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Democratization and FDI dynamics: Counterfactual evidence from Indonesia

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ABSTRACT

This paper examines how a discrete transition from autocracy to democracy affects FDI inflows, shifting the focus from cross-country average marginal effects to within-country dynamic adjustment. Using Indonesia's 1998 democratic transition as a case study, the analysis applies low-rank panel methods – matrix completion and interactive fixed effects counterfactual estimator – to construct a counterfactual trajectory of FDI inflows under continued autocracy. The results reveal a pronounced and economically large decline in FDI inflows relative to the counterfactual in the immediate post-transition period. This negative treatment effect diminishes over time, with observed FDI inflows converging toward the counterfactual path in the long run. However, no sustained democratic premium is observed within the sample period. These findings suggest that the impact of democratization on FDI inflows in Indonesia follows a time-dependent adjustment pattern, consistent with the cross-country trajectory identified by Lacroix et al. (2021), rather than reflecting a uniform effect. A range of robustness checks supports the stability of the findings, including placebo-in-time and equivalence tests, alternative treatment timings, donor pool sensitivity analyses, and variations in the pretreatment period.

KEYWORDS

Indonesia, democratization, FDI, counterfactual analysis, low-rank structure

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1. INTRODUCTION

Whether democracies attract more foreign direct investment (FDI) than autocracies remains one of the most debated – and unresolved – questions in political economy. Despite decades of research, there is no clear empirical consensus. On the one hand, democracies are often argued to provide secure property rights, transparent institutions, and credible commitments that foreign investors value (e.g., Jensen, 2003). On the other hand, autocracies are sometimes argued to be equally – if not more – attractive to investors when they can offer political stability, decisive policymaking, and insulation from redistributive pressures (e.g., Asiedu & Lien, 2011; Li & Resnick, 2003). This lack of agreement is not merely academic, as "attracting FDI has become one of the integral parts of economic development strategies in many countries" (Jensen, 2003, p. 587).

A central limitation of the existing literature is its predominant focus on the *level* of democracy. Most empirical studies examine the relationship between marginal changes in democracy scores and FDI inflows, typically using cross-country or panel regressions. While this approach has generated a large body of evidence, its findings are highly sensitive to specification choices, measurement strategies, and sample composition, resulting in mixed and often contradictory conclusions (Desbordes & Verardi, 2017). A major issue in this literature is that, by focusing on average marginal effects, it implicitly assumes a uniform effect of the *level* of democracy. As a result, it conflates discrete regime transitions with incremental changes occurring within an existing democracy or autocracy. Accordingly, this line of the literature abstracts from the process of democratic change itself.

This limitation motivates the present study. Accordingly, rather than asking whether more democracy is associated with higher or lower FDI inflows on average, this paper shifts the focus to a complementary question: how do foreign investments respond over time to the replacement of an autocratic regime? This shift in perspective draws on theories of political transitions, which emphasize that regime change can temporarily increase uncertainty even if it improves long-run institutional credibility (Acemoglu et al., 2019; Persson & Tabellini, 2009). Olson's (1993) theory highlights a potential trade-off between the long-run stability of (dynastic) autocratic regimes and the gradual development of credible institutions under democracy that support investment.

Despite the theoretical importance of these dynamics, empirical evidence on the effects of discrete regime transitions on FDI inflows remains limited. Only a few studies examine this question explicitly, among which Lacroix et al. (2021) is a prominent contribution. Their analysis highlights the importance of the nature of the regime change, showing that while democratic transitions on average do not lead to an increase in FDI inflows, consolidated transitions – those that do not revert to autocracy within the first five years – eventually generate a significant increase in FDI inflows in the long run. Specifically, the findings suggest that the credibility gains associated with democracy materialize only after a period of institutional stabilization, typically taking about ten years to emerge. However, by relying on cross-country variation and methods like difference-in-differences or propensity score matching, Lacroix et al. (2021) provide a pooled estimate of the two types of transition but do not explicitly trace the counterfactual trajectory of a specific transition over time.

To complement these insights, this paper adopts a counterfactual framework to examine the dynamic effects of a discrete transition from autocracy to democracy. The core contribution lies in providing a within-country, counterfactual-based characterization of the full adjustment path following a regime change. Specifically, by shifting the unit of analysis from cross-country averages to a continuous counterfactual trajectory, the paper moves beyond a binary "positive or negative" effect. It traces how FDI inflows evolve relative to a scenario in which the prior regime persists.

Indonesia offers a particularly informative case. For more than three decades under President Suharto[1] the country was governed by a highly centralized autocracy with "sultanistic" features (Fukuoka, 2012). While politically repressive, the regime delivered relative macroeconomic stability and a predictable policy environment.[2] The collapse of the Suharto regime in 1998 marked the beginning of a rapid and far-reaching democratic transition, transforming Indonesia into one of the largest democracies in the developing world.[3] This episode provides a natural setting to examine a counterfactual question: how would FDI inflows have evolved had the authoritarian regime persisted?

However, the regime change almost coincided with the 1997 Asian Financial Crisis, which affected Indonesia with exceptional severity (McLeod, 2000). This overlap creates a key identification challenge: observed post-transition dynamics may reflect both democratization and crisis-related effects. To address this challenge, the paper adopts a counterfactual framework based on low-rank panel methods (Athey et al., 2021; Bai, 2009; Candès & Recht, 2009; Xu, 2017), employing two estimators: matrix completion and the interactive fixed-effects counterfactual estimator. These methods model outcomes as driven by a low-dimensional latent structure capturing common shocks while allowing for heterogeneous responses across countries. However, the methods' ability to recover a credible counterfactual is, in fact, an empirical question rather than an assumption. The analysis, therefore, places strong emphasis on validation and robustness, including placebo-in-time and equivalence tests, alternative treatment timings, leave-one-out donor pool sensitivity checks, donor group restrictions, shorter pre-treatment windows, and leave-one-out tests for pre-treatment years. In addition, cross-validation is used to determine model complexity. Across these exercises, the estimated treatment effects remain stable in sign, magnitude, and dynamic pattern.

The empirical results reveal a clear and robust dynamic pattern. Following democratization, FDI inflows fall substantially below the counterfactual benchmark, with the largest negative effects occurring in the immediate post-transition period. The negative treatment effects diminish over time and the observed FDI inflows converge toward the counterfactual trajectory. This suggests that the regime change in Indonesia does not appear to generate a clear "democratic premium" over the prior autocratic regime's performance within the observed period.

The dynamics of the counterfactual FDI inflows are consistent with Indonesia's post-1998 political trajectory. The early democratic period was marked by political instability,

[1]Indonesia became independent in 1945 and initially functioned as a democracy, albeit an unstable one, until 1957. The first president, Sukarno, transformed the democratic system into a "guided democracy," in which the main sources of power were the military and the Indonesian Communist Party (Webber, 2006). Suharto became the country's second president following Sukarno's resignation.

[2]Note that the Indonesian economy has experienced sustained growth since 2000, averaging 5–6% per year, and its FDI inflows as a % of GDP have been consistently high, comparable to China (own calculation based on World Bank, 2025).

[3]Indonesia's regime change following the collapse of the Suharto regime in 1998 was successful, as evidenced by the fact that its *polity2* score shifted from −7 before 1998 to +8 thereafter and has remained at that level. Note that a score of at least +6 is required to be classified as a democracy.

fragmentation, weak party discipline, separatist conflicts, and persistent corruption (Webber, 2006). For foreign investors, the environment was highly uncertain: the authoritarian promise of policy stability had disappeared, while the credibility of the new democratic institutions had not yet been fully established. Over time, however, the introduction of competitive elections, decentralized governance through the "big bang" laws (Aspinall & Fealy, 2003), and the establishment of new institutions – most notably the Constitutional Court and the Corruption Eradication Commission in 2002 (Butt & Lindsey, 2010) – coincided with a gradual adjustment in foreign investment behavior, mirroring the attenuation of the negative estimated treatment effect.

The paper's contribution is twofold. First, by providing a counterfactual-based analysis of Indonesia's 1998 democratic transition, it complements the existing literature, which has primarily documented observed FDI trends without constructing an explicit benchmark of how investment would have evolved under continued autocratic rule (e.g., Lindblad, 2015; MacIntyre, 2001; McLeod, 2000). Second, this study extends the findings of Lacroix et al. (2021) by tracing the full within-country adjustment path following democratization, rather than examining its two types – transition and consolidation – in isolation.

The remainder of this paper proceeds as follows. Section 2 reviews the empirical literature on the relationship between democracy and FDI. Section 3 outlines the empirical strategy and data. Section 4 presents the results and robustness checks. Section 5 discusses the findings, emphasizing the distinction between short- and long-run trajectories. Section 6 concludes.

2. BRIEF REVIEW OF THE LITERATURE

At a broad level, the literature on the relationship between democracy and FDI can be traced back to two influential early contributions, Jensen (2003) and Li and Resnick (2003). Building on these foundations, the literature developed two contrasting perspectives on how democracy affects FDI inflows. One strand argues that democratic institutions enhance FDI by improving policy credibility, transparency, and the protection of property rights. The other emphasizes that democracy may deter investment due to political competition, regulatory constraints, and increased policy uncertainty. As a result, the empirical literature has not reached a clear consensus on the direction or magnitude of the effect. A key reason for this divergence is that empirical studies differ substantially in econometric specification, measurement, and sample composition, leading to widely varying estimates (Desbordes & Verardi, 2017). Despite these differences, most contributions share a common feature: they conceptualize democracy primarily through a *level* variable and examine its cross-country variation at a given point in time.

The first line of research highlights the potentially ambiguous or even negative effects of democracy on FDI. Early contributions argue that investors may prefer autocracies because they provide a "high degree of future certainty concerning the factors decisive for determining the results of investment decisions" (Resnick, 2001, p. 385). Li and Resnick (2003) refine this argument by showing that democracy has countervailing effects: while stronger property rights protection encourages FDI, democratic constraints may deter investment through stricter regulation, labor protections, limits on investor incentives, and pressures to protect domestic firms. However, once property rights protection is accounted for, the relationship between democracy and FDI inflows turns negative – a phenomenon the authors refer to as a "reversal of fortune".

Subsequent studies find similarly mixed results. Some papers report insignificant or conditional effects of democracy depending on model specification and institutional context (Mathur & Singh, 2011; Yang, 2007). Meta-analytical evidence further highlights that the democracy–FDI relationship is mediated by multiple channels, including political risk, labor costs, and capital controls, and varies depending on how FDI is measured (Li et al., 2018). More recent work emphasizes that the effect of democracy is highly context-dependent, varying with development level, sectoral composition, and institutional environment (Adam & Filippaios, 2007; Holmes et al., 2025; Rommel et al., 2025).

As a contrast to this strand of the literature, Jensen's (2003) seminal contribution emphasizes the role of veto players and audience costs in constraining arbitrary policy change, finding large positive effects of democracy on FDI inflows. Subsequent work refines and qualifies this argument. Ahlquist (2006) shows that FDI is more sensitive to democratic institutions than portfolio investments, though the estimated effects are modest. Several replication and reexamination studies argue that earlier negative findings are driven by measurement choices, outliers, and functional form assumptions (Choi, 2009; Choi & Samy, 2008; Jakobsen & Soysa, 2006). Once these issues are addressed, democracy typically exhibits a positive association with FDI. Other contributions demonstrate that democracy's effect is conditional on income levels, natural resource dependence, and the structure of host economies (Asiedu & Lien, 2011; Doces, 2010; Guerin & Manzocchi, 2009). More recent studies further refine democracy measures, showing that effective or practiced democracy – rather than formal institutions alone – can enhance FDI inflows (Basu et al., 2023; Desbordes & Verardi, 2017).

Despite differences in findings, most empirical studies rely on cross-country or panel regressions that estimate the average marginal effect of democracy *level* on FDI inflows. While methodological refinements – such as fixed effects (Adam & Filippaios, 2007), dynamic panels (e.g., Doces, 2010), gravity models (e.g., Guerin & Manzocchi, 2009), and robust estimators (e.g., Yang, 2007) – have improved identification, the central conceptual framework has remained largely unchanged. A common limitation is the implicit assumption in most studies that changes in democracy *level* have uniform effects, regardless of whether they occur during periods of transition or within already stable democratic environments. Accordingly, this approach abstracts from the possibility that political regimes evolve through discrete transitions, where short-run adjustment and long-run consolidation may have very different implications for investment.

An important exception is Lacroix et al. (2021), who investigate the impact of democratic transitions on FDI inflows using difference-in-differences regression on a broad panel of 115 developing countries between 1970 and 2014. Their findings challenge the notion of a uniform "democratic premium" by showing that democratic transitions do not, on average, increase FDI inflows. Instead, they identify a crucial distinction between transition and consolidation types of regime change, arguing that positive investment effects materialize only when transitions become consolidated, meaning they do not revert to autocracy within the first five years. The authors argue that while democratic institutions are inherently attractive to foreign investors, the initial phase of democratization is often overshadowed by heightened political risk and the threat of institutional reversal. According to their analysis, it typically takes about ten years following a transition for a statistically significant increase in FDI inflows to emerge. However, when controlling for subjective measures of political risk, they find that the underlying effect of democratization is positive even in the short run, suggesting that the initial stagnation of FDI inflows is primarily driven by investor uncertainty about the stability of the new regime rather than the democratic institutions themselves.

The present study shifts the focus from democracy *levels* to the dynamic effects of regime change, and drawing on Lacroix et al. (2021), it examines how foreign investment evolves after democratization relative to a counterfactual scenario in which the previous regime persists in Indonesia.

3. EMPIRICAL STRATEGY AND DATA

3.1. Why a low-rank model?

This section outlines the empirical approach used to analyze how Indonesia's 1998 transition from autocracy to democracy affected FDI inflows. When addressing this question, the key empirical objective is to recover the counterfactual trajectory of Indonesia's FDI inflows in the absence of democratization. This requires constructing an estimate of what would have happened in the absence of the regime change, using information from a set of donor (untreated) countries.

However, the presence of large global macroeconomic shocks – most notably the 1997 Asian Financial Crisis and the 2008 Global Financial Crisis – poses challenges for commonly used counterfactual methods. Specifically, in difference-in-differences (DID) and two-way fixed effects (TWFE) methods, identification relies on the parallel trends assumption, which may be violated when countries are differentially affected by large common shocks. The synthetic control method (SCM) (Abadie, 2021; Abadie et al., 2010) uses fixed unit weights, which limits its ability to capture asymmetric shocks. Its extensions, such as the augmented synthetic control method (ASCM) (Ben-Michael et al., 2021) and the synthetic difference-in-differences method (SDID) (Arkhangelsky et al., 2021), may also be restrictive in the present case. More specifically, in ASCM, the bias correction relies on relatively smooth adjustments to improve pre-treatment fit, which may be insufficient to capture large shifts in the outcome variable, as may be the case in Indonesia. As for SDID, it builds on the assumption that a combination of weighting and common trend adjustment can approximate the counterfactual path, which may be violated when the treated unit experiences a large, idiosyncratic shock with a nonlinear recovery pattern.

Due to these concerns, this study adopts an imputation-based counterfactual framework based on a low-rank representation of the outcome matrix, which provides a flexible way to capture heterogeneous and time-varying responses to common shocks. Two complementary estimators are employed: matrix completion (MC) and the interactive fixed effects counterfactual estimator (IFEct). The following section presents the econometric model, the empirical strategy and the data.

3.2. Econometric model

Both MC and IFEct estimators belong to the broader class of low-rank panel data methods used in causal inference with panel data (Athey et al., 2021; Bai, 2009; Xu, 2017). Both methods rely on the assumption that the counterfactual outcome matrix can be approximated by a latent low-dimensional structure. However, they differ substantially in how this latent structure is modeled and estimated. The IFEct estimator is based on an explicit factor model in which the latent component is parameterized through interactive fixed effects (Bai, 2009). By contrast, the MC estimator does not impose a specific factor decomposition; instead, it treats the recovery of missing counterfactual outcomes as a low-rank matrix recovery problem solved through nuclear norm regularization (Athey et al., 2021; Candès & Recht, 2009).

Let Y_{it} denote the observed outcome for unit $i = 1, \ldots N$ and time $t = 1, \ldots T$, and let $Y \in \mathbb{R}^{N \times T}$ denote the full outcome matrix whose entries are Y_{it}. In causal panel settings with treatment, some entries – namely, post-treatment outcomes for treated units under no treatment (i.e., counterfactual outcomes) – are unobserved and must be recovered from the data. The counterfactual outcome matrix is assumed to be well approximated by a latent low-rank structure. Formally, the structural component of the outcome matrix can be written as a low-rank matrix $\mathbf{L}$, with $\text{rank}(\mathbf{L}) = r \ll \min(N, T)$.

Let $Y_{it}(0)$ denote the counterfactual outcome in the absence of treatment, given by (Athey et al., 2021; Candès & Recht, 2009)

$$Y_{it}(0) = X_{it}'\beta + L_{it} + \varepsilon_{it},$$

where X_{it} is a vector of observed covariates, L_{it} is an unobserved latent component capturing systematic variation across units and over time, and ε_{it} is an idiosyncratic disturbance term. The latent component L_{it} is assumed to be well-approximated by a low-rank structure, meaning that the counterfactual outcome matrix can be represented by a small number of underlying latent dimensions driving common dynamics across units and periods.

Observed outcomes deviate from the counterfactual outcomes due to treatment exposure. The observed outcome is given by (Athey et al., 2021)

$$Y_{it} = Y_{it}(0) + \tau_t D_{it},$$

where D_{it} is a treatment indicator which equals one for treated units in post-treatment periods and zero otherwise, and τ_t denotes a time-varying average treatment effect on the treated (ATT).[4] Combining these expressions yields the reduced-form representation

$$Y_{it} = \tau_t D_{it} + X_{it}'\beta + L_{it} + \varepsilon_{it},$$

which serves as a general representation of the observed outcome under latent low-rank structure assumptions commonly used in the causal panel literature. The MC and IFEct estimators differ in how the latent component is modeled and estimated.

The IFEct imposes a specific factor structure on the latent component L_{it}:

$$L_{it} = \alpha_i + \delta_t + \lambda_i' f_t,$$

where α_i are unit fixed effects, δ_t are time fixed effects, λ_i represents unit-specific loadings[5] and f_t denotes time-varying latent factors[6] (Bai, 2009). Under this formulation, IFEct corresponds to a structured, factor-based representation of the same underlying low-rank structure (Xu, 2017).

[4]The time-varying treatment effect τ_t is consistent with the standard low-rank panel literature, where treatment effects are typically identified at the aggregate (time) level rather than at the unit-time level, while unit-level heterogeneity is absorbed by the latent factor structure (Athey et al., 2021; Bai, 2009; Xu, 2017).

[5]They capture heterogeneous impacts arising from each unit's unobserved characteristics.

[6]Latent factors are to be understood as time-varying trends that affect each unit differently.

This specification relies on the following assumptions (Bai, 2009, pp. 1240–1243).

Assumption 1: low-rank factor structure. The outcome matrix admits an approximate low-rank representation, meaning that a small number of latent common factors drive the evolution of outcomes across units and over time.

Assumption 2: sufficient variation in data. Identification of the factor structure requires sufficient cross-sectional and temporal variation in the data.

Assumption 3: weak dependence in idiosyncratic errors. The idiosyncratic error term may exhibit heteroskedasticity and weak serial and cross-sectional dependence, but such dependence is sufficiently limited relative to the latent common component.

Assumption 4: exogeneity. The idiosyncratic error term is assumed to be mean-independent of the regressors and latent factors, ruling out systematic dependence between the idiosyncratic error term and the regressors.

Under these conditions, the latent component can be consistently recovered together with the counterfactual outcome path in IFEct.

The MC method does not impose an explicit factor decomposition on L_{it}; instead, it assumes only that the latent outcome matrix is approximately low-rank and recovers it using nuclear norm regularization (Athey et al., 2021; Candès & Recht, 2009). The estimator solves the following regularized optimization problem:

$$\min_{\mathbf{L}} \left\{ \sum_{(i,t)\in\Omega} (Y_{it} - L_{it})^2 + \lambda \parallel \mathbf{L} \parallel_* \right\}$$

where Ω denotes the set of observed outcomes used for estimation, consisting of both pre- and post-treatment observations for untreated units and pre-treatment observations for the treated units, $\parallel \mathbf{L} \parallel_*$ is the nuclear norm (the sum of the singular values of $\mathbf{L}$), and $\lambda>0$ is a regularization parameter controlling the effective rank of the solution (Athey et al., 2021; Candès & Recht, 2009). The nuclear norm acts as a convex surrogate for the rank constraint, encouraging a low-rank approximation while allowing the data to determine the complexity of the underlying structure.

Once the latent low-rank component of the outcome matrix, denoted $\widehat{\mathbf{L}}$, has been estimated from the observed data, the missing counterfactual outcomes are recovered as

$$\widehat{Y}_{it}(0) = \widehat{L}_{it}, \text{for } (i,t) \notin \Omega,$$

where $\widehat{Y}_{it}(0)$ represents the estimated counterfactual outcome matrix obtained from the low-rank model. The treatment effect is then defined as the deviation between the observed outcome and its estimated counterfactual counterpart:

$$\widehat{\tau}_{it} = Y_{it} - \widehat{Y}_{it}(0)$$

where $\widehat{\tau}_{it}$ denotes the estimated treatment effect for unit i in period t.

Identification in the MC framework similarly relies on the assumption that the counterfactual outcome matrix is approximately low-rank and that the observed entries contain sufficient information to recover the latent structure (Athey et al., 2021; Candès & Recht, 2009; Xu, 2017). In causal panel data applications, an additional requirement of sufficient overlap between treated and untreated units over time is typically imposed. In practice, this means that identification relies on observing (1) pre-treatment outcomes for treated units and (2) both pre- and post-treatment outcomes for untreated units, which together provide information about the latent low-rank structure used to reconstruct missing counterfactual outcomes (e.g., Athey et al., 2021; Xu, 2017).

In summary, both IFEct and MC estimators assume that the counterfactual outcome matrix can be approximated by a low-rank representation. However, they differ fundamentally in how this structure is modeled and estimated: IFEct imposes an explicit interactive factor structure following Bai (2009), while MC treats the latent structure more flexibly and recovers it through nuclear norm regularization without specifying the number or form of latent factors (Athey et al., 2021; Xu, 2017).

3.3. Estimation strategy

A central feature of the empirical strategy is that Indonesia's democratic transition almost coincides with the 1997 Asian Financial Crisis, while approximately a decade later, the 2008 Global Financial Crisis represents an additional major macroeconomic shock. However, these two episodes differ in their role for identification within the low-rank model. The 1997 crisis may raise identification concerns because it was particularly severe in Indonesia relative to countries in the donor pool[7] (IMF, 2007), and occurs in close temporal proximity to the treatment year, potentially confounding the effects of democratization with crisis-related dynamics. By contrast, the 2008 Global Financial Crisis occurs entirely within the post-treatment period and therefore does not affect the identification of the initial treatment effect in 1998. Given its global nature, it can be viewed as a typical macroeconomic shock with heterogeneous effects across countries.[8] In this sense, it corresponds more closely to the type of globally shared macroeconomic shock that low-rank models are designed to accommodate through latent common components (Athey et al., 2021; Xu, 2017), and is therefore less likely to threaten identification of the initial 1998 treatment effect.

Building on the above, the key identification issue – namely, whether the effects of the 1997 crisis can be adequately "separated" from those of democratization – should be addressed explicitly. Within the low-rank model, this ultimately depends on whether the latent low-rank

[7]The formation of the donor pool is discussed in Section 3.4; the full list of donor countries is reported in Table A/1 in the Appendix.

[8]As emphasized by Eichengreen (2010), the 2008 Global Financial Crisis constituted a global macro-financial shock that transmitted unevenly across emerging markets, with country-specific outcomes driven by differences in financial openness, external financing dependence, and macroeconomic policy space. Concerning its impact on donor countries, in emerging Asia, the transmission channel operated mainly through a collapse in external demand. However, the macroeconomic impact was partly mitigated by large-scale fiscal and monetary stimulus as well as stronger domestic demand conditions (Eichengreen, 2010). For instance, the shock was comparatively mild in Indonesia (Athukorala & Chongvilaivan, 2010); by contrast, Vietnam was more exposed (Van, 2009). African economies were primarily affected through declines in commodity prices, reductions in remittance inflows, and tighter external financing conditions (Lane & Milesi-Ferretti, 2011). Specifically, North African economies were relatively severely affected through external channels such as reduced oil revenues, tourism, and European demand (Aly, 2011; IMF, 2009; IMF, 2010; World Bank, 2009).

structure can absorb the crisis-related dynamics. However, this cannot be verified directly and a priori, and must therefore be evaluated indirectly through model diagnostics. For this reason, the empirical strategy is explicitly validation-driven in assessing whether the latent structure adequately captures crisis-related dynamics. Rather than assuming that latent factors fully absorb crisis-related variation, the analysis evaluates this through multiple diagnostics. More specifically, placebo-in-time tests and equivalence tests assess whether the model generates spurious treatment effects in periods without actual treatment. If the model were unable to account for crisis-related dynamics, this would be expected to manifest in poor pre-treatment fit or systematic placebo effects, particularly around the onset of the crisis, consistent with the identification conditions underlying the low-rank framework in Section 3.2. The absence of such patterns provides indirect support for the adequacy of the counterfactual approximation.

Although the identifying assumptions underlying low-rank models cannot be directly verified, several features of the global FDI inflows and the 1997 Asian Financial Crisis suggest that the low-rank framework may still provide a reasonable approximation for the counterfactual outcome in the present application. These features do not validate the identifying assumptions per se, but provide contextual support for the applicability of a low-rank approximation in this empirical setting.

First, FDI inflows are widely documented to co-move, at least partly, in response to common macroeconomic forces. This pattern of the FDI inflows is consistent with the international finance literature, which emphasizes that FDI flows to emerging markets are significantly driven by global “push” factors, such as international liquidity conditions and changes in global risk appetite (Calvo et al., 1993; Forbes & Warnock, 2012). These common forces generate the co-movement across countries that may justify the use of a low-rank representation.

Second, although Indonesia experienced an exceptionally severe contraction, the 1997 Asian Financial Crisis was not purely idiosyncratic to Indonesia. Empirical evidence suggests that the crisis affected a broad range of the donor countries in Asia, North Africa, the Middle East, and Sub-Saharan Africa (see Table A/1 in the Appendix) through trade, commodity price, and financial channels. Thus, although Indonesia experienced an exceptionally severe contraction (Athukorala & Chongvilaivan, 2010), the crisis itself was not confined to Indonesia alone, but represented a partially shared macroeconomic shock with heterogeneous cross-country effects.[9] This heterogeneity is compatible with the low-rank framework, which allows common shocks to affect countries with different intensities through heterogeneous factor loadings. Consequently, Indonesia may load more strongly on the crisis-related component than other countries.

Third, the countries in the donor pool reacted to the 1997 crisis in markedly different ways, both in terms of timing and the speed of recovery, and the duration of the crisis effects also varied considerably across units (see footnote 9). Rather than producing a single uniform

[9]Growth rates indicate a broad-based slowdown or contraction around 1997 in Asia (own calculation). While Indonesia experienced an exceptionally severe downturn (GDP growth falling to −13.1% in 1998), other economies also exhibited notable declines, including Singapore (−2.2% from 7.5%), Vietnam (from 8.2% in 1997 to 5.8%) and Laos (from 6.9 to 4.0%). Economies in North Africa and the Middle East were affected mainly through declining oil and commodity prices and weaker external balances, although direct financial contagion remained limited (Chabrier, 1999; IMF, 1999a, 1999b). Similarly, Sub-Saharan African economies were affected indirectly through commodity prices, export demand, and trade competition, while financial spillovers remained relatively limited because of lower financial integration (Calamitsis, 1999; Harris, 1999).

response across countries, the crisis generated heterogeneous adjustment paths over time. Since the estimation procedure reconstructs missing counterfactual outcomes from the observed entries of the outcome matrix, this heterogeneity provides informative variation that can be exploited in the estimation procedure (consistent with the sufficient variation requirement discussed in Section 3.2). Moreover, because the estimation uses all available observed entries in the outcome matrix, both before and after the crisis for untreated units and before treatment for treated units, the procedure is able to incorporate variation in crisis timing and recovery patterns across countries when reconstructing the missing counterfactual outcomes.

Finally, the 1997 Asian Financial Crisis did not disrupt the existing co-movement structure in international capital flows, including FDI. As documented by Kaminsky and Reinhart (2000), emerging markets continued to exhibit substantial co-movement in capital flows and asset prices due to common exposure to global liquidity and financial shocks. Thus, despite the exceptional severity of the Indonesian crisis episode, the post-crisis period was still characterized by systematic cross-country co-movements rather than purely idiosyncratic fluctuations, suggesting that the underlying co-movement structure remained broadly intact.

The above considerations suggest that the 1997 crisis is not inconsistent with the low-rank representation and does not require being explicitly "separated" from the latent structure ex ante. Instead, the identification strategy primarily relies on the assumption that the shock can be sufficiently approximated within a low-dimensional latent structure, while the adequacy of this approximation is assessed empirically through pre-treatment fit (placebo-in-time and equivalence tests). For this reason, the empirical strategy emphasizes validation rather than relying solely on maintained assumptions. While the low-rank structure assumption is a maintained identifying assumption, the empirical diagnostics provide ex post evidence on whether this assumption is consistent with the observed data in the present application. Nevertheless, note that given the exceptional severity of the Indonesian crisis episode, it is not possible to fully rule out that part of the estimated treatment effect reflects residual crisis dynamics. As a precaution against potential misspecification, the year 1997 is excluded from the pre-treatment period in the empirical analyses in order to reduce the influence of potentially extreme observations on the estimation of the latent factor structure.[10] The empirical strategy should therefore be interpreted as reducing – rather than fully eliminating – this identification concern.

To determine model complexity in a data-driven manner, cross-validation is used in both IFEct and MC estimations. In MC, the effective rank is selected through the regularization parameter λ, while in IFEct the number of latent factors is chosen to minimize mean squared prediction error (MSPE). Model performance is evaluated using pre-treatment fit diagnostics, namely, placebo-in-time tests, and equivalence tests following Liu et al. (2024). The credibility of the results is further assessed through a series of robustness exercises designed to evaluate the stability of both the estimated treatment effects and the latent low-rank structure underlying the counterfactual reconstruction. To account for the gradual nature of democratization, the analysis considers alternative treatment onsets (1999 and 2000), providing a check on the sensitivity of the results to treatment timing. Placebo-based diagnostics are conducted by assigning fictitious treatment dates in the pre-treatment period (1995 and 1996) in order to assess

[10]Note that excluding the year 1997 does not result in a time shift within the model. The treatment onset remains fixed at 1998, and the chronological order of the observations is preserved.

whether the estimators generate spurious treatment effects. Both MC and IFEct specifications are estimated using alternative sets of covariates (a parsimonious and a full set), allowing an assessment of whether the estimated treatment effects depend on the inclusion of additional controls. The sensitivity of the results to the composition of the donor pool is explicitly assessed through robustness exercises. In particular, I conduct systematic leave-one-out analyses in which potentially influential donor countries (including large economies and regional neighbors) are sequentially excluded, together with the exclusion of three donor groups (civil war countries, Francophone Sub-Saharan countries, and high-income oil-producing countries), in order to evaluate whether the estimated treatment effects are disproportionately driven by particular donors or donor subgroups. In addition, shorter pre-treatment windows are used to assess the sensitivity of the recovered latent structure to the length of the pre-treatment period, while pre-treatment-year leave-one-out tests evaluate whether specific pre-treatment observations exert disproportionate influence on the estimated counterfactual trajectory.

To address concerns regarding outcome measurement (see Li, 2009), I employ three alternative measures of FDI inflows commonly used in the literature: (1) FDI inflows per capita, (2) total FDI inflows, and (3) FDI inflows as a % of GDP. Using multiple outcome measures allows for a systematic assessment of the robustness of the results to measurement choices.

Because balance-of-payments-based FDI data frequently include zero and negative values, reflecting disinvestment and intra-firm financial flows, the distribution of raw FDI inflows variables is highly skewed and deviates substantially from normality. To accommodate the negative observations while maintaining a logarithmic-like scale (see Bellemare & Wichman, 2020), I employ the inverse hyperbolic sine (IHS) transformation[11] (Burbidge et al., 1988) which is defined as $\mathrm{IHS}(x) = \ln\left(x + \sqrt{x^2 + 1}\right)$.

Unlike the standard ln (x+1) adjustment employed frequently in the literature, the IHS transformation is defined for the entire real line, including zero and negative values, making it uniquely suited for BoP-based FDI inflows data.

3.4. Data and variables

The outcome variable (Y_{it}) is measured by an FDI inflows metric based on FDI inflows (BoP, current US$) per capita, obtained from the World Development Indicators (World Bank, 2025). The treatment indicator (D_{it}) equals one for Indonesia in the post-1998 period and zero otherwise. The covariate vector (X_{it}) includes variables selected based on theoretical relevance, prior empirical studies, and data availability. These are as follows: (1) GDP per capita, capturing overall economic development (World Bank, 2025), (2) GDP growth rate, reflecting short-run macroeconomic performance (World Bank, 2025),[12] (3) the primary school enrollment ratio, used as a proxy for human capital (World Bank, 2025),[13] (4) the KOF Index of Economic Globalisation (KOF Swiss Economic Institute, 2024), measuring openness to international trade and finance, and (5) nominal GDP (World Bank, 2025), capturing the size of the economy.

[11]Note that Pence (2006) documents that beyond effectively handling the mass of zero and negative observations, the IHS transformation can stabilize variance and reduce the influence of extreme values.

[12]For Djibouti, data is taken from UNCTAD (2025).

[13]For Singapore, data is taken from UNESCO UIS (2025).

Donor countries (untreated units) consist of countries that did not experience a stable democratic transition throughout the sample period (1984–2014). To determine them, I use the *polity2* score from the Polity5 Project (Center for Systemic Peace, 2021), which runs from −10 to +10 to measure democracy.[14] Following a stability-oriented definition of democracy, countries that transitioned but failed to maintain a *polity2* score above 6 for at least four consecutive years are classified as non-democratic. The four-year window is chosen to approximate a full electoral cycle, ensuring that the transition reflects sustained institutional change rather than short-lived political fluctuations. This threshold minimizes the risk that temporary or unstable regime shifts contaminate the donor group. More specifically, the classification is robust to alternative window lengths (e.g., 3 or 5 years), which yield very similar donor compositions.[15]

This procedure yields an initial set of 57 non-democratic countries for which Polity5 data is available. From this set, I exclude communist economies (Cuba and North Korea), countries subject to sustained trade embargoes during the pre-treatment period (Iraq, Iran, Libya, and Sudan), and countries with serious data limitations. Further restrictions are not imposed on the donor pool in order to preserve sufficient cross-sectional variation, which is essential for identifying the latent factor structure in low-rank models (Chen et al., 2021). Since both the MC and IFEct models can accommodate unbalanced panels (Liu et al., 2024), I do not impose a fully balanced dataset, which would unnecessarily reduce the sample size and potentially weaken the identification of the latent low-rank structure. Accordingly, I retain countries with sporadically missing observations and do not apply imputation procedures. The resulting donor pool consists of 39 countries, from Asia and Africa, with a few exceptions, and is composed of developing economies. Table A/1 in the Appendix reports pre-treatment period averages of the covariates and the three measures of FDI inflows for Indonesia and the donor countries. For transparency, all reported values are based on the raw (untransformed) data.

To satisfy the data requirements of the MC and IFEct estimator (Chen et al., 2021; Liu et al., 2024) and ensure model stability, I employ a panel covering the period 1984–2014. This timeframe is chosen to strike a crucial balance regarding the length of the pre-treatment period. On one hand, the pre-treatment period must be sufficiently long to reliably estimate unobserved factors and their heterogeneous loadings (Chen et al., 2021; Xu, 2017). On the other hand, it must not be excessively long, as incorporating observations from earlier periods with substantially different macroeconomic or institutional conditions is a particularly salient concern for developing economies. Furthermore, the length of the post-treatment period is specifically selected to capture the prolonged adjustment process that often follows democratization, allowing for the observation of long-run effects rather than just immediate shifts.

[14]Countries with values between −10 and −6 are considered autocracies; democracies have values between 6 and 10; and anocracies fall between −5 and +5.

[15]More specifically, the use of a five-year window does not change the set of donors, whereas the use of a three-year window would include only Sri Lanka.

4. RESULTS

4.1. Baseline results

The MC and IFEct estimators[16] are implemented in parallel, as the relative performance of the two methods may vary across specifications and cannot be determined a priori. Model selection is therefore guided by predictive performance, as measured by the mean squared prediction error. To determine the appropriate level of model complexity, I rely on cross-validation procedures that evaluate predictive performance in the pre-treatment period. However, the role of cross-validation differs between the two estimators. In MC, cross-validation is used to select the regularization parameter λ, which governs the effective rank of the recovered low-rank matrix. In contrast, in the IFEct model, cross-validation is used to determine the number of latent factors, which directly determines the dimensionality of the factor structure.[17] This approach ensures that model complexity is determined objectively by the data, rather than imposing it a priori.

I use two sets of covariates: a parsimonious set (KOF Globalisation index, GDP growth rate, GDP per capita) and a full set (KOF Globalisation index, nominal GDP, GDP per capita, GDP growth rate, primary school enrollment ratio). The baseline relies on a parsimonious set of covariates, ensuring that a core set of theoretically relevant variables is sufficient to construct a credible counterfactual. In contrast, a richer specification including a full set of covariates is used as a robustness check.[18] Graphical results are reported only for the baseline (parsimonious) specification to conserve space; however, for each method and outcome variable, three figures are presented (Figures 1–18)[19] – the estimated treatment effects (ATT), the placebo-in-time test, and the equivalence test – ensuring full comparability across specifications. The corresponding results for the full set of covariates are reported exclusively in Table 1.

Consistent with the identification strategy, a necessary condition for credible counterfactual approximation is that the model can closely reproduce Indonesia's pre-treatment trajectory, which is not merely a descriptive requirement but a central identification diagnostic: Failure to reproduce the pre-treatment trajectory would suggest that the latent structure may be insufficient to capture the relevant macroeconomic dynamics underlying the counterfactual process. This condition appears to be satisfied based on graphical and statistical diagnostics. While some pre-treatment years may display small deviations (see Figures 1, 4, 7, 10, 13, and 16), these do not exhibit a systematic pattern over time. Moreover, their magnitude remains limited relative to the post-treatment effects (see the mean of the pre-treatment yearly ATTs in Table 1), supporting the interpretation that systematic pre-treatment divergence is absent.

Turning to the post-treatment period shown in the treatment effects figures, the treatment effect estimates suggest a consistent pattern across all outcome measures. The estimated post-

[16]I use the Stata package *fect* (distribution date: 20200605). The equivalence test uses 10 pre-periods, while all other parameters – including bootstrap procedures, seeds, and cross-validation – adhere to the default settings.

[17]Note that the optimal number of latent factors in the IFEct estimations is one or two across the estimations presented below, as determined by the lowest MSPE in cross-validation.

[18]The full covariate specification is not reported for the FDI as a % of GDP variable, as including both GDP per capita and nominal GDP renders the model numerically unstable and prevents convergence of the estimation algorithms.

[19]Throughout the study, the estimator type and the FDI inflows measure used in a given specification are indicated in parentheses below each figure.

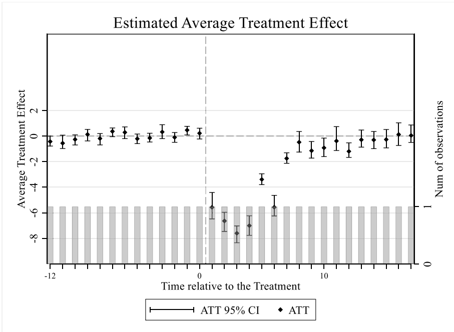

Fig. 1. Treatment effects (MC, FDI inflows per capita)

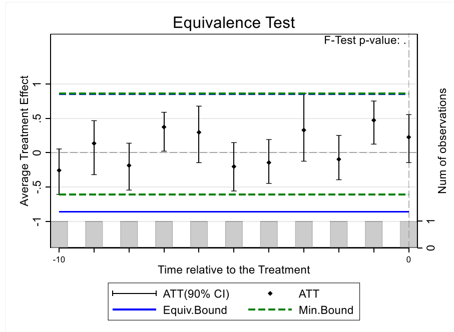

Fig. 2. Equivalence test (MC, FDI inflows per capita)

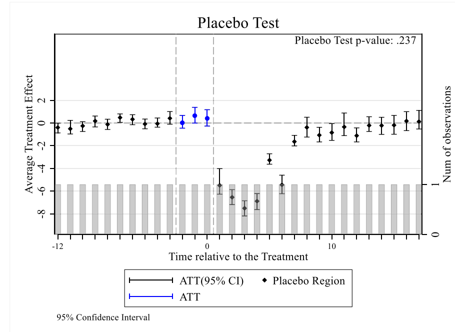

Fig. 3. Placebo-in-time test (MC, FDI inflows per capita)

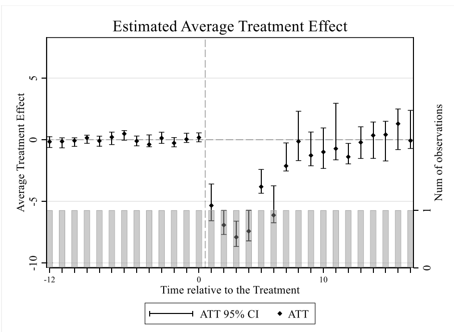

Fig. 4. Treatment effects (IFEct, FDI inflows per capita)

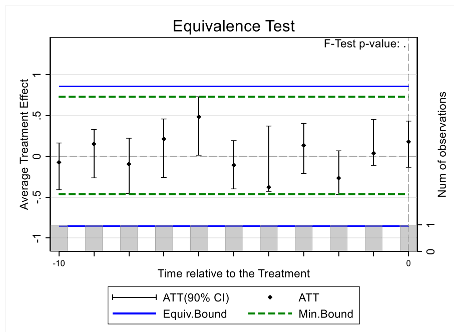

Fig. 5. Equivalence test (IFEct, FDI inflows per capita)

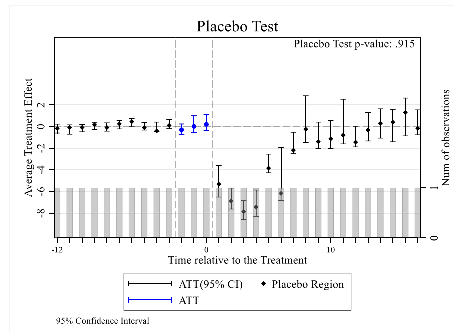

Fig. 6. Placebo-in-time test (IFEct, FDI inflows per capita)

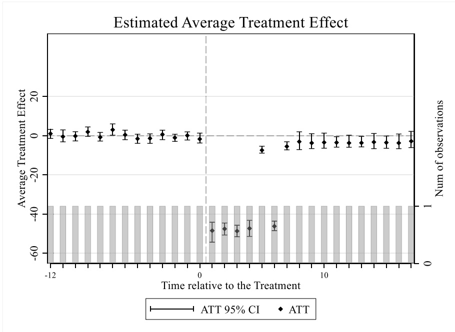

Fig. 7. Treatment effects (MC, total FDI inflows)

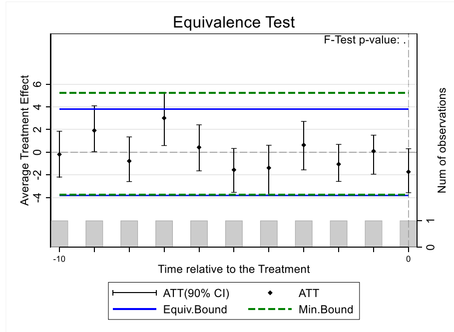

Fig. 8. Equivalence test (MC, total FDI inflows)

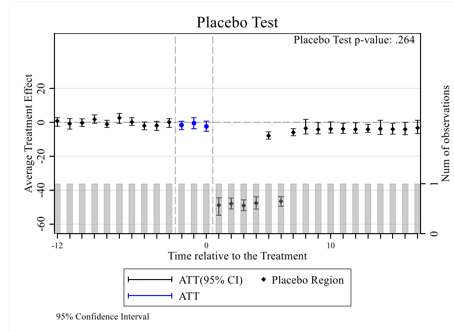

Fig. 9. Placebo-in-time test (MC, total FDI inflows)

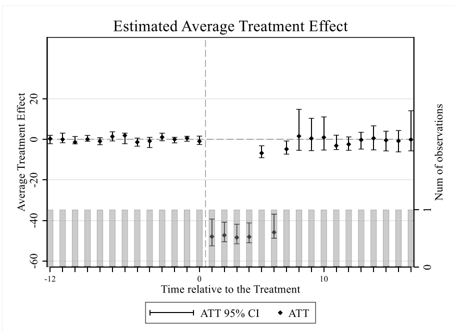

Fig. 10. Treatment effects (IFEct, total FDI inflows)

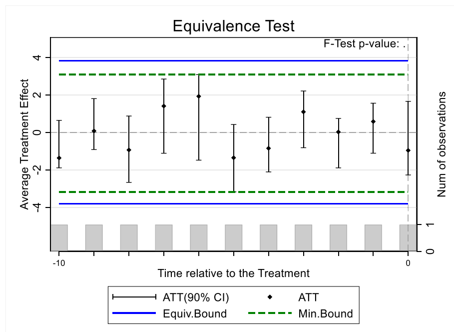

Fig. 11. Equivalence test (IFEct, total FDI inflows)

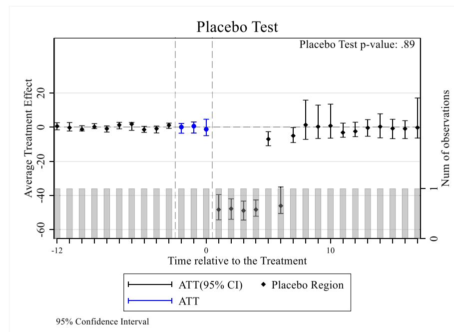

Fig. 12. Placebo-in-time test (IFEct, total FDI inflows)

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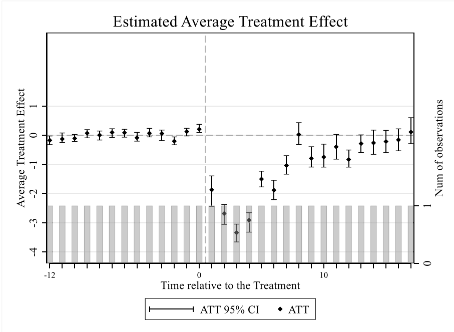

Fig. 13. Treatment effects (MC, FDI inflows as a % of GDP)

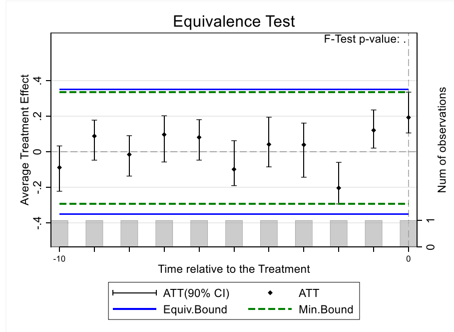

Fig. 14. Equivalence test (MC, FDI inflows as a % of GDP)

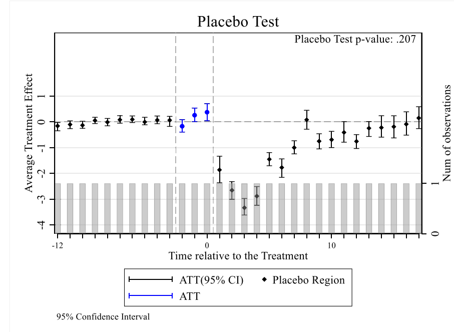

Fig. 15. Placebo-in-time test (MC, FDI inflows as a % of GDP)

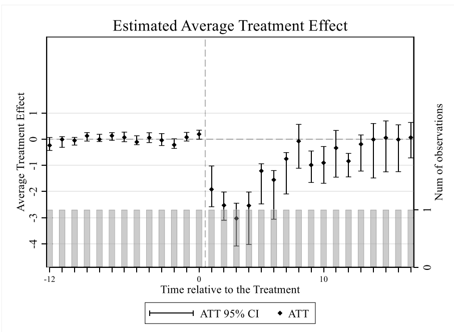

Fig. 16. Treatment effects (IFEct, FDI inflows as a % of GDP)

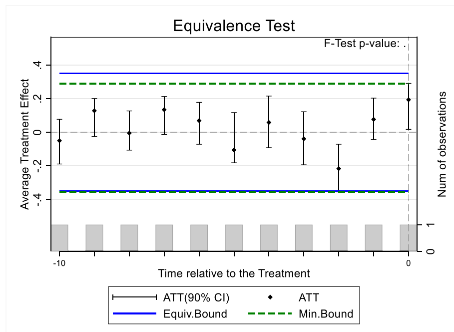

Fig. 17. Equivalence test (IFEct, FDI inflows as a % of GDP)

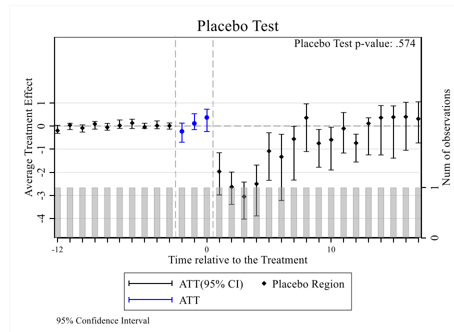

Fig. 18. Placebo-in-time test (IFEct, FDI inflows as a % of GDP)

Table 1. Estimation results based on IHS-transformed FDI inflows measures under parsimonious and full covariate specifications with MC and IFEct estimators

	FDI inflows per capita		Total FDI inflows		FDI inflows as a % of GDP	
	MC	IFEct	MC	IFEct	MC	IFEct
baseline: parsimonious set of covariates						
MSPE	6.0663	8.7761	121.7494	147.5722	0.978	1.0816
overall post-treatment ATT	−2.486	−2.491	−16.806	−14.866	−1.109	−0.985
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
short-run ATT	−6.042	−6.275	−39.895	−39.709	−2.466	−2.245
medium-run ATT	−1.967	−2.128	−12.394	−9.533	−0.891	−0.853
long-run ATT	−0.318	−0.047	−3.467	−0.931	−0.294	−0.180
mean pre-treatment ATT	−0.020	−0.016	0.137	0.075	−0.018	−0.016
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.237$	$p = 0.915$	$p = 0.264$	$p = 0.890$	$p = 0.207$	$p = 0.574$
equivalence test	pass	pass	fail at $t = -7$	pass	pass	pass
full set of covariates						
MSPE	6.1897	8.6857	126.392	149.0191		
overall post-treatment ATT	−2.488	−2.495	−16.751	−13.945		
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$		
short-run ATT	−6.029	−6.294	−39.737	−36.807		
medium-run ATT	−1.969	−2.130	−12.330	−8.992		
long-run ATT	−0.328	−0.042	−3.490	−1.154		
mean pre-treatment ATT	−0.018	−0.017	0.112	0.131		
placebo test	pass	pass	pass	pass		
	$p = 0.245$	$p = 0.880$	$p = 0.310$	$p = 0.501$		
equivalence test	pass	pass	pass	pass		

Notes: All ATT estimates are based on inverse hyperbolic sine (IHS)-transformed outcome variables. “Short-run”, “medium-run”, and “long-run” ATT values represent descriptive averages of yearly ATT estimates over the corresponding post-treatment windows and are not based on separate formal inference procedures.

transition deviations suggest that FDI inflows in Indonesia fell substantially below the estimated counterfactual trajectory in the immediate aftermath of the democratic transition, particularly during the first 4–6 years. In the medium run, the magnitude of the negative treatment effect decreases, while in the long run, the estimated treatment effects converge toward zero, indicating a (partial) recovery over time.

To complement the graphical evidence, Table 1 reports the corresponding estimation results for each specification. Across all specifications, the overall post-treatment ATT is negative and statistically significant. Note that statistical inference in the low-rank model follows the parametric bootstrap procedure proposed by Xu (2017), and fundamentally differs from the asymptotic testing typically employed in traditional panel data models. The parametric bootstrap procedure constructs confidence intervals by resampling residuals' time series while preserving their temporal dependence structure (Xu, 2017, p. 64). This implies that uncertainty is assessed at the level of the estimated counterfactual trajectory, rather than through a sequence of independent year-by-year hypothesis tests. In each bootstrap iteration, the full model is re-estimated, generating a distribution of treatment effects both for individual years and for the average effect over the post-treatment period. As a consequence, the year-specific ATT estimates and their confidence intervals – displayed as vertical bars around the point estimates in the figures – should not be interpreted as a set of independent hypothesis tests.

As shown in Table 1, for FDI inflows per capita, the overall post-treatment ATT is approximately -2.49 under both MC and IFEct. For total FDI inflows, it ranges between roughly -14 and -16.8, depending on the estimator. In the parsimonious specification, FDI as a % of GDP likewise exhibits a negative and statistically significant effect, with overall post-treatment ATT values close to -1.[20] The similarity of estimates across estimators and specifications suggests that the results are not primarily driven by specific modeling choices or particular covariates, but instead reflect a stable pattern in the data.

Table 1 also presents averages of the yearly ATTs over selected post-treatment windows. The short run is defined as the first five years after democratization (1998–2002), capturing the immediate adjustment period. The medium run corresponds to years 6–10 after the transition, while the long run covers the remaining post-treatment period. These averages are reported for descriptive purposes to illustrate the dynamic adjustment of treatment effects over time. In the short run, the period-average ATT is more than double the magnitude of the overall ATT. In the medium run, the magnitude of the period-average decreases but remains economically meaningful, in the long run, it becomes substantially smaller and approaches zero, indicating a gradual attenuation of the initial effects. This dynamic pattern is consistent across both estimators and specifications.

To assess the credibility of the results, I conduct placebo-in-time[21] and equivalence tests following Liu et al. (2024). The placebo-in-time test (see Figures 3, 6, 9, 12, 15, and 18) evaluates whether the model detects non-existent treatment effects prior to the actual treatment. For this application, the interval $[-2, 0]$ relative to the treatment year is designated as the placebo period.

[20]The treatment effects for the different FDI inflows measures naturally vary in magnitude, given that they are measured in different units. See Section 5 for a discussion.

[21]Unlike the traditional "manual" placebo approach (*in-time placebo*) – which typically involves re-estimating the model with an arbitrarily chosen "fake" treatment year in the pre-treatment period – the *placebo-in-time* test utilizes a more sophisticated cross-validation logic (Liu et al., 2024). While a manual fake-year test serves as a general falsification check to ensure the model does not produce "ghost effects" at random, it often requires truncating the sample and may fail to capture the nuances of the latent low-rank structure near the actual event. In contrast, the placebo-in-time test performed here (focusing on the $[-2, 0]$ window) acts as a rigorous out-of-sample diagnostic. The model excludes observations from the years immediately preceding democratization and uses the latent low-rank structure estimated from the remaining pre-treatment period to predict them. By demonstrating that the placebo-test ATT is statistically and economically indistinguishable from zero during this critical window, the results are consistent with the interpretation that democratization can be treated as a structural shift and that the subsequent divergence in FDI is unlikely to reflect a continuation of pre-existing trends.

During this test, observations within this range are treated as if the intervention had occurred earlier, and the corresponding ATT is estimated (see Liu et al., 2024, for more details). Across all specifications, the estimated placebo effects are small and statistically insignificant, with P-values well above conventional thresholds, indicating that the model does not detect artificial effects in the pre-treatment period.

While the placebo-in-time test assesses whether pre-treatment effects are statistically different from zero, the equivalence test provides a more stringent diagnostic by evaluating whether pre-treatment deviations are economically small (see Liu et al., 2024). Specifically, the equivalence test examines whether the confidence intervals of the estimated pre-treatment effects lie within a pre-specified equivalence bound around zero, implying that average pre-treatment deviations are economically negligible. The equivalence bound is set to 0.36 times the residual SD (Liu et al., 2024). Figures 2, 5, 8, 11, 14, and 17 report the results of the equivalence test. The vertical bars represent the 90% confidence intervals of the estimated pre-treatment effects, while the blue solid lines indicate the equivalence bounds. The green dotted lines represent the minimum bounds, which provide an additional reference for the magnitude of acceptable pre-treatment deviations relative to the residual variation in the data. The test evaluates whether the estimated pre-treatment effects (including their confidence intervals) remain within the equivalence range. As shown in the figures, this condition is satisfied across all specifications with a single exception (MC estimation with total FDI inflows in year -7). Moreover, with that single exception, the confidence intervals also remain within the minimum bounds, further suggesting that the latent low-rank structure provides an adequate approximation of Indonesia's pre-treatment trajectory prior to democratization.

An additional important result shown in Table 1 is that the parsimonious specification consistently achieves lower MSPE values across estimators (except for one specification), indicating superior predictive performance relative to the full specification. This suggests that the baseline model already captures the relevant variation in the data, and that including additional covariates does not improve – and may even reduce – model fit.[22] Furthermore, within the present application, the MC estimator generally achieves lower MSPE values, suggesting somewhat stronger predictive performance relative to IFEct.

In summary, the results support a consistent pattern: democratization in Indonesia is associated with a substantial decline in FDI inflows in the short and medium run relative to the counterfactual, followed by a convergence toward the counterfactual path, without evidence of a sustained positive treatment effect. The findings are robust across the applied methods, and outcome measures, and are supported by strong pre-treatment fit and diagnostic tests, which reduces the likelihood that the findings are driven by model-specific artifacts.

[22]Covariates in the low-rank model are included primarily to improve pre-treatment fit and to help discipline the estimation of the latent factor structure, rather than to enable conventional inference on their individual coefficients. In this sense, their role differs fundamentally from that in standard regression settings. Crucially, both MC and IFEct can also be estimated without covariates; however, in such specifications the pre-treatment fit deteriorates noticeably in this application. This reinforces the interpretation that covariates are useful insofar as they enhance the credibility of the counterfactual, rather than for identifying structural mechanisms (e.g., through variables such as corruption), which lies beyond the scope of the present design.

4.2. Alternative treatment onsets

As a sensitivity check, the baseline specification using alternative treatment onsets, namely, 1999 and 2000 is re-estimated. This addresses potential concerns regarding the precise timing of the democratization process. Figures 19–24 report the results for the MC estimator with the baseline covariates, using FDI inflows per capita as the outcome variable.

A key pattern emerges across these specifications.[23] The estimated treatment effects display a very similar dynamic profile to the baseline results; however, the timing of the treatment shifts mechanically with the imposed treatment year: the negative treatment effects observed in the baseline specification now appear partly in the pre-treatment period. As a result, the pre-treatment fit deteriorates, and both the placebo-in-time and equivalence tests perform worse.

The fact that the estimated treatment effects "move" in tandem with the imposed treatment date indicates that the model captures a time-specific change in 1998, rather than detecting spurious patterns driven by noise or model specification. At the same time, the deterioration in pre-treatment fit under alternative treatment timings highlights the importance of correctly specifying the treatment onset. When the treatment year is shifted forward, part of the true post-treatment adjustment period is incorrectly assigned to the pre-treatment window, which mechanically violates the identifying assumption of no pre-treatment effects. This likely contributes to the weaker performance in placebo and equivalence tests under these alternative specifications.

To sum up, these results provide support for the baseline identification strategy. They suggest that the estimated negative treatment effects are tightly linked to the actual timing of Indonesia's democratic transition and its immediate aftermath, rather than being driven by arbitrary model choices.

4.3. Leave-one-out and group restriction donor sensitivity checks

To ensure that the estimated treatment effects are not driven by specific donor countries, a leave-one-out procedure is conducted.[24] This involves three types of sensitivity checks. First, potentially key donor countries are excluded one by one, namely, China, Vietnam, Egypt, Algeria, and Laos. These countries are selected based on their economic size, regional relevance, or potential influence on the estimation given their weight in global FDI dynamics. Second, a regional group – Francophone Sub-Saharan African countries – is partially excluded. Third, two additional subsets of countries are excluded: countries affected by civil war, to ensure that the results are not driven by structurally dissimilar or unstable economies, and the four countries with the highest income per capita. For group-level exclusions (Francophone Africa and countries affected by civil war), partial exclusion is implemented instead of removing all such countries from the donor pool, thereby avoiding a substantial reduction in the number of donors. Table 2 contains the results for all three FDI inflows variables, using both MC and IFEct methods.

[23]The results presented here focus on FDI inflows per capita using the MC estimator for expositional clarity. The same qualitative patterns emerge when using the alternative outcome measures (total FDI inflows and FDI as a % of GDP) and when applying the IFEct estimator. These additional results are omitted for brevity but are available from the author upon request.

[24]Unlike standard implementations of the Synthetic Control Method, the Stata and R *fect* package does not provide an automated LOO procedure for donor sensitivity. Therefore, LOO analyses are implemented manually for a selected set of potentially influential donors and donor groups.

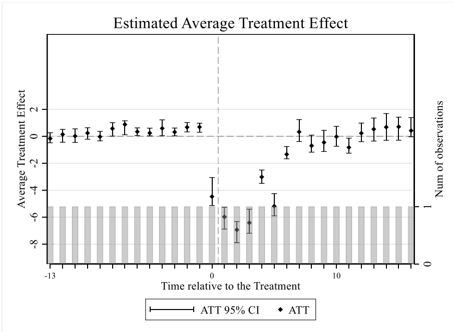

Fig. 19. Treatment effects with 1999 as the treatment year (MC, FDI inflows per capita)

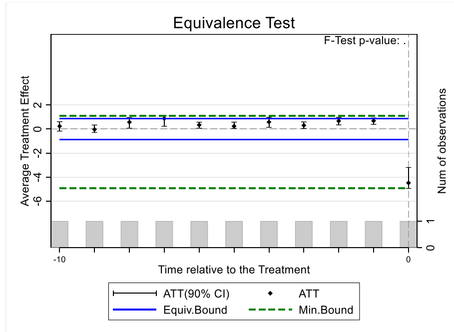

Fig. 20. Equivalence test with 1999 as the treatment year (MC, FDI inflows per capita)

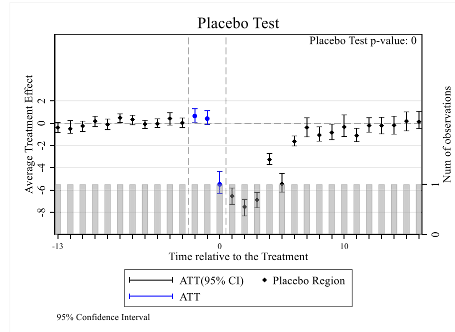

Fig. 21. Placebo test with 1999 as the treatment year (MC, FDI inflows per capita)

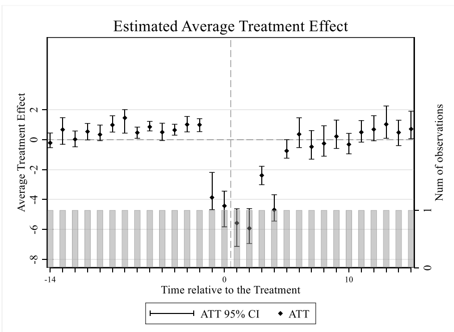

Fig. 22. Treatment effects with 2000 as the treatment year (MC, FDI inflows per capita)

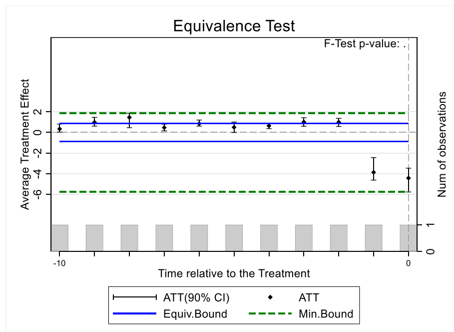

Fig. 23. Equivalence test with 2000 as the treatment year (MC, FDI inflows per capita)

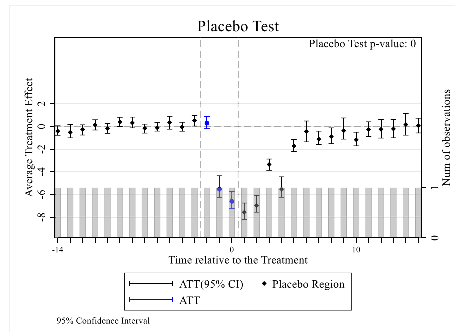

Fig. 24. Placebo test with 2000 as the treatment year (MC, FDI inflows per capita)

Table 2. Leave-one-out sensitivity analysis for various FDI inflows measures with MC and IFEct estimators

	FDI inflows per capita		Total FDI inflows		FDI inflows as a % of GDP	
	MC	IFEct	MC	IFEct	MC	IFEct
Baseline						
MSPE	6.0663	8.7761	121.7494	147.5722	0.9780	1.0816
overall post-treatment ATT	−2.486	−2.491	−16.806	−14.866	−1.109	−0.985
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.237$	$p = 0.915$	$p = 0.264$	$p = 0.890$	$p = 0.207$	$p = 0.574$
equivalence test	pass	pass	pass	pass	pass	pass
China excluded						
MSPE	6.3900	7.7628	122.4201	173.2404	0.9850	1.0267
overall post-treatment ATT	−2.695	−2.510	−16.960	−14.344	−1.124	−0.977
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.404$	$p = 0.925$	$p = 0.289$	$p = 0.464$	$p = 0.168$	$p = 0.427$
equivalence test	pass	pass	fail	pass	pass	fail
Viet Nam excluded						
MSPE	6.2401	9.6369	131.2137	149.4422	0.9994	1.0482
overall post-treatment ATT	−2.490	−2.555	−17.613	−13.978	−1.148	−0.806
	$p = 0.000$	$p = 0.042$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.031$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.163$	$p = 0.907$	$p = 0.441$	$p = 0.428$	$p = 0.152$	$p = 0.626$
equivalence test	pass	pass	fail	pass	fail	pass
Egypt excluded						
MSPE	6.3221	7.8331	125.8015	194.3156	0.9505	1.0160
overall post-treatment ATT	−2.589	−2.521	−16.894	−15.525	−1.135	−1.046
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.240$	$p = 0.944$	$p = 0.281$	$p = 0.847$	$p = 0.235$	$p = 0.508$
equivalence test	fail	pass	fail	pass	pass	pass

(continued)

Table 2. Continued

	FDI inflows per capita		Total FDI inflows		FDI inflows as a % of GDP	
	MC	IFEct	MC	IFEct	MC	IFEct
Algeria excluded						
MSPE	6.3814	7.7343	143.1769	181.1308	1.0228	1.0375
overall post-treatment ATT	−2.493	−2.419	−17.562	−15.548	−1.128	−0.991
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.013$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.204$	$p = 0.903$	$p = 0.206$	$p = 0.793$	$p = 0.168$	$p = 0.591$
equivalence test	fail	pass	fail	pass	pass	pass
Lao PDR excluded						
MSPE	6.3458	8.7144	130.4482	189.3728	1.0255	1.0333
overall post-treatment ATT	−2.543	−2.508	−17.441	−13.827	−1.111	−0.860
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.007$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.152$	$p = 0.958$	$p = 0.265$	$p = 0.500$	$p = 0.051$	$p = 0.320$
equivalence test	fail	pass	fail	pass	pass	pass
Francophone Sub-Saharan Africa excluded (Cote d'Ivoire, Cameroon, Gabon, Equatorial Guinea, Guinea, Togo, Burkina Faso)						
MSPE	6.1646	8.4884	122.9148	163.7276	0.9646	1.0803
overall post-treatment ATT	−2.373	−2.338	−15.607	−12.228	−1.082	−1.146
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	fail	pass	pass	pass
	$p = 0.732$	$p = 0.894$	$p = 0.04$	$p = 0.553$	$p = 0.438$	$p = 0.537$
equivalence test	pass	pass	fail	pass	pass	pass
Civil war countries excluded (Angola, Congo, Dem., Congo, Rep., Rwanda, Chad)						
MSPE	5.4621	12.7083	105.9841	229.8336	0.8280	1.9012
overall post-treatment ATT	−2.755	−2.821	−17.127	−16.0378	−1.165	−1.101
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.545$	$p = 0.581$	$p = 0.200$	$p = 0.654$	$p = 0.435$	$p = 0.965$
equivalence test	pass	fail	pass	fail	fail	fail

(continued)

Table 2. Continued

	FDI inflows per capita		Total FDI inflows		FDI inflows as a % of GDP	
	MC	IFEct	MC	IFEct	MC	IFEct
High-income countries excluded (United Arab Emirates, Kuwait, Qatar, Saudi Arabia)						
MSPE	4.6059	7.6105	101.7075	131.1815	0.9967	1.0869
overall post-treatment ATT	−2.300	−1.969	−15.353	−14.974	−1.123	−1.028
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	fail	pass	pass	pass	pass	pass
	$p = 0.028$	$p = 0.127$	$p = 0.569$	$p = 0.200$	$p = 0.148$	$p = 0.207$
equivalence test	fail	fail	pass	fail	fail	fail

Note: All ATT estimates are based on inverse hyperbolic sine (IHS)-transformed outcome variables.

The central finding is that the estimated treatment effects exhibit a high degree of stability. Across all LOO specifications, the overall ATT remains negative and statistically significant at the 1% level. For FDI inflows per capita, the estimated effects remain tightly clustered between approximately −2.34 and −2.75. For total FDI inflows, the ATT ranges from roughly −12.23 to −17.61, while for FDI as a % of GDP, the estimates remain close to −1 across both estimators. This narrow range suggests that the main results are not driven by any individual donor or by the particular subsets included in the analysis. Both MC and IFEct yield qualitatively similar treatment effects across all donor exclusion exercises, strengthening confidence that the findings are not estimator-specific.

In terms of model fit, excluding individual donors has only a limited impact on MSPE values, which remain broadly stable across specifications. The MC estimator produces lower MSPE values than the IFEct estimator, consistent with the results obtained using the full donor pool. The placebo-in-time test passes for all individual donor exclusions, suggesting that the estimated effects are not driven by spurious post-treatment divergence arising in the pre-treatment period. The equivalence test also passes in most specifications, although some sensitivity emerges when particular donors are excluded. This suggests that certain donor units contribute to achieving tighter pre-treatment fit, even though their exclusion does not materially affect the estimated treatment effects. Importantly, the ATT estimates exhibit a high degree of stability across all individual donor exclusions, indicating that the substantive findings are not driven by any single donor country.

Excluding the Francophone Sub-Saharan African group does not materially affect the estimated ATT, which remains very similar to the baseline across all specifications. Changes in MSPE are small and not systematic, and validation results remain largely unchanged, with only a single failure in the equivalence test. Overall, this suggests that this group is not influential for either model fit or the estimated treatment effects.

However, the impact of excluding the other two groups of countries is more nuanced. When excluding the subset of civil war countries, the MSPE decreases under MC but increases substantially under the IFEct estimator, suggesting that these countries contribute differently to the latent low-rank structure recovered by the two estimators. At the same time, the equivalence test fails in most specifications, indicating that their exclusion weakens the stability of the estimated counterfactual despite mixed effects on model fit. However, the estimated ATT remains stable in sign and broadly similar in magnitude across all outcome variables relative to the baseline values.

A related pattern emerges when excluding high-income oil-exporting donors. Their exclusion leads to a reduction in MSPE in several specifications, suggesting an improved pre-treatment fit in purely numerical terms. However, this improvement is not consistently accompanied by better validation performance, as the equivalence test passes only in one specification, indicating that lower MSPE values do not necessarily correspond to a more credible counterfactual approximation. With the exclusion of these high-income countries, the observed pattern is consistent with potential overfitting to pre-treatment dynamics rather than a genuine improvement in overall model performance. It should be noted, however, that excluding structurally distinct donor groups necessarily alters the latent low-rank structure underlying the estimator. The key robustness criterion is therefore whether the substantive conclusion regarding the direction and persistence of the democratization effect remains unchanged, rather than whether model fit measures remain identical across specifications, particularly when the estimated treatment effects themselves remain substantively stable.

In conclusion, the LOO test results support the validity of the baseline specification by demonstrating that it achieves an appropriate balance between model fit and validation performance. While the exclusion of specific groups affects model fit in different ways, the estimated treatment effects remain stable across all specifications, indicating that the results are not driven by particular donor countries or donor subsets.

4.4. In-time placebo test

I further assess the credibility of the identification strategy by conducting in-time placebo tests, in which the treatment is artificially assigned to a “fake” treatment year (1995 and 1996), prior to Indonesia’s actual democratic transition. Using multiple placebo dates allows testing whether the estimated treatment effects are sensitive to the specific timing of the treatment or instead reflect confounding time patterns or common shocks. If the baseline results were driven by such factors, similar post-treatment dynamics in FDI inflows would be expected under these “fake” treatment timings, as observed in the baseline specification.

However, as shown in Figures 25–36, assigning the treatment to earlier “fake” years does not generate a treatment effect beginning from the “fake” date itself. Instead, the estimated treatment effects remain close to zero in the period between the “fake” year and the actual democratization year. The negative treatment effect only emerges at the true treatment year and closely mirrors the baseline dynamics thereafter. This pattern indicates that the estimator does not attribute the effect to the artificially assigned treatment timing, but instead detects a break that aligns with the actual treatment timing.

4.5. Shorter pre-treatment window

As a further robustness exercise, I re-estimate the model using a shorter pre-treatment window. However, shortening the pre-treatment period requires caution, as the length of the pre-treatment window plays a critical role in low-rank estimators, since a sufficiently long time horizon is needed to reliably estimate unobserved factors and their heterogeneous loadings (Chen et al., 2021;

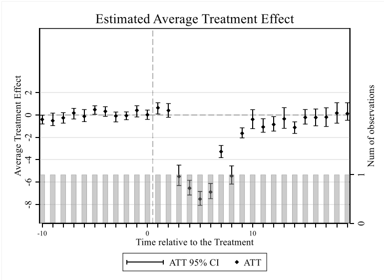

Fig. 25. Treatment effects with 1995 as the treatment year (MC, FDI inflows per capita)

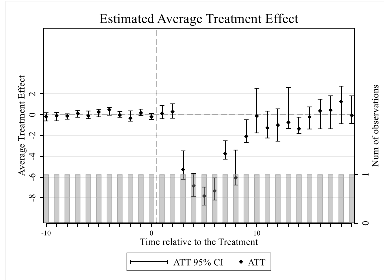

Fig. 26. Treatment effects with 1995 as the treatment year (IFEct, FDI inflows per capita)

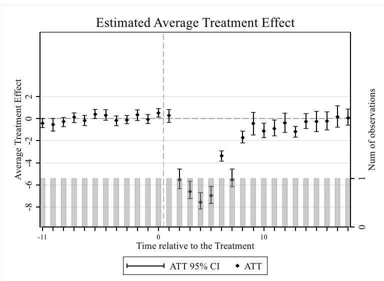

Fig. 27. Treatment effects with 1996 as the treatment year (MC, FDI inflows per capita)

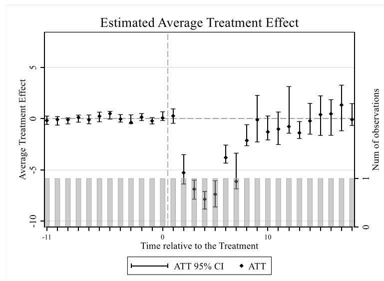

Fig. 28. Treatment effects with 1996 as the treatment year (IFEct, FDI inflows per capita)

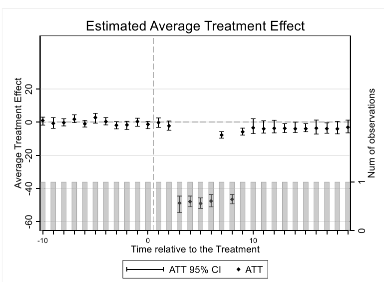

Fig. 29. Treatment effects with 1995 as the treatment year (MC, total FDI inflows)

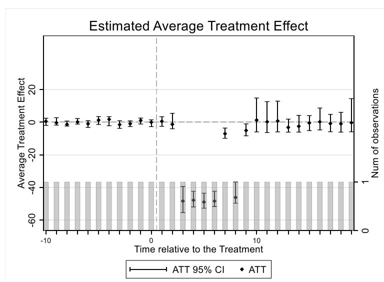

Fig. 30. Treatment effects with 1995 as the treatment year (IFEct, total FDI inflows)

Xu, 2017). Accordingly, the pre-treatment window is shortened only moderately – by five years – so as not to compromise the estimation of the latent low-rank structure unduly.

The results in Table 3 show that shortening the pre-treatment window does not materially affect the estimated treatment effects. Across all outcome variables and both estimators, the

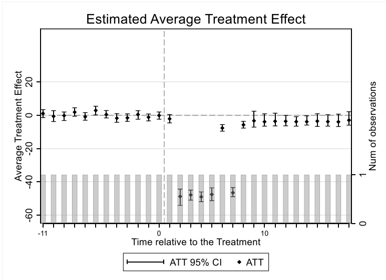

Fig. 31. Treatment effects with 1996 as the treatment year (MC, total FDI inflows)

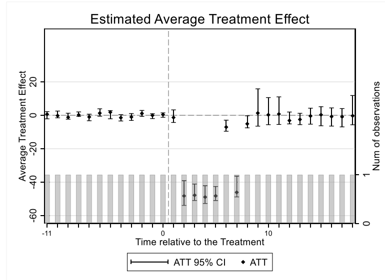

Fig. 32. Treatment effects with 1996 as the treatment year (IFEct, total FDI inflows)

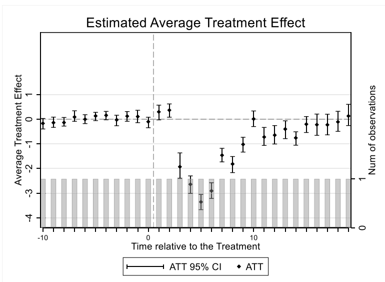

Fig. 33. Treatment effects with 1995 as the treatment year (MC, FDI inflows as a % of GDP)

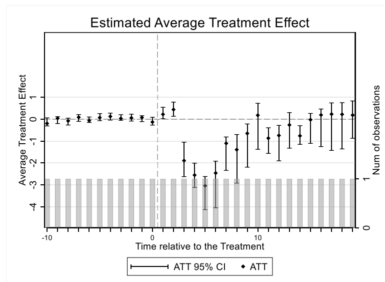

Fig. 34. Treatment effects with 1995 as the treatment year (IFEct, FDI inflows as a % of GDP)

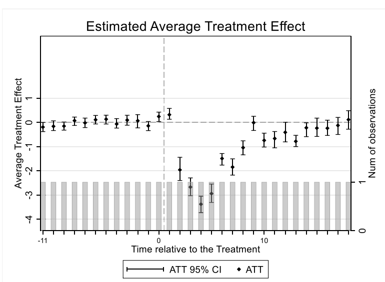

Fig. 35. Treatment effects with 1996 as the treatment year (MC, FDI inflows as a % of GDP)

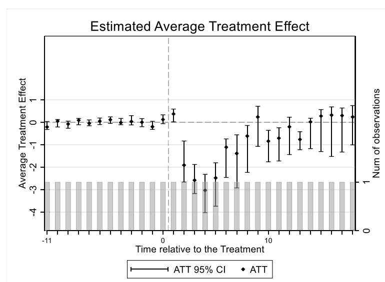

Fig. 36. Treatment effects with 1996 as the treatment year (IFEct, FDI inflows as a % of GDP)

ATT remains negative, statistically significant, and very similar in magnitude to the baseline estimates. This suggests that the main findings are not sensitive to a shorter pre-treatment horizon.

However, the diagnostic tests indicate some deterioration in model fit. While the placebo-in-time test continues to pass, the equivalence test fails in several cases under the shorter pre-treatment

Table 3. Robustness checks with a shorter pre-treatment window for various FDI inflows measures with MC and IFEct estimators

Pre-treatment period: 1989–1996						
	FDI inflows per capita		Total FDI inflows		FDI inflows as a % of GDP	
	MC	IFEct	MC	IFEct	MC	IFEct
MSPE	6.4541	12.1004	120.6837	142.0863	1.0559	1.1836
overall post-treatment ATT	−2.761	−2.539	−16.888	−16.090	−1.226	−1.276
	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$	$p = 0.000$
placebo test	pass	pass	pass	pass	pass	pass
	$p = 0.345$	$p = 0.603$	$p = 0.336$	$p = 0.173$	$p = 0.304$	$p = 0.363$
equivalence test	fail	pass	fail	fail	pass	pass

Note: All ATT estimates are based on inverse hyperbolic sine (IHS)-transformed outcome variables.

window. This pattern is consistent with a reduction in available information, as the shorter time dimension limits the precision with which the latent low-rank structure can be recovered.

In sum, these findings highlight the importance of an appropriately chosen pre-treatment window. It seems that the baseline specification provides a good balance between having enough pre-treatment information for reliable estimation and avoiding the inclusion of structurally different earlier periods. Overall, the results suggest that the estimated treatment effects are not driven by short-run fluctuations in the pre-treatment period.

4.6. Leave-one-out test over the pre-treatment period

I implement an LOO over the pre-treatment period[25] to assess whether the estimated treatment effects are sensitive to the inclusion of specific pre-transition years. This test is motivated by the possibility that individual observations in the pre-treatment period may exert disproportionate influence on the estimation of the latent low-rank structure and, consequently, on the reconstructed counterfactual trajectory.

Specifically, each pre-treatment year is sequentially excluded, the model is re-estimated, and the resulting treatment effects are averaged across all iterations. In each iteration, cross-validation is used to select the model parameters (including the number of latent factors or regularization terms), ensuring that the estimation remains fully data-driven and comparable to the baseline specification. This approach provides a systematic way to evaluate the stability of the estimates with respect to the time dimension of the data. Figures 37–39 present the estimated treatment effects for the three FDI measures using the MC estimator.[26] The results are nearly

[25]This analysis is implemented by using the R *fect* packages.

[26]The IFEct results exhibit the same qualitative patterns and are therefore not reported for brevity; full results are available upon request.

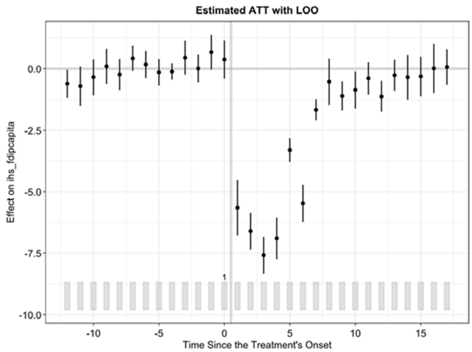

Fig. 37. Treatment effects with LOO (MC, FDI inflows per capita)

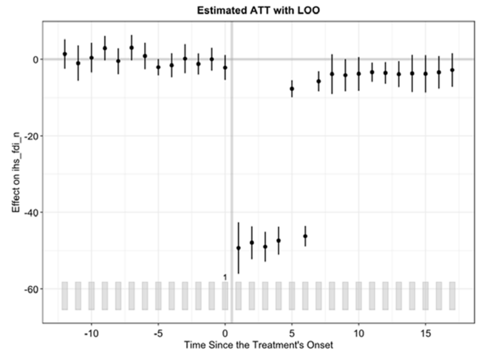

Fig. 38. Treatment effects with LOO (MC, total FDI inflows)

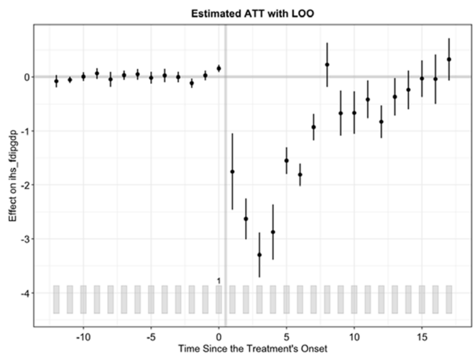

Fig. 39. Treatment effects with LOO (MC, FDI inflows as a % of GDP)

identical to the baseline estimates in both magnitude and dynamic pattern. In all cases, the short-run negative treatment effects, as well as their subsequent attenuation over time, are preserved.

These findings indicate that the estimated treatment effects are not driven by particular pre-treatment years. More broadly, they provide additional support for the stability of the inferred low-rank structure and the credibility of the constructed counterfactual trajectory.

5. DISCUSSION OF THE RESULTS

The results can be interpreted in light of the transition–consolidation distinction by Lacroix et al. (2021), who emphasize that the impact of democratization on FDI inflows depends on the success of democratic regime change. In their framework, democratization, on average, does not generate immediate gains, and positive effects emerge only in consolidated transitions about ten years after the regime change. Their findings imply a gradual improvement in FDI inflows as the new democratic regime stabilizes over time. In contrast to their cross-country perspective, the low-rank estimators used in this study enable recovery of a country-specific counterfactual trajectory of investment dynamics. This allows the analysis to trace how FDI inflows in Indonesia evolve relative to a scenario in which the pre-transition regime persists.

Following the 1998 transition, observed FDI inflows in Indonesia remain substantially below the estimated counterfactual benchmark, with the largest negative treatment effects occurring in the immediate post-transition period. Over time, this gap largely disappears, indicating convergence to the counterfactual path rather than persistent divergence. By tracing this full dynamic adjustment, this study moves beyond the stylized fact that the consolidation of a transition matters (Acemoglu et al., 2019; Lacroix et al., 2021; Persson & Tabellini, 2009), and quantifies the depth and duration of the post-transition adjustment along a continuous counterfactual path. However, the economic magnitude of the estimated treatment effects cannot be directly interpreted from the ATT values reported in Table 1, as the outcome variables are transformed using the inverse hyperbolic sine (IHS) transformation. To provide a more intuitive interpretation, the overall post-treatment ATT is scaled relative to the average level of FDI inflows in the pre-treatment period. This normalization expresses the overall estimated treatment effect in relation to a typical pre-transition FDI level. As shown in Table 4,[27] the resulting ratio is relatively stable across all three outcome measures, suggesting that the estimated deviation from the counterfactual trajectory was economically large relative to typical FDI inflows during the Suharto era. Since both the ATT estimates and the pre-treatment averages are measured on the IHS-transformed scale, this ratio should be interpreted as a relative magnitude indicator rather than a direct percentage or level comparison.

However, in contrast to the long-run positive treatment effects documented for consolidated transitions by Lacroix et al. (2021), the Indonesian recovery does not translate into a clear positive treatment effect relative to the counterfactual: while the negative impact of democratization relative to the counterfactual trajectory attenuates over time, a sustained relative increase is not detected. This difference can be understood in light of the fact that Lacroix et al. (2021) report average effects across transitions, whereas the positive impact of democratic consolidation typically emerges around ten years after regime change. In the Indonesian case, this adjustment may imply that any positive democratic premium, if present at all, emerges only over a substantially longer horizon.

Table 4. Economic magnitude of the overall ATT

	FDI inflows per capita	Total FDI inflows	FDI inflows as a % of GDP
overall post-treatment ATT	−2.486	−16.806	−1.109
mean pre-treatment FDI inflows	1.8824	12.5464	0.7460
relative treatment effect (ratio)	−1.32	−1.33	−1.49

Note: All ATT estimates are based on inverse hyperbolic sine (IHS)-transformed outcome variables.

[27]For brevity, the calculation is for the MC method, but the ratio shows the same pattern for IFEct as well.

One possible interpretation consistent with these results is that foreign investors initially perceived the transition period as more uncertain than the preceding authoritarian regime. In this interpretation, the relative stability associated with the Suharto regime – an Olsonian stationary bandit (Olson, 1993) – may initially have outweighed the still-emerging credibility of the new democratic institutions, which is illuminated by MacIntyre's (2001) analysis of veto players. Under the Suharto regime, extreme power concentration – effectively a single veto player – generated personalized credibility and a predictable policy environment that was attractive to foreign capital.[28] As he argues, the democratic transition shifted the political landscape toward a configuration with multiple veto players and fragmented authority. This transition may have been associated with increased policy uncertainty from the perspective of foreign investors, as the personalized guarantees of the prior regime vanished before the new, impersonal democratic institutions – such as the Constitutional Court or the Corruption Eradication Commission – could establish their own track record of credibility (Butt & Lindsey, 2010). The subsequent recovery of the estimated counterfactual gap may be consistent with a gradual stabilization of the new democratic institutions and the settling of the "big bang" decentralization laws (Aspinall & Fealy, 2003). However, the absence of a "democratic premium" relative to the counterfactual suggests that for foreign investors, the process of democratization is initially a source of volatility that outweighs the long-term institutional benefits[29] for a protracted period.

Although the results highlight the impact of democratic consolidation in Indonesia, the analysis itself may offer insights to reconcile the contradictory empirical findings in the democracy–FDI literature (see Section 2). As argued before, cross-country and panel regression approaches that estimate average marginal effects of democracy levels may implicitly pool observations from different phases of the democratization process. As a result, they may combine short-run transitional declines with later stabilization, leading to negative, insignificant, or mixed estimates. The Indonesian case suggests, however, that such findings may reflect variation along a common adjustment path rather than fundamentally contradictory relationships. Overall, the results suggest that the relationship between democratization and FDI is inherently dynamic, and that part of the disagreement in the literature may stem from differences in whether studies capture transitional or more consolidated institutional environments.

6. SUMMARY

This study examined how Indonesia's post-1998 FDI inflows evolved relative to an estimated counterfactual trajectory associated with the country's democratic transition. Using low-rank

[28]This environment closely mirrors the consolidated autocracy described by Aidt and Albornoz (2011), where the alignment of interests between the ruling elite and foreign investors results in a "highly FDI-friendly" regime with minimal tax burdens on capital.

[29]Well-established democracies provide a more secure environment for long-term investment, since autocracies, by their nature, cannot guarantee the indefinite protection of property and contractual rights (Olson, 1993, p. 574): "The main obstacle to long-run progress in autocracies is that individual rights even to such relatively unpolitical or economic matters as property and contracts can never be secure, at least over the long run".

estimators (MC and IFEct), the analysis traced Indonesia's post-transition FDI trajectory relative to a constructed counterfactual in which the authoritarian regime persisted.

The results reveal a clear and systematic dynamic adjustment pattern. In the immediate aftermath of the transition, FDI inflows fall sharply below the counterfactual benchmark, indicating a pronounced short-run negative treatment effect. This gap is economically large in the early post-transition years, then narrows over time, and largely disappears in the longer run. Taken together, the findings suggest that the estimated deviations from the counterfactual trajectory associated with the democratic transition were strongly time-dependent, with distinct short-, medium-, and long-run phases. Over time, FDI inflows converge toward the counterfactual path, but there is no evidence of a democratic premium within the observed period. These findings are robust across alternative outcome measures, estimators, treatment timings, "fake" treatment years, donor pool compositions, and variations in the pre-treatment period. In addition, placebo-in-time and equivalence tests do not indicate systematic deviations in the pre-treatment period, supporting the credibility of the identification strategy.

However, the results should be interpreted with caution. They reflect the dynamics of the Indonesian case, shaped by the country's specific political and economic context, and therefore cannot be directly generalized to other settings. Rather than providing a general estimate of the effect of democratization on FDI inflows, the findings illustrate how such effects may unfold over time in a particular democratization episode. If anything, a more cautious takeaway is that part of the mixed empirical evidence in the literature (see Section 2) may stem from cross-country approaches that pool observations across different phases of democratization: by combining periods of short-run adjustment with later stages of stabilization, such estimates may obscure the underlying dynamic pattern and lead to heterogeneous or ambiguous average effects. In this sense, the Indonesian case highlights the importance of distinguishing between short- and long-run effects when evaluating the relationship between democratization and FDI inflows. However, given the exceptional severity of the 1998 crisis episode in Indonesia, the estimated post-transition deviations should not be interpreted as isolating the effect of democratization alone. Rather, they are more appropriately interpreted as estimated deviations from a counterfactual trajectory associated with the broader transition period, within which some residual crisis-related dynamics may remain.

Future research could extend this framework to other country cases to assess whether similar dynamic adjustment patterns are observable in different regional and institutional contexts. Comparative evidence across multiple democratization episodes would help clarify the extent to which the Indonesian experience reflects a more general transition mechanism or a country-specific trajectory.

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Appendix

Table A/1. Indonesia's and donor countries' pre-treatment-period means of outcome and covariate variables

Country	Total FDI	FDI per capita	FDI over GDP	KOF Index	Primary school	Growth	GDP per capita	Nominal GDP
Indonesia	1,649,076,923	8.50	1.07	42.55	114.72	6.48	1,513	124,612,978,718
donor countries								
Angola	213,244,769	17.93	3.06	36.91	75.96	1.56	2,504	5,596,535,463
Bahrain	351,453,708	657.45	5.97	61.05	109.98	4.79	20,312	5,062,883,073
Burkina Faso	5,063,452	0.51	0.22	23.11	32.39	4.05	344	2,484,928,799
Cameroon	30,457,139	2.93	0.34	32.72	94.19	−0.11	1,402	11,329,316,216
Central African Rep.	1,657,944	0.60	0.22	26.07	71.05	1.16	511	1,139,069,037
Chad	19,164,703	3.19	1.53	23.49	45.60	3.69	597	1,407,204,441
China	13,079,476,923	11.04	2.37	37.28	119.95	10.79	1,038	429,363,194,315
Congo, Dem. Rep.	−1,371,538	−0.04	−0.01	32.52	82.65	−2.35	770	7,370,669,848
Congo, Rep.	49,061,157	19.50	2.01	48.49	124.75	0.87	2,389	2,359,293,680
Cote d'Ivoire	91,462,356	6.88	0.83	42.15	71.53	1.77	1,873	10,303,838,018
Djibouti	1,272,265	2.16	0.29	41.20	33.11	−0.58	1,895	441,968,235
Egypt	841,618,382	14.83	1.99	51.81	87.79	4.79	1,930	44,458,350,310
Equatorial Guinea	47,240,685	83.18	27.31	37.36	135.03	12.40	484	110,761,330
Eswatini	48,462,315	55.02	5.27	44.05	93.86	7.42	1,794	993,121,694
Gabon	−17,140,727	−8.77	−0.16	47.61	144.34	2.55	8,390	4,444,436,219
Guinea	11,912,538	1.81	0.29	34.07	36.24	3.96	539	6,764,013,794
Indonesia	1,649,076,923	8.50	1.07	42.55	114.72	6.48	1,513	124,612,978,718
Jordania	19,360,196	6.31	0.35	61.07	98.08	2.58	3,653	5,610,079,545
Kuwait	148,348,982	88.49	0.58	63.35	89.49	7.46	22,999	21,937,575,456
Lao PDR	28,390,769	5.79	1.76	26.33	100.98	5.44	645	1,371,611,573
Mauritania	5,257,691	2.69	0.35	33.76	53.59	2.63	1,444	1,638,610,537
Morocco	232,185,708	9.15	0.71	46.18	72.43	4.73	1,655	28,360,839,069
Mozambique	19,577,692	1.31	1.17	30.17	71.31	3.86	207	3,264,435,887
Myanmar	107,411,467	2.58	3.31	28.27	101.04	2.60	190	2,699,032,089
Nigeria	775,233,460	7.80	1.29	40.55	91.31	2.64	1,458	75,211,930,427

(continued)

Table A/1. Continued

Country	Total FDI	FDI per capita	FDI over GDP	KOF Index	Primary school	Growth	GDP per capita	Nominal GDP
Oman	108,140,756	66.16	1.01	45.88	79.89	6.10	17,607	10,899,381,376
Papua New Guinea	139,109,674	34.45	3.77	37.56	58.77	4.73	2,173	3,817,254,546
Qatar	53,060,769	102.91	0.64	52.60	99.03	3.08	39,542	6,884,742,208
Rwanda	9,939,308	1.46	0.50	21.27	71.91	0.05	352	1,853,657,160
Saudi Arabia	369,950,391	51.88	0.32	60.42	98.72	1.60	26,579	118,220,826,323
Singapore	4,824,181,681	1481.90	10.30	80.23	102.34	7.85	23,378	45,106,820,918
Suriname	−69,323,077	−171.43	−10.88	43.28	134.24	0.23	6,501	703,070,811
Tanzania	27,721,641	0.92	0.35	33.89	72.36	3.68	511	8,506,758,808
Togo	1,995,902	0.60	0.11	42.84	92.93	2.73	675	1,785,559,190
Tunisia	211,730,434	24.57	1.54	53.99	114.47	4.08	2,153	12,638,384,191
Uganda	29,386,923	1.43	0.61	25.52	70.76	5.08	377	4,508,337,027
United Arab Emirates	118,298,462	54.57	0.22	58.72	99.80	3.21	65,250	49,324,518,576
Viet Nam	622,932,269	8.76	4.07	33.38	108.66	6.51	729	17,468,304,439
Zimbabwe	16,601,771	1.45	0.23	44.01	109.74	3.49	1,582	7,257,300,538