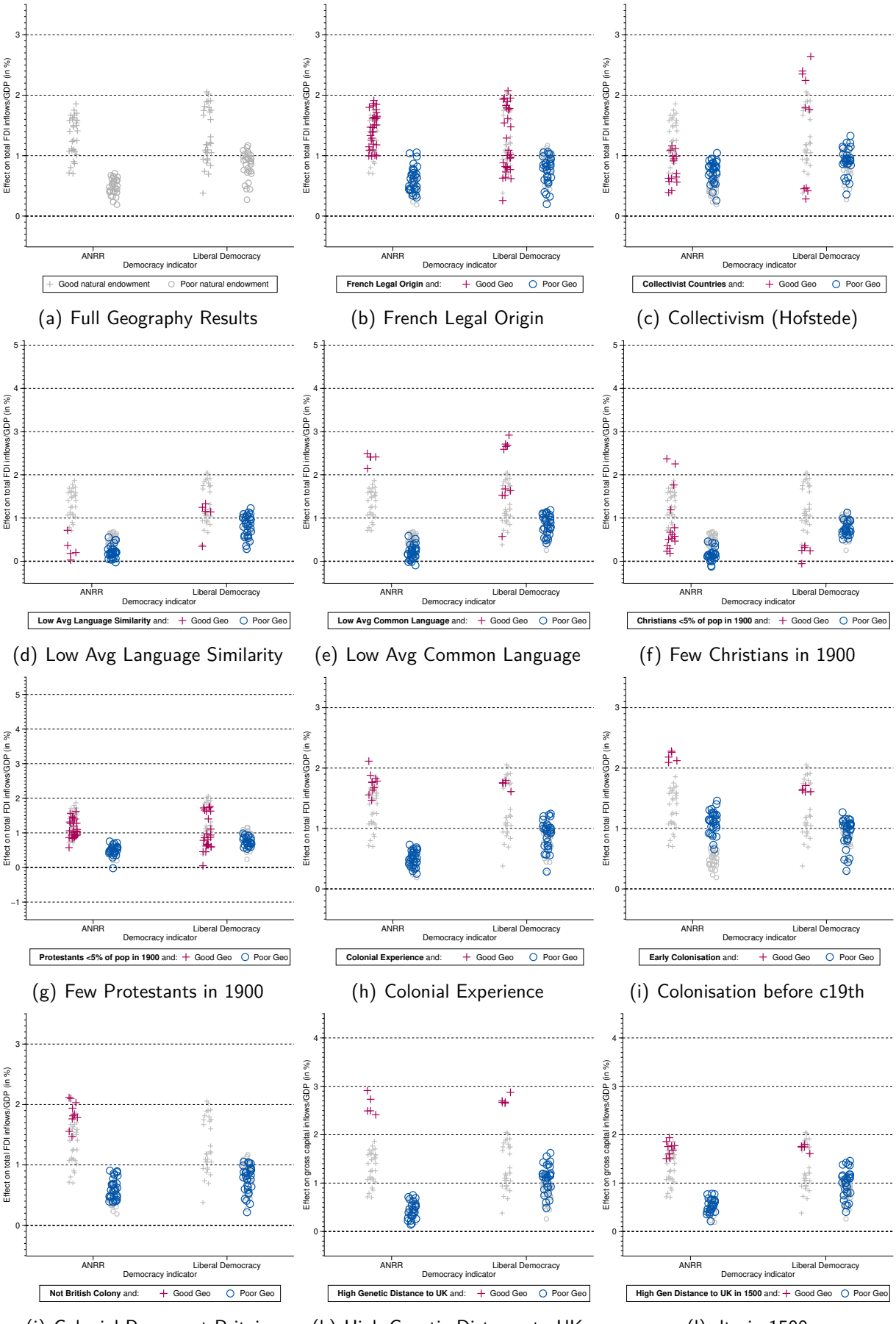
(d) Low Avg Language Similarity (e) Low Avg Common Language (f) Few Christians in 1900

(g) Few Protestants in 1900 (h) Colonial Experience (i) Colonisation before c19th

(j) Colonial Power not Britain (k) High Genetic Distance to UK (l) dto in 1500

Notes: The plots present robust ATET estimates for the causal effect of democracy on capital inflows by geography (in grey, as in Figure 4) and by alternative deep determinants (in colour) *within* these geographical groupings.

Figure 8: Alternative Deep Determinants 'within' Geography – FDI inflows



(a) Full Geography Results (b) French Legal Origin (c) Collectivism (Hofstede)

(d) Low Avg Language Similarity (e) Low Avg Common Language (f) Few Christians in 1900

(g) Few Protestants in 1900 (h) Colonial Experience (i) Colonisation before c19th

(j) Colonial Power not Britain (k) High Genetic Distance to UK (l) dto in 1500

Notes: The plots present robust ATET estimates for the causal effect of democracy on FDI inflows by geography (in grey, as in Figure 4) and by alternative deep determinants (in colour) *within* these geographical groupings.

we use our previous proxies for distant culture based on (d) language similarity, and (e) common language use. We now can draw on data on religious tradition and investigate countries with historically low population shares (<5%) that are Christian (respectively Protestant) in (f) and (g), constructed from Brown and James (2019) data for the year 1900.

Panels (h) to (j) focus on history, namely colonial experience. In addition to the proxies for colonial experience in (h) we focus on 'early colonialisation' (before the 19th century) in (i), and countries where Britain was *not* the (dominant) colonial power[28] in (j) — all measures are constructed from data in Becker (2019). Finally, we investigate whether genetic distance to the UK, present or historical (from Spolaore and Wacziarg, 2013), can provide additional insights. These transcend culture or history and could be referred to as European 'heritage'.

We present results for gross capital flows and FDI flows in Figures 7 and 8, respectively. Estimates for French Legal Origin in panel (b) are larger or (in the FDI case) qualitatively identical to the full sample results and hence a detrimental effect of geography via French legal origin is not confirmed in the data. Looking at collectivism as a proxy for culture in (c), we again see either higher treatment effects or broadly similar effects to those in the full sample results by geography, although results are weaker for FDI flows. Results for language similarity provide only few estimates, whereas average common language has a much richer set. Although results are at times weaker, we would still suggest that 'good' geography estimates in panels (d) and (e) of the two Figures are typically larger than 'poor' geography ones. The same holds if we proxy culture using religion, a low population share (<5%) in 1900 of Christians or of Protestants only — panels (f) and (g).[29] Results in panels (h) to (j) for various proxies of colonial experience in 'good' geography samples show a tendency for treatment effects to be at least similar if not higher than for the full sample case. While we found strong evidence above that countries with colonial experience have substantially lower treatment effects, this analysis suggests that our stark geographic patterns are not distorted by an association between 'poor' geography and colonial history. Finally, an investigation of genetic dissimilarity to the UK in panels (k) and (l), the latter for data from 1500, suggests that our geography-based results are robust to this distinction.

We reiterate that all of the results presented for the 'good' geography subsamples (the individual + markers), with the exception of French legal origin, 'few Protestants' and two of the colonial heritage results for the ANRR definition, are based on averages for just nine or ten country estimates, and hence should be taken with a grain of salt. This caveat aside, the narrative of a clear distinction in the economic consequences of democratic regime change along geographic lines we have developed in our earlier analysis does not obviously disappear when we allow for geography to affect outcomes by shaping legal origin, culture, colonial experience, or genetic makeup of the

[28] Existing work argues that British colonial rule is linked to more institutional development (Paine, 2019). This does not automatically extend to economic prosperity, although anecdotally such a link is frequently made.

[29] Research by Stulz and Williamson (2003) on financial development suggests Catholic countries extend fewer creditor rights than Protestant ones, suggesting the latter definition may be more suitable.

population.[30] This is an important insight: we have not found any strong evidence for an indirect effect of geography via these alternative channels.

6 Concluding remarks

Why capital flows to some countries but not others has long puzzled economists, until improvements in the quality of institutions were motivated and empirically confirmed as one important factor. In this paper, we have connected this literature with the recent work on democracy and growth, asking whether the democratic dividend observed in the latter literature can be isolated in the patterns of capital inflows as well, one of a range of plausible transmission channels for improved economic prosperity following a shift from autocracy to democracy. We begin by establishing that a significant step change in the quality of institutions (democratic regime change) unequivocally causes higher rates of capital inflows (total or FDI inflows). These average effects are economically large and robust to alternative definitions of democratic regime change.

Our point of departure from this combination of democracy and capital flows is that we argue for strong heterogeneity in the relationship *across countries*. Studying and identifying the underlying causes that shape this heterogeneity is important because policymakers and the populace alike may otherwise have unrealistic expectations of the economic effects of regime change.

We motivate and empirically demonstrate across a range of specifications and robustness checks that geography (proxied by measures of climate and disease environment) appears to capture the differential patterns across countries well, much better than alternative structural ('deep') determinants related to culture or legal origins. In countries with favourable geography (temperate climate, low disease environment) democratic regime change gives a substantial boost to total capital inflows (especially inflows of FDI), whereas when geographic endowment is comparatively worse (tropical climate, higher disease environment) the effect is substantially lower. The exception when distinguishing alternative deep determinants is colonial experience (history), which leads to dramatically lower capital flow effects upon regime change.

Why does democracy aid prosperity in some but not other countries? Geography represents a structural determinant of the magnitude of capital inflows which dominates and hence partially eradicates the benefits of significant institutional change: our definition of geography (climate, disease environment) correlates with the concentration of the export basket, the complexity of production, goods trade costs, and the volatility of aggregate commodity terms of trade of a country — all these represent risk factors which make 'poor' geography countries disproportionally less attractive for foreign capital inflows. While democracy causes economic prosperity, including higher capital inflows, it does so unequally, and our research suggests that the resulting patterns strongly correlate with geographic characteristics.

[30] It bears reminding that we find in Section 4 that colonial experience strongly correlates with poor economic returns (capital/FDI inflows) to democratic regime change.

Our final consideration is for the character of the relationship between unfavourable geography and (in relative terms) worse economic performance after democratic regime change. Is it 'nature' itself which leads to these outcomes (a direct effect), or does 'nature' affect other deep determinants which in turn lead to the observed patterns (indirect effect)? Although we cannot apportion relative contributions, and are also not able to make any claims for causality, we provide analysis which studies whether alternative unfavourable deep determinants can provide some additional insights related to this question. Adopting a host of unfavourable characteristics, we find that 'good' geography countries often fare even better in terms of the magnitude of effects for capital inflows (e.g. French legal origin, collectivism, few Protestants), and on the whole we see no clear evidence for cultural, legal origin, historical, or genetic factors to be the real (indirect) driver of the strong geographic patterns we reveal.

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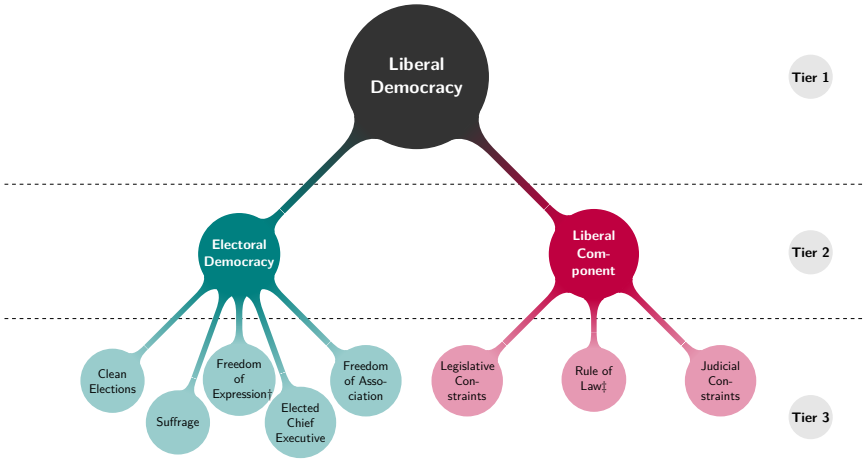
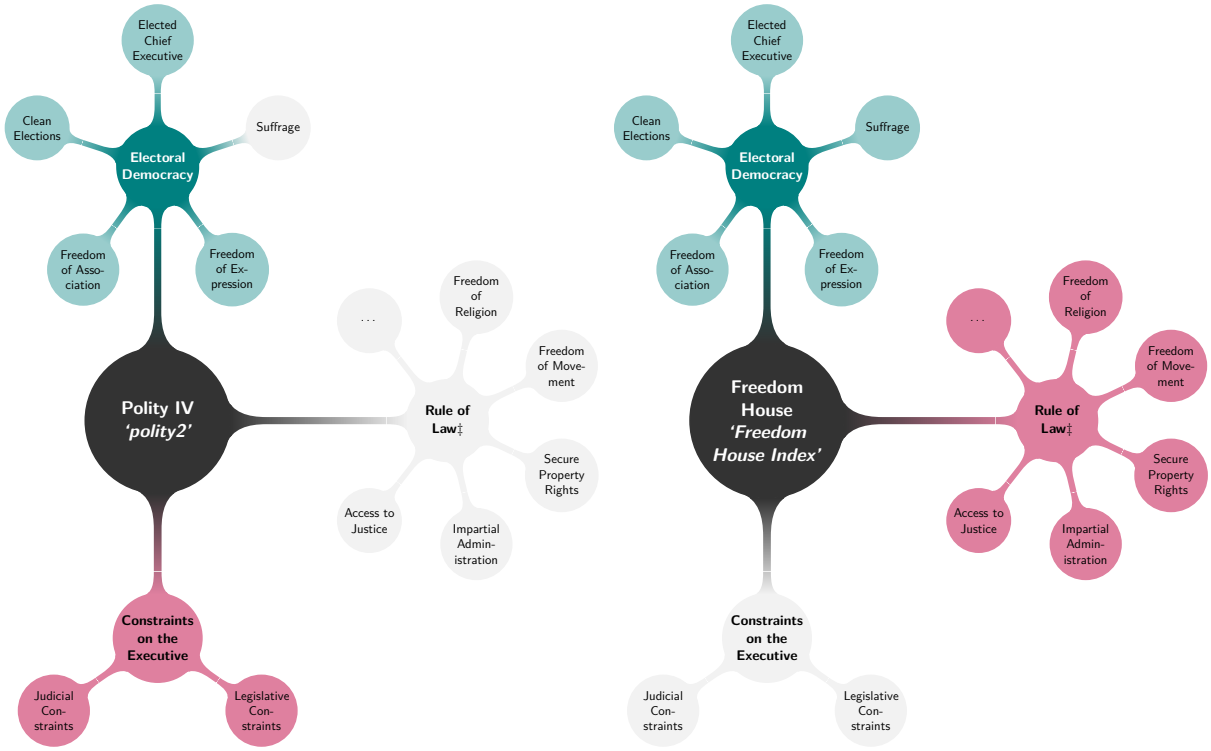
Online Appendix – Not Intended for Publication

A Data, Sample Makeup and Descriptives

Figure A-1: Definitions of Democratic Institutions



(a) V-Dem Definition of Liberal Democracy and its Components



(b) The indices (Polity, FHI) combined in the ANRR definition of Democracy

Notes: The images present (a) the V-Dem conceptualisation of liberal democracy, and (b) an attempt at integrating the Polity IV 'polity2' and Freedom House FHI into the V-Dem framework. The lower panel provides greater distinction within the 'Rule of Law' set of institutions for reference. Institutions, concepts and practices shaded in light gray are *not* covered by the index in question. † This includes 'alternative sources of information'. ‡ In its entirety this component covers 'Individual Liberties and Equality before the Law.'

Capital Flow Data — Detailed Definitions

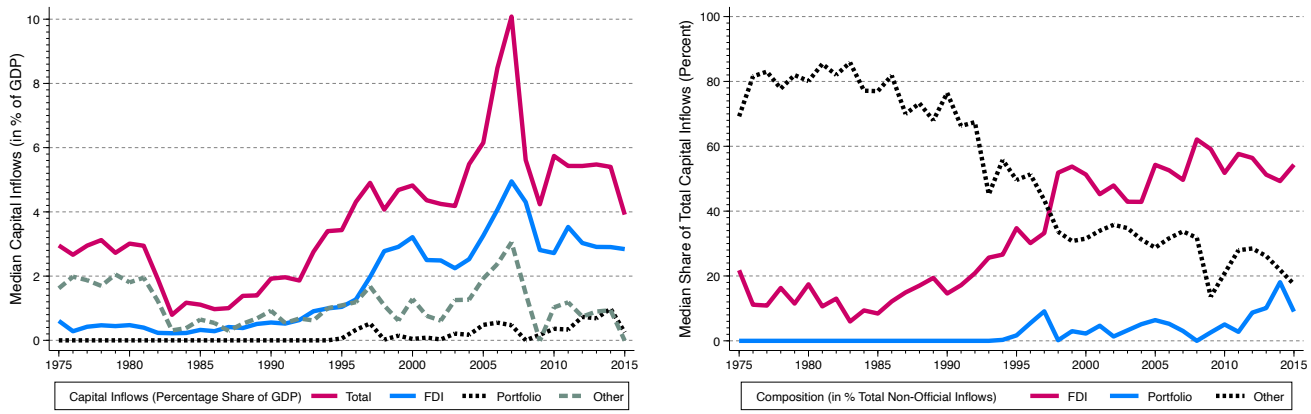
(1) *Total Non-Official Capital Inflows*, defined as `icapfl-iothfg`: Total inflows less other inflows to official sector. Total inflows are made up of `ifdi+ipf+idrvtv+iothf`: FDI inflows, portfolio inflows, derivative inflows and other inflows. The resulting flow is expressed in percent of GDP in US$ (`icapflp_gdp`).

(2) *FDI inflows*, expressed in percent of GDP in US$ (`ifdi_gdp`).

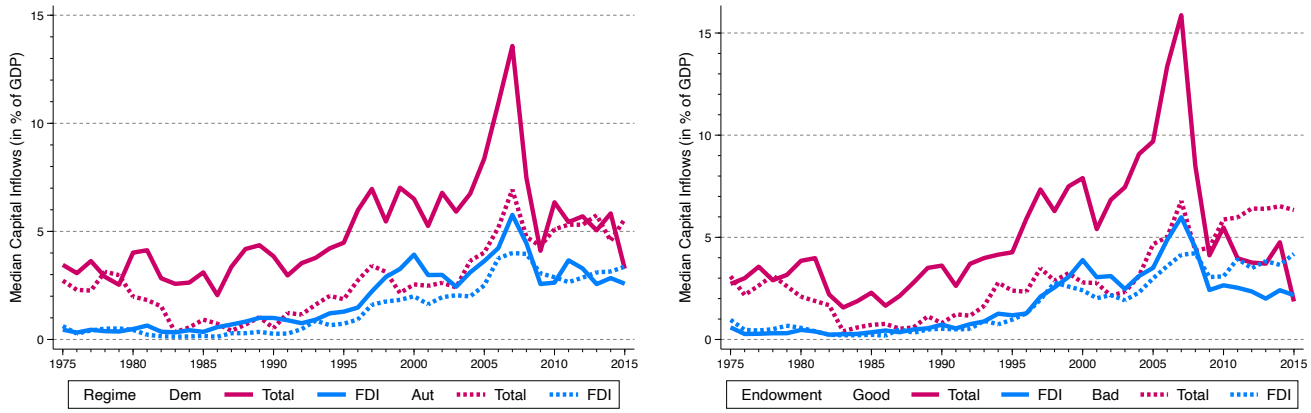
(3) *Total Net Non-Official Inflows*, defined as `ncapfl-nothfg`: Total net inflows less net other inflows to the official sector. Total net inflows are made up of `nfdi +npf+ndrvtv+nothf`: net FDI inflows, net portfolio inflows, net derivative inflows, and net other inflows. The resulting flow is expressed in percent of GDP in US$ (`ncapflp_gdp`).

For more details, see Bluedorn et al. (2013). The data start in 1970, however, we do not use the first five years of data: our empirical setup would imply that a mere 1 or 2 control group countries were available for 1970-74.

Figure A-2: Composition and Evolution of Capital Inflows



(a) Evolution (left) and Composition of Median Capital Inflows across all countries



(b) Evolution of Median Capital Inflows by Political Regime (left) and Geography

Notes: We present median capital flows (in percent of total inflows or in percent of GDP) for all countries in Panel (a) and by political regime and geography in Panel (b). Regime is defined by the V-Dem ERT variable (not countries experiencing democratic regime change), 'poor' geography by being located below the full sample average absolute latitude.

Table A-1: Sample Makeup – Capital Flow Analysis

	ISO	Country	Start	End	Obs	Miss	Income	CapFlow/GDP		ANRR Democracy Definition						Liberal Democracy Definition					
								Start	End	Always	Treat	AbsLat		Ctrl	AbsLat		Always	Treat	AbsLat		Ctrl	AbsLat	
												Hi	Lo		Hi	Lo			Hi	Lo		H	Lo
1	AGO	Angola	1985	2015	31		LMC	3.7	-1.5					26		26					31		31
2	ALB	Albania	1981	2015	35		UMC	0.0	10.5		30	30						35	35				
3	ARG	Argentina	1976	2015	40		UMC	-1.0	1.7		35	35						40	40				
4	ARM	Armenia	1993	2015	23		UMC	-2.1	9.4		18	18									23	23	
5	AUS	Australia	1970	2015	46		HIC	2.8	5.9	41							46						
6	AUT	Austria	1970	2015	46		HIC	3.5	-2.8	41							46						
7	AZE	Azerbaijan	1995	2015	21		UMC	12.9	8.5					16	16						21	21	
8	BDI	Burundi	1985	2015	31		LIC	0.8	8.0		26		26								31		31
9	BEL	Belgium	2002	2015	14		HIC	19.3	0.7	9							14						
10	BEN	Benin	1974	2014	41		LMC	2.4	5.3		37		37					41		41			
11	BFA	Burkina Faso	1974	2014	36	5	LIC	1.6	18.6		32		32					36		36			
12	BGD	Bangladesh	1976	2015	40		LMC	0.0	2.0		35	35									40	40	
13	BGR	Bulgaria	1981	2015	35		UMC	-1.2	4.9		30	30						35	35				
14	BIH	Bosnia & Herzeg	1998	2015	18		UMC	4.0	3.5					13	13			18	18				
15	BLR	Belarus	1993	2015	23		UMC	1.2	4.7		18	18						23	23				
16	BOL	Bolivia	1976	2015	40		LMC	2.1	6.1		35		35					40		40			
17	BRA	Brazil	1975	2015	41		UMC	5.5	5.9		36		36					41		41			
18	BRB	Barbados	1970	2013	44		HIC	15.2	10.6	41							44						
19	BWA	Botswana	2000	2015	16		UMC	1.9	3.9	11							16						
20	CAF	Central Afr Rep	1977	1994	18		LIC	1.4	-0.2		18		18								18		18
21	CAN	Canada	1970	2015	46		HIC	4.4	9.3	41							46						
22	CHE	Switzerland	1977	2015	39		HIC	-0.5	3.8	34							39						
23	CHL	Chile	1975	2015	41		HIC	-1.9	10.0		36	36						41	41				
24	CHN	China	1982	2015	34		UMC	0.2	4.3					29	29						34	34	
25	CIV	Cote d'Ivoire	1975	2013	39		LMC	4.7	3.7		36		36								39		39
26	CMR	Cameroon	1977	2015	39		LMC	1.7	1.3					34		34					39		39
27	COG	Congo, Rep	1978	2007	30		LMC	8.0	10.0		30		30								30		30
28	COL	Colombia	1970	2015	46		UMC	2.4	7.7	41								46		46			
29	COM	Comoros	1981	2012	25	7	LMC	1.0	4.7		23		23								25		25
30	CPV	Cabo Verde	1977	2015	39		LMC	0.1	6.8		34		34					39		39			
31	CRI	Costa Rica	1977	2015	39		UMC	9.6	11.2	34							39						
32	CYP	Cyprus	1976	2015	40		HIC	4.4	27.8	35							40						
33	CZE	Czech Rep	1993	2015	23		HIC	7.6	5.7	18							23						
34	DEU	Germany	1971	2015	45		HIC	2.0	0.9	40							45						
35	DJI	Djibouti	1991	2015	25		LMC	5.0	18.2		20		20								25		25
36	DNK	Denmark	1975	2015	41		HIC	1.7	0.7	36							41						
37	DOM	Dominican Rep	1977	2015	39		UMC	1.6	6.0		34		34								39		39
38	DZA	Algeria	1977	2015	26	13	LMC	9.8	1.1					21	21						26	26	
39	ECU	Ecuador	1976	2015	40		UMC	0.7	1.1		35		35					40		40			
40	EGY	Egypt	1977	2015	39		LMC	-4.4	1.7					34	34						39	39	
41	ESP	Spain	1975	2015	41		HIC	2.7	-1.4		36	36						41	41				
42	EST	Estonia	1993	2015	23		HIC	14.6	7.1	18							23						
43	ETH	Ethiopia	1977	2012	36		LIC	0.1	1.7		34		34								36		36
44	FIN	Finland	1975	2015	41		HIC	10.1	-16.3	36							41						
45	FRA	France	1975	2015	41		HIC	3.0	0.0	36							41						
46	GAB	Gabon	1978	2005	28		UMC	-2.6	1.0					28		28					28		28
47	GBR	United Kingdom	1970	2015	46		HIC	3.5	-0.6	41							46						
48	GEO	Georgia	1997	2015	19		UMC	7.3	11.5	14								19	19				
49	GHA	Ghana	1975	2015	41		LMC	0.5	7.8		36		36					41		41			
50	GIN	Guinea	1986	2013	28		LIC	-0.3	18.6		25		25								28		28
51	GMB	Gambia	1978	2012	30	5	LIC	1.0	-3.0		28		28								30		30
52	GNB	Guinea-Bissau	1982	2013	29	3	LIC	0.0	2.8		26		26								29		29
53	GRC	Greece	1976	2015	39	1	HIC	3.4	-9.8	34							39						
54	GTM	Guatemala	1977	2015	39		UMC	3.1	4.5		34		34					39		39			
55	HKG	Hong Kong	1998	2015	18		HIC	-85.6	64.6	13											18		18
56	HND	Honduras	1974	2015	42		LMC	4.6	7.9		37		37								42		42
57	HRV	Croatia	1993	2015	23		HIC	1.9	2.7		18	18						23	23				
58	HTI	Haiti	1971	2015	45		LIC	0.9	2.2		40		40								45		45
59	HUN	Hungary	1982	2015	34		HIC	0.0	-2.6		29	29						34	34				
60	IDN	Indonesia	1981	2015	35		UMC	0.2	5.1		30		30					35		35			
61	IND	India	1975	2015	41		LMC	0.0	6.9	36								41		41			
62	IRL	Ireland	1974	2015	42		HIC	13.7	76.4	37							42						
63	IRN	Iran	1981	2000	20		UMC	0.2	-0.2					20	20						20	20	
64	ISL	Iceland	1976	2015	40		HIC	1.3	-9.0	35							40						
65	ISR	Israel	1970	2015	46		HIC	5.9	3.1	41							46						
66	ITA	Italy	1970	2015	46		HIC	3.8	0.4	41							46						
67	JAM	Jamaica	1976	2015	40		UMC	-1.4	7.2	35								40		40			
68	JOR	Jordan	1972	2015	44		UMC	0.0	9.8					39	39						44	44	
69	JPN	Japan	1977	2015	39		HIC	0.0	0.5	34							39						
70	KAZ	Kazakhstan	1995	2015	21		UMC	7.3	7.7					16	16						21	21	

(Continued overleaf)

Table A-1: Sample Makeup – Capital Flow Analysis (continued)

	ISO	Country	Start	End	Obs	Miss	Income	CapFlow/GDP		ANRR Democracy Definition						Liberal Democracy Definition					
								Start	End	Always	Treat	AbsLat Hi	AbsLat Lo	Ctrl	AbsLat Hi	AbsLat Lo	Always	Treat	AbsLat Hi	AbsLat Lo	Ctrl	AbsLat H	AbsLat Lo
71	KEN	Kenya	1975	2014	40		LMC	2.4	6.9		36		36								40		40
72	KGZ	Kyrgyz Republic	1993	2015	23		LMC	1.8	8.5		18	18									23	23	
73	KHM	Cambodia	1992	2014	23		LMC	1.6	18.5		19		19								23		23
74	KOR	South Korea	1976	2015	40		HIC	5.2	-0.2		35	35						40	40				
75	KWT	Kuwait	1975	2015	41		HIC	-0.8	2.2					36	36						41	41	
76	LAO	Lao	1984	2015	32		LMC	0.4	9.3					27		27					32		32
77	LBN	Lebanon	2002	2015	14		UMC	10.5	15.5		9	9									14	14	
78	LBR	Liberia	1979	2015	21	16	LIC	0.1	36.5		16		16					21		21			
79	LBY	Libya	1977	2013	37		UMC	-0.1	1.1					34	34						37	37	
80	LKA	Sri Lanka	1975	2015	41		LMC	-0.8	6.1	36								41		41			
81	LSO	Lesotho	2000	2015	16		LMC	5.3	5.0	11								16	16				
82	LTU	Lithuania	1993	2015	23		HIC	-0.7	-0.4	18							23						
83	LUX	Luxembourg	2002	2015	14		HIC	701.0	1327.6	9							14						
84	LVA	Latvia	1992	2015	24		HIC	4.8	10.1		19	19					24						
85	MAR	Morocco	1975	2015	41		LMC	0.8	7.2					36	36						41	41	
86	MDA	Moldova	1994	2015	22		LMC	8.4	7.2	17								22	22				
87	MDG	Madagascar	1974	2013	40		LIC	0.3	8.1		37		37								40		40
88	MEX	Mexico	1979	2015	37		UMC	4.2	7.7		32	32						37	37				
89	MLI	Mali	1975	2014	40		LIC	0.0	3.3		36		36					40		40			
90	MLT	Malta	1971	2015	45		HIC	6.7	42.9	40							45						
91	MMR	Myanmar	1998	2015	18		LMC	4.2	3.2					13		13					18		18
92	MNG	Mongolia	1981	2015	35		LMC	0.0	29.9		30	30						35	35				
93	MOZ	Mozambique	1981	2015	35		LIC	0.0	40.2		30		30								35		35
94	MRT	Mauritania	1981	2015	22	13	LMC	8.4	17.4					18		18					22		22
95	MUS	Mauritius	1976	2015	40		HIC	-2.8	-7.2	35							40						
96	MWI	Malawi	1977	2015	39		LIC	2.5	9.3		34		34					39		39			
97	MYS	Malaysia	1974	2015	42		UMC	5.5	6.7					37		37					42		42
98	NAM	Namibia	2000	2015	16		UMC	7.8	10.5	11							16						
99	NER	Niger	1974	2013	40		LIC	2.1	18.0		37		37					40		40			
100	NGA	Nigeria	1977	2015	38	1	LMC	1.0	4.0		33		33					38		38			
101	NIC	Nicaragua	1977	2015	39		LMC	8.7	12.3		34		34					39		39			
102	NLD	Netherlands	1970	2015	46		HIC	9.4	-18.1	41							46						
103	NOR	Norway	1975	2015	41		HIC	8.1	10.0	36							41						
104	NPL	Nepal	1981	2015	35		LMC	0.3	2.1		30	30						35	35				
105	NZL	New Zealand	1972	2015	44		HIC	2.3	3.1	39							44						
106	OMN	Oman	1974	2015	42		HIC	-5.0	3.5					37		37					42		42
107	PAK	Pakistan	1976	2015	40		LMC	1.5	0.8		35	35									40	40	
108	PAN	Panama	1977	2015	39		HIC	120.0	18.0		34		34					39		39			
109	PER	Peru	1977	2015	39		UMC	0.7	8.6		34		34					39		39			
110	PHL	Philippines	1977	2015	39		LMC	4.3	2.7		34		34					39		39			
111	POL	Poland	1976	2015	40		HIC	8.1	3.2		35	35						40	40				
112	PRT	Portugal	1975	2015	41		HIC	-0.2	-5.9		36	36						41	41				
113	PRY	Paraguay	1975	2015	41		UMC	5.1	2.9		36		36					41		41			
114	RUS	Russian Fed	1994	2015	22		UMC	0.6	2.5		17	17									22	22	
115	RWA	Rwanda	1976	2015	40		LIC	-0.2	3.8					35		35					40		40
116	SAU	Saudi Arabia	1971	2015	45		HIC	-1.0	1.6					40	40						45	45	
117	SDN	Sudan	1977	2015	39		LIC	1.1	3.3		34		34								39		39
118	SEN	Senegal	1974	2014	41		LMC	3.4	7.9		37		37					41		41			
119	SGP	Singapore	1972	2015	44		HIC	19.1	38.2	39											44		44
120	SLE	Sierra Leone	1977	2014	35	3	LIC	1.9	26.6		31		31								35		35
121	SLV	El Salvador	1976	2015	40		LMC	3.6	5.4		35		35					40		40			
122	STP	Sao Tome & Pr	1974	2015	33	9	LMC	4.1	4.9		27		27					33		33			
123	SVK	Slovak Republic	1993	2015	23		HIC	6.9	7.7	18							23						
124	SVN	Slovenia	1993	2015	23		HIC	0.9	2.6	18							23						
125	SWE	Sweden	1970	2015	46		HIC	1.5	-6.0	41							46						
126	SWZ	Eswatini	2000	2015	16		LMC	7.4	0.0					11	11						16	16	
127	SYC	Seychelles	1976	2015	40		HIC	17.2	28.9					35		35					40		40
128	SYR	Syria	1977	2010	34		LIC	2.0	3.8					34	34						34	34	
129	TCD	Chad	1981	1994	14		LIC	-0.2	-2.2					14		14					14		14
130	TGO	Togo	1974	2015	42		LIC	-10.6	6.9					37		37					42		42
131	THA	Thailand	1975	2015	41		UMC	3.1	3.9		36		36					41		41			
132	TJK	Tajikistan	2002	2015	14		LIC	8.7	4.3					9	9						14	14	
133	TTO	Trinidad & Tob	1975	2011	37		HIC	3.5	-13.8	36							37						
134	TUN	Tunisia	1976	2015	40		LMC	8.3	5.2					35	35			40	40				
135	TUR	Turkey	1974	2015	42		UMC	0.7	8.4		37	37						42	42				
136	TZA	Tanzania	1976	2015	40		LMC	-0.4	6.6					35		35					40		40
137	UGA	Uganda	1980	2015	36		LIC	-0.1	5.5		31		31								36		36
138	UKR	Ukraine	1994	2015	22		LMC	6.8	4.4	17								22	22				
139	URY	Uruguay	1978	2015	38		HIC	1.8	10.6		33	33						38	38				
140	USA	United States	1970	2015	46		HIC	-0.3	5.0	41							46						

(Continued overleaf)

Table A-1: Sample Makeup – Capital Flow Analysis (continued)

	ISO	Country	Start	End	Obs	Miss	Income	CapFlow/GDP		ANRR Democracy Definition						Liberal Democracy Definition					
								Start	End	Always	Treat	AbsLat		Ctrl	AbsLat		Always	Treat	AbsLat		Ctrl	AbsLat	
												Hi	Lo		Hi	Lo			Hi	Lo		H	Lo
141	VEN	Venezuela	1970	2015	45	1	UMC	0.2	1.7		40		40					45		45			
142	VNM	Vietnam	1996	2015	20		LMC	11.9	6.9					15		15					20		20
143	YEM	Yemen	1990	2015	26		LIC	-0.5	0.7					21		21					26		26
144	ZAF	South Africa	1998	2015	18		UMC	8.5	1.0	13													
145	ZMB	Zambia	1978	2015	33	5	LMC	5.8	10.0		28		28					33		33			
146	ZWE	Zimbabwe	1981	2015	21	14	LMC	1.7	8.5		16		16								21		21

Notes: We present the sample makeup for the capital flow analysis (1975-2015). Income indicates the World Bank Income Level category (Low - LIC, Lower Middle - LMC, Upper Middle - UMC, and High - HIC). We report the gross capital inflow over GDP for the first and last year of the country series, in percent. The remaining columns indicate treated and controls samples (total number of observations, respectively) for two democracy definitions: that by ANRR and the V-Dem Liberal Democracy (sample mean cutoff). 'Always' refers to countries that were democracies throughout the sample period, 'treat' to the treated sample, where absolute latitude ('Abslat') 'Hi' and 'Lo' provide the split for one of the many deep determinants we apply in our analysis. 'Ctrl' is the control sample, again split into 'Hi' and 'Lo' absolute latitude. Absolute latitude is one of the six geography proxies (plus four more in robustness checks) we adopt in the paper, in addition to proxies for culture, history and legal origin.

Table A-2: Deep Determinants: Pairwise Correlations

	Geography						LO	Culture			History
	Malaria Ecology	Malaria Risk	Disease Prevalence	Zero Temperate	Some Tropics	Absolute Latitude	French Leg.Orig.	Cult. Dist. from UK	Common Language	Language Similarity	Exper. Colo'ism
Malaria Ecology	1.00 108										
Malaria Risk	0.58 108	1.00 109									
Disease Prevalence	0.52 108	0.50 109	1.00 109								
Zero Temperate	0.60 99	0.48 99	0.39 99	1.00 99							
Some Tropics	0.79 108	0.63 109	0.54 109	0.60 99	1.00 109						
Absolute Latitude	0.78 108	0.55 109	0.49 109	0.71 99	0.81 109	1.00 109					
French Legal Origin	0.09 108	-0.04 109	0.03 109	0.02 99	-0.02 109	0.09 109	1.00 109				
Cult. Distance from UK	0.47 102	0.41 102	0.26 102	0.56 99	0.53 102	0.41 102	-0.16 102	1.00 102			
Common Language	0.13 108	0.31 109	0.21 109	0.41 99	0.15 109	0.18 109	-0.15 109	0.38 102	1.00 109		
Language Similarity	0.28 108	0.35 109	0.30 109	0.35 99	0.31 109	0.38 109	-0.24 109	0.47 102	0.48 109	1.00 109	
Experienced Colonialism	0.60 107	0.47 108	0.49 108	0.36 99	0.57 108	0.59 108	0.21 108	0.22 102	0.05 108	0.07 108	1.00 108

Notes: We present the pairwise correlation coefficients and sample sizes for the time-invariant deep determinants: geography (×6 proxies), French legal origin and culture (×3). This is for the treatment and control samples only, using Liberal Democracy as the regime change definition. Results are virtually identical if we use the full sample (including countries which have been democratic throughout the sample period). The mean (median) for 15 geography correlations is 0.60 (0.58), and for 3 culture correlations 0.44 (0.47). The mean (median) of 18 correlation between geography and culture is 0.32 (0.33), of 6 correlations between geography and history is 0.51 (0.53), of 6 correlations between geography and legal origin is 0.03 (0.02), of 3 correlations between history and culture is 0.11 (0.07), and of 3 correlations between legal origin and culture is -0.18 (-0.16).

B Results Tables, Diagnostics and Robustness Checks

B.1 Main Analysis — Diagnostic tests

Table B-1: Diagnostic Tests — PCDID Capital Flow Analysis

Democracy Indicator		ANRR		LibDem		Poly		Liberal		N
Deep Determinant Group		0	1	0	1	0	1	0	1	
Panel A: Total Non-Official Capital Inflows										
Controls: none										
Geography	Alpha $t > 1.96$	0.17	0.50	0.33	0.17	0.67	0.50	0.00	0.00	30
Alternative Deep Det	Alpha $t > 1.96$	0.80	0.60	0.40	0.20	0.60	0.40	0.00	0.00	25
Controls: export/trade										
Geography	Alpha $t > 1.96$	0.17	0.17	0.33	0.00	0.50	0.50	0.17	0.00	30
Alternative Deep Det	Alpha $t > 1.96$	0.60	0.40	0.40	0.20	0.60	0.20	0.00	0.00	25
Geography	$\chi^2(p) < 0.1$	0.00	0.13	0.00	0.17	0.00	0.27	0.00	0.00	30
Alternative Deep Det	$\chi^2(p) < 0.1$	0.00	0.00	0.00	0.00	0.00	0.16	0.00	0.00	25
Controls: export/trade, GDP pc growth, population growth										
Geography	Alpha $t > 1.96$	0.17	0.67	0.33	0.00	0.67	0.17	0.33	0.00	30
Alternative Deep Det	Alpha $t > 1.96$	0.40	0.80	0.40	0.20	0.60	0.80	0.20	0.00	25
Geography	$\chi^2(p) < 0.1$	0.67	0.93	0.33	0.93	0.83	1.00	1.00	1.00	30
Alternative Deep Det	$\chi^2(p) < 0.1$	0.60	0.96	0.40	0.52	0.52	1.00	1.00	1.00	25
Panel B: FDI Inflows										
Controls: none										
Geography	Alpha $t > 1.96$	0.33	0.67	0.00	0.00	0.50	0.17	0.33	0.17	30
Alternative Deep Det	Alpha $t > 1.96$	0.40	0.40	0.20	0.20	0.40	0.20	0.20	0.00	25
Controls: export/trade										
Geography	Alpha $t > 1.96$	0.33	1.00	0.00	0.00	0.50	0.17	0.17	0.00	30
Alternative Deep Det	Alpha $t > 1.96$	0.40	0.40	0.20	0.20	0.40	0.40	0.20	0.00	25
Geography	$\chi^2(p) < 0.1$	0.07	0.13	0.00	0.17	0.00	0.17	0.00	0.00	30
Alternative Deep Det	$\chi^2(p) < 0.1$	0.00	0.04	0.04	0.00	0.00	0.08	0.00	0.00	25
Controls: export/trade, GDP pc growth, population growth										
Geography	Alpha $t > 1.96$	0.00	1.00	0.33	0.17	0.50	0.33	0.17	0.17	30
Alternative Deep Det	Alpha $t > 1.96$	0.40	0.40	0.20	0.40	0.40	0.80	0.40	0.20	25
Geography	$\chi^2(p) < 0.1$	0.30	0.83	0.37	1.00	0.80	1.00	1.00	1.00	30
Alternative Deep Det	$\chi^2(p) < 0.1$	0.32	0.84	0.36	0.64	0.72	1.00	1.00	1.00	25

Notes: The table reports the rejection frequency across the 25 or 30 models analysed for each cell (and presented in Figure 4 in the maintext). The 'Alpha' test is for weak parallel trends, so if the null hypothesis is rejected the PCDID specification may be misspecified: we want to see *very low* rejection rates, like for the 'Liberal Component'. The χ^2 test is for bad controls, so if the null hypothesis is rejected ($p < 0.1$) we should not include this (set of) control(s): again, we want to see *very low* rejection rates, like for the models with exports/trade as additional control. There are five alternative factor augmentations and four (alternative deep determinants) or six (geography) proxies for deep determinants, hence 25 or 30 models for each of the two deep determinant groups, respectively. These 25 or 30 models are represented in each estimate 'cloud' of the aforementioned figures.

Table B-2: Statistical Significance — PCDID Capital Flow Analysis

Democracy Indicator	ANRR		LibDem		Poly		Liberal		N
Deep Determinant Group	0	1	0	1	0	1	0	1	
Panel A: Total Non-Official Capital Inflows									
Controls: none									
Geography	0.93	0.67	1.00	0.60	0.70	0.80	1.00	0.63	30
Alternative Deep Det	0.00	0.24	0.60	0.72	0.56	0.24	0.72	0.12	25
Controls: export/trade									
Geography	0.97	0.97	1.00	0.80	0.23	1.00	1.00	0.60	30
Alternative Deep Det	0.68	0.76	0.84	0.88	0.68	0.76	0.76	0.92	25
Controls: export/trade, GDP pc growth, population growth									
Geography	0.13	0.00	0.53	0.60	0.30	0.53	0.93	0.10	30
Alternative Deep Det	0.48	0.76	0.84	0.68	0.56	0.68	0.72	0.84	25
Panel B: FDI Inflows									
Controls: none									
Geography	0.93	0.97	0.90	0.73	0.83	1.00	0.83	0.77	30
Alternative Deep Det	0.72	1.00	0.88	0.92	0.76	1.00	0.88	0.96	25
Controls: export/trade									
Geography	1.00	0.97	0.97	1.00	0.87	1.00	0.93	1.00	30
Alternative Deep Det	0.68	1.00	0.96	1.00	0.76	1.00	0.92	0.96	25
Controls: export/trade, GDP pc growth, population growth									
Geography	0.90	0.53	1.00	0.87	0.93	1.00	0.97	0.83	30
Alternative Deep Det	0.60	0.92	1.00	0.96	0.88	1.00	0.96	0.92	25

Notes: The table reports the rejection frequency across the 25 or 30 models analysed for each cell (and presented in Figure 4 in the maintext). These are for the t-tests (10% level) of the robust mean PCDID estimates (computed using the non-parametric variance estimator of Pesaran, 2006): if we see very high rejection rates this equates to statistical significance of the individual ATET presented in aforementioned figures. There are five alternative factor augmentations and four (alternative deep determinants) or six (for alternative deep determinants only five) proxies for deep determinants, hence 25 or 30 models for each of the two deep determinant groups, respectively. These 25 or 30 models are represented in each estimate 'cloud' of the aforementioned figures.

Table B-3: Comparison of Means — Reversal Propensity and Treatment Length

	Reversal to Autocracy				Average Years in Democracy			
	ANRR	**LibDem**	**Poly**	**Liberal**	**ANRR**	**LibDem**	**Poly**	**Liberal**
Malaria Ecology	0.55	0.43	0.51	0.44	0.09	0.30	0.55	0.53
Malaria Risk	0.16	0.17	0.22	0.14	0.25	0.02	0.20	0.12
Historical Disease	0.96	0.27	0.81	0.67	0.19	0.96	0.76	0.62
Some tropical land	0.72	0.25	0.82	0.49	0.26	0.51	0.82	0.19
Zero temperate land	0.01	0.03	0.21	0.01	0.08	0.31	0.39	0.18
Absolute Latitude	0.66	0.25	0.73	0.50	0.25	0.67	0.53	0.44
French LO	0.22	0.65	0.50	0.73	0.17	0.26	0.09	0.76
Distant Culture	0.10	0.23	0.85	0.21	0.03	0.33	0.42	0.25
Low Avg Common Lang	0.03	0.52	0.21	0.94	0.00	0.01	0.00	0.15
Low Avg Lang Similarity	0.02	0.28	0.44	0.28	0.00	0.16	0.40	0.68
Colonial Experience	0.44	0.58	0.18	0.25	0.32	0.56	0.68	0.62

Notes: The table reports the p-values of difference in means tests for (a) the reversal of 'treated' countries to autocracy, and (b) the average years spent in democracy, comparing countries with 'good' and 'poor' deep determinants. A high p-value suggests that the average country with 'good' deep determinants does not have a statistically significantly different propensity to revert to autocracy (number of years in democracy) than the average country with 'poor' deep determinants.

Table B-4: Bad control tests for alternative measures of trade

Democracy	All Definitions				ANRR & LibDem only			
Dep Var	Total Cap Flows		FDI Flows		Total Cap Flows		FDI Flows	
Deep Determinant	'Good'	'Poor'	'Good'	'Poor'	'Good'	'Poor'	'Good'	'Poor'
Geography								
Exports/Trade	0%	14%	2%	12%	0%	15%	3%	15%
Trade/GDP	99%	93%	95%	64%	98%	85%	90%	40%
Exports/GDP	79%	28%	79%	29%	21%	21%	67%	0%
Alternative Deep Determinants								
Exports/Trade	4%	6%	6%	4%	8%	3%	12%	2%
Trade/GDP	90%	87%	86%	79%	88%	74%	88%	62%
Exports/GDP	58%	48%	49%	50%	40%	30%	32%	26%

Notes: In robustness checks to our main results we add exports/trade as an additional control to the PCDID model. Alternative measures for a trade variable were also considered: trade openness (total trade/GDP) and exports/GDP. In this table we report (separately for total capital flows and FDI flows) the share of specifications for which a test whether each of these additional control variables constitutes a bad control fails: 0% indicates that no specification failed the test, 99% that virtually all specifications failed the test. We split results by geography and alternative deep determinants as well as 'good' versus 'poor' attributes. In the left panel, each cell for 'geography' reports the results covering 120 specifications (6 proxies times 5 alternative factor augmentations times 4 definitions of democracy), in the 'alternative deep determinants' case these are 100 specifications (only 5 proxies). In the right panel we have 60 and 50 specifications, respectively.

B.2 Main Analysis — Result tables

(see overleaf)

Table B-5: Democracy, Geography and Total Non-Official Capital Inflows (1975-2015)

	Disease Environment						Climate					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Malaria Ecology		Malaria Risk		Hist. Disease Index		Tropical Land		No Temperate Land		|Latitude| > MD	
	Low	High	Low	High	Low	High	None	Some	No	Yes	Yes	No
ANRR democracy	2.694***	0.768*	3.551***	0.558	2.405***	0.734*	2.440**	0.760**	1.761**	0.856**	2.894***	0.866**
	[0.883]	[0.416]	[1.076]	[0.381]	[0.876]	[0.412]	[1.095]	[0.378]	[0.867]	[0.371]	[0.956]	[0.435]
Treated Countries	22	47	23	46	26	43	23	46	21	44	23	46
Treated Observations	617	1470	626	1461	763	1324	637	1450	616	1368	650	1437
Control Countries	13	17	10	21	11	20	17	14	9	18	16	15
Control Observations	342	448	265	560	267	558	451	374	252	491	416	409
Alpha test (t)	-1.23	-1.98	-2.56	-0.56	-1.25	-0.84	-0.70	-1.00	-1.07	-5.55	-0.35	-4.63
Alternative factor augmentation												
2 factors	2.219**	0.572	2.826***	0.291	3.084***	0.342	2.476***	0.695*	3.090***	0.636*	2.126***	0.599
3 factors	2.737***	0.874**	3.054***	0.383	3.121***	0.685*	2.684**	0.782**	2.545**	0.637*	2.278**	0.784*
4 factors	2.694***	0.768*	3.551***	0.558	2.405***	0.734*	2.440**	0.760**	1.761**	0.856**	2.894***	0.866**
5 factors	2.330**	0.836*	3.315***	0.612	2.309***	0.781**	2.392**	0.665*	0.959	0.991**	2.411***	0.864*
6 factors	2.970***	0.247	2.870***	0.281	2.293***	0.871**	2.306***	0.452	0.109	0.730**	2.306***	0.645*
Liberal Democracy Index > median	2.605***	1.014*	3.588***	-0.049	2.608***	1.048**	2.224***	0.985	2.035***	1.222***	2.377***	1.375**
	[0.845]	[0.592]	[0.785]	[0.484]	[0.847]	[0.477]	[0.858]	[0.599]	[0.779]	[0.452]	[0.858]	[0.595]
Treated Countries	24	27	23	28	22	29	22	29	22	26	22	29
Treated Observations	801	1029	760	1070	709	1121	718	1112	745	996	716	1114
Control Countries	17	40	16	42	18	40	22	36	11	40	20	38
Control Observations	498	1262	440	1360	551	1249	630	1170	321	1288	588	1212
Alpha test (t)	-0.13	-1.74	-6.72	0.10	0.63	-1.90	-4.53	0.02	-0.91	0.10	0.48	-2.48
Alternative factor augmentation												
2 factors	1.868***	1.618***	3.946***	0.292	2.929***	1.745***	3.228***	0.324	1.712***	1.204**	1.928***	1.862***
3 factors	2.634***	1.357**	3.773***	0.757	2.627***	1.592***	2.715***	0.953*	2.100***	1.299**	1.792***	1.703**
4 factors	2.605***	1.014*	3.588***	-0.049	2.608***	1.048**	2.224***	0.985	2.035***	1.222***	2.377***	1.375**
5 factors	2.559***	0.940**	3.739***	0.019	2.391***	1.032**	2.275**	0.557	1.942**	1.166***	2.483***	0.827
6 factors	2.812***	0.661	3.879***	-0.054	1.599***	1.027**	2.264**	0.385	1.731**	1.037**	2.409***	0.760

Notes: This table is for the analysis by geography. We present robust mean estimates from PCDID regressions of total non-official capital inflows and a democracy dummy defined as indicated in each result panel — these can be interpreted as Average Treatment Effects on the Treated (ATET). These results are for the model *without any additional controls*. The 12 different models in each panel are for sample splits determined by 'disease environment' and 'climate', in each case we use three proxies for these factors of geography, separating 'good' geography in the odd columns and 'poor' geography in the even columns. In a lower part of each panel we report the ATET estimates for specification with two to six factors. See Table 1 for all other details.

(x)

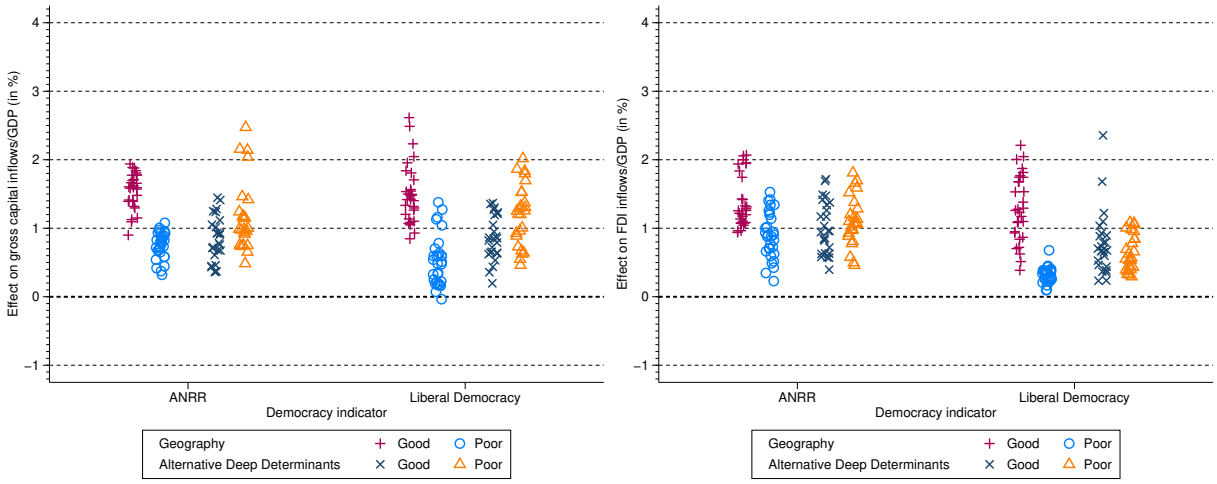
Table B-6: Democracy, Alternative Deep Determinants and Total Non-Official Capital Inflows (1975-2015)

	Legal Origin		**Culture**						**History**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	French LO		**Cultural Distance**		**Common Language**		**Language Similarity**		**Colonial Exp**	
	No	Yes	Low	High	Low	High	High	Low	No	Yes
ANRR democracy	-0.801*	0.196	1.421	0.280	0.358	-0.091	0.290	-0.027	1.947	-0.255
	[0.418]	[0.373]	[0.878]	[0.304]	[0.397]	[0.495]	[0.435]	[0.631]	[1.244]	[0.333]
Treated Countries	23	46	24	42	38	31	39	30	19	50
Treated Observations	673	1414	722	1281	1130	957	1175	912	521	1566
Control Countries	7	24	10	19	22	9	21	10	8	23
Control Observations	173	652	264	513	584	241	520	305	175	650
Alpha test (t)	-13.88	-3.22	-11.40	-10.42	-10.92	-4.73	-10.35	-10.76	-9.52	-1.22
Alternative factor augmentation										
2 factors	-0.132	0.620	0.481	0.210	0.346	0.319	0.196	0.234	1.921*	-0.075
3 factors	-0.147	0.368	1.162	0.332	0.545	0.003	0.259	-0.021	1.906*	-0.108
4 factors	-0.801*	0.196	1.421	0.280	0.358	-0.091	0.290	-0.027	1.947	-0.255
5 factors	-0.554	0.369	1.106	0.210	0.273	-0.205	0.125	-0.259	1.972	0.097
6 factors	-0.111	0.456	0.728	0.318	0.365	0.052	0.251	0.016	2.041	0.068
Liberal Democracy Index > median	1.319*	2.300***	1.364	0.881	1.886**	1.766**	1.930**	1.861***	2.675**	0.937*
	[0.773]	[0.704]	[0.939]	[0.624]	[0.773]	[0.767]	[0.760]	[0.613]	[1.150]	[0.492]
Treated Countries	20	31	23	26	24	27	25	26	18	33
Treated Observations	653	1177	770	989	835	995	903	927	566	1264
Control Countries	16	42	16	37	40	18	42	16	11	46
Control Observations	530	1270	495	1163	1244	556	1256	544	296	1486
Alpha test (t)	-14.03	-0.45	-0.22	-1.29	0.20	-8.50	-5.94	1.16	-1.02	-0.57
Alternative factor augmentation										
2 factors	1.911*	1.892***	3.183***	0.567	1.646**	1.988***	1.874**	1.625**	3.037***	0.725
3 factors	1.844*	2.058***	1.352**	0.826	1.419**	2.033***	1.805**	1.532**	2.956***	0.945*
4 factors	1.319*	2.300***	1.364	0.881	1.886**	1.766**	1.930**	1.861***	2.675**	0.937*
5 factors	0.827	1.597***	1.710*	0.645	1.373**	1.638***	1.397**	2.066***	2.783**	0.349
6 factors	0.635	1.251**	1.240	0.557	1.086*	0.976*	1.391*	1.586**	2.432**	0.335

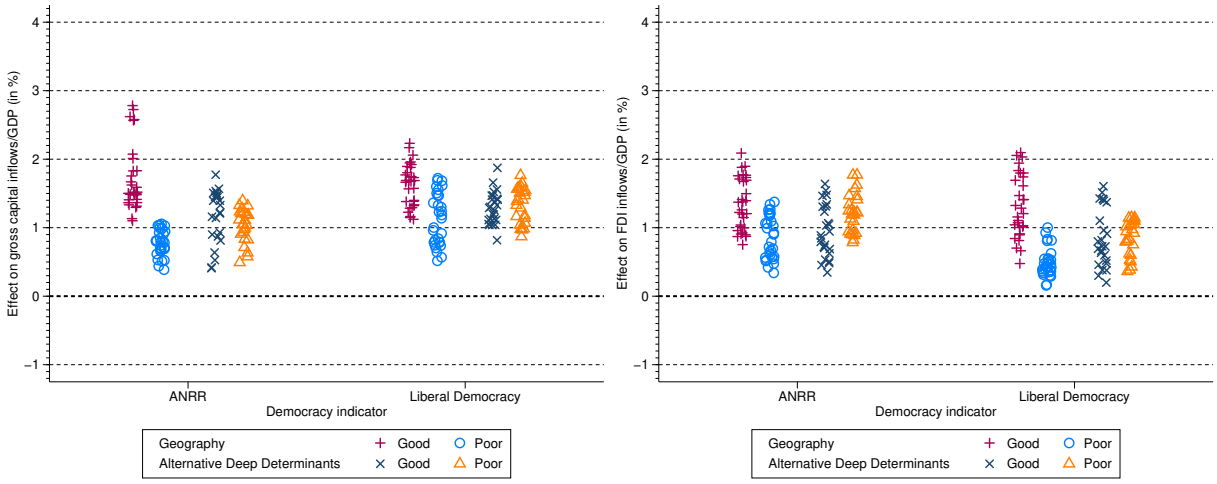
Notes: This table presents the analysis by alternative deep determinants — French Legal Origin (LO), three measures for cultural clusters, and two for colonial experience. Odd (even) columns are for 'good' ('poor') deep determinants. See Table B-5 for all other details.

B.3 Analysis using per capita capital flow definition

Figure B-1: Democracy, Geography and Capital Inflows (per capita definition)



(a) Total Non-Official Capital Inflows (left) and Total FDI Inflows (right) – no controls

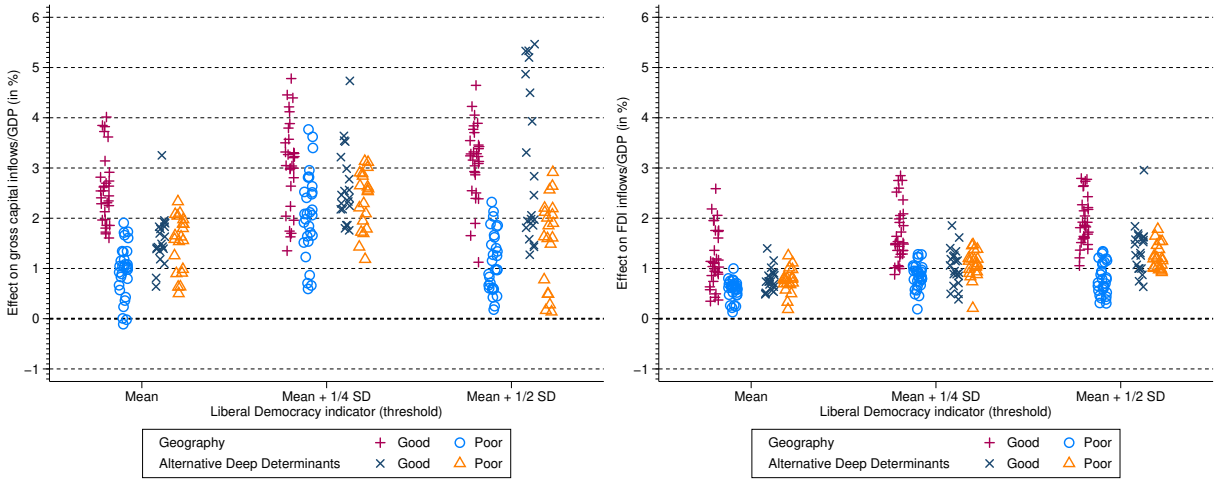


(b) Total Non-Official Capital Inflows (left) and Total FDI Inflows (right) – exports/trade as additional control

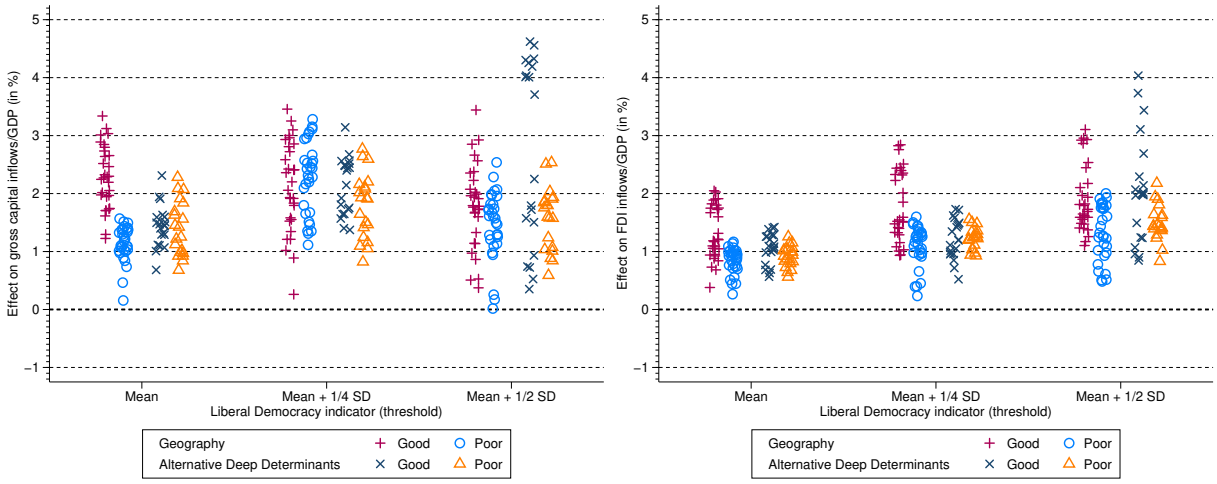
Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on capital inflows by geography (+ and o for good and poor geography, respectively), using four different definitions of democratic regime change. These are the results using per capita capital inflows as dependent variable (transformed using the inverse hyperbolic sine function). See Figure 4 in the maintext for all other details. The Alpha weak parallel trend test is rejected across all specifications and 'good' versus 'poor' samples in 83% of the total capital flow and 63% of the FDI flow models presented — this suggests misspecification of the PCDID models used. Results of the Alpha test, analysis of 'bad controls' and statistical significance are therefore not presented in detail (available on request).

B.4 Capital flow analysis using tighter democracy thresholds (V-Dem data)

Figure B-2: Democracy, Geography and Capital Inflows



(a) Total Non-Official Capital Inflows (left) and Total FDI Inflows (right) – no controls

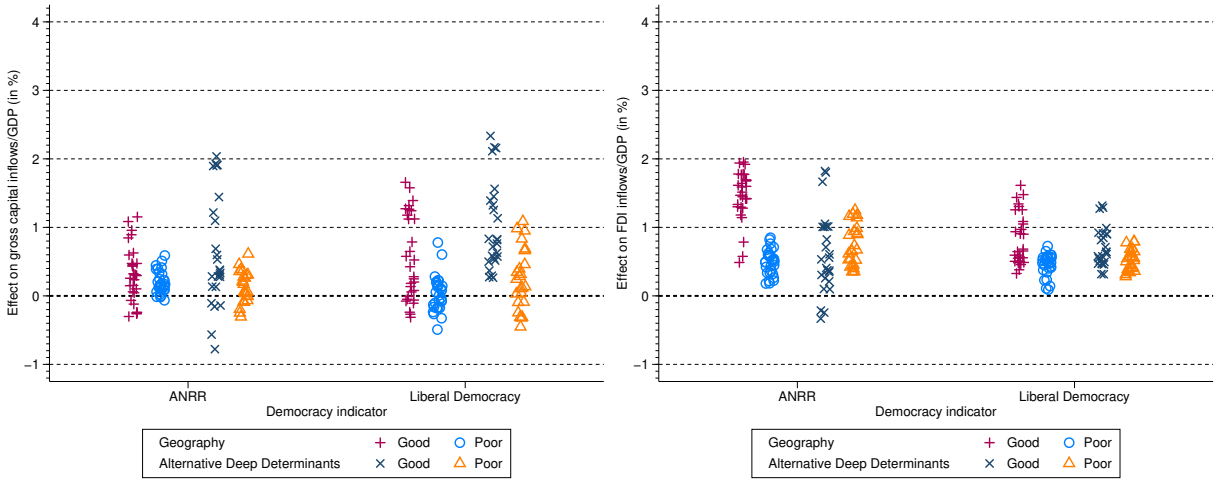


(b) Total Non-Official Capital Inflows (left) and Total FDI Inflows (right) – exports/trade as additional control

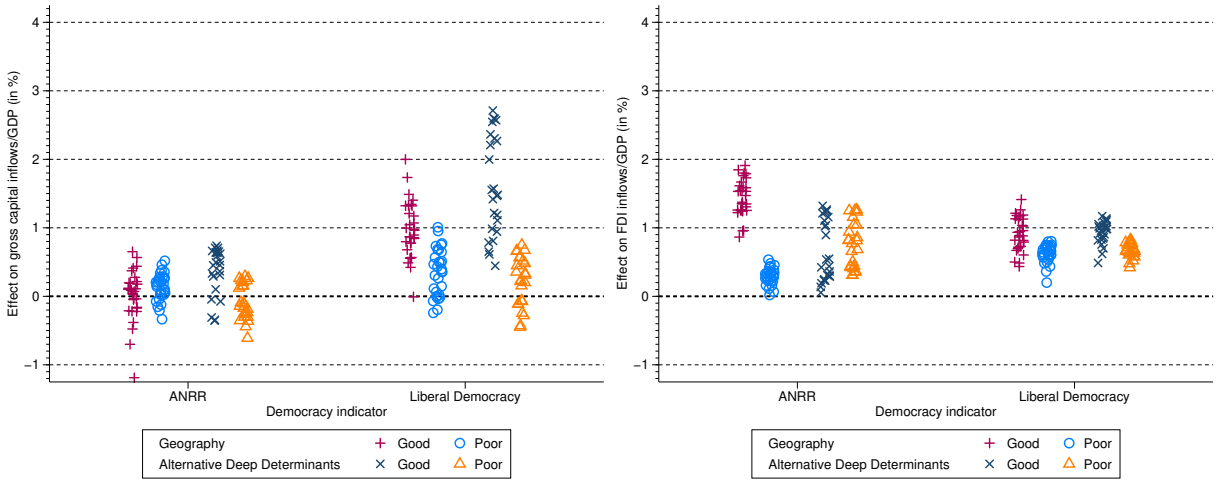
Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on capital inflows by geography (+ and o for good and poor geography, respectively), using different definitions of democratic regime change based on the V-Dem liberal democracy index: (i) the mean (across all countries in the sample for 1975-2015, regardless of whether they were always democracies, always autocracies or democratisers); (ii) the mean plus 1/4 of a standard deviation; and (iii) the mean plus 1/2 of a standard deviation. Like in the analysis in the maintext a high proportion (>60%) of the models including a full set of control variables (in contrast to the models only including only exports/trade) fail the 'bad control' test and are therefore not presented. The Alpha weak parallel trend test is rejected across all specifications and 'good' versus 'poor' samples in only 13% of the total capital flow and 3% of the FDI flow models presented — this indicates that underlying PCDID models are not misspecified. See Figure 4 in the maintext for all other details. In all plots we exclude the results for colonial experience, which mirroring the results in Figure 5 in the maintext highlight substantial heterogeneity between countries which did (low ATETs) and those which did not (high ATETs) experience colonialism.

B.5 Capital flow analysis using net (non-official) capital inflows

Figure B-3: Democracy and Net (Non-Official) Capital Inflows



(a) Net Non-Official Capital Inflows (left) and Net FDI Inflows (right) – no controls



(b) Net Non-Official Capital Inflows (left) and Net FDI Inflows (right) – exports/trade as additional control

Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on net non-official capital inflows (left column) and net FDI inflows (right column). See Figure 4 in the maintext for all other details. The Alpha weak parallel trend test is rejected across all specifications and 'good' versus 'poor' samples in 67% of the total capital flow and 58% of the FDI flow models — this suggests misspecification of the PCDID models used. Results of the Alpha test, analysis of 'bad controls' and statistical significance are therefore not presented in detail (available on request).

B.6 Capital flow analysis using alternative geography proxies

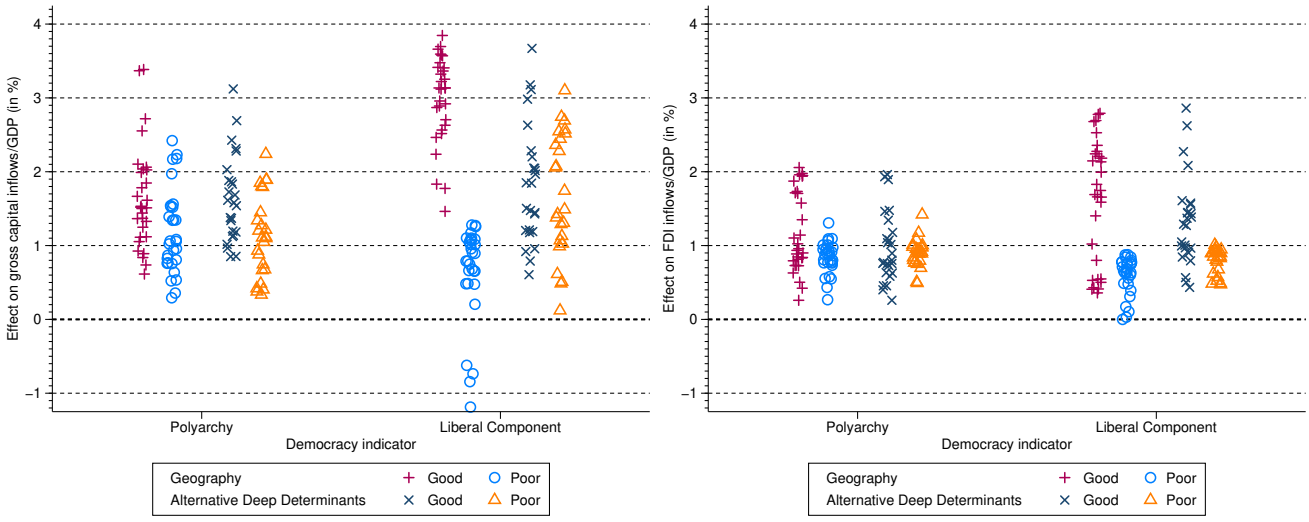
Table B-7: Total Capital Inflows – Alternative Proxies of Geography (1975-2015)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Landlocked		**High UV Radiation**		**Few Frost Days**		**Low Ag-Suitability**	
	No	Yes	No	Yes	No	Yes	No	Yes
ANRR democracy	1.286***	0.129	1.465*	0.597*	1.915*	0.589*	1.022**	0.461
	[0.390]	[0.700]	[0.799]	[0.339]	[1.039]	[0.333]	[0.465]	[0.495]
Treated Countries	50	18	23	42	21	44	37	28
Treated Observations	1564	500	654	1329	582	1401	1140	843
Control Countries	22	7	15	14	15	14	8	21
Control Observations	663	128	380	397	390	387	177	600
χ^2 test (p)	0.78	0.02	0.45	0.30	0.61	0.65	0.78	0.73
Alpha test (t)	-0.06	-7.23	-0.66	-2.04	-0.50	-3.07	-1.42	-2.20
Alternative factor augmentation								
2 factors	1.262***	-0.012	1.228*	0.573	1.715*	0.620*	1.135**	0.649
3 factors	1.324***	0.063	1.568**	0.631*	1.803**	0.617*	1.021**	0.521
4 factors	1.286***	0.129	1.465*	0.597*	1.915*	0.589*	1.022**	0.461
5 factors	1.112***	0.186	1.909**	0.291	1.984**	0.599	0.683	0.500
6 factors	1.137***	0.186	1.967**	0.441	1.735*	0.704**	0.531	0.664
Liberal Democracy	1.617***	0.848	2.321***	0.794	2.786***	0.827	1.418***	1.693**
Index > median	[0.490]	[0.913]	[0.860]	[0.569]	[1.078]	[0.631]	[0.495]	[0.673]
Treated Countries	37	13	21	27	21	27	32	16
Treated Observations	1380	428	704	1015	678	1041	1117	602
Control Countries	42	14	19	33	18	34	19	33
Control Observations	1405	346	547	1086	517	1116	574	1059
χ^2 test (p)	0.62	0.06	0.97	0.15	0.81	0.40	0.63	0.53
Alpha test (t)	-1.17	-1.32	0.73	0.51	0.34	0.54	0.52	0.52
Alternative factor augmentation								
2 factors	1.853***	1.368**	1.667*	1.093**	1.633*	1.023**	1.497***	2.063***
3 factors	1.714***	0.938	1.510*	0.603	2.372**	0.878*	1.424***	1.679***
4 factors	1.617***	0.848	2.321***	0.794	2.786***	0.827	1.418***	1.693**
5 factors	1.712***	0.910	2.499***	0.848	3.167***	0.734	1.270***	1.850***
6 factors	1.624***	1.130	2.394**	0.500	2.678**	0.715	1.181***	1.519***
Polyarchy	1.891***	0.126	0.276	1.064**	1.684	1.063**	0.903*	1.610***
Index > median	[0.433]	[0.762]	[0.732]	[0.414]	[1.052]	[0.463]	[0.516]	[0.605]
Treated Countries	45	14	24	32	23	33	36	20
Treated Observations	1665	451	800	1209	734	1275	1256	753
Control Countries	34	13	15	28	15	28	14	29
Control Observations	1120	323	433	892	443	882	417	908
χ^2 test (p)	0.38	0.32	1.00	0.14	0.88	0.22	0.45	0.79
Alpha test (t)	-3.12	-1.91	-0.94	0.39	-1.04	0.23	-0.73	-1.62
Alternative factor augmentation								
2 factors	1.858***	0.922	0.716	1.471***	1.063	1.361***	1.018*	1.685**
3 factors	1.541***	0.704	0.942	1.243***	1.446	1.071**	0.881*	1.577**
4 factors	1.891***	0.126	0.276	1.064**	1.684	1.063**	0.903*	1.610***
5 factors	1.830***	0.232	0.574	1.079**	1.513	0.700*	0.939*	1.846***
6 factors	1.743***	0.279	0.976	1.143***	1.601	0.672*	0.954	1.384**
Liberal Component	1.815***	0.574	2.375***	0.816	2.750***	0.881*	1.102*	2.004***
Index > median	[0.669]	[0.907]	[0.872]	[0.531]	[0.925]	[0.534]	[0.668]	[0.567]
Treated Countries	43	13	24	30	22	32	30	24
Treated Observations	1606	449	820	1138	745	1221	1069	897
Control Countries	31	12	15	27	15	27	18	24
Control Observations	1000	287	401	867	412	856	544	724
χ^2 test (p)	0.61	0.01	0.33	0.83	0.41	0.84	0.48	0.92
Alpha test (t)	0.59	-2.81	0.02	0.67	-0.21	0.62	0.67	-0.86
Alternative factor augmentation								
2 factors	2.241***	1.221	2.172**	1.118**	2.456***	1.266**	1.551**	2.051***
3 factors	1.877***	0.676	2.550***	0.727	2.830***	0.93	1.192*	2.097***
4 factors	1.815***	0.574	2.375***	0.816	2.750***	0.881*	1.102*	2.004***
5 factors	1.803***	0.53	2.126**	0.803	2.640***	0.901	1.177*	2.273***
6 factors	1.726***	0.624	2.240**	0.014	2.843***	0.218	1.032*	1.522***

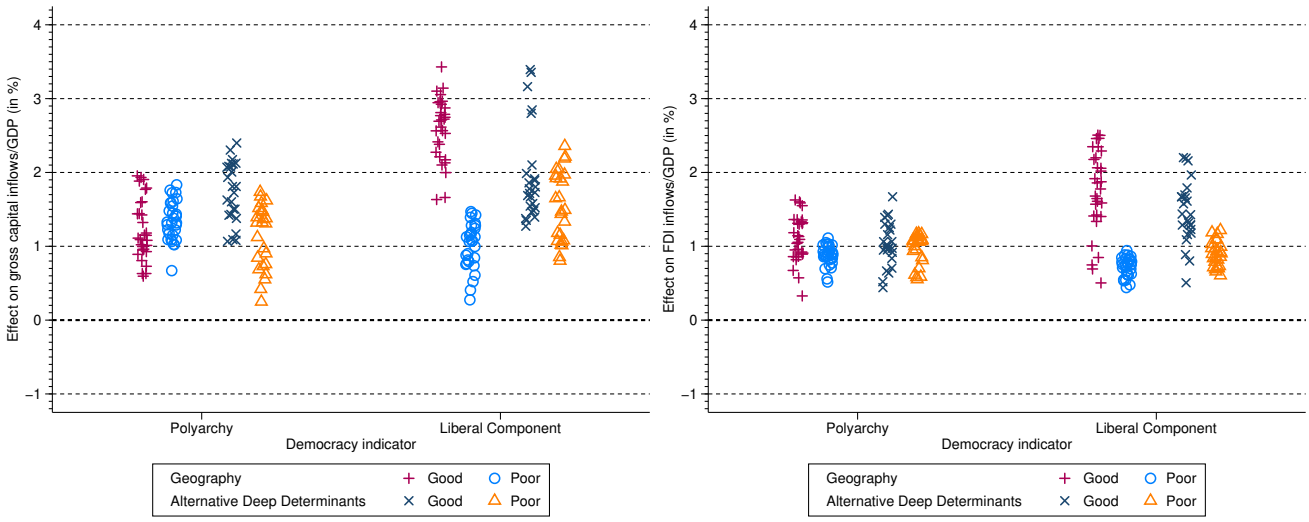
Notes: This table presents the analysis for a number of alternative proxies for geography — whether a country is landlocked, high UV radiation exposure, low number of frost days per year, and low suitability for agriculture. These results are for the model with exports/trade as additional control.

B.7 Analysis using building blocks of liberal democracy

Figure B-4: Building Blocks of Liberal Democracy



(a) Total Non-Official Capital (left) and FDI (right) Inflows – no controls



(b) Total Non-Official Capital (left) and FDI (right) Inflows – exports/trade as control

Notes: The plots present robust ATET (Mean Group PCDID) estimates for the causal effect of democracy on capital inflows by geography (+ and o for 'good' and 'poor' geography, respectively) and alternative deep determinants (x for non-French LO/proximate culture/no colonial experience and △ for French LO/distant culture/colonial experience). We distinguish polyarchy and the liberal component, the two building blocks of liberal democracy. See notes to Figure 4 for all other details. Diagnostic tests and statistical significance of ATETs are reported in Appendix Tables B-1 and B-2.

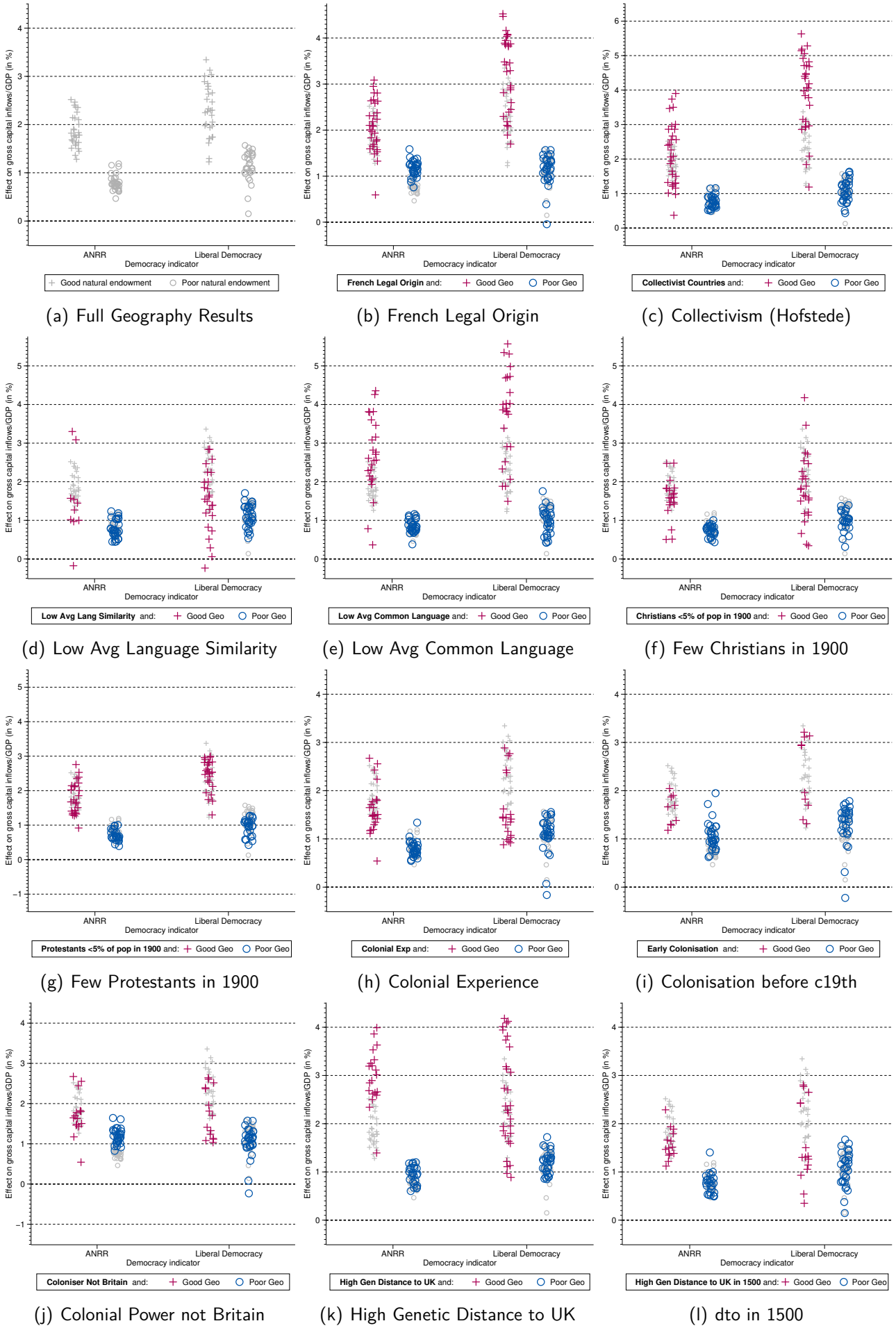
C Deep Determinants within Geography Samples

Table C-1: Alternative Deep Determinants within Geography — Sample sizes

	LO	Culture					History			Genetics	
N	French	Hofst	CL	LS	Christ	Prot	Col	Col c19	Non-Brit	UK	UK c16
Countries with Good Geography											
2	0	0	0	0	0	0	0	0	0	0	0
3	0	0	0	0	0	0	0	0	0	0	0
4	0	0	0	0	0	0	0	10	5	0	0
5	0	0	0	0	0	0	0	5	10	0	15
6	0	0	5	5	0	0	10	5	0	0	0
7	0	0	10	5	10	0	0	5	10	25	10
8	0	20	10	10	10	0	15	0	5	0	0
9	0	10	0	5	5	0	0	5	0	5	0
10	5	0	5	5	0	0	5	0	0	0	5
11	15	0	0	0	0	0	0	0	0	0	0
13	10	0	0	0	0	0	0	0	0	0	0
19	0	0	0	0	0	20	0	0	0	0	0
20	0	0	0	0	0	5	0	0	0	0	0
Sum> 9	30	10	5	10	5	25	5	5	0	5	5
Sum> 7	30	30	25	25	25	25	20	10	15	30	15
Countries with Poor Geography											
11	0	0	0	0	5	0	0	0	0	0	0
12	0	0	0	0	15	0	0	0	0	0	0
13	0	0	0	0	5	0	0	0	0	0	0
14	0	0	0	5	0	0	0	5	0	0	0
15	0	0	5	5	0	0	0	5	0	0	0
16	5	10	0	15	0	0	0	5	5	0	0
17	0	20	15	5	0	0	0	0	5	0	0
18	5	0	5	0	0	0	0	5	5	5	0
19	0	0	5	0	0	0	0	10	0	5	5
20	15	0	0	0	0	0	0	0	10	20	0
21	5	0	0	0	0	0	0	0	5	0	5
22	0	0	0	0	0	5	0	0	0	0	5
23	0	0	0	0	0	0	10	0	0	0	0
24	0	0	0	0	0	5	0	0	0	0	15
25	0	0	0	0	0	15	10	0	0	0	0
27	0	0	0	0	0	0	10	0	0	0	0
Sum	30	30	30	30	25	25	30	30	30	30	30

Notes: This table presents the number of treated countries N (in rows) in the subsample analysis by legal origin, culture, history, and genetics in Section **??** of the maintext. The entries report the number of specifications, e.g. the '5' for 'French LO' in the row marked '10' of the 'Good Geography' panel means that there were 5 specifications with 10 countries with good geography which had 'French legal origin'. Note that we exclude specifications from analysis in Section **??** if $N < 9$ and that we focus on the ANRR and liberal democracy definitions of regime change only. Below, in Figures C-1 and C-2, we exclude $N < 7$ as a robustness check — dotted lines highlight these two sample restrictions. Cultural proxies: Hofst is the dummy for 'collectivist' countries, CL is low average common language and LS low language similarity. Historical proxies: Col indicates any colonial experience, Col c19th is for colonisation prior to the 19th century, Non-Brit indicates if the (most enduring) colonial power was *not* Britain. Genetic proxies: UK is genetic distance from the UK, UK c16 is the genetic distance from the UK in 1500.

Figure C-1: Alternative Deep Determinants 'within' Geography – gross capital inflows



(a) Full Geography Results (b) French Legal Origin (c) Collectivism (Hofstede)

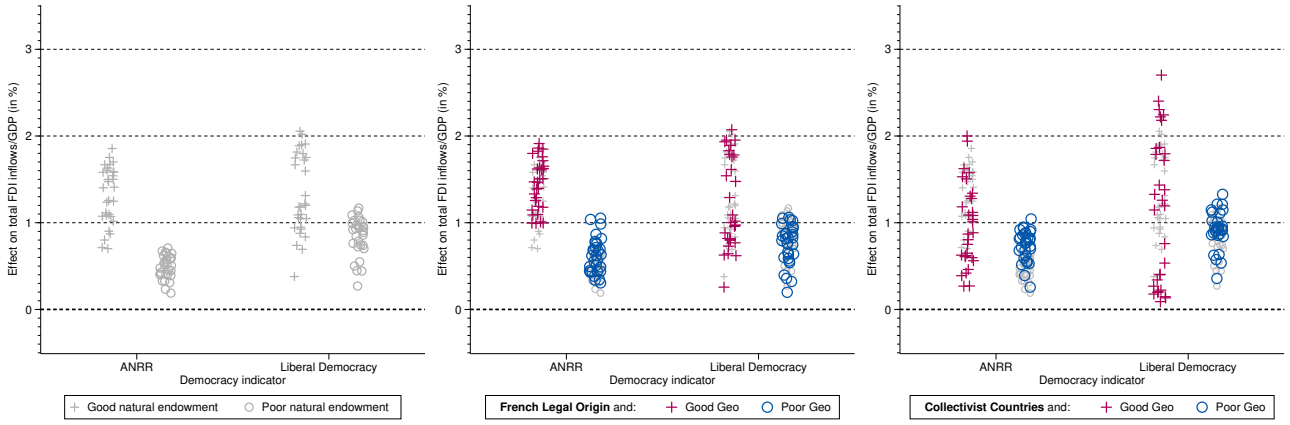
(d) Low Avg Language Similarity (e) Low Avg Common Language (f) Few Christians in 1900

(g) Few Protestants in 1900 (h) Colonial Experience (i) Colonisation before c19th

(j) Colonial Power not Britain (k) High Genetic Distance to UK (l) dto in 1500

Notes: The plots present robust ATET estimates for the causal effect of democracy on capital inflows by geography (in grey, as in Figure 4) and by alternative deep determinants (in colour) *within* these geographical groupings. Minimum of 7 countries for ATET.

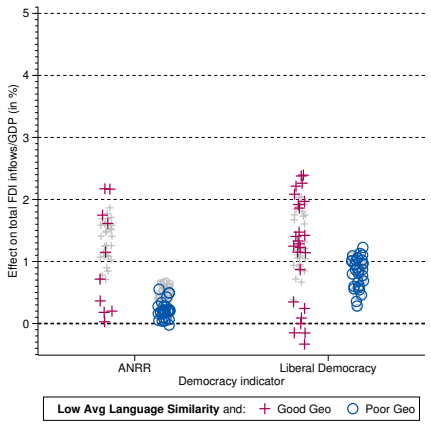
Figure C-2: Alternative Deep Determinants 'within' Geography – FDI inflows



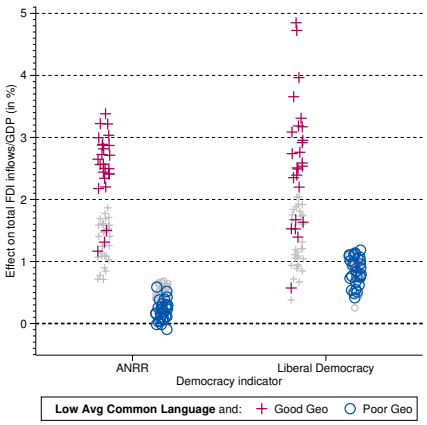
(a) Full Geography Results

(b) French Legal Origin

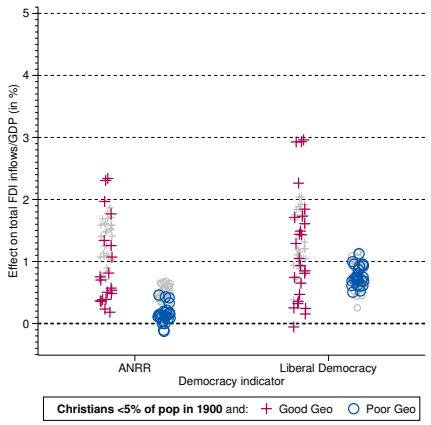
(c) Collectivism (Hofstede)



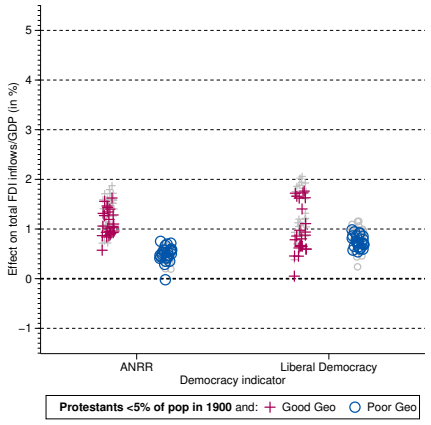
(d) Low Avg Language Similarity



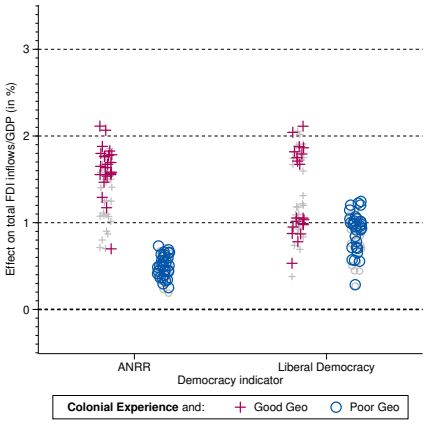
(e) Low Avg Common Language



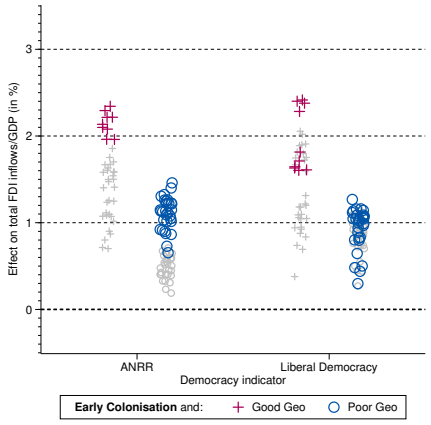
(f) Few Christians in 1900



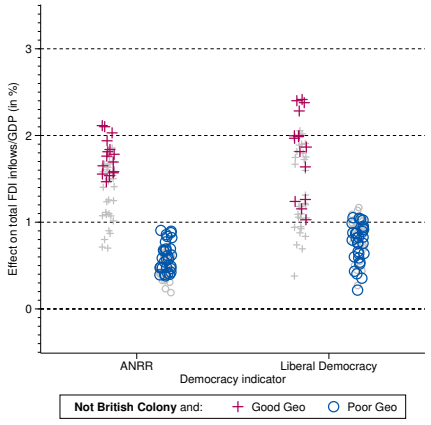
(g) Few Protestants in 1900



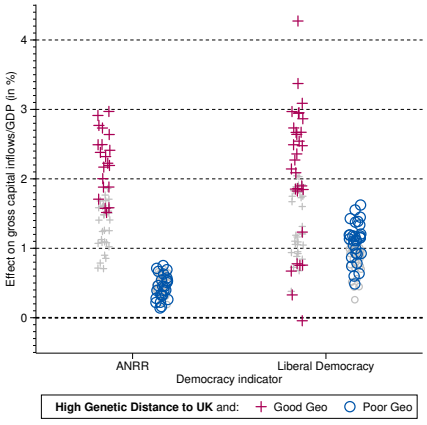
(h) Colonial Experience



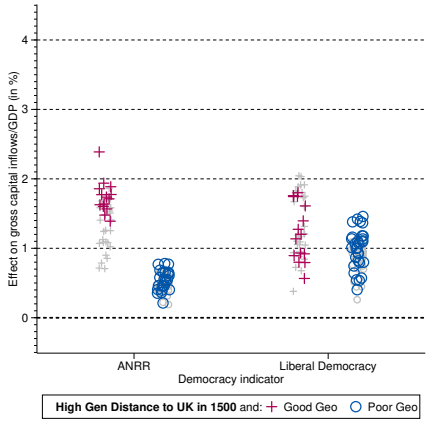
(i) Colonisation before c19th



(j) Colonial Power not Britain



(k) High Genetic Distance to UK



(l) dto in 1500

Notes: The plots present robust ATET estimates for the causal effect of democracy on FDI inflows by geography (in grey, as in Figure 4) and by alternative deep determinants (in colour) *within* these geographical groupings. Minimum of 7 countries for ATET.