

Lone Wolves, Sleeping Elephants or Predator Hawks? Institutional Shocks and Counterfactual Long-Run Development Paths¹

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Abstract

This paper examines the importance of institutional shocks to long-run development. Our empirical method offers a clear empirical test to distinguish between three models of institutional shocks. We define gradual institutional change without a major shock, institutional change imposed by a major shock, and structural breakdowns. Drawing on several case studies from a sample of 56 countries for the period 1820-2015, we identify a series of institutional shocks and construct a synthetic counterfactual scenario of long-run development in response to a shock. Our evidence suggests that the long-run impact of institutional shocks on growth and development depends on the initial and pre-shock country-level characteristics. Gradual institutional changes help foster long-run development while changes imposed by a shock produce a large but temporary deviation from the long-run growth equilibrium. By contrast, institutional breakdowns produce a permanent change of long-run growth path, and depend on the nature of the shock.

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1. Introduction

Economic growth and its institutional determinants are by now a large body of literature. Since the work of Douglas North and others, economists have been promoting different theories to explain how institutions shape economic development (North 1990, 2005). Many different empirical studies have provided for multiple assessments of the role institutions in determining successful and unsuccessful economic growth.

In their classical survey of the literature, Acemoglu et al (2005) summarize the most important ideas. Institutions shape economic features, therefore, they matter for growth. Institutions constraint economic actors, they produce (positive or negative) incentives (from investment decisions to acquisition of human capital and technology), and they influence the organization of production at micro and macroeconomic levels. Consequently, institutions are important determinants for economic outcomes. The authors point out that a vast economic literature analyzes the extent to which institutions help or hurt these economic outcomes. Variables such as distribution of political power, social organization, legal institutions (enforcement of property rights), constraints on the executive (power-holders), likelihood of capture and rent seeking are important. In a related article (Acemoglu and Johnson 2005), it is suggested that there are two kinds of institutions, property rights institutions (they limit expropriation by the state) and contracting institutions (they facilitate exchange between private parties). The authors conclude that property rights institutions prevail in the long run, they shape growth, whereas contracting institutions seem to be less important (because individuals can find alternative ways to ineffective contracting institutions). These findings seem to contradict the work of La Porta et al (2008) who suggest, in a different strain of the literature, that both property and contracting institutions (labeled under common and civil law systems) determine successful growth. Other possible explanations such as geography and culture also provide for concurring powerful explanatory variables (Acemoglu and Robinson 2012).

Within the New Institutional Economics, a new theme has emerged from this vast literature on institutions and growth – institutional reform. If certain institutions promote economic growth, it is only natural that economists should discuss how less successful economies should adjust and change their institutions in order to narrow the GDP gap (or other relevant economic indicators) and make them more successful economies. Nevertheless, there are important considerations. If institutions are endogenous, not only they are slow to change, but the needed redistribution of political power is challenging. Alternatively, institutional reform could be driven by external shocks. However, such mechanism requires a confluence of factors (beliefs, leadership, opportunity) that makes success difficult to achieve. For example, Alston (2017) describes two cases – United States 1783-1789 and Brazil 1985-2015. Yet the results seem hardly deterministic and chance seems to play a significant role. Alston et al (2018) add two other examples, Argentina 1912-1955 (from checks and balances to populism) and Ecuador 1998-2016 (from neoliberal to inclusive politics), that illustrate how external shocks seem better at determining institutional failure rather than actual successes.

A key point in all these discussions is how institutional changes explain growth trajectories. Natural experiments are rare. In other words, it is difficult to compare the same economy with and without institutional changes in order to assess the extent to which these shocks made a significant difference in the long run. Acemoglu et al (2005) discuss two intriguing examples. One is European colonies. Not exactly the same economies, but similar areas of the world subject to difference patterns of European expansion. The literature seems to uncover a multitude of factors that could explain current successes and failures - “latitude specific” technology, extracting institutions (determined by mortality rates of settlers), legal transplants. A more significant institutional “laboratory”, however, is Korea. Subject to the

same political, social, cultural and legal institutions until 1945, it suffers an external shock in the period 1945-1953, and it is divided in two since then. Institutions differ considerably between North and South Korea after 1953. Up to the late 1950s there is no big difference in terms of GDP per capita. However, by the late 1960s differences emerge - South Korea was one of the Asian miracles while North Korea was a central planning economy. By the 1990s the difference in terms of GDP per capita was astronomical - USD 16,100 in South Korea against USD 1,000 in North Korea by 2000. In 2017 (estimation), the numbers were about USD 30,000 against USD 1,000 respectively. Clearly political and legal institutions in South Korea promote economic growth whereas in North Korea foster poverty and economic stagnation.

Institutional natural experiments such as Korea are rare. In this article, we develop an alternative methodology to test for the impact of institutional shocks. Using a sophisticated econometric approach, we construct a synthetic counterfactual that allows a comparison between the same economy with and without a particular shock. This comparison can confirm standard results from short-term analysis (i.e., how a particular shock affected GDP growth in the following years) but, more importantly, it allows long-term analysis to be discussed (i.e., whether or not a particular shock changed growth trajectories for a long period of time).

We start with a theoretical concern, the distinction between change imposed by shock (a temporary change with impact in GDP growth but no long-term significance) and structural change (a permanent boost or breakdown with impact in GDP growth for a long period of time). With the exception of Korea and maybe the European colonization process, the difference between shock and structural breakdown, for example, is usually unclear in the institutional literature since the window of observation is limited (a few years, maybe a decade) and conclusive results are difficult. In other words, there is a tendency to treat shock and breakdown, specifically in the case of political institutions, as equivalent concepts because the counterfactual does not exist. The advantage of our approach is to document this distinction, provide for an alternative exploratory analysis and help our understanding of institutional changes that determine long-run success or failure.

We should recognize that our empirical method does not fully resolve the identification issues inherent in the empirical work on institutions and long-run development. Our method relies on the parallel trend assumption by estimating the synthetic long-run development in the absence of the unanticipated institutional shocks. The key issue revolves around the spillovers of trade, conflict and migration influenced directly by the postulated institutional shocks. Such spillovers might render the parallel trend assumption questionable since the separation of treated country from its control peers would not yield a valid inference on the counterfactual outcomes. We remedy these concerns by excluding the countries experiencing the same type of institutional shock in the same year as the treated country from the donor pool (Abadie et. al. 2010). The exclusion of simultaneously treated countries from the donor pool ensures that the donor countries do not suffer from the institutional imitation of the shock of the treated country which makes counterfactual outcomes comparison consistent both in time and space. This particular exclusion also partially alleviates the spillovers of donors to the treated countries which might arise from trade, conflict, migration and other external factors at hand since the donor group is not affected by the same institutional shock as the treated country. Finally, we might want to interpret the results with caution when the analyzed countries are large economies since these possible concerns are less relevant for small opened economies.

The conceptual framework is discussed in section 2. The following sections explain the empirical methodology to construct counterfactuals (section 3) and the choice of data, including the selection of countries to test the methodology (section 4). We bring together

countries from Latin America, Europe, Africa and Asia that have deserved careful analysis by economists. The results are discussed by type of institutional change, gradual, shock, and breakdown (section 5). Final remarks close the article (section 6).

2. Conceptual Framework and Institutional Theory

We distinguish three models of institutional change:

(i) **Gradual change** – there is no major event introducing institutional change, but a set of continuous institutional changes which reinforce long-run growth paths. Ideally, a fully democratic regime operates by gradual change – negotiated institutional reform that helps growth. For example, rather than constitutional shocks, gradual change is achieved by constitutional amendments that mature as political power is slowly reshaped.

(ii) **Change imposed by shock** – there is a major (external) event that imposes institutional change that impacts growth in the short-run (boosting or reducing growth, depending on the nature of the shock) but does not derail the long-run path.

(iii) **Structural change** – there is a major event that changes the long-run path (boosting or reducing long-run growth, depends on the nature of the shock). An institutional breakdown would be an example of a structural change.

The literature in the New Institutional Economics makes a clear distinction between what we label gradual change (intrinsically endogenous) and the two other forms of change (naturally exogenous). However, it tends to confuse both shock and structural change because, in a standard short-run analysis (for example, within a decade), they look alike. Our method introduces a clear and clean empirical test to distinguish these three models of institutional change. Empirically, all three contours can be tested. If an economy has gradual change only, we should observe close to zero effect of any plausible exogenous shock on growth paths (keeping the long-term trajectory unchanged but with a clear positive slope). However, if change imposed by shock is prevalent, then there could be a large but temporary effect on the growth path but without impact toward long-run outcomes. Finally, if there is a structural change, then we should observe something like a clear divergence between actual and estimated (synthetic counterfactual) performance.

Notice we test our empirical strategy with events that have been discussed in the literature as major shocks. Therefore, to a large extent, the choice of events is not driven by our methodology, but rather by an exogenous understanding of political and economic history. Many historical events can be seen as having all three elements – an endogenous element (reflecting the gradual change of socioeconomic determinants), an exogenous element that imposes short-term consequences, and an exogenous element with long-term consequences. Our conceptual understanding is to identify the preponderant element. If all three characteristics coexist in a single event, we denote such event as structural change when the long-run growth path seems affected in a statistically significant way. If not, then such event is a candidate to change imposed by shock. By the same reasoning, we test for statistical impact on growth in the short-run. If it fails this second test, we understand that event to be gradual change.

3. A Formal Model of Institutional Shocks

Suppose one can observe a finite set of countries $i = 1, 2, \dots, J + 1$, which may be exposed to the set of independent institutional shocks. Without the loss of generality, suppose that only the

first country is exposed to the shock shift while the remaining J number of countries in its neighborhood are the potential control units in the donor pool used to evaluate the long-run effect of institutional shocks. Let $\ln y_{i,t}^{No-Shock}$ be the economic outcome of interest for i -th country in the absence of the institutional shock at time t within the discrete time horizon $t = 1, 2, \dots, T$, and let T_0 denote the number of pre-shock periods from the time horizon such that $1 \leq T_0 < T$. Suppose the economic outcome of interest in the presence of the institutional shock is denoted as $\ln y_{i,t}^{Shock}$, and assume that the period of institutional shock lasts from $T_0 + 1$ to T .

Assumption #1: *The institutional shock in country i at time T_0 is independent of the economic outcome before the shock period so that for $t \in \{1, \dots, T_0\}$ and $i = \{1, 2, \dots, N\}$, we have $\ln y_{i,t}^{Shock} = \ln y_{i,t}^{No-Shock}$.*

Assumption #1 immediately implies that the institutional shock has no prior impact on the economic outcome of interest. If the institutional shock is anticipated, the outcome before the intervention may possibly react to the shock and violate Assumption #1. In such setup, T_0 may be adjusted to be the first period in which the outcome of interest may react to the institutional shock. Following [Abadie and Gardeazabal \(2003\)](#), [Rosenbaum \(2007\)](#) and [Abadie et. al. \(2010\)](#), we further assume that the countries exposed to the institutional shocks do not interfere, and exclude the potentially interfering countries from the donor pool to ensure that the economic outcome of the unaffected countries are not influenced by the institutional shocks taking place in the affected countries.

Let $\lambda_i = \ln y_{i,t}^{Shock} - \ln y_{i,t}^{No-Shock}$ describe the effect of the institutional shock for country i at time t , such that $\lambda_i = (\lambda_{i,T_0+1}, \dots, \lambda_{i,T})$, and let $D_{i,t} = 1 \cdot [(i \in J+1) \rightarrow \{0, 1\}]$ be a simple linear indicator function which takes the value of 1 if the i -th country is exposed to the institutional shock at time t , and 0 otherwise. Hence, the observed economic outcome for country i at time t is given by:

$$\ln y_{i,t} = \ln y_{i,t}^{No-Shock} + \lambda_{i,t} \cdot D_{i,t} \quad (1)$$

Our goal is to estimate the effect of institutional shocks on the economic outcome of interest. By default, Eq. (1) implies that the level of outcome in the absence of the institutional shocks invokes the counterfactual unobserved to the econometrician. Only the first country from the finite set is affected by the institutional shock strictly after the period T_0 , so with $1 \leq T_0 \leq T$, the distribution of institutional shock is described by the following set:

$$D_{i,t} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

which implies that for $t > T_0$, our aim is to estimate the sequence $\lambda_i = (\lambda_{i,T_0+1}, \dots, \lambda_{i,T})$ to construct the counterfactual level of economic outcome without the institutional shock given by:

$$\lambda_{1t} = \ln y_{1,t}^{Shock} - \ln y_{1,t}^{No-Shock} = \ln y_{1,t} - \ln y_{1,t}^{No-Shock} \quad (3)$$

where $\ln y_{1,t}^{Shock}$ is observed ex-ante while $\ln y_{1,t}^{No-Shock}$ is the unobserved missing counterfactual in the absence of institutional shock which we aim to estimate. The major drawback to the missing counterfactual approach is that $\ln y_{1,t}^{No-Shock}$ is unobserved and needs to be estimated ex-post. Suppose that the counterfactual economic outcome is given by the latent factor model with unobserved components:

$$\ln y_{1,t}^{No-Shock} = \theta_t + \eta_t \mathbf{Z}_i + \delta_t \mathbf{M}_i + \varepsilon_{i,t} \quad (4)$$

where θ_t is an unknown common factor of the treated unit with the control countries from the donor pool with constant linear between-country factor loadings, $\mathbf{Z}_i \in \mathbb{R}^r$ is a $r \times 1$ vector of unobserved covariates unaffected by the institutional shock, η_t is an $1 \times r$ vector of unknown parameters, $\delta_t \in \mathbb{R}^F$ is a $1 \times F$ vector of unobserved common factors, and $\mathbf{M}_i \in \mathbb{R}^F$ is a $F \times 1$ vector of unknown factor loadings. The unobserved country-specific and time-varying transitory shocks are given by the stochastic disturbance term, $\varepsilon_{i,t}$. Furthermore, we impose the zero conditional independence assumption on the behavior of the transitory shocks, which implies that $E(\varepsilon_{i,t} | \mathbf{Z}, \mathbf{M}) = 0$, to ensure that the counterfactual is consistently estimated, and that the transitory shocks fail to exhibit a non-zero covariance with the set of common and unknown factor loadings. Hence, Assumption #1 allows heterogeneous responses to multiple unobserved factors by embedding the time trend models into the counterfactual economic outcome.

The basic idea behind Assumption #1 is to reweight the control group from the donor pool so that a synthetic country is set to match $\mathbf{Z}_i \in \mathbb{R}^r$ on a subset of pre-shock characteristics of the affected country. By default, $\mathbf{M}_i \in \mathbb{R}^F$ is automatically matched with the synthetic counterfactual process. Matching pre-shock characteristics of the affected country to its similar counterparts unaffected by the institutional shock at time T_0 ensures that the unobserved country-level heterogeneity bias is not projected out the counterfactual model specification.

Suppose that the donor pool comprises $J-1$ countries excluding the affected ones. Let $J \times 1$ vector of weights be described by $\mathbf{W} = (w_2, w_3, \dots, w_{J+1})'$ such that $w_j \geq 0$ for $j = 2, \dots, J+1$ and $w_2 + \dots + w_{J+1} = 1$. Each particular value of the vector \mathbf{W} represents a potential synthetic control. The synthetic control is a weighted average of control countries sharing similar pre-shock characteristics captured by $\mathbf{Z}_i \in \mathbb{R}^r$. Pointwise, the economic outcome of interest for each synthetic control, indexed by \mathbf{W} , is given by:

$$\sum_{j=2}^{J+1} w_j \ln y_{jt} = \theta_t \cdot \sum_{j=2}^{J+1} w_j \mathbf{Z}_j + \delta_t \cdot \sum_{j=2}^{J+1} w_j \mathbf{M}_j + \sum_{j=2}^{J+1} w_j \varepsilon_{jt} \quad (5)$$

where the vector of weights from the donor pool on the synthetic counterpart of the shock-affected country, $(\omega_2^*, \dots, \omega_{J+1}^*)$, is such that $\sum_{j=2}^{J+1} \omega_j^* \ln y_{j,1} = \mathcal{Y}_{11}$, $\sum_{j=2}^{J+1} \omega_j^* \ln y_{j,2} = \mathcal{Y}_{12}$,

$$\sum_{j=2}^{J+1} \omega_j^* \ln y_{j,T_0} = \mathcal{Y}_{1T_0}, \text{ and } \sum_{j=2}^{J+1} \omega_j^* \mathbf{Z}_j = \mathbf{Z}_1.$$

Theorem #1: Suppose $\exists \mathbf{W}^*$ is such that the synthetic country matched the affected country in the pre-shock period, $\sum_{j=2}^{J+1} \omega_j^* \ln y_{j,t} = \ln y_{1,t}, \forall t \in \{1, \dots, T_0\}$, and $\sum_{j=2}^{J+1} \omega_j^* \mathbf{Z}_j = \mathbf{Z}_1$ where $\sum_{t=1}^{T_0} \delta_t' \delta_t$ is non-singular. For all $t > T_0$, the difference between the synthetic counterfactual and actual economic

outcome, $E \left[\ln y_{1,t}^{\text{No-Shock}} - \sum_{j=2}^{J+1} \omega_j^* \ln y_{j,t} \right] \rightarrow 0$, disappears as $T_0 \rightarrow \infty$, or if T_0 is large relative to $\mathcal{E}_{i,t}$.

Theorem #1 implies that under the standard conditions, the synthetic control with $\mathbf{W} = (\omega_2, \omega_3, \dots, \omega_{J+1})'$ replicates the missing counterfactual. Then, an approximately unbiased estimator of $\lambda_{1,t}$ is given by the underlying difference between the observed economic outcome and the synthetic control holding the scale of transitory shocks constant. This suggests that:

$$\hat{\lambda}_{1,t} = \ln y_{1,t} - \sum_{j=2}^{J+1} \omega_j^* \ln y_{j,t} \quad (6)$$

For $t \in \{T_0 + 1, \dots, T\}$ as an estimator of $\lambda_{1,t}$. The set of counterfactual outcomes weighted by the additive weights from the donor pool can hold only if $(\ln y_{11}, \dots, \ln y_{1,T_0}, \mathbf{Z}_1')$ inside the convex hull of $\{(\ln y_{21}, \dots, \ln y_{2,T_0}, \mathbf{Z}_2'), \dots, (\ln y_{J+1,1}, \dots, \ln y_{J+1,T_0}, \mathbf{Z}_{J+1}')\}$. If the standard conditions for the missing counterfactual hold, the synthetic control group is constructed as a weighted combination of the unaffected countries such that Theorem #1 holds. This enables us to match the pre-shock covariate-level characteristics from the data-generating process for $\ln y_{i,t}$ by the synthetic control group.

The fit of the synthetic counterfactual outcome for the country affected by the institutional shock may be poor if the interpolation biases of the constant linear model are large relative to the sample size. The traditional approach advocated by [Abadie et al. \(2012\)](#) and [Cavallo et al. \(2013\)](#) is to adjust the underlying model specification with the appropriate covariates set to avoid the poorly fitted synthetic control, or to remove the observations with pre-shock root mean square prediction error (RMSE) greater than $\sqrt{3}$ multiplied by average pre-shock RMSE ([Acemoglu et al. 2016](#)). We attempt to minimize the biases ex-ante by setting the root mean square prediction error within the 10% of pre/post fit, which ensures that country-level weights from the donor pool fail to produce implausible counterfactual. Restricting the root mean prediction error within the 10% pre/post fit neighborhood ensures that the countries from the donor pool share very similar characteristics with the treated country exposed to the institutional shocks. This implies that:

$$\frac{\sum_{t=T_0+1}^T (\ln y_{j,t} - \ln y_{i,t}^{No Shock})}{\sum_{t=1}^{T_0} \ln y_{j,t} - \ln y_{i,t}^{No Shock}} \leq .10 \quad (7)$$

Our approach is to generalize the difference-in-differences model traditional used in such setup. The Adding the fixed-effects to the model specification, the underlying effect of the institutional shock is obtained only if δ_t is constant for all $t = 1, 2, \dots, T$ where $T_0 \in T$. Even though the covariates and unobserved confounding covariates may yield reasonably unbiased effect of institutional shocks on the economic outcomes, the effect is restricted to the covariates constant over time, which can be eliminated using the standard first-differencing transformation. The latent factor model from Eq. (4) overcomes the time-invariance of unobserved characteristics and allows these to vary over time, and, hence, does not eliminate the unobserved factors. A synthetic control group for the affected country of the form $\sum_{j=2}^{J+1} \omega_j^* \mathbf{Z}_j = \mathbf{Z}_1$, and $\sum_{j=2}^{J+1} \omega_j \mathbf{M}_j = \mathbf{M}_1$ provides a reasonably unbiased estimator of $\ln y_{i,t}^{No Shock}$. Such conditions approximately hold under the standard conditions which implies that the factor model in Eq. (4) yields the synthetic control group both from common and unobserved factors that can fit \mathbf{Z}_1 , and the set of pre-shock economic outcomes if \mathbf{Z}_1 fits \mathbf{M}_1 .

Proposition #1: *Suppose the latent factor for the unobserved counterfactual holds so that $\theta_t + \eta_t \mathbf{Z}_i + \delta_t \mathbf{M}_i + \varepsilon_{i,t}$ is a reasonable approximation of the economic outcome before the institutional shock. Assuming zero conditional mean independence on the transitory shocks, $\hat{\lambda}_{1,t} = \ln y_{1,t} - \sum_{j=2}^{J+1} \omega_j^* \ln y_{j,t}$ yields an approximately unbiased effect of institutional shock. The equilibrium condition in Eq. (5) yields three regimes of institutional shock:*

(i) *If $\ln y_{i,t}^{No Shock} < \ln y_{i,t}^{Shock}$ for all $t > T_0$ in non-random treatment group, denoted $i = J-1$ then $\hat{\lambda}_{1,t} \cong 0$ implies that an institutional shock is asymptotically equivalent to gradual institutional change for any set of random variables $F = f\{Z, M : \Theta, t \in (t \leq T_0 \leq T)\} \rightarrow i^{+/-}$.*

(ii) *If $\ln y_{i,t}^{No Shock} > \ln y_{i,t}^{Shock}$ for all $t > T_{0+k}$ where $k \notin \{T_0 - t\}$, but $\ln y_{i,t}^{No Shock} \approx \ln y_{i,t}^{Shock}$ as $T_{0+k} \rightarrow T$, then we have a unique solution $\{\hat{\lambda}_{1,t-k} < 0, \hat{\lambda}_{1,t+k} > 0\}$ where $k \in T$ and $k \notin T_0$, which is asymptotically equivalent to the institutional change imposed by a shock at time T_0 as $t+k \rightarrow T$ for any set of random variables $F = f\{Z, M : \Theta, t \in (t \leq T_0 \leq T)\} \rightarrow i^{+/-}$.*

(iii) *If $\ln y_{i,t}^{No Shock} > \ln y_{i,t}^{Shock}$ for all $t > T_0$, there exists a unique solution $\hat{\lambda}_{1,t} < 0$, which is asymptotically equivalent to the institutional breakdown for any set of random variables $F = f\{Z, M : \Theta, t \in (t \leq T_0 \leq T)\} \rightarrow i^{+/-}$ as $t \rightarrow T$.*

The proposition #1 suggests three possible directions of institutional change. Under regime (i), $\hat{\lambda}_{1,t} \cong 0$ which implies that the synthetic control group is a sufficient approximation of the

treated country affected by the institutional change with no major deviation from the long-run growth path. The equilibrium dynamics under regime (ii) is characterized by the institutional change imposed by a major shock at time T_0 . The shock is associated with a temporary deviation of the economic outcome from its long path so that $\ln y_{i,t}^{No\ Shock} > \ln y_{i,t}^{Shock}$ holds up until the juncture point k where $\{k \in T\} > T_0$ so that the long-run equilibrium path of economic outcome is restored after k -th time period. Hence, $\hat{\lambda}_{1,t+T_0} < 0$ and $\hat{\lambda}_{1,T_0+k} > 0$. The equilibrium dynamics under regime (iii) suggests that the institutional shock is equivalent to the institutional breakdown. The breakdown implies a permanent deviation of the economic outcome on the downward path relative to the level in the period before institutional change so that $\hat{\lambda}_1 < 0$ for each time subset after T_0 . The regimes of institutional shocks under (ii) and (iii) clearly impose non-zero long-run shock effects except that regime (ii) imposes no major long-run penalty while the opposite holds for the series of institutional shocks under regime (iii).

The ultimate question regarding the institutional shocks pertains to the composition of the synthetic control group. If Assumption #1 holds, the affected countries should follow the parallel covariate-level trends before T_0 to construct the unobserved counterfactual. To avoid excessively large extrapolation, we ex-ante restricting the donor pool to the set of countries which ensures that post/pre-institutional shock RMSE is within 10% bound as implies by Eq. (7). Such ex-ante restriction effectively ensures that the missing counterfactual is not driven by the subset of countries possibly violating the parallel trend assumption.

Assumption #2 (Parallel Trends): *The economic outcome in the treated country before the institutional shocks follows the parallel trend in the unaffected countries. Hence, the identification of the effect of institutional shocks is possible as long as $E(\ln y_{i,t}^{t-T_0} - \ln y_{j,t}^{t-T_0} | D_{i,t} = 1, \mathbf{X}_{i,t}) = E(\ln y_{i,t}^{t-T_0} - \ln y_{j,t}^{t-T_0} | D_{i,t} = 0, \mathbf{X}_{i,t})$ holds regardless of the composition of the donor pool $j = 2, \dots, J+1$ if the underlying condition in Eq. (7) is satisfied.*

Assumption #3 (Independence of Institutional Shock Conditional on Past Outcomes). *Similar economic outcome is anticipated in the pre-institutional shock is anticipated so that the outcome without the treatment imposed by institutional shock is similar to the outcome path in the affected country is similar to the outcome path in the post-shock period which implies that $\ln y_{i,t}^{No\ Shock} \perp (\mathbf{X}_{i,t}, \ln y_{i,t}^{Shock})$.*

Once Assumption #1 through #3 are met, the discrepancy between the affected and unaffected countries before the institutional shock at time T_0 can be measured upon the error restriction in Eq. (7). Let \mathbf{W} be a $J \times 1$ vector of non-negative weights such that $\mathbf{W} = (w_2, \dots, w_{J+1})'$ for $j = 2, \dots, J+1$, we have $w_2 + \dots + w_{J+1} = 1$. Each value from \mathbf{W} represents an unrestricted weighted average of the control countries from the donor pool, and serves as a potential synthetic control for the country affected by the institutional shock. The convexity of combinations from the untreated countries ensures that the weights, by default, are not larger than unity. The economic outcome of interest is given ex-ante across the discrete time horizon $t = 1, 2, \dots, T$ for the country affected by the institutional shocks, and for the countries from

the donor pool unaffected by such a shock. Let $\mathbf{G} = (g_1, g_2, \dots, g_{T_0})$ represent a linear combination of pre-shock outcome for the $s = 1, 2, \dots, T_0$ pre-shock time horizon subset of T so that:

$$\ln \bar{y}_{i,t}^{-\mathbf{G}} = \sum_{s=1}^{T_0} g_s \ln y_{i,t}^{No\ Shock}$$

where $g_1 = g_2 = \dots = g_{T_0-1} = 0$ and $g_{T_0} = 1$ imply that the weighted combination of outcome is the value of outcome immediately before the institutional shock, $\ln \bar{y}^{-\mathbf{G}} = \ln y_{i,T_0}$. In addition, if $g_1 = g_2 = \dots = g_{T_0-1} = 1/T_0$, then $\ln \bar{y}_{i,t}^{-\mathbf{G}} = T_0^{-1} \sum_{s=1}^{T_0} \ln y_{i,s}$ is a simple unweighted average of pre-shock outcome period. Let \mathbf{P} represent a linear combination defined by the vector set $\mathbf{G} = \mathbf{G}_1, \dots, \mathbf{G}_P$. Suppose the pre-shock characteristics of the country affected by the institutional shock are described by the covariate vector $\mathbf{X}_1 = \{\mathbf{Z}'_1, \mathcal{Y}_1^{G_1}, \dots, \mathcal{Y}_1^{G_P}\}$ with the $k \times 1$ dimension, where $k = r + P$. Analogously, let $\mathbf{X}_0 = \{\mathbf{Z}'_j, \mathcal{Y}_j^{G_1}, \dots, \mathcal{Y}_j^{G_P}\}$ be the covariate vector of the characteristics for the countries unexposed to the institutional shock of the treated country.

The key question pertains to the distance in covariate-level characteristics between the affected country and its counterpart unaffected by the institutional shock. We minimize the pre-shock distance between the affected and unaffected countries, and choose the vector \mathbf{W}^* which suggests that the covariate distance between the affected country and the unaffected ones, $\|\mathbf{X}_1 - \mathbf{X}_0 \mathbf{W}\|$, is minimized to ensure that post/pre RMSE is within 10% bound as advocated by Eq. (7), subject to $w_2 \geq 0, \dots, w_{J+1} \geq 0$, and $w_2 + \dots + w_{J+1} = 1$. The underlying criteria in Eq. (7) ensures that the synthetic control conditions hold approximately for the weight vector. Hence, only a couple of linear combinations are considered in all available periods for pre-shock outcome variable. Our measure of mismatch in covariate characteristics between affected and unaffected countries follows the approach advocated by [Abadie et. al. \(2010\)](#). More specifically, we use a positive semi-definite a fully symmetric $r \times r$ \mathbf{V} matrix in the characteristics-based distance minimization between the affected countries and unaffected ones:

$$\|\mathbf{X}_{1,j} - \mathbf{X}_{0,i} \mathbf{W}\|_{\mathbf{V}} = \sqrt{(\mathbf{X}_{1,j} - \mathbf{X}_{0,i} \mathbf{W})' \mathbf{V} (\mathbf{X}_{1,j} - \mathbf{X}_{0,i} \mathbf{W})} \quad (8)$$

where \mathbf{W} is the distance-minimizing vector of positive weights used to match the treated and unaffected countries in terms of covariate characteristics before the institutional shock, $\mathbf{X}_{1,j}$ is the covariate-level vector for the treated country, and $\mathbf{X}_{0,i}$ is the covariate-level vector of the unaffected country. Following [Abadie et. al \(2010\)](#), we avoid excessively large interpolation biases arising from the potentially non-linear relationship between the explanatory variables and the economic outcome of interest. Imposing the 10% acceptance threshold on post/pre-RMSE from Eq. (7) and ignoring non-linearities, the weight vector \mathbf{W} yields the set of positive and additive weights that match the treated country with the unaffected one before the institutional shock with less than 10% discrepancy between the

treated country and the synthetic control group. This implies that the composition of synthetic control group from the donor pool is effectively restricted to the countries that are most similar to the treated country for any given value of the $\mathbf{X}_{1,j}$. The optimum choice of \mathbf{V} affects the size of the post/pre RMSE and approximates the path of the outcome variable in the affected country before the institutional shock using a linear combination of countries from the donor pool with the set of positive weights from \mathbf{W} . [Abadie and Gardeazabal \(2003\)](#) select \mathbf{V} to minimize the post/pre RMSE for the entire pre-treatment period. In spite of the distance minimization, the discrepancy between the treated country and the control units may still be large which renders the unobserved counterfactual implausible. Our approach is to reshape the donor pool to match the treated country with the synthetic control group for the entire pre-shock period under the condition that RMSE is in the neighborhood of 10% error bound. One might object that the number of countries in the synthetic control group may be small. But this comes at the benefit of approximating the unobserved counterfactual for the treated country with a reasonable degree of precision, which helps us avoid the trap of artefact-based counterfactual. Such an artefact would not approximate the outcome trajectory of the treated country in the absence of the institutional shock, which could have non-trivial implications for the subsequent inference.

4. Data

4.1. The Dependent Variable

Our dependent variable is real per capita GDP adjusted for purchasing power parities (PPP) using Geary-Khamis conversion at 1990 constant prices. The data on per capita GDP is from Bolt and Van Zanden (2014) First Update of the Maddison (2010) database for the period 1820-2011. Wherever possible, discontinuous series is decomposed into a continuous per capita series using the aggregate region-level per capita GDP series to approximate the growth rates and per capita income levels in the between-benchmark years. Using the PPP conversion for the 1990 benchmark year, we calculate real per capita GDP for the period 2012-2015 using the revised real per capita GDP growth rates from *World Economic Outlook, April 2017 Edition*. We consider the recent historical reconstruction of real per capita GDP for United States ([Sutch 2007](#)), United Kingdom ([Broadberry et. al. 2015](#)), Spain ([Alvarez-Nogal and Prados de la Escosura 2013](#)), Portugal ([Reis 2011](#)), Sweden ([Schön and Krantz 2012](#)), Germany ([Pfister 2011, Burhop and Wolf 2005](#)), Italy ([Malanima 2011, Bafiggi 2011](#)) Considering the recently reconstructed per capita GDP for Switzerland ([Halbeisen et. al. 2017](#)), Slovenia ([Spruk 2018](#)), and Greece ([Kostelenos et. al. 2013](#)), post-1850 series is directly linked to the pre-1850 series back until the year 1820 using the previous estimates by [Maddison \(2007\)](#). For Latin America, the income per capita estimates from [Prados de la Escosura \(2009\)](#) are considered together with the recently updated estimates starting in 1820 for Argentina ([Newland and Poulson 1998, Della Paolera et. al. 2003, Newland and Ortiz 2001](#)), Brazil ([Leef 1982, Goldsmith 1986](#)), Chile ([Diaz et. al. 2007](#)), Colombia ([Kalmanovitz Krauter and Lopez Rivera 2009](#)), Mexico ([Coatsworth 1989](#)), Uruguay ([Bertola et. al. 1998](#)), and Venezuela ([Baptista 1997](#)). For South Africa, the historical reconstruction of per capita GDP for the Capy Colony by [Fourie and Van Zanden \(2013\)](#) is considered. Per capita GDP estimates are comparable across and within countries given a common benchmark year for the constant price adjustment and the usual PPP adjustment for international comparison purposes, and reflect the long-term development trends, tendencies, and trajectories as a first-order approximation of the economic development paths in the long term.

4.2 Covariates

4.2.1 Demographic and Health Covariates

The set of demographic and health covariates comprises population size, population density, population growth rate, life expectancy at birth and fertility rate. The data on population size and population growth is from [Maddison \(2007\)](#). Population density is expressed as the number of inhabitants within a country-level territory per square km. The data on population density is from *International Data Base* of U.S. Census Bureau for the period 1950-2015. For the period 1820-1950, we calculate population density by dividing the number of inhabitants with the size of the country area (in km²) for each individual year using the data on historical boundaries from [Klein Goldwijk et. al. \(2017\)](#). The data on life expectancy and fertility rate is from 2017 revision of *World Population Prospects* by United Nations for the period from 1960 onwards.

4.2.2 Institutional Covariates

The set of institutional covariates consists of the variables measuring the institutional structure and quality of institutions. We assemble covariates that capture judicial independence, corruption, rule of law, efficiency of public sector, political stability, economic freedom, and the structure of political institutions. The data on judicial independence is from [Feld and Voigt \(2003\)](#) and [Voigt et. al. \(2015\)](#) using de jure and de facto indices of judicial independence to capture the full set of effects on growth and development. The data on the control of corruption, quality of regulation, rule of law, public sector effectiveness and political stability is from [Kaufmann et. al. \(2011\)](#). These time-varying indicators of institutional quality capture the essential aspects of quality and resilience of institutional structure on country-year basis, namely the likelihood of government destabilization, quality of public services, the ability of government to implement sound policies and regulations to promote private sector development, strength of contract enforcement, property rights, the likelihood of crime and violence, as well as the extent of state capture by elites and private interests. The ability of these indicators to proxy the institutional quality is further strengthened by overcoming time-invariance of indicators traditionally used for such purpose. The data on economic freedom is from 2017 Index of Economic Freedom ([Miller and Kim 2017](#)), and includes the aggregate index composed of twelve different indices measuring the rule of law, size of government, regulatory efficiency, and open markets. The data on civil liberties is assembled from Freedom House annual reports for the period from 1973 onwards. The data on the structure of political institutions is from [Persson et. al. \(2003\)](#) and include the binary indicators of presidential system, majoritarian vs. proportional representation and a binary variable indicating whether or not the country is a federation. We also use the Polity IV composite measure of political institutions from [Marshall et. al. \(2016\)](#) to further capture the structure of political institutional framework.

4.2.3 Geography Covariates

The data on physical geography is from [Nunn and Puga \(2012\)](#). The set of geography-related covariates comprises binary variables indicating whether the country is an island, and whether or not the country is landlocked. We also consider the geography variables measuring soil

quality, fraction of the desert area, size of the area (in km²), fraction of the area in the tropical zone, the fraction of the area within 100km of ice-free coast, geo-centric terrain ruggedness proxy, latitude and longitude coordinates. These indicators allow us to control for the potential time-invariant effects of physical geography on growth and development.

4.2.4 Legal History Covariates

The data on the country-level legal history is from [La Porta et. al. \(2008\)](#). We proxy the country-level legal history by distinguishing between different legal families broadly aligned between the civil law and common law tradition. We match the per capita GDP data for the period 1820–2015 with the full set of binary variables indicating the legal family to which the national legal systems belong. The binary variables indicate whether or not the national legal systems belong to (i) British common law, (ii) French civil law, (iii) German civil law, and (iv) Scandinavian civil law tradition.

4.2.5 Culture Covariates

The set of culture covariates comprises three commonly used measures to address the relationship between culture and long-run economic outcomes. The first covariate captures latent cultural dimensions proposed by [Hofstede \(2003\)](#), namely power distance, level of individualism vs. collectivism, masculinity vs. femininity, uncertainty avoidance, long-term vs. short-term normative orientation, and indulgence vs. restraint. Following [Maseland \(2013\)](#) and [Klassing \(2013\)](#), we compute the first principal component as a latent quantitative measure of cross-country cultural differences. The second covariate captures the level of trust in a society. Using the data from 2015 Gallup World Poll, we compute the fraction of adult population agreeing with the statement that complete strangers can be trusted following [Knack and Keefer \(1997\)](#), [Temple and Johnson \(1998\)](#), [Zak and Knack \(2001\)](#), [Beugelsdijk et. al. \(2004\)](#), [Roth \(2009\)](#), and [Bjørnskov \(2012\)](#). The third covariate proxies the level of social capital to control for its effect on growth and development as suggested by [Putnam et. al. \(1994\)](#), [Helliwell and Putnam \(1995\)](#), [Knack \(2002\)](#), [Routledge and Von Amsberg \(2003\)](#), [Iyer et. al. \(2005\)](#), and [Beugelsdijk and Van Schaik \(2005\)](#). Following [Spruk and Keseljevic \(2016\)](#), we compute the first principal component from the battery of social capital variables: (i) marriage rate (in %), (ii) rate of volunteering (in %), (iii) the rate of helping strangers (in %), (iv) family and friends support (in %), and (v) the share of population donating to charity (in %). The resulting variables is a latent covariates proxying social capital differences across countries. We also consider the data on fractionalization by [Alesina et. al. \(2003\)](#) and use the three variables measuring ethnic, linguistic and religious fractionalization to further address the link between culture and long-run development

4.2.6 Human Capital and Macroeconomic Covariates

The set of human capital and macroeconomic covariates consists of the variables capturing human capital investment stock and the macroeconomic environment across and within countries. The human capital covariate comprises a combined index of average years of education ([Barro and Lee 2013](#)) and returns to schooling ([Psacharopoulos 1994](#)) is derived from the country-level Mincer equation estimates. The set of macroeconomic covariates consists of the share of investment in GDP (in %), trade openness defined as exports plus imports divided by the GDP. The former captures the level of capital intensity while the latter

captures the economy-level openness to international trade in goods and services. Both variables are standard covariates in cross-country growth and development regressions (Levine and Renelt 1992, Sala-i-Martin 1997, Frankel and Romer 1997, Sala-i-Martin et. al. 2004).

4.2.7 Initial Persistence Covariates

The set of initial persistence covariates comprises the level of per capita GDP in benchmark years and the year prior to the institutional shock following Barro (1991) and Barro and Sala-i-Martin (2004) conditional convergence framework. We also collect the data on the persistence of institutional, health and demographic variables in the initial year when the covariates values are observed. The full set of initial persistence covariates other than the dependent variable comprises the level of life expectancy in 1960, fertility rate in 1960, and the level of civil liberties in 1973. Following Acemoglu and Robinson (2006), the set of institutional persistence covariates consists of the aggregate Polity IV indicator and constraints on the executive variable in the year 1820.

4.3. Sample

Our sample comprises 58 countries² for the period 1850-2015 which totals 10,976 country/year-level observation. Table 1 summarizes the key covariate-level descriptive statistics for the full sample along with the covariate-specific averaging period.

TABLE 1 [INSERT HERE]

We have selected a few countries to illustrate the methodological use of the synthetic counterfactual. Particularly, we use specific countries to detect and document the three models of institutional change that we have reviewed in the theoretical section. Our selection of countries follows closely a diversity of situations that provide for a more informed analysis of institutional change.

We include countries from Latin America (Argentina, Brazil, Mexico) since they have been widely discussed by economists (Alston and Gallo 2010, Alston et al. 2010). A few European experiences are also analyzed (Spain, Portugal, Austria, Hungary, Slovenia, Italy). They highlight how we can have gradual change or institutional shock in the old continent. The Philippines and South Africa are also two interesting experiences, with significant political change in the last 150 years and influenced by the common law tradition. Finally, we included Egypt, Jordan, Turkey (all part of the former Ottoman Empire) and Morocco since they share political and religious challenges.

² Algeria, Argentina, Australia, Austria, Belgium, Brazil, Burma, Canada, Chile, China, Colombia, Czech Republic, Denmark, Egypt, Finland, France, Germany, Greece, Hong Kong, Hungary, India, Indonesia, Iran, Iraq, Ireland, Italy, Jamaica, Japan, Jordan, Lebanon, Malaysia, Mexico, Morocco, Nepal, Netherlands, New Zealand, Norway, Philippines, Poland, Portugal, Slovenia, South Africa, South Korea, Spain, Sri Lanka, Sweden, Switzerland, Syria, Thailand, Tunisia, Turkey, United Kingdom, United States, Uruguay, Venezuela, and Vietnam.

Table 2 indicates, for each example, the relevant institutional changes that are tested in our econometric analysis. They are essentially changes of political regime or major events like war or European Union membership. These events are chosen exogenously to the empirical methodology. They are the product of our understanding of political and economic history.

TABLE 2 [INSERT HERE]

5. Results

In this section, we present several case studies on the gradual institutional institutional change, change imposed by a shock, and on the structural change. Table 2 presents the full list of institutional breaks for the full set of case studies considered.

5.1. Gradual Institutional Change

5.1.1. Austria

In Figure 1, we present the long-run development path of Austria in the period 1820-2015 as a reference country of gradual institutional change. The figure presents a series of counterfactual scenarios in response to the set of institutional shocks. The evidence largely suggests that the institutional shocks experienced by Austria encouraged the long-run development path without major events introduction abrupt institutional changes but a set of continuous institutional changes achieved through gradual adjustments reinforcing long-run growth and development. However, not all shocks have the same implications for long-run development.

The evidence suggests that in the absence of the 1867 dual monarchy with Hungary, the counterfactual Austria would sustain higher economic growth compared to the real Austria. The counterfactual Austria is composed of the weighted combination of countries sharing similar pre-1867 institutional, cultural, and geographic characteristics, and long-term growth trends: The synthetic control group comprises Denmark (54%), Germany (29%), and Switzerland (17%). The 1867 dual monarchy does not appear to be a source of institutional breakdowns or an external shock with short-run growth effect since by the end of the estimation period, the synthetic Austria converges with the real Austria. A similar implication arises from the 1907 introduction of universal male suffrage. In the long run, the counterfactual path of Austria's development in the absence of the universal male suffrage is below the real growth path which suggests that early institutional reforms towards democracy benefitted Austria's economic development. The synthetic Austria without universal male suffrage is composed of Germany (90%), and Switzerland (10%) which appear to be the largest donors to the counterfactual series. A similar outcome is indicated by the collapse of the Austrian Empire in 1918, which led to the gradual dissolution of Austria into numerous small states. Our results show that, in the long run, the real Austria outperforms the counterfactual Austria and suggests that the imperial economic and political organization has been a net burden for long-run development. The long-run Austria appears to be poorer by 10% ($=22,247/24,661$) had the Austrian-Hungarian not collapsed, and poorer by 9% in the absence of 9% universal suffrage in 1907.

FIGURE 1 [INSERT HERE]

In 1955, Austria promulgated the State Treaty (*Österreichischer Staatsvertrag*) which re-established it as a sovereign and independent state. Our evidence shows that the State Treaty

turns out to be another gradual institutional shock benefitting Austria's long-run development. The counterfactual process suggests that Austria in the absence of the State Treaty would follow similar growth and development path than its peers from the synthetic control group until 1970s. The synthetic control group comprises Germany (34%), Netherlands (32%), Slovenia (22%), and Italy (11%). After 1970s, Austria sustained an upward growth and development trend while its peers embarked on the path of growth slowdown. The magnitude of long-run impact of the State Treaty is not trivial. In the absence of the State Treaty our estimates imply that Austria's per capita GDP by 2015 would falter by 14% ($=24,661/21,504$). The institutional shock with the most profound and long-lasting implications for Austria's long-run development appears to be the 1983 loss of absolute majority by Austrian social democrats (SPÖ) in the parliamentary elections. The end result of the election was a coalition government with the right-wing *Freedom Party of Austria* (FPÖ) which led to the major policy change, and marks the end of the Kreisky period in the postwar Austria. It also led to the decisive policy change towards structural reforms, simplification of the tax code, and privatization of state-owned enterprises. Our results show that the loss of absolute majority by the SPÖ party is associated with a gain in long-run development by 13.7% ($= 24,661/21,676$) compared to the counterfactual scenario of continuous socialist absolute majority in the national parliament. In the absence of the majority loss, our results imply that synthetic Austria would follow the growth and development trends of Netherlands (32%), Germany (31%), Slovenia (20%) and a couple of other countries where the equivalent majority loss did not occur, which indicate a notable slowdown of long-run growth and development. We replicate the synthetic control relationship exploiting the 1995 EU membership, and find no effects on long-run development. The evidence also confirms our Proposition #1 and theoretical notion on the existence and beneficial effects of gradual institutional changes for long-run development. Across the full set of synthetic estimation checks, the ratio between post-shock and pre-shock prediction error is consistently low, and within 15%, which implies that the counterfactual scenario is unlikely to be confounded by the unobservable sources of variation or other idiosyncratic shocks unreflected by the full set of covariates. Figure 2 summarizes the composition of synthetic control group for Austria for the full set of institutional shocks.

FIGURE 2 [INSERT HERE]

5.1.2. Brief Reference to Italy

Our results for Italy highlight the typical path of the long-run development driven by the gradual institutional changes. The 1861 unification of the North and South appears to have produced a temporary institutional change where the counterfactual Italy without the unification outperforms the real Italy with the unification. The temporary shock of the unification tends to disappear by 1910, which is consistent with the evidence on the economic effects of the unification by de [Oliveira and Guerriero \(2014\)](#). Pre-unification Italy shares the growth and development trends and characteristics of the Netherlands (49%), China (26%), United Kingdom (18%), and India (5%).

Our evidence also suggests that the fascist dictatorship in 1922 does not appear to have been a major constraint on long-run growth and development since the post-1922 development trajectory outperforms the counterfactual scenario. The synthetic control group for pre-1922 Italy comprises a weighted combination of Netherlands (61%), India (23%), and Portugal (15%) with 8% post/pre-shock prediction error, which suggests that alternative shocks are unlikely to affect the counterfactual long-run development path. However, the postwar transition to democracy in 1946 exhibits a similar gap between the real Italy and counterfactual Italy under fascist dictatorship in favor of the former. Our evidence suggests

that if Italy remained under the fascist dictatorship, its postwar growth would be substantially slower than the one of Italy under democracy. Likewise, the pre-1946 Italy shares the growth and development characteristics of the Netherlands followed by Portugal and India. In contrast, the collapse of the old party system in 1992 does not appear to be a discernable institutional shock with long-lasting implications. However, the real and counterfactual development series converge by 2015 suggesting that the collapse of the old party system, at best, appears to be a temporary long-run development shock. The synthetic control for pre-1992 Italy consists of the Netherlands (46%), Portugal (44%), Slovenia (5%), and Greece (3%). Figure A1 in the Appendix presents the real versus counterfactual long-run growth and development paths for Italy in response to the four institutional shocks.

5.2 Institutional Change Imposed by a Shock

5.2.3. Turkey

In Figure 3, we present the effects of institutional shocks on counterfactual long-run development of Turkey as a reference country for the institutional changes imposed by a major event. The evidence suggests that the series of institutional shocks in Turkey appear to have a marked short-run growth impact which tends to disappear in the long run. The most obvious institutional shock emanates from the breakup of the Ottoman Empire in 1920. Our results show that the collapse of the Ottoman Empire and the subsequent founding of the Republic of Turkey under Kemal Atatürk had a marked positive impact on short-run growth but no permanent long-run impact as the per capita GDP of real Turkey and its synthetic counterpart converge in the long run with no discernable difference. The counterfactual Turkey in the hypothetical absence of the collapse of Ottoman Empire is a weighted combination of Thailand (42%), Italy (22%), China (10%), Algeria and Uruguay (both 9%) and a couple of other countries.

A similar implication is conveyed by the transition to multiparty democracy in 1950 when the first free elections were won by the *Democratic Party*. In contrast to the Austrian experience with the transition to democracy, the Turkish case suggests that the transition to democracy appears to be a persistent constraint on long-run development as a net effect of populist redistribution and weak protection of private property rights inherent to the setup where democracy is introduced in the context of widespread poverty (Aghion, Alesina, and Trebbi et al. 2003). The counterfactual Turkey in the absence of the transition to multiparty democracy follows the covariate-level trends of Thailand (44%), Italy (20%), Portugal (18%), Iraq (10%), and a couple of other countries. The long-run growth and development loss of the 1950 transition to multiparty democracy, implied by our counterfactual estimates, is equivalent to 5.5% ($=9,897/9,373$). By contrast, the long-run growth and development loss associated with the 1960 army coup equals 7% ($=10,097/9,373$).

A different realm of implications emanates from the 1960 army coup against the ruling Democratic Party, and the subsequent coup in 1971. Our estimates suggest that the per capita GDP level of the counterfactual Turkey tends to converge with the level of real Turkey regardless of whether the 1960 coup or 1971 coup, which suggests that both institutional shocks were major stimuli to long-run development with a sizeable short-run, and negligible long-run impact. We also replicate the synthetic control setup by exploiting the 1982 transition to democracy. The transition comprises the constitutional reform with the creation of the seven-year presidency, and reduced the size of the national parliament to a single chamber. The long-run implications of the transition to democracy suggests that the long-

run Turkey in the absence of either 1980 military coup or without the transition to democracy in 1982 is no better off in terms of per capita income than its synthetic counterpart. In particular, the counterfactual Turkey shares the pre-1982 covariate-level growth and development characteristics and trends of Portugal (32%), China (28%), Chile (9%), Italy (8%), and a couple of other countries with a minor weight share.

FIGURE 3 [INSERT HERE]

The counterfactual process of Turkey's long-run development is unlikely to be contaminated by the idiosyncratic effects and unobservables that could create an elusive and uninformative counterfactual. The ratio between the post and pre-shock prediction error for the full set of growth and development covariates is in the range between 10% and 10.7% for the 1950 multiparty democracy, the 1982 transition to democracy and for the 1960, 1971, and 1980 coups. Considering the collapse of the Ottoman Empire, the error is equivalent to 6.7%. The evidence and diagnostic checks on the counterfactual scenario suggest that the series of institutional changes considered for Turkey clearly follow the pattern of the institutional change imposed by a shock. If the shocks had a profound effect on growth and development with long-lasting implications, the size of the prediction error should have been notably larger than the current estimates imply. In addition, the post/pre-shock prediction error also confirm an accurate and adequate replication of Turkey's long-run growth process implied by the synthetic group. Figure 4 summarizes the composition of synthetic control group for Turkey for the full set of institutional changes.

FIGURE 4 [INSERT HERE]

5.2.2 Morocco

Figure 5 presents the long-run effects of institutional change imposed by a shock drawing on the case of Morocco. The evidence on the counterfactual path of Morocco's long-run development suggests that a series of externally imposed institutional shocks benefitted long-run growth substantially while other shocks had a more short-run effect on growth but failed to alter the long-run equilibrium. Two such shocks are the creation of the Spanish Protectorate in 1882 and the French rule in 1912. A conventional view on the long-term effects of Spanish and French rule suggests that country where the Spanish and French rule was imposed had worse economic outcomes, and slower growth than the colonies where the British rule and the common law were introduced (La Porta et. al. 1998, 2008, Mahoney 2001, Lange et. al. 2006). Our evidence on Morocco suggests that externally imposed shocks such as the creation of protectorate in 1882 by Spain, and in 1912 by France had a strong positive impact on long-run development. The creation of the protectorate is associated with a substantial gain in long-run development relative to the counterfactual scenario without such a shock. In particular, our counterfactual estimates imply that in the absence of the creation of the protectorate in 1882 would lead to 32% ($=4,624/3,491$) drop in long-run development by 2015. In a similar vein, our counterfactual estimates imply that in the absence of the creation of French protectorate, contemporary Morocco's per capita GDP would be 41% lower than the actual level. The counterfactual Morocco without the 1882 and 1913 shock shares nearly identical pre-shock trends than the real Morocco. The synthetic Morocco is a weighted combination of countries sharing similar pre-shock growth and development trends, institutional, geographic and non-institutional characteristics, namely, Nepal (34%), Tunisia (31%), Algeria (26%), and a couple of others. The composition of the synthetic control group is similar for the 1912 shock with Nepal (50%), Egypt (27%), and Malaysia (19%) providing the largest weights in the donor pool. The root mean square prediction error is 0.7% for the 1882 shock, and 1.9 percent for the 1912 shock. It suggests that the counterfactual is unlikely

to be driven by shocks other than the creation of the protectorate. The evidence on the externally imposed shocks by Spanish and French rule highlights the benefits of political centralization imposed on a nearly lawless society prior to the French and Spanish rule.

FIGURE 5 [INSERT HERE]

A significant institutional shock to Morocco's long-run development with a sizeable short-run impact but negligible long-run impact evolved upon the independence from France in 1956 which led to the rule of King Mohammed V. The onset of independence is associated with a sizeable short-run boost in long-run growth, which appears to be driven by the growth trends in Morocco's nearest counterparts without such a shock in 1956 such as Iran, Tunisia, Jamaica and Nepal. Had Morocco followed the growth and development trends in these countries with similar pre-independence characteristics, our evidence implies that the counterfactual and real growth and development paths of Morocco would almost perfectly converge. Similar implications for long-run development emanate from the 1961 rule by King Hassan upon the death of his predecessor. The shock seems to have produced a sizeable short-run impact but by 1980s, the two series begin to move in tandem until the 2010 when the gap between the real and counterfactual Morocco is negligible. For both shocks, the predictor error is within the 15% bound suggesting that it is unlikely that alternative shocks around both break dates affect the counterfactual path of long-run development. In both cases, the counterfactual Morocco is a linear combination of Iran, Tunisia and several other donor countries.

Slightly different implications of the shock are posited by the onset of King Mohammed VI. Rule in 1999. The evidence suggests that the counterfactual Morocco without such a shock is substantially poorer than the real Morocco. The counterfactual estimate highlights the benefits of economic liberalization and social reforms introduced after 1999 on growth and development. The benefits are apparent since the counterfactual and real growth and development paths tend to diverge after 1999. The synthetic Morocco is a weighted combination of countries where such institutional reforms were not introduced to such a degree, or were nearly absent such as Nepal (36%), Syria (22%), and Tunisia (14%) and a couple of other donor countries. By 2015, the counterfactual Morocco is poorer by 29% with 11% post/pre-shock margin of error. In a similar vein, the counterfactual Morocco without the U.S Free Trade Agreement (FTA) in 2006, and without the 2011 Arab Spring is poorer than the real Morocco. In the absence of FTA with the United States, the long-run growth and development would falter by 17%. For the Arab Spring, our estimates imply a 10% improvement in per capita GDP by 2015. Figure 6 summarizes the composition of the synthetic control group for Morocco for the full set of institutional shocks.

FIGURE 6 [INSERT HERE]

5.2.3 Jordan

Figure 7 presents the long-run effects of institutional changes for Jordan is another reference country of the institutional change imposed by a shock. The evidence is similar to the Moroccan case study with institutional changes imposed by an external shock producing a short-run growth and development stimuli but no long-term benefit.

In case of Jordan, six institutional shocks are considered. First, the independence from Britain in 1946 (with King Abdullah I) is associated with higher growth until the 1970s compared to the counterfactual, and with a growth slowdown in 1980-1990 period. Afterwards, the real and synthetic Jordan converge until 2010, and diverge in the post-2010 period. Similar

consequences of the shock are posited by the Constitution of Jordan in 1952 (under Kings Talal and Hussein) and with the 1965 Treaty with Saudi Arabia, which changed the territorial boundaries and had important implications for the distribution of natural resources. In all three cases, the synthetic Jordan is a weighted combination of countries sharing similar pre-shock growth trends, institutional, geographic and other characteristics implied by the covariates: Tunisia (50%), Lebanon (17%), New Zealand (12%) along with a couple of other donor countries.

We also replicate the long-run growth and development model by exploiting the 1973 Yom Kippur War where Jordan co-opted with Egypt and Syria in attacking Israel. The evidence suggests that the war itself does not represent a discernable institutional shock per se since the counterfactual path of Jordan's growth and development appears to be driven by the pre-1973 trends, namely the 1965 Treaty with Saudi Arabia over the territorial boundaries. In both cases, per capita GDP of Jordan converges with its synthetic counterpart and arguably confirms our notion of the institutional changes imposed by a shock with a small or negligible long-run effect. Discernable long-run effects are not detected by the rise of the King Abdullah II. Rule in 1999 and by the Arab Spring in 2010.

Across the full set of institutional shocks considered for Jordan, the synthetic control group is stable and comprises countries with similar geographic and institutional characteristics prior to the shock such as Tunisia, Lebanon, New Zealand, Iraq, Malaysia, and Uruguay. Figure 8 presents the full composition of the synthetic control group in more detail. The counterfactual Jordan without the series of institutional shocks consistently converges with the real Jordan. The difference between real Jordan and synthetic Jordan is negligible at best, and confirms the prevalence of institutional changes imposed by a major shock. The post/pre-shock prediction error appears to be low for the 1946 and 1952 shocks (around 14%), and tends to increase for the subsequent shocks with the similar behavior of real and counterfactual development paths for Morocco.

FIGURE 7 [INSERT HERE]

FIGURE 8 [INSERT HERE]

5.2.4. Slovenia

Our results for Slovenia highlight substantial long-run development benefits of institutional changes imposed by a series of institutional shocks. Compared to the other countries, the long-run counterfactual development path of Slovenia almost always outperforms its real counterpart. The series of economic and political reforms starting with the reign of the Francis Joseph I in 1848 appear to be a source of long-run growth gains. Such reforms consolidated the Austrian rule against external threats such as Hungarian and Balkan uprising. Our results show that the Austrian rule improved rather than hampered Slovenia's long-run development considerably. The long-run development benefits are considerable. In the absence of the 1907 universal suffrage law, the counterfactual Slovenia by 2015 appears to be 27% poorer than its real counterpart. Similar results hold when we exploit the 1867 formation of the dual monarchy shock. Pre-1867 and pre-1907 Slovenia follows the growth and development trends of the countries with similar pre-shock characteristics such as Austria, Hungary, Czech Republic, Denmark, Switzerland, and a couple of others.

In contrast, the collapse of the Austrian empire in 1918 appears to be a minor institutional breakdown where the counterfactual Slovenia in 2015 would be 12% richer (=20,660/18,303) than the actual Slovenia, and would have the per capita income level similar to Germany and

France. The synthetic control group for pre-1918 Slovenia is a weighted linear combination of Austria (56%), South Korea (17%), Poland (14%), and Greece (10%). Subsequent institutional shocks such as the 1929 dictatorship do not seem to be a constraint on Slovenia's long-run development. As in case of Austria, the long-run Slovenia down to present day after the annexation by Nazi Germany is richer than Slovenia without the annexation although the magnitude of the gain is small. The major shock to Slovenia's long-run development is posited by the 1948 breakup with Eastern Block after Yugoslavia formed an alliance with the United States. The breakup with Eastern Europe appears to have a substantial long-run development dividend. In quantitative terms, the breakup with Eastern Block in 1948 is associated with 33% ($=12,209/18,303$) higher per capita income in 2015. If Slovenia remained an ally of the Soviet Union, its growth and development trends would mimic a linear combination of Hungary (57%) and Greece (26%) with smaller weight shares of Denmark (7%), Hong Kong (5%), Belgium (4%), and a couple of others. The evidence largely suggests that while Austrian and German rule was a net benefit for Slovenia's long-run development, state formation with Serbs/Croats in 1919 and a short-lived alliance with Soviet Union and Eastern Europe were a net burden. Figure A2 in the Appendix presents Slovenia's real and counterfactual long-run development paths.

5.2.5. Portugal and Spain

Figure A3 presents the counterfactual long-run development paths for Portugal. The evidence suggests that overthrow of the monarchy in 1910 and the 1926 Estado Novo regime appear to be temporary institutional shock, which improved long-run development. In the long run, the beneficial effects of the institutional reforms promulgated in 1910 and the effects of Estado Novo disappear as the real and counterfactual Portugal converge by the end of our estimation period. The synthetic control group for pre-1910 Portugal comprises Turkey (43%), Italy (39%), South Korea (8%), and China (7%) which yields only 3% post/pre-shock prediction error, and confirms the relevance of the counterfactual long-run development path. Similarly, the growth and development process of the synthetic Portugal in the hypothetical absence of Estado Novo regime is best described by a linear combination of Italy (42%), China (30%), Poland (13%), Greece (5%), South Korea (3%), Tunisia (3%), Uruguay (2%), and Brazil (1%).

A slightly different pattern is posited by the 1974 Carnation Revolution. The left-wing military coup against the Salazar regime resulted in a communist government. Our evidence suggests that the shock imposed by the revolutionary overthrow of the Estado Novo regime appears to a minor form of institutional breakdown as the long-run counterfactual Portugal is substantially richer than the real Portugal. More precisely, Portugal in 2015 without the communist coup would be 23% richer ($=17,154/13,912$) than Portugal with the communist coup. The counterfactual estimate implies that in the absence of the coup, Portugal's per capita income in 2015 would be similar to that of Italy and Slovenia. The synthetic Portugal without the communist coup consist of the linear combination of the Netherlands (32%), South Korea (30%), Greece (19%), Brazil (15%), and Tunisia (2%). Despite the short-lived communist coup d'etat, its adverse effects on long-run development are arguably long-lasting.

In 1975, the communist government was overthrown in a military coup, which later resulted in the transition to democracy. The coup against the communist government and the subsequent transition to democracy appear to be a major institutional shock with a large-scale positive impact on long-run development. In particular, the real Portugal after the coup strongly outperforms the counterfactual Portugal. The counterfactual scenario is arguably the one in which the communist government remained in power. If the communist remained in power, our estimates suggest that Portugal's long-run development would suffer. In quantitative terms, Portugal in 2015 in response to the overthrow of the communist

government is 20% richer ($=13,912/11,498$) than Portugal with the communists in power in 1975. In the counterfactual scenario, per capita income would falter and approach the levels of Greece and Argentina. Hence, if the communists stayed in power, Portugal would probably not join the EU and lean towards the Latin American populist model. The synthetic control group for pre-1975 Portugal consists of Brazil (47%), Netherlands (19%), Spain (16%), Greece and South Korea (each 8%). Compared to the coup against communist government, the EU membership in 1986 appears to be a temporary institutional shock with a positive short-run growth impact which largely disappeared by early 2000s.

Figure A4 presents the real vs. counterfactual development paths for Spain. The sequence of institutional changes suggests that the institutional shocks and their impact on long-run development were short-lived. This particular holds either for 1873 First Spanish Republic, 1923 dictatorship or 1931 Second Republic. The onset of the civil war in 1936 is associated with a marked but temporary drop in per capita income which largely disappeared by late 1960s. Nevertheless, the magnitude of per capita GDP penalty following the civil war is substantial. The synthetic control group for pre-civil Spain consists of the combination of Belgium (40%), Colombia (13%), Ireland (8%), Philippines (7%), South Korea (6%), Turkey (5%), United States (4%), Canada (3%), India (3%), Portugal (3%), Slovenia (2%), and Finland (1%) with 4% post/pre shock prediction error. Compared to Portugal, the transition to democracy in 1976 does not seem to have produced an institutional shock to long-run development. By contrast, our evidence suggests that the EU membership in 1986 is a temporary institutional shock to Spain's long-run development which largely disappeared by mid-2000s. The synthetic control group for pre-EU Spain consists of Slovenia (48%), Belgium (26%), China (15%), Canada (6%), and Syria (3%) which best describe Spain's pre-EU institutional development, growth and development trends, and geographic characteristics.

5.3. Institutional Breakdowns

We elaborate on three case studies of institutional breakdowns where the series of institutional shocks shaped the long-run growth path through a structural change where the real and counterfactual long-run development paths diverge as a result of the shock.

5.3.1. Egypt

Figure 9 presents the evolution of long-run development of Egypt following the sequence of institutional shocks that led to the breakdown indicated by our Proposition #1. The evidence on the balance between the real and counterfactual paths of long-run development suggest that the institutional shocks not only played a pivotal role in Egypt's growth and development, but also indicate the type of shocks triggering structural change and the long-lasting gap between the real and counterfactual series.

In 1882, Egypt underwent the British occupation after the de facto takeover of the Suez Canal construction by the British government in the midst of the near default of Egypt on its debt. We exploit the de facto takeover of Suez Canal as the first institutional shock and show that the shock is associated with the long-run loss of development. The absence of the 1882 takeover not only tends to produce a substantial short-run growth benefit but also tends to foster the long-run growth and development potential. By 2015, the difference between the counterfactual Egypt without the takeover and the real Egypt with the takeover is 16% ($=5,218/4,498$) in favor of the counterfactual. In 1914, Egypt became a de jure British protectorate. The establishment of the protectorate in 1914 seems to have deepened the long-run development costs of this particular institutional breakdown. In the long run, the 1914 institutional shock is associated with 19% drop in per capita GDP relative to the

counterfactual scenario of no establishment of British protectorate. The synthetic Egypt consists of the linear weighted combination of countries sharing similar pre-shock trends such as Tunisia (60%), Iran (20%) and a couple of others without a similar type and timing of the shock. The case of Egypt under the British rule starkly contrasts with the beneficial long-run effects of French and Spanish rule in Morocco. The startling comparison of Egypt and Morocco inevitably suggests that different societies respond differently to the similar set of institutional shock. The prediction error in both cases is 0.3% and 0.1%, respectively, suggesting almost non-existent discrepancy between real Egypt and its synthetic counterpart prior to the shocks.

The effects of the de facto 1882 and de jure 1914 British rule do not appear to be driven by the 1928 institutional shock, namely by the foundation of the Muslim Brotherhood movement. Another institutional shock leading to the breakdown appears to be the 1948 attack on Israel by the Arab League. Our evidence suggests that the shock led to the short-run growth and development slowdown. The counterfactual Egypt and its real counterpart briefly converge in early 1980s, and tend to diverge in early 1990s. In the long run, the counterfactual Egypt without the 1948 Arab-Israeli war ($=5,431/4,498$) is 20% richer than the real Egypt with such a shock. In 1952, Egypt became a republic after the coup d'etat led by Gamal Abdel Naser. As an institutional shock, the Naser rule appears to be an institutional breakdown with persistently negative implications for Egypt's long-run development. The adverse effects of the Naser rule seem to be immediate and persistent. In particular, long-run Egypt without Naser rule is 29% richer than Egypt in the aftermath of the Naser rule. The synthetic Egypt in the absence of Naser's rule sharing similar pre-shock trends and characteristics consists of the weighted combination of Algeria (31%), Tunisia (27%), China (20%), Vietnam (19%), and a couple of other countries from the donor pool.

FIGURE 9 [INSERT HERE]

A distinctive institutional break occurred in 1970 when the death of Naser was followed by the Sadat dictatorship. Sadat regime re-oriented Egypt's Cold War allegiance from the Soviet Union to the United States. Alongside, the regime initiated economic policies to reduce government regulation and promote foreign direct investment. Not surprisingly, our evidence shows that the Sadat regime appears to be a short-term net benefit for Egypt's long-run development. With less than 5% margin of post/pre-shock prediction error, Egypt with the Sadat dictatorship is significantly better off in terms of long-run development than the counterfactual Egypt without the regime. Countries such as Algeria, Vietnam and China are the only three donors to the synthetic Egypt for the 1970 Sadat dictatorship. In the long term, the synthetic Egypt and real Egypt converge together with almost non-existent gap by the end of our estimation period.

Another institutional break occurred in 1981 when the assassination of the Sadat resulted in the rule of Hosni Mubarak. Our evidence suggests that the long-run effects of the Mubarak rule are almost identical to the effects of the Sadat dictatorship, as the real and synthetic Egypt move in tandem and do not drift apart. Starkly different implications follow from the 1992 rise of insurgent Islamist terrorism. Our evidence does not indicate that pre-1992 trends affect the counterfactual path of long-run development. The synthetic Egypt is composed of Tunisia (52%), Burma (24%), China (18%), Australia (4%), and New Zealand (1%), which provides the basis of a valid counterfactual as these countries did not experience the equivalent wave of insurgent Islamist terrorism but had similar geographic and institutional characteristics to Egypt as well as comparable growth trends. The discrepancy between the real Egypt and its synthetic counterpart is 5.1%, and confirms the existence of breakdown. In the long run, Egypt after the insurgent Islamist terrorism is 64% poorer ($=7,405/4,498$) than a

counterfactual Egypt without the wave of Islamist terrorism in 1992. In a startling comparison with Morocco, the effect of Arab Spring on Egypt's long-run development appears to be negative and suggests the institutional shock was more similar to the breakdown that led to persistent instability and de facto military rule. Our estimates imply that a counterfactual Egypt without the Arab Spring is 26% richer than Egypt after the Arab Spring.

FIGURE 10 [INSERT HERE]

5.3.2. Mexico

Following the independence from Spain in 1821, Mexico experienced prolonged institutional instability and civil war. The first major break point is posited by the 1846 Mexican-American War starting the U.S annexation of Texas and by the subsequent military conflict. Our estimates imply that in the absence of the war, Mexico would largely avoid the economic decline lasting until 1860 when Mexican per capita income dropped below the pre-independence war level. The counterfactual scenario is also characterized by a slower growth and development until the Mexican Revolution in 1917 while the real and counterfactual trajectories converge in the post-WW2 period but tend to diverge after 2000. By mid-2010s, Mexico without the 1846 Mexican-American war is 13% richer ($=9,996/8,821$) than Mexico despite the war. The post/pre prediction error is about 0.3% and suggests that the counterfactual does not seem to suffer from alternative institutional shocks. The synthetic Mexico without the Mexican-American war is a weighted combination of Colombia (35%), Brazil (25%), Turkey (20%), and a couple of others, including the United States, which comprises 7% base weight of the synthetic Mexico. The French occupation of Mexico in 1860s gradually led to the Second Mexican Empire in 1867 which itself represents a major institutional shock. The establishment of the Second Empire appears to be a long-run growth and development benefits, which has its origins in the beneficial effects of the reforms introduced by Maximillian I. such as the adoption of the modern criminal code, and the modernization of infrastructure, particularly railways. Due to subsequent institutional changes, the long-run Mexico with the Second Empire tends to converge with its synthetic counterpart. The synthetic Mexico for the 1867 institutional shock is fairly uniform and consists of the weighted combination of Brazil (85%), South Korea (14%), and United States (1%). Figure 11 presents the full set of counterfactual estimates for Mexico.

FIGURE 11 [INSERT HERE]

A somewhat similar boon to Mexico's long-run development emanates from the Díaz dictatorship in 1876. The onset of the dictatorship is associated with widespread and consistent improvements in long-run development compared to the counterfactual scenario without the dictatorship. The effects of the dictatorship were long-lasting as the real Mexico outpaces the synthetic Mexico in the period 1876-1993. Afterwards, the synthetic Mexico without the Porfiriato-like institutional shock outperforms the real Mexico. It suggests that in the long-run, Porfiriato turned from being a net benefit for long-run development to the net burden and mimicked the characteristics of institutional breakdown slightly similar to the ones discussed in the case of Egypt. By 2015, Mexico without Díaz dictatorship is 11% richer than the synthetic Mexico ($=9,816/8,281$) without such an institutional shock. In contrast, the 1917 Mexican Revolution does not seem to be a discernable institutional shock influencing the long-run development since the counterfactual scenario is shaped by the pre-existing trends. On the other hand, the major institutional breakdown came with the establishment of the Institutional Revolutionary Party (PRI), which designates the shift to one-party rule that lasted more than 70 years. The post/pre prediction error is 12.4%, which indicates that the counterfactual long-run development path of Mexico under PRI rule is unlikely to pick the

institutional shocks other than one-party rule in 1929. Despite the strong growth in the initial years of PRI rule, the real and synthetic series drift apart by early 1980s, and the long-run development of counterfactual Mexico is better off without the PRI rule. In quantitative terms, the real Mexico by 2015 is 71% poorer ($=14,185/8,281$) with the PRI rule than Mexico without the PRI rule. The synthetic Mexico is a weighted linear combination of Argentina (50%), South Korea (42%), Chile (6%), and Morocco (2%). The end of the PRI rule in 2000 earmarks Mexico's transition to democracy. The evidence suggests that the transition to democracy failed to uphold long-run growth and development benefits and led to the institutional breakdown since the counterfactual Mexico is better off than the real Mexico. The growth and development effects of the institutional shock in 2000 are unlikely to be driven by the implausible counterfactual since the post/pre-shock prediction error is about 14% and is within the conventional acceptance bounds. The counterfactual Mexico without the transition to democracy is a weighted combination of 11 countries sharing similar pre-2000 growth trends and covariate-level characteristics with Colombia (34%) and Argentina (19%) providing the largest weight from the donor pool. Mexico with the transition to democracy is 24% poorer ($=10,301/8,281$) than Mexico without such transition following the growth trends in the countries sharing institutional, geographic and other covariate-specific similarities before the shock. Figure 12 presents the composition of the synthetic control group for Mexico.

FIGURE 12 [INSERT HERE]

5.3.3. Brazil

Figure 13 presents the results and counterfactual estimates for Brazil. Compared to the Spanish America, Brazil followed a different pattern of institutional development. After the independence from Portugal in 1822, Brazil became an empire under the rule of Emperor Pedro I. In 1889, the military coup d'état under the leadership of Deodoro de Fonseca deposed the second emperor, Pedro II. In spite of the de jure constitutional democracy, de facto political institutions of the old republic were based exclusively on patron-client relationships, and were highly oligarchic and personalistic. The de facto political power was concentrated in the hands of locally dominant oligarchs who kept the practice of dispensing favors in return for loyalty. The dominant political class comprised coffee planters and agrarian oligarchs exploiting weak state capacity to devolve power to the local agrarian oligarchies. Extreme concentration of wealth and landownership was reminiscent of feudal aristocracies. About 464 largest landowners held more than 270,000 km² of land while 464,000 small and medium-sized farms occupied 157,000 km². One form of manifestation of the patron-client relationship were the presidential and governor elections were exchange of favors between politicians and large landowners was a norm to control the votes of the population in exchange for favors. Our evidence suggests that the old republican regime appears to be a net burden for Brazil's long-run development. The counterfactual per capita GDP of the synthetic Brazil without the 1889 shock suggests that Brazil would have experienced a markedly stronger growth in the absence of the 1889 coup d'état and the associated institutional framework of the old republic. The actual and counterfactual growth paths after the 1889 coup d'état converge until early 1980s while afterwards the counterfactual Brazil exhibits sustained growth after 1980. In the long run, the counterfactual Brazil without the old oligarchic regime is 41% richer ($=9,798/6,902$) than the actual Brazil in the aftermath of the old oligarchic regime. The synthetic Brazil without the 1889 shock is a weighted combination of countries matching the parallel growth and development trends, namely, Portugal (35%), Colombia (31%), China (20%), and a couple of other countries from the donor pool. The post/pre-shock prediction error is 1.9% of the estimation margin which suggests that the counterfactual scenario is unlikely to be driven by the alternative shocks.

In 1930, a military junta led by Getúlio Vargas deposed the old oligarchic regime and overthrew the oligarchic coffee plantation owners. The junta shifted the balance of de facto economic and political power toward an urban middle class and business interested that promoted industrialization and rapid modernization of infrastructure. The Vargas regime advocated moderate social reforms and used a state interventionist policy by utilizing tax concessions to promote industrialization, but at the same time the regime leaned heavily towards economic nationalism, creation of state monopolies and trade protectionism along the Portugal's Estado Novo parallel. Our evidence suggests that in the short run, the 1930 institutional shock seemed to have encouraged growth and development as the counterfactual growth path outperforms the real growth path. In the period 1960-1980, the two series converge but afterwards, the counterfactual Brazil without the 1930 shock again outperforms the real Brazil with the shock. In the long run, the counterfactual Brazil without the 1930 shock is 46% richer (=10,097/6,902) than the actual Brazil. The key donors to the synthetic Brazil without the shock include Colombia (46%), China (22%), Venezuela (16%), and United States (6%) among a couple of other countries with minor weight share.

In 1945, Vargas was overthrown in a coup d'état which marked the return to the democratic rule. The democratic regime under the presidency of Eurico Gaspar Dutra embarked on the liberalization of the fascist-influenced Estado Novo regime, and introduced the electoral law, and allowed free and regular elections. The Dutra regime also pursued a different set of economic policies than the Vargas regime. It broke up state interventions and adopted policies to encourage the expansion of the manufacturing sector. Our evidence suggests that the 1946 institutional shock turns out to be a net benefit for Brazil for the period 1945-2000 when the counterfactual scenario without the shock produces a slower long-run development path than the actual one with only 6% post/pre-shock prediction error. After 2000, the counterfactual development trajectory outpaces the actual development path. The key donors to the synthetic Brazil without the 1946 transitional republic include China (39%), Colombia (37%), and Uruguay (12%).

In 1964, the democratic regime fell to the military coup in response to threat of the communist revolution and Soviet control. The military regime entrusted the economic policy to the group of economic advisors under the leadership of Antônio Delfim Netto. The group favored free-market policies with macroeconomic stabilization, and lesser state intervention along with large-scale modernization of infrastructure. These policies were pivotal in Brazil's rapid growth episode in the period 1964-1985. Our evidence suggests the 1964 institutional shock produced a rapid acceleration of growth and development for the period 1964-2000. After 2000, the counterfactual again outperforms the real Brazil, and depletes the long-run growth benefits of institutional reforms of the military regime. The synthetic Brazil without the 1964 military dictatorship consists of a weighted combination of China (45%), Venezuela (20%), Colombia (17%), Uruguay (10%), and a couple of other countries with a small weight share.

Lastly, we examine the contribution of Brazil's return to democracy and civilian rule in 1985 to its long-run development. In 1985, Brazil embarked on the path towards large-scale democratization. Race and literacy-related voting qualifications were ended in 1988. Apart from the macroeconomic stabilization, the presidency of Fernando Henrique Cardoso and Fernando Collor Mello pursued the economic policies with an emphasis on free trade and privatization of government-owned enterprises. In 2000, the election of Lula da Silva turned Brazil from the reform agenda to the populist redistribution such as minimum wage increases. Our evidence suggests that democratization in 1985 appears to be an institutional breakdown. The counterfactual Brazil after the wave of democratization consistently outperforms the real Brazil after the transition to democracy. The counterfactual scenario exhibits a notable

growth acceleration compared to substantially slower growth in the post-1990 period. In quantitative terms, the counterfactual Brazil is 53% richer ($=10,614/6,902$) than the real Brazil after the democratization. Prior to democratization, Brazil's growth and development trends are similar to the high-growth countries such as China. This implies that the reliance on pre-1985 trends might produce a markedly different growth and development trajectory. In particular, the synthetic Brazil is a weighted combination of China (45%), Venezuela (23%), Colombia (19%), Canada (13%), and Iraq (4%). Figure 13 presents the composition of the synthetic control group for Brazil across the full set of institutional shocks. The counterfactual scenario implied by the synthetic control group has 8.6% post/pre-shock prediction error, which implies that it is quite unlikely that pre-1985 shocks affecting the level and direction of the scenario. Hence, the case of Brazil suggests that democratization appears to have been an institutional breakdown, and a net burden on long-run development. Our evidence is consistent with the view that democratization is not always beneficial for growth (Lipset 1959, Weede 1983, 1996, Barro 1996, Tavares and Wacziarg 2001, Aghion et. al. 2007, Glaeser et. al. 2007).

FIGURE 13 [INSERT HERE]

5.3.4. Hungary

The case of Hungary largely testifies to the persistence of institutional breakdowns with long-lasting negative impact on long-run development. In Figure A5, the counterfactual and real paths of long-run growth and development are presented for Hungary. The first institutional shock considered are the institutional reforms of civil rights in 1848. In spite of the 1848 revolution, the counterfactual Hungary outperform the real Hungary with civil rights reforms. The 1848 shock came with almost non-existent short-run effects but sizeable long-run effects. Prior to the 1848 revolution, the synthetic Hungary is a linear combination of Poland (40%), Slovenia (38%), Czech Republic (12%), Germany (7%), and Argentina (1%). Post/pre-1848 prediction error is about 1.5%, which suggests that alternative shocks and institutional reforms are unlikely to shape post-1848 growth and development path. Hungary in 2015 without the 1848 turmoil is 61% poorer ($=15,063/9,309$) than Hungary with pre-1848 trends. A similar breakdown is evident with the formation of dual monarchy. The formation of dual monarchy with Austria produced a short-run growth and development benefit, which turned out to be a net burden in the long-term perspective. The collapse of the empire in 1918 is also associated with the divergent long-run development path relative to the counterfactual scenario. In the absence of collapse, Hungary would have experienced notably stronger post-WW2 growth. The synthetic Hungary prior to the 1918 collapse of the empire is a weighted combination of Poland (34%), Czech Republic (31%), Argentina (11%), Slovenia (10%), South Korea (8%), Austria (2%), and Germany (1%). In a stark contrast to Austria and Slovenia, the alliance with Nazi Germany in 1940 appears to be another institutional breakdown with sizeable negative effects on long-run development although the loss of per capita income is small compared to pre-1940 shocks. The most powerful institutional breakdown is posited by the communist rule and Soviet occupation in 1944. Our counterfactual series for Hungary is almost identical to the series on real Hungary with 4.3% post/pre-shock prediction error. The synthetic Hungary with the pre-1944 growth and development trends consists of Poland (30%), Czech Republic (21%), Germany (17%), Lebanon (14%), South Korea (6%), Greece (5%), and Austria (3%). In the absence of the communist rule in 1944, the counterfactual Hungary in 2015 would be richer by 29% ($=12,088/9,309$) which confirms the high long-run growth and development cost of Soviet occupation and communist rule. By the same token, the transition to democracy in 1990 also appears to be a breakdown. Hungary with pre-1990 trends is consistently richer than than Hungary after 1990. The synthetic Hungary with pre-1990 trends is composed of Czech Republic (41%), Poland (35%), Slovenia (7%), Jordan (5%),

Lebanon and South Korea (each 3%), France (1%), Brazil (1%). In relative terms, the long-run cost of the 1990 breakdown is smaller than the cost of the 1944 communist rule.

5.3.5. Brief References to Argentina, Philippines and South Africa

Figure A6, A7, and A8 in the Appendix display the counterfactual versus real development paths for Argentina, Philippines and South Africa. In all three countries, the institutional shocks seem to have produced the breakdowns with permanent divergence between the real and counterfactual path of growth and development. The Argentine experience is particularly noteworthy. The 1853 Constitution does not appear to be a breakdown per se as the synthetic Argentina exhibits a substantially slower growth and development. In the long run, both series converge which suggests that the institutional changes after the 1853 constitution largely condemned the economic benefits of the 1853 Constitution. Pre-1853 Argentina shares the growth and development characteristics of Uruguay (78%), Brazil (10%), Venezuela (4%), Algeria (2%), India and Turkey (each 1%). The most pervasive and costly breakdowns are indicated by the 1912 Sáenz Peña Law which led to the surge of populist distribution and facilitated the seeds of the military coup in 1930. In the absence of the 1912 Sáenz Peña Law, counterfactual Argentina is 1.02 times richer ($=21,162/10,437$) than its real counterpart. The absence of the military coup in 1930 and the reliance on pre-1930 trends is associated with 66% increase in per capita ($=17,326/10,437$) by 2015. Pre-1930 synthetic Argentina consists of Uruguay (49%), Mexico (22%), Venezuela (19%), Syria (5%), Slovenia (2%), and Algeria (1%). While the Peronist rule in late 1940s and early 1950s does not seem to have produced an institutional breakdown, the military dictatorship in 1976 appears to be a pervasive breakdown with a substantial long-run growth and development cost with 35% per capita income penalty ($10,437/14,185$) by 2015 as a result of the military dictatorship. The synthetic Argentina is a weighted mix of Chile, Uruguay, and Venezuela. Similar to the case of Hungary, the transition to democracy in 1983 does not comprise an institutional change imposed by a shock but rather an institutional breakdown since the counterfactual Argentina outperforms its real counterpart.

The cases of Philippines and South Africa are more puzzling. Nevertheless, both case studies testify to the persistence of institutional breakdowns. In case of Philippines, the U.S occupation and the subsequent Philippine Autonomy Act are associated with a marked short-term growth and development cost but moderate long-term cost as the counterfactual Philippines goes in parallel with the real Philippines for both institutional shocks alike. Pre-U.S-imposed shock Philippines are a linear combination of India (70%), Indonesia (17%), and Spain (11%). The most pervasive breakdown occurred with the Japanese occupation in 1942 and subsequent institutional shocks such as land reform, Marcos dictatorship, and various republican institutional experiments. In all these cases, the counterfactual Philippines are better off in terms of growth and development than the real Philippines. Pre-Japanese occupation Philippines are a linear combination of Brazil (37%), Japan (31%), South Korea (16%), and Colombia (14%). Without the Japanese occupation, the counterfactual Philippines are 3.4 times richer ($=12,718/3,721$) than the real Philippines by 2015. Similar magnitude of long-term growth and development cost emanates from the Marcos dictatorship, land reform and the republican institutional experiment in 1986. In case of South Africa, the Apartheid regime appears to be the most pervasive institutional breakdown. In the absence of the Apartheid regime, the reliance on pre-1948 growth and development trends is associated with 32% higher per capita income ($=6,836/5,160$) in 2015. The synthetic South Africa in pre-Apartheid years is a weighted linear combination of Iran (27%), Canada (25%), Syria (22%), Jamaica (20%), Nepal (2%), India (1%). Although the long-term growth and development cost of Apartheid regime is lower than the cost of Japanese occupation in Philippines or communist

rule in Hungary, the absolute magnitude is substantial and suggest that the regime comprised a major barrier to broad-based growth and development.

6. Conclusions

Korea is a unique “laboratory” to test the impact of institutional changes in growth trajectories because economists can study two countries subject to the same cultural background for a long period of time (until 1945) and exposed to different institutional shocks (since the 1950s). There are few readily available experiments to explore the interaction between institutional change and economic performance. In this article, we develop an empirical methodology that aims at constructing a possible synthetic counterfactual. This synthetic counterfactual provides a new environment to analyze how specific institutional variations affect growth. Our counterfactual estimations posit a valid inference on the long-term development implications of the institutional shocks. Our strategy is to exclude the set of countries undergoing the same shock as the treated country from the donor pool to ensure a valid inference on the counterfactual long-term development paths.

There are three distinct models of institutional evolution, namely, gradual change, shock and structural breakdown. In the absence of an existing counterfactual, the distinction between shock and breakdown was understandably blurred in the literature. Our methodology provides for an exploratory e clean test with the synthetic counterfactual. We have discussed examples (such as Mexico or Brazil for breakdowns, Turkey or Morocco for shocks) to illustrate the different between short-term impacts on growth versus changes in long-term growth trajectories.

Our methodology introduces a systematic identification of institutional change. In that light, our article offers an empirical methodology to explore and classify institutional evolution. It does not explain why a specific change (for example, the Mexican Porfiriato versus the Brazilian 1889 Republic or constitutional reforms in Jordan versus Egypt) is more like a shock or more like a breakdown. However, a systematic identification of institutional change based on the synthetic counterfactual adds to the discussion on which determinants or characteristics of change seem to matter in order to enhance long-term growth. An exhaustive identification and classification of institutional change should be able to uncover the aspects that seem to determine when it goes from a shock to a breakdown.

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Table 1: Covariate-Level Descriptive Statistics

	Obs	Averaging Period	Mean	StD	Min	Max
<i>Panel A: The Dependent Variable</i>						
Real GDP Per Capita	10,976	1820–2015	3848.30	5228.03	335	34,146
<i>Panel B:</i>						
Population Size	10,976	1820–2015	15.92	1.64	9.90	21.03
Population Density	10,976	1820–2015	3.38	1.47	0.041	7.883
Log Normalized Population Growth	10,976	1820–2015	0.012	0.014	-0.342	0.260
Life Expectancy at Birth	3,136	1960–2015	49.13	32.68	35.21	83.84
Fertility Rate	3,136	1960–2015	2.36	3.013	1.076	8.427
<i>Panel B: Institutional Covariates</i>						
De Jure Judicial Independence	10,976	1820–2015	0.64	0.15	0.32	0.93
De Facto Judicial Independence	10,976	1820–2015	0.66	0.22	0.16	1
Control of Corruption	1,121	1996–2015	0.531	1.128	-1.672	-2.585
Government Effectiveness	1,121	1996–2015	0.632	0.993	-2.088	2.358
Political Stability and Absence of Violence	1,121	1996–2015	0.096	1.006	-3.180	1.663
Quality of Regulation	1,121	1996–2015	0.532	1.005	-2.344	2.189
Rule of Law	1,121	1996–2015	0.514	1.033	-1.990	2.120
Economic Freedom Index	756	1996–2015	66.371	8.711	34.3	83.1
Polity2 Score	10,976	1820–2015	-1.00	7.77	-10	10
Constraints on the Executive	10,976	1820–2015	3.80	2.76	1	7
Majoritarian vs. Proportional Representation	10,976	1820–2015	0.176	0.381	0	1
Presidential vs. Parliamentary Electoral System	10,976	1820–2015	0.201	0.401	0	1
Federalism	10,976	1820–2015	0.212	0.408	0	1
FH Civil Liberties Score	2,584	1973–2015	3.25	2.11	1	7
<i>Panel C: Geography Covariates</i>						
Island	10,976	1820–2015	0.16	0.36	0	1
Landlocked	10,976	1820–2015	0.10	0.30	0	1
Terrain Ruggedness	10,976	1820–2015	1.46	1.15	0.005	5.043
Latitude	10,976	1820–2015	27.44	27.16	-41.80	64.48
Longitude	10,976	1820–2015	25.18	65.77	-112.98	171.47
Soil	10,976	1820–2015	41.04	22.17	0.007	96.076
Desert	10,976	1820–2015	2.20	5.42	0	23.27
Tropical	10,976	1820–2015	18.98	36.11	0	100
Nearest Coast within 100 km	10,976	1820–2015	44.93	36.14	0	100
<i>Panel D: Legal History Covariates</i>						
British Common Law	10,976	1820–2015	0.25	0.43	0	1
French Civil Law	10,976	1820–2015	0.48	0.49	0	1
German Civil Law	10,976	1820–2015	0.17	0.38	0	1
Scandinavian Civil Law	10,976	1820–2015	0.05	0.22	0	1
<i>Panel E: Culture Covariates</i>						
Culture (First Principal Component)	10,976	1820–2015	-0.293	1.306	-3.35	1.52
Trust	10,976	1820–2015	27.77	15.50	6.70	74.20
Social Capital (First Principal Component)	10,976	1820–2015	0.498	1.53	-2.39	3.70
Ethnic Fractionalization	10,976	1820–2015	0.318	0.219	0.002	0.751
Linguistic Fractionalization	10,976	1820–2015	0.276	0.251	0.002	0.865
Religious Fractionalization	10,976	1820–2015	0.388	0.242	0.003	0.860
<i>Panel F: Human Capital and Macroeconomic Covariates</i>						
Index of Human Capital	3,696	1950–2015	2.27	0.72	1.01	3.87
Investment Share of GDP	3,696	1950–2015	0.229	0.086	0.006	0.606
Trade Openness	3,696	1950–2015	0.452	0.437	0.0009	4.614
<i>Panel G: Initial Persistence</i>						
Initial GDP Per Capita	56	1820	791.09	368.31	335	2,074
Initial Life Expectancy	56	1960	44.76	28.71	40.03	73.54
Initial Fertility Rate	56	1960	2.045	2.412	2.001	7.123
Initial Polity2 Score	56	1820	-6.75	4.13	-10	9
Initial Constraints on the Executive	56	1820	2.17	2.11	1	7

Table 2: Institutional Break Dates Across Country-Level Case Studies

Argentina	
1853	Constitution
1916	Saenz Peña Law
1930	Coup d'etat
1946	Election of Péron
1955	Coup d'etat
1966	Revolución Argentina
1973	Return of Péron
1976	Dirty War
1983	Democracy
Austria	
1848	Franz Joseph I Rule
1867	Dual Monarchy
1907	Universal male suffrage
1919	Collapse of Austrian-Hungarian Empire
1920	First Austrian Republic
1933	Dollfuss Dictatorship
1938	Annexation by Nazi Germany (Anschluss)
1955	The Second Republic
1983	The Loss of Socialist Absolute Majority
1995	EU Membership
Brazil	
1889	Old Republic
1930	Transitional Republic
1946	New Republic
1964	Dictatorship
1988	New Constitution/Democracy
Egypt	
1882	De Facto British Rule
1914	De Jure British Rule
1928	Foundation of Muslim Brotherhood
1948	Arab-Israeli War
1952	Naser Rule
1970	Sadat Dictatorship
1981	Mubarak Rule
2010	Arab Spring
Hungary	
1848	Civil rights reforms; Franz Joseph I Rule
1867	Dual Monarchy
1918	Collapse of Austrian-Hungarian Empire
1940	Tripartite Pact
1944	Communist Rule
1989	Third Republic
2004	EU Membership
Italy	
1871	Unification
1923	Fascist Dictatorship
1946	Democracy; collapse of Monarchy
1992	Collapse of the Old Party System
Jordan	
1905	Shoubak Revolt
1910	Karak Revolt
1916	Great Arab Revolt

1921	Establishment of Emirate of Transjordan
1946	Independence from Britain
1952	Constitution of Jordan
1965	Treaty with Saudi Arabia that changes boundaries and natural resources
1973	Yom Kippur War
1999	King Abdullah II Rule
2010	Arab Spring
Mexico	
1821	War of Independence
1846	Mexican-American War
1866	Mexican Empire
1876	Porfiriato
1917	February Revolution
1929	PRI Election/Regime
2000	PAN Election/Democracy
Morocco	
1884	Spanish Protectorate
1912	French Protectorate
1921	Rif War
1956	Independence from France
1961	King Hassan II Rule
1999	King Mohammed VI Rule
2006	Free Trade Agreement with the U.S.
2011	Arab Spring
Philippines	
1900	American Rule
1916	<i>Philippine</i> Autonomy Act
1942	Japanese occupation
1946	Third Republic
1963	Land Reform
1965	Marcos Dictatorship
1981	Fourth Republic
1986	Fifth Republic
Portugal	
1910	First Republic, overthrow of King Manuel II
1926	Military regime, followed by Salazar dictatorship
1974	Carnation revolution against dictatorship, rapidly taken over by communists
1975	Democracy, coup against communist government
1986	EU Membership
Slovenia	
1848	Civil rights reforms; Franz Joseph I Rule
1867	Dual Monarchy
1907	Universal male suffrage
1919	Kingdom of Yugoslavia
1929	Dictatorship
1941	Fascist Dictatorship
1943	Civil War
1950	Breakup with Eastern Bloc
1990	Independence
2004	EU Membership
South Africa	
1880/81	Boer War

1910	Formation of Union of South Africa
1948	Apartheid
1994	New Constitution
Spain	
1868	Glorious Revolution & First Spanish Republic
1874	Return of monarchy; King Alfonso XII
1923	Primo Rivera dictatorship
1931	Second Spanish Republic; resignation of King Alfonso XIII
1936/39	Civil War; Franco dictatorship
1976	Democracy; King Juan Carlos I
1986	EU Membership
Turkey	
1876	First Constitutional Era (to 1878)
1908	Second Constitutional Era and Young Turk Revolution
1923	End of Ottoman Empire; Republic of Turkey; Atatürk Era
1950	Multiparty Democracy
1960	Coup d'etat
1971	Coup d'etat
1980	Coup d'etat
1982	Democracy/ new Constitution
2003	Erdogan Era

Figure 1: Gradual Institutional Change: The Case of Austria, 1820-2015

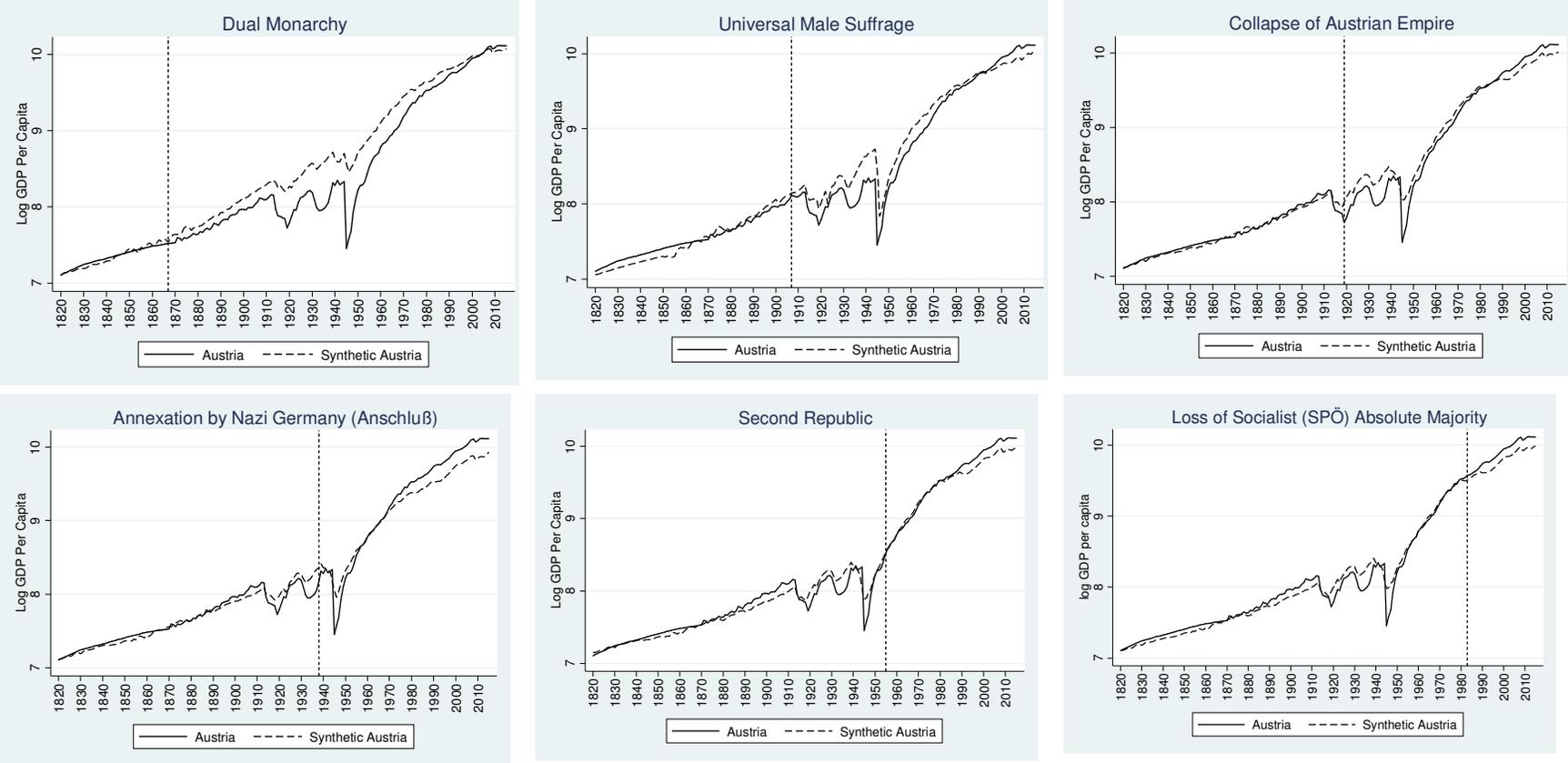


Figure 2: Synthetic Control Group for Austria, 1820-2015

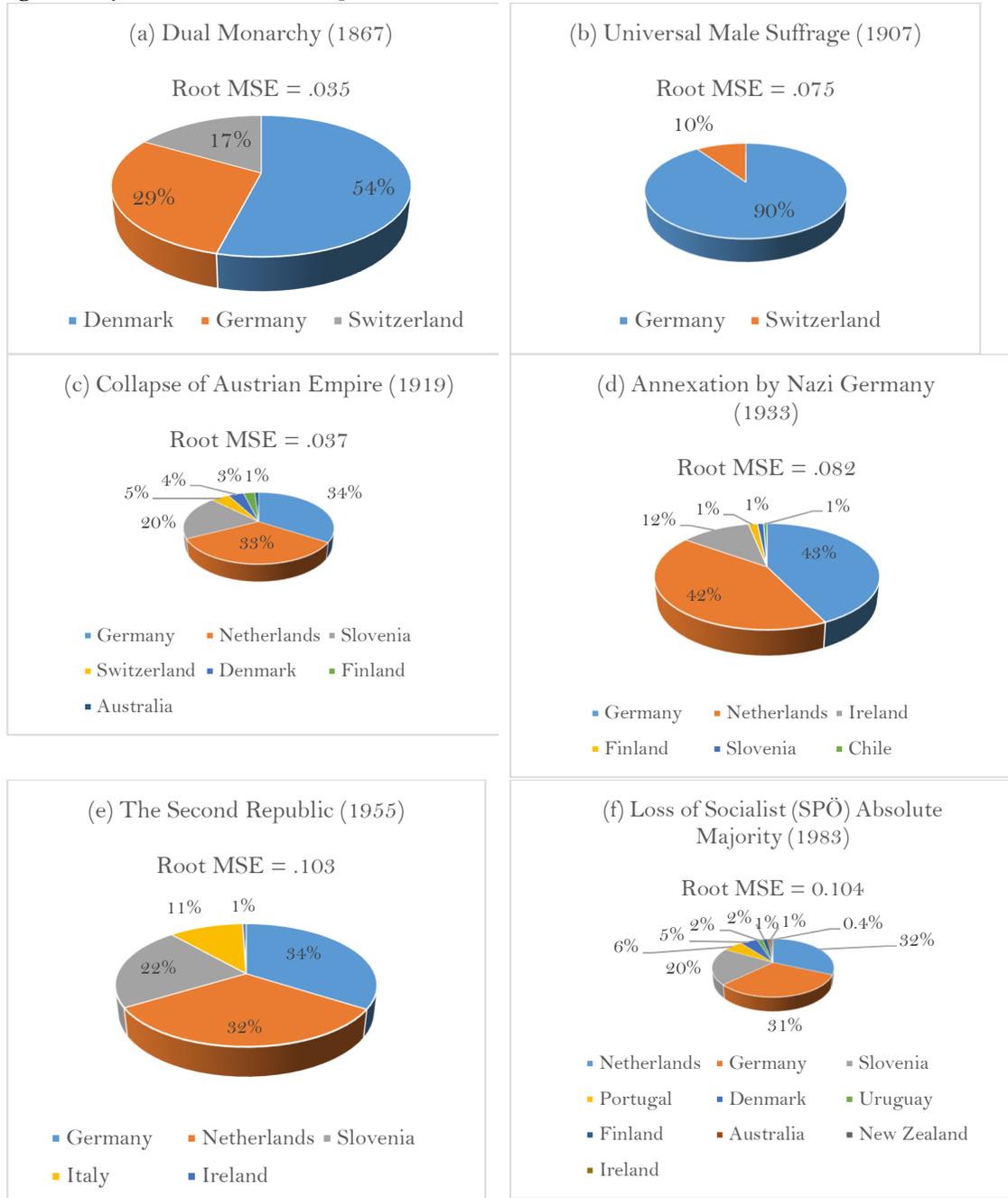


Figure 3: Institutional Change Imposed by a Shock: The Case of Turkey, 1820-2015

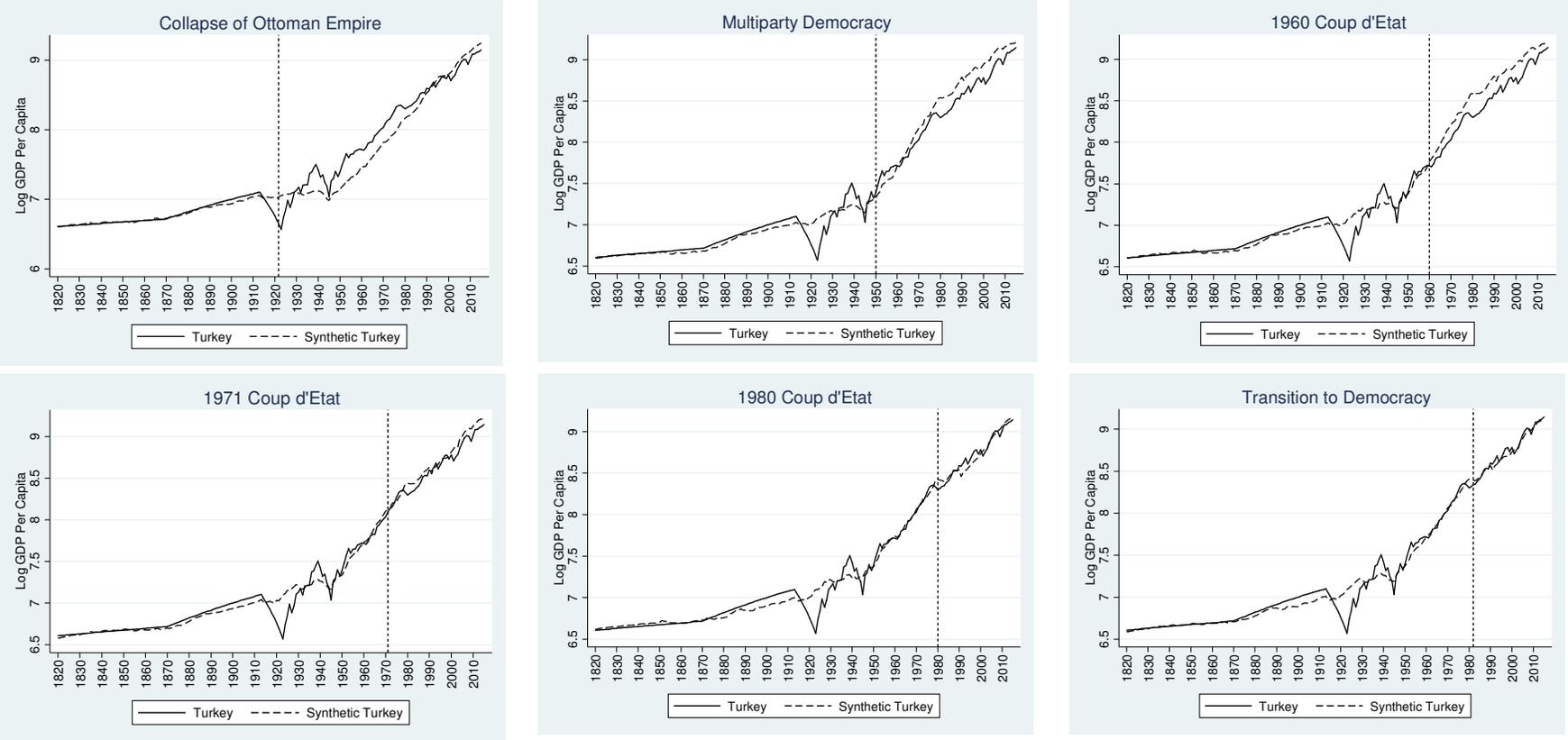


Figure 4: Synthetic Control Group for Turkey, 1820-2015

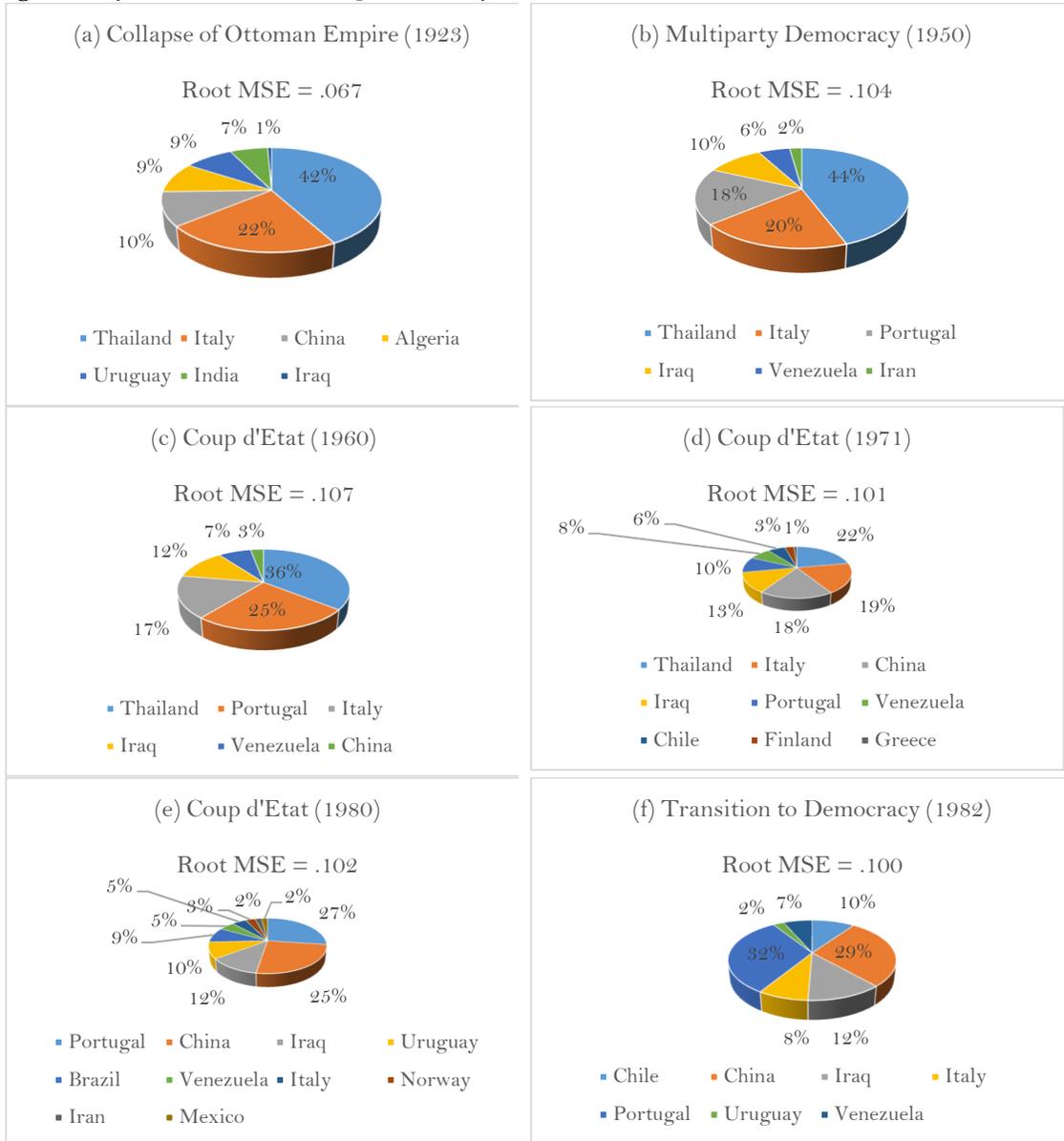


Figure 5: Institutional Change Imposed by a Shock: The Case of Morocco, 1820-2015



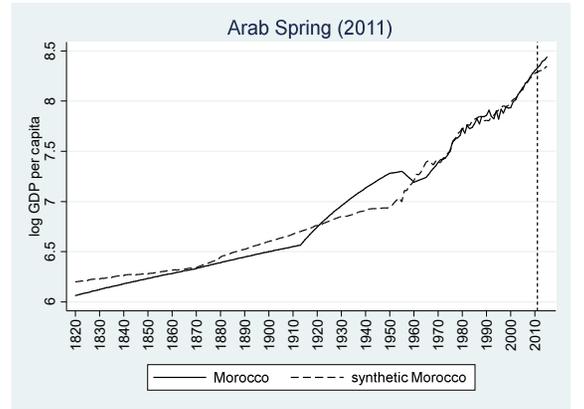
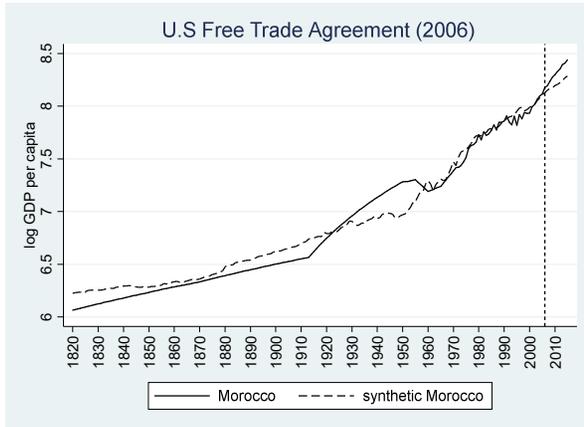
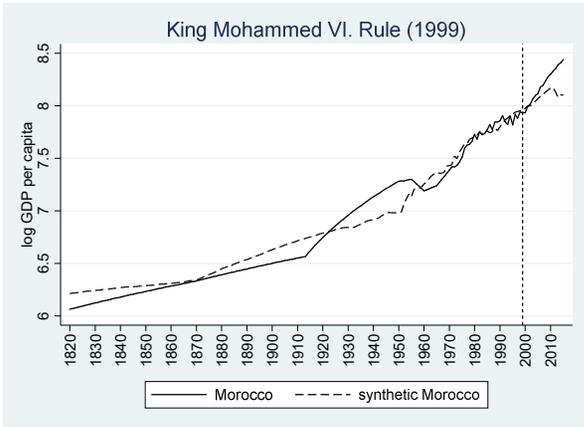
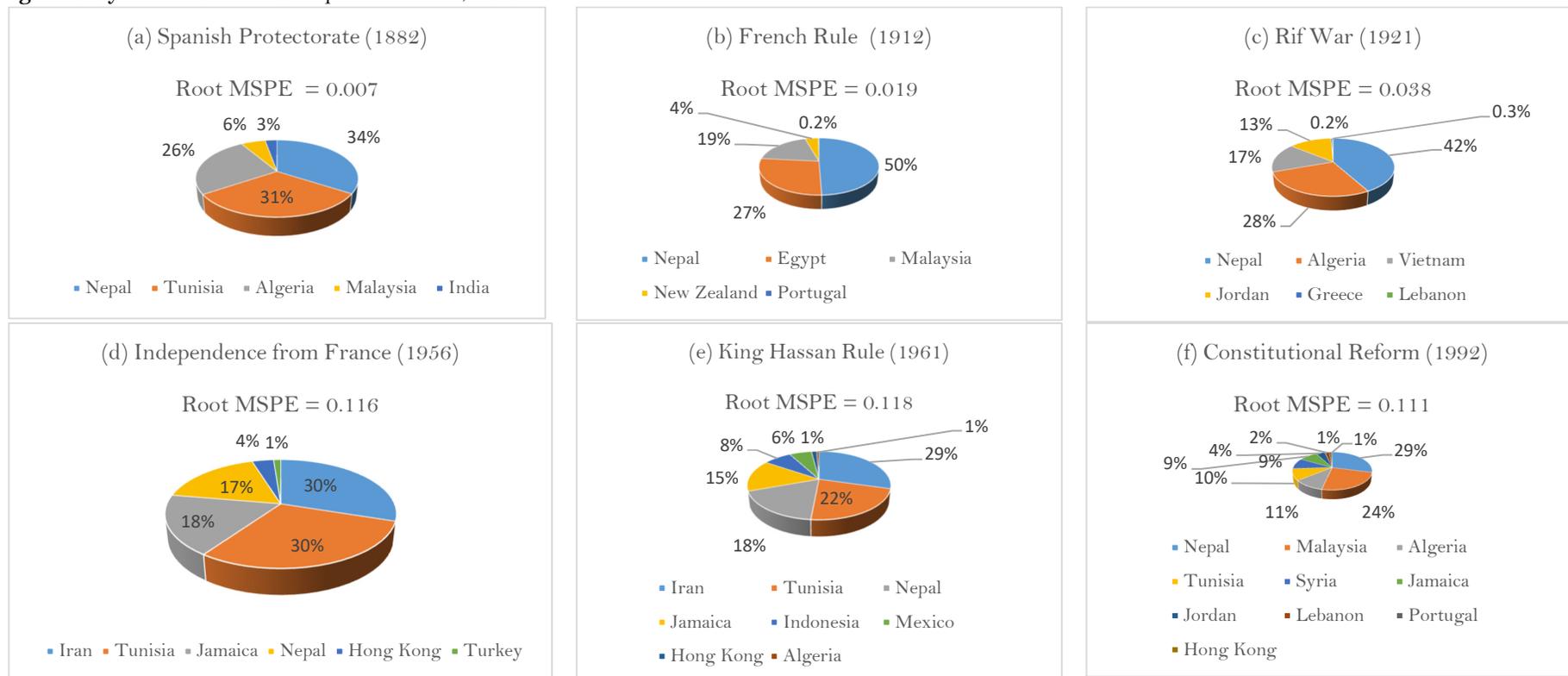
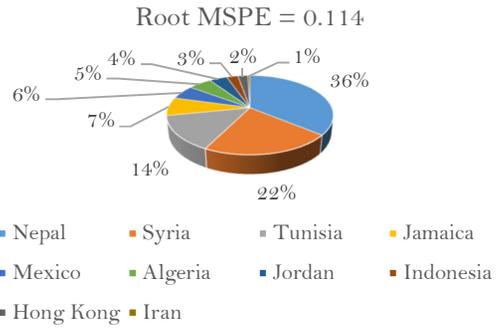


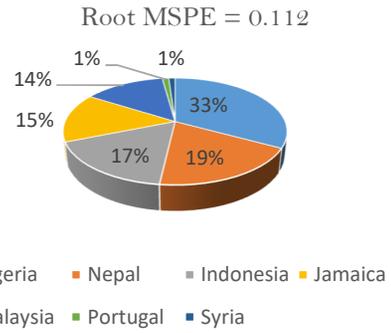
Figure 6: Synthetic Control Group for Morocco, 1820-2015



(g) King Mohammad VI. Rule (1999)



(h) U.S Free Trade Agreement (2006)



(i) Arab Spring (2011)

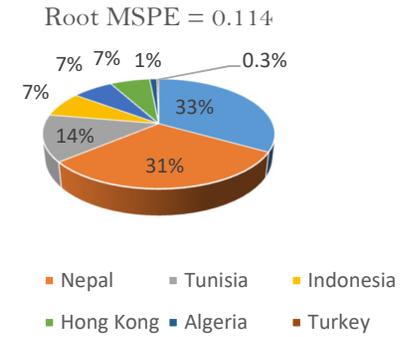


Figure 7: Institutional Change Imposed by a Shock: The Case of Jordan, 1820-2015

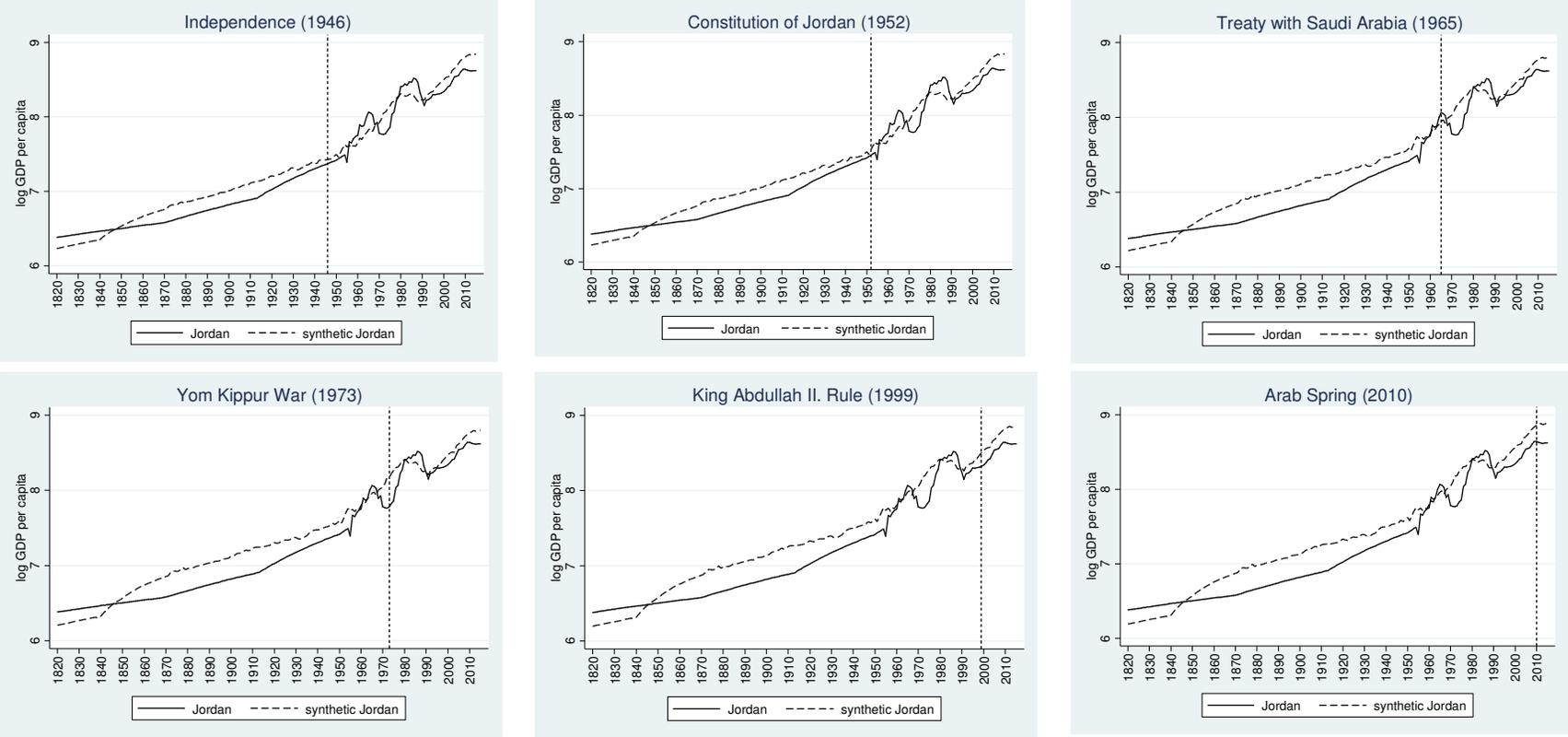


Figure 8: Synthetic Control Group for Jordan, 1820-2015

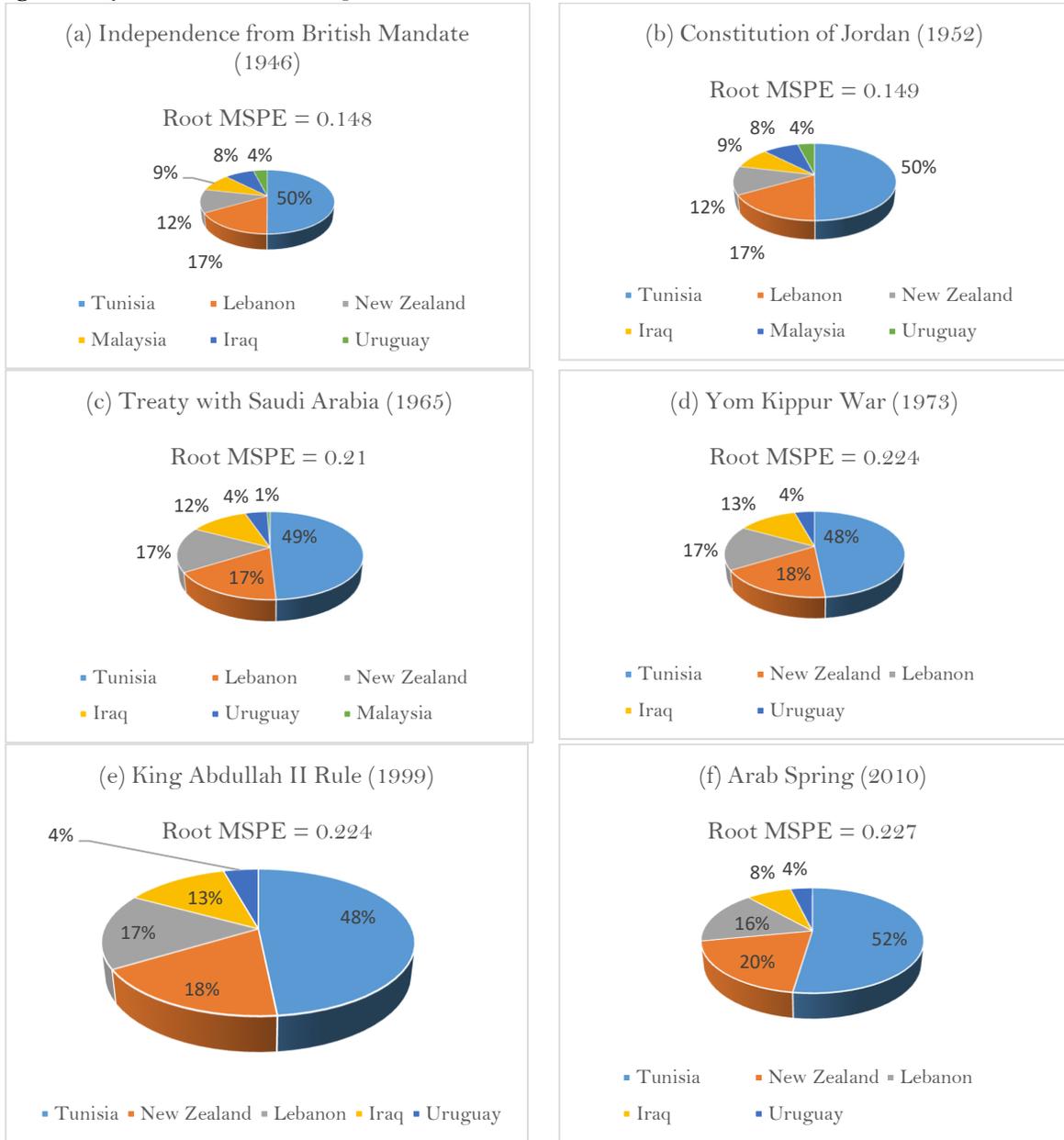


Figure 9: Institutional Breakdowns: The Case of Egypt, 1820-2015

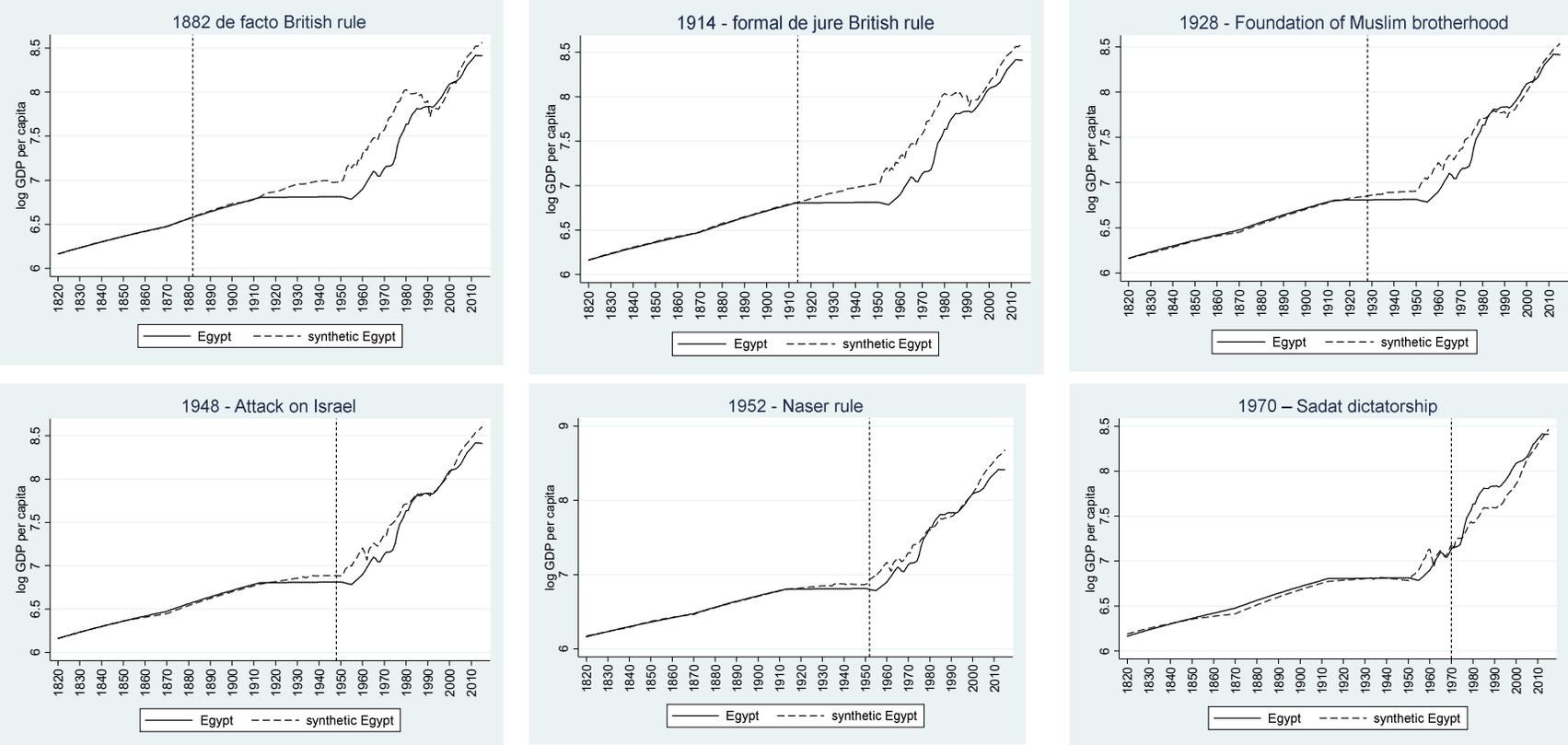
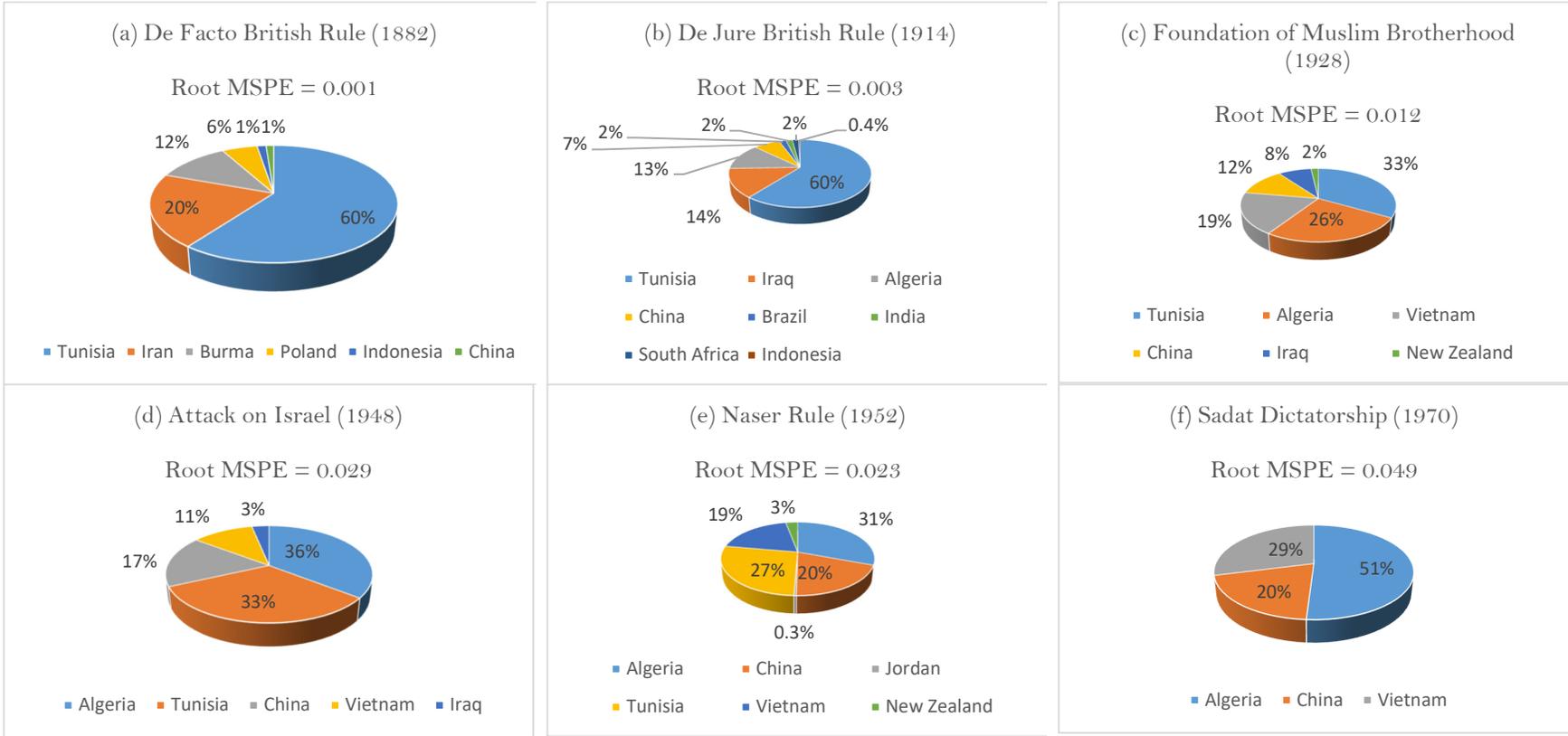
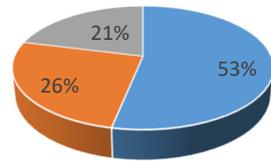


Figure 10: Synthetic Control Group for Egypt, 1820-2015



(g) Mubarak Rule (1981)

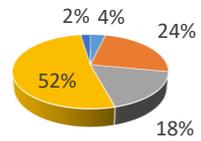
Root MSPE = 0.054



■ Algeria ■ China ■ Vietnam

(h) Rise of Islamist Terrorism (1992)

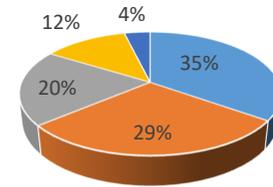
Root MSPE = 0.051



■ Australia ■ Burma ■ China
■ Tunisia ■ New Zealand

(i) Arab Spring (2010)

Root MSPE = 0.061



■ Algeria ■ Tunisia ■ China ■ Burma ■ Vietnam

Figure 11: Institutional Breakdowns: The Case of Mexico, 1820-2015

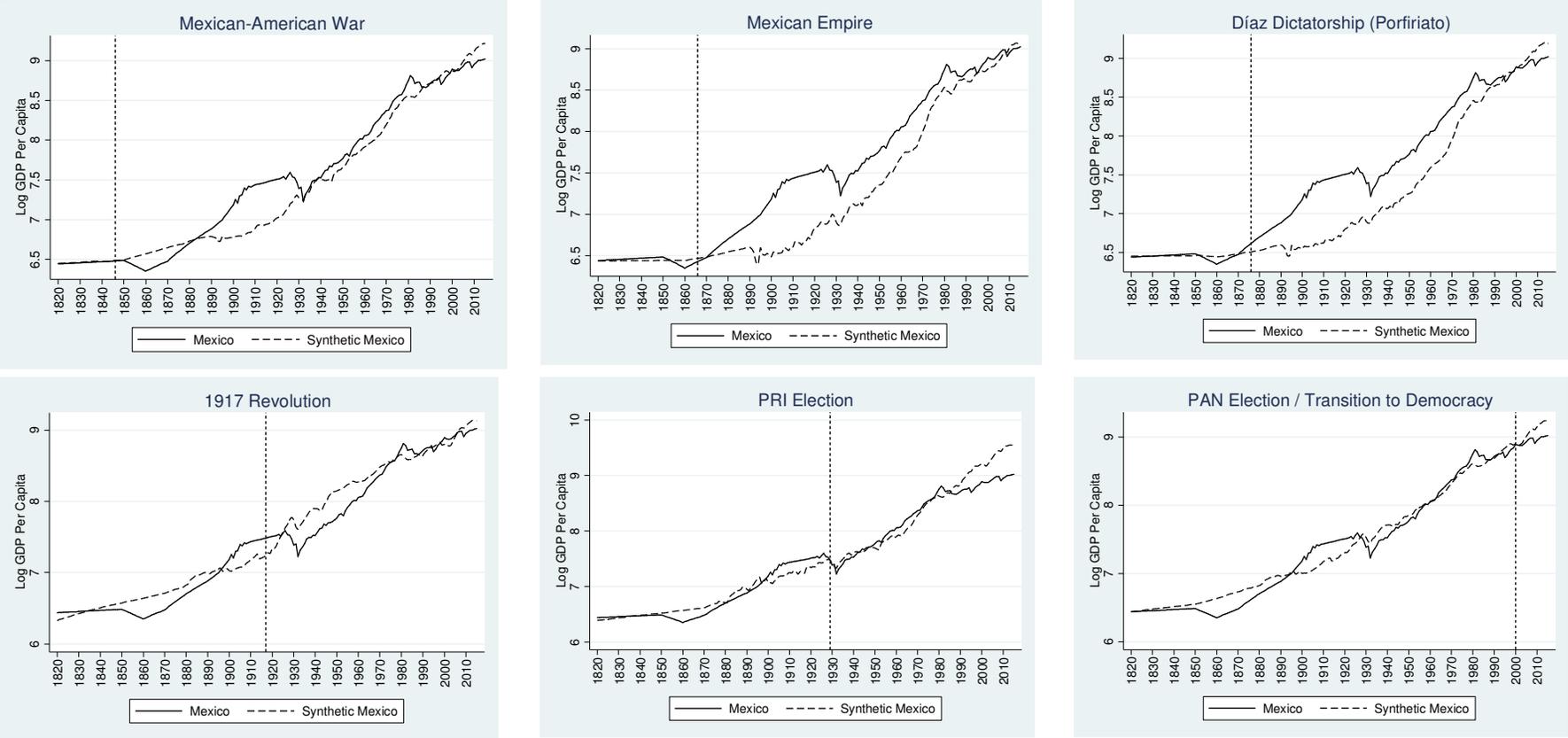


Figure 12: Synthetic Control Group for Mexico, 1820-2015

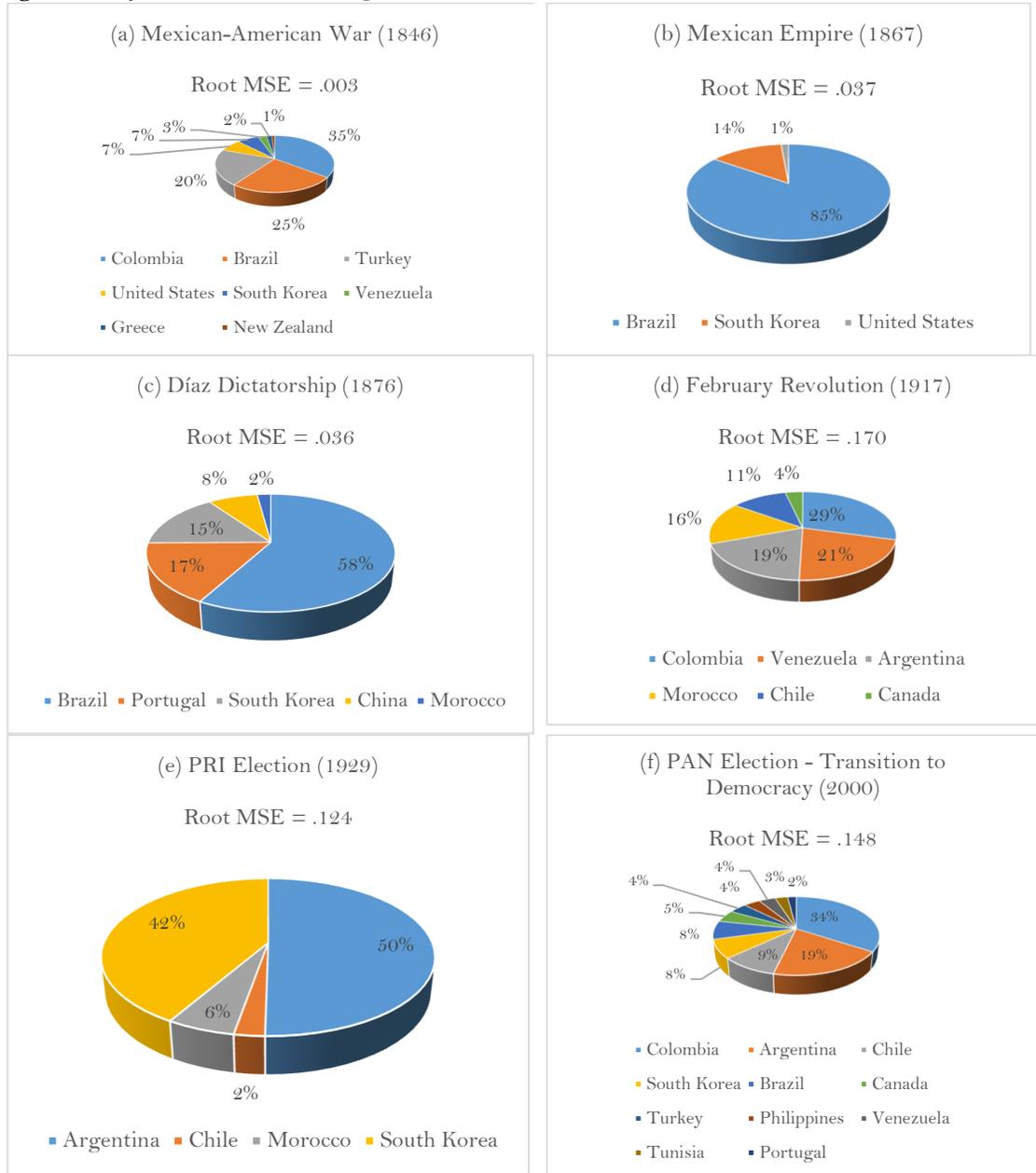


Figure 13: Institutional Breakdowns: The Case of Brazil, 1820-2015

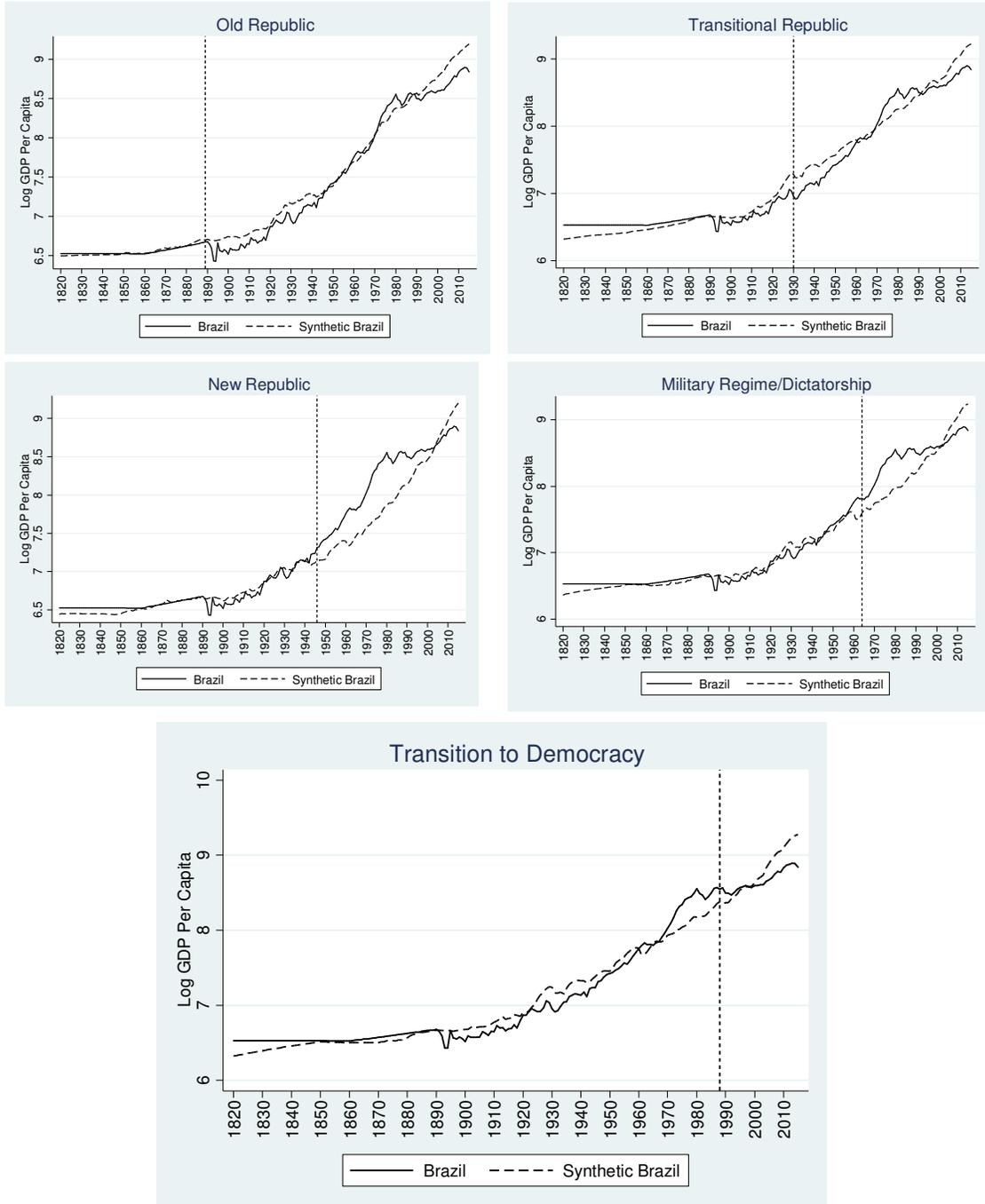
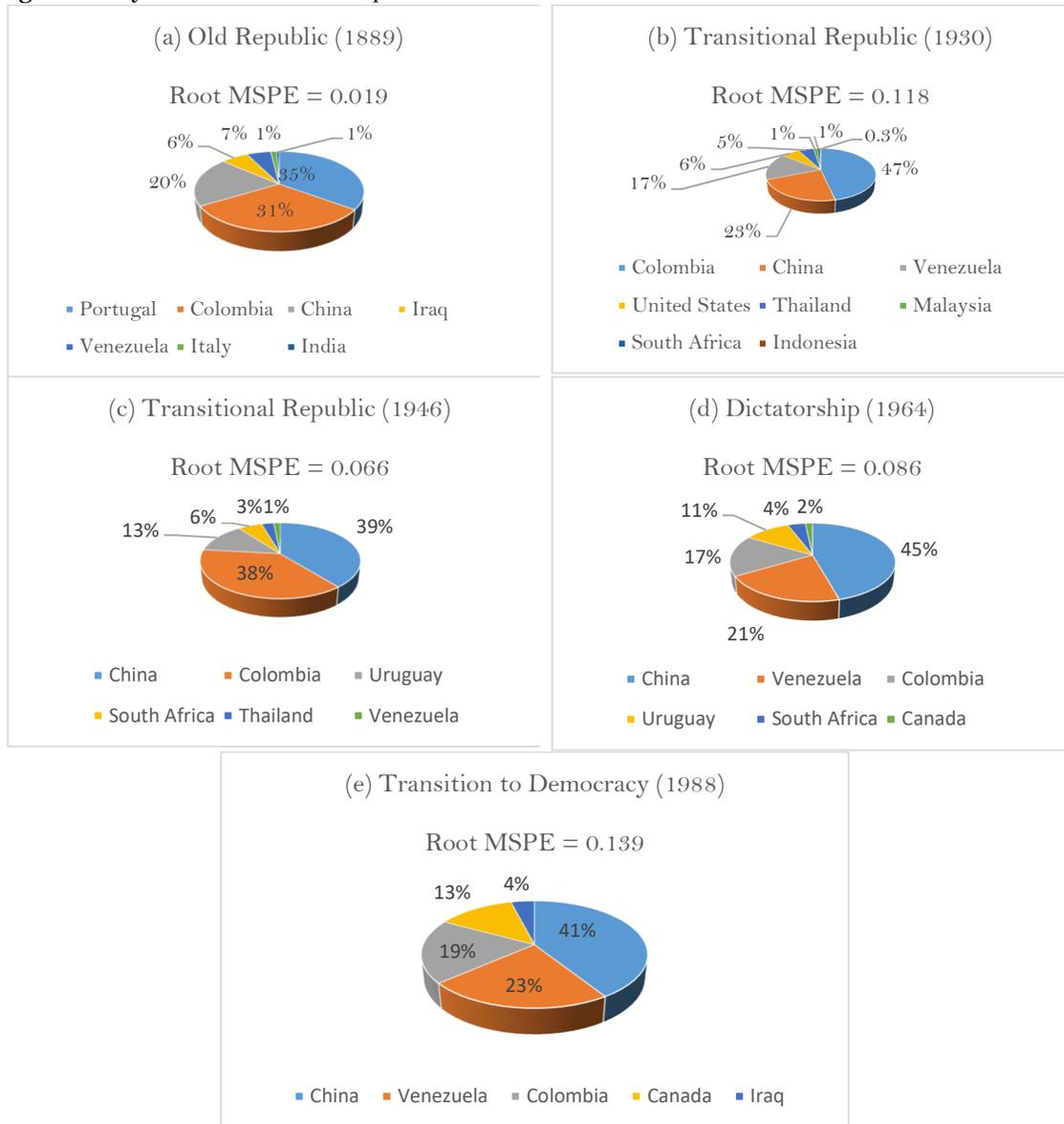


Figure 14: Synthetic Control Group for Brazil, 1820-2015



Appendix: Additional Country-Level Case Studies on Institutional Change and Counterfactual Long-Run Development Paths, 1820-2015

Figure A1: Gradual Institutional Change: The Case of Italy, 1820-2015

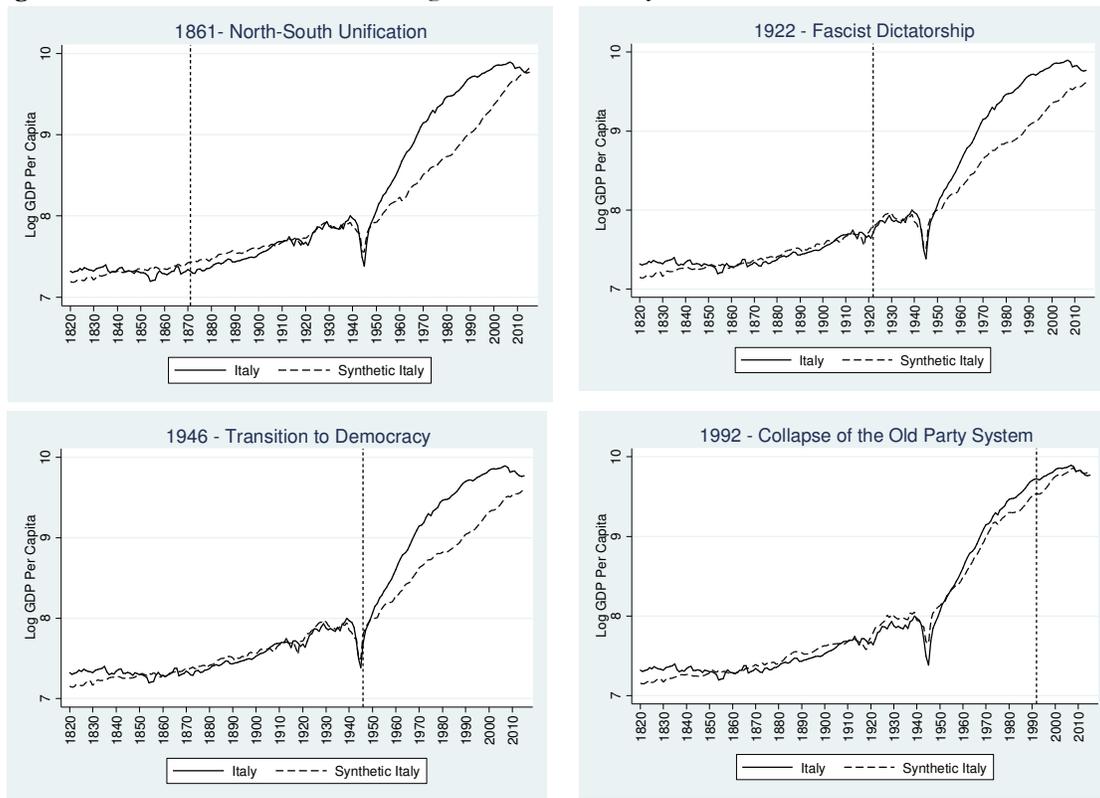


Figure A2: Institutional Change Imposed by a Shock: The Case of Slovenia, 1820-2015

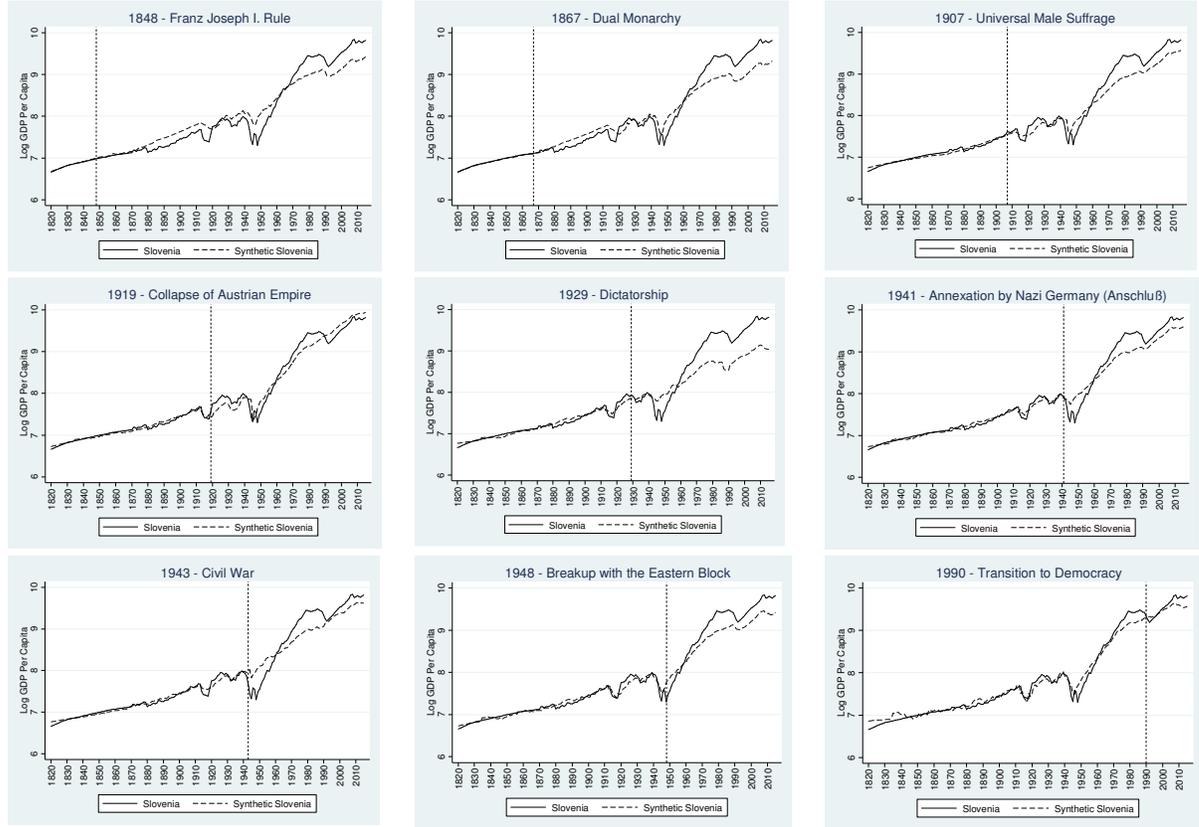


Figure A3: Institutional Change Imposed by a Shock: The Case of Portugal, 1820–2015

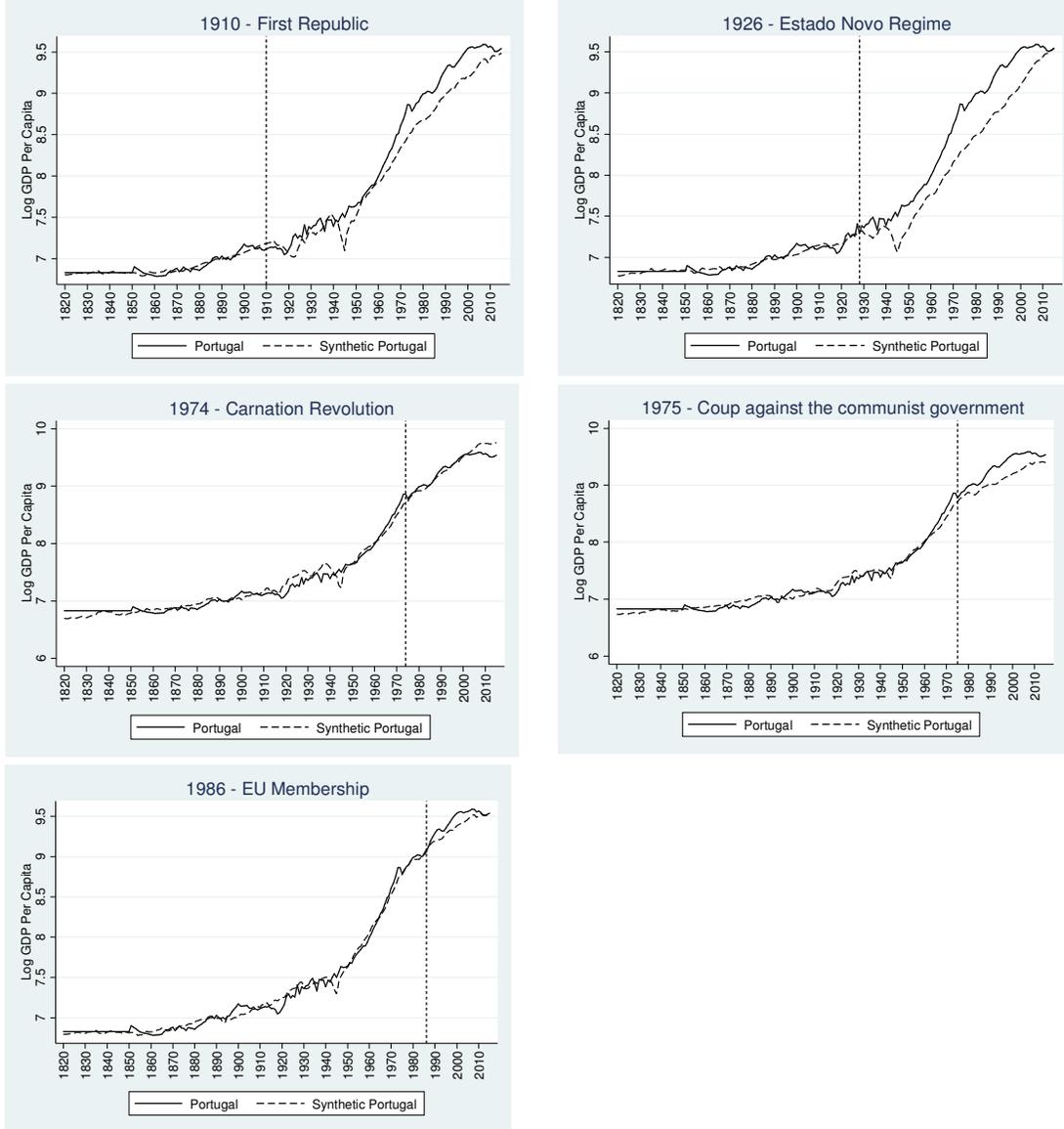


Figure A4: Institutional Change Imposed by a Shock: The Case of Spain, 1820-2015

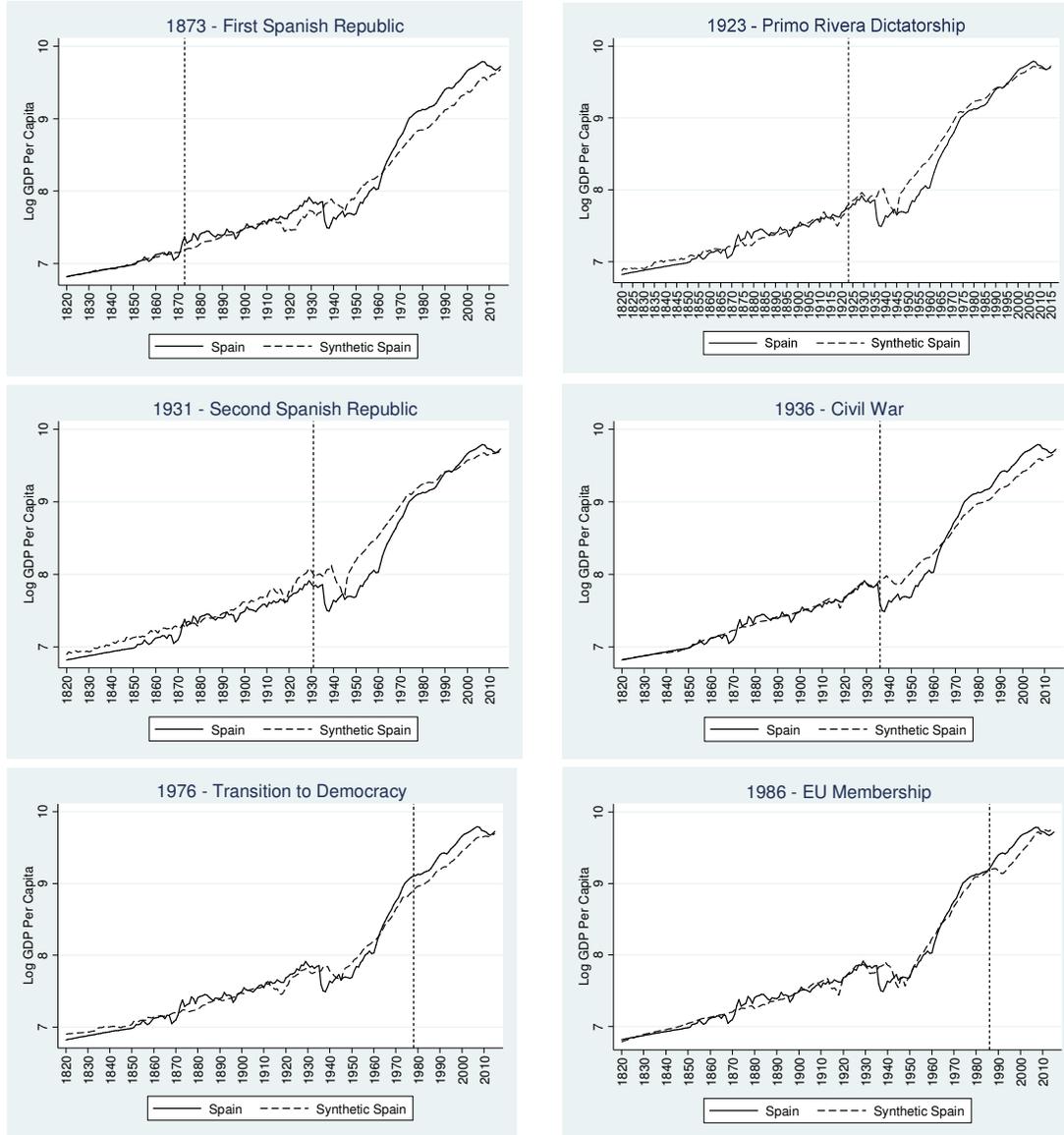


Figure A5: Institutional Breakdowns: The Case of Hungary, 1820–2015

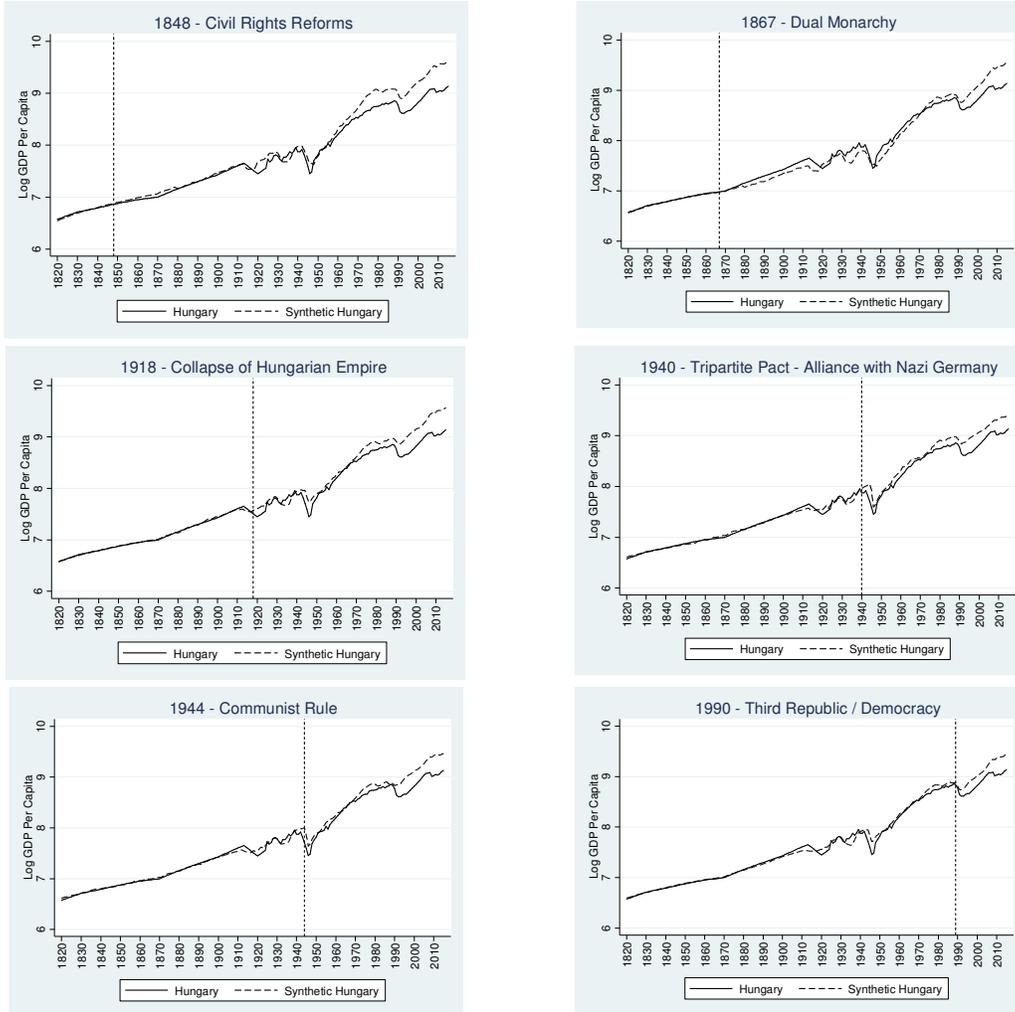


Figure A6: Institutional Breakdowns: The Case of Argentina, 1820–2015

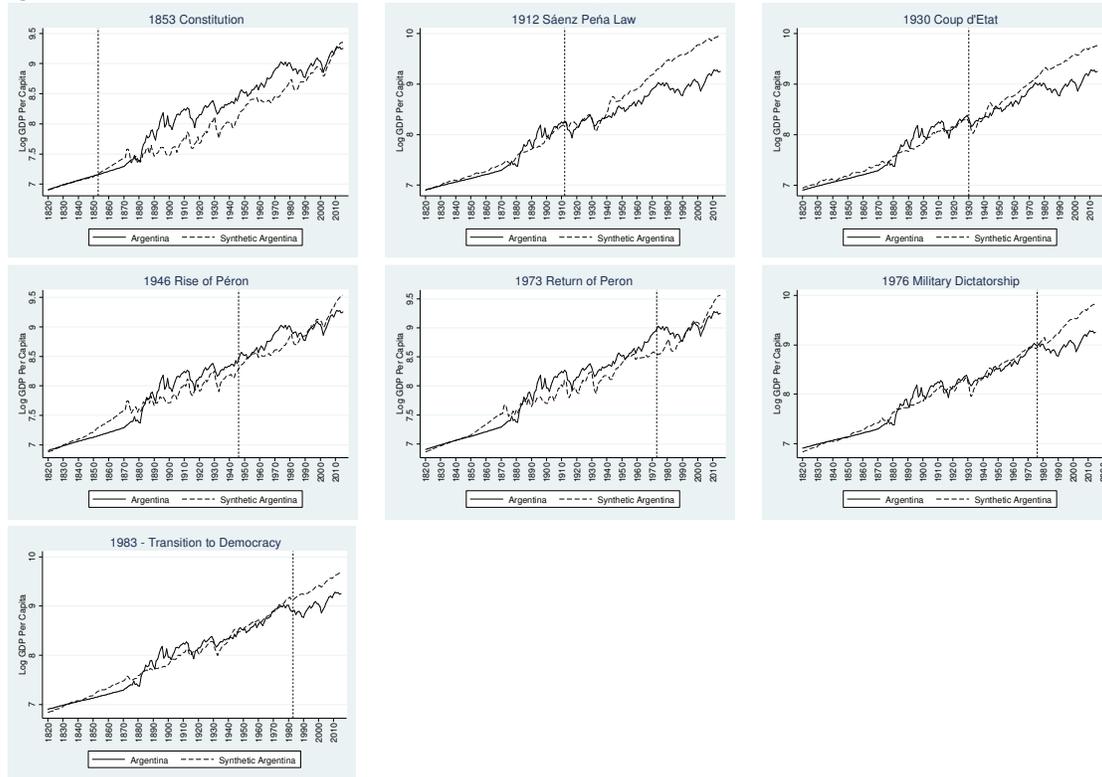


Figure A7: Institutional Breakdowns: The Case of South Africa, 1820-2015

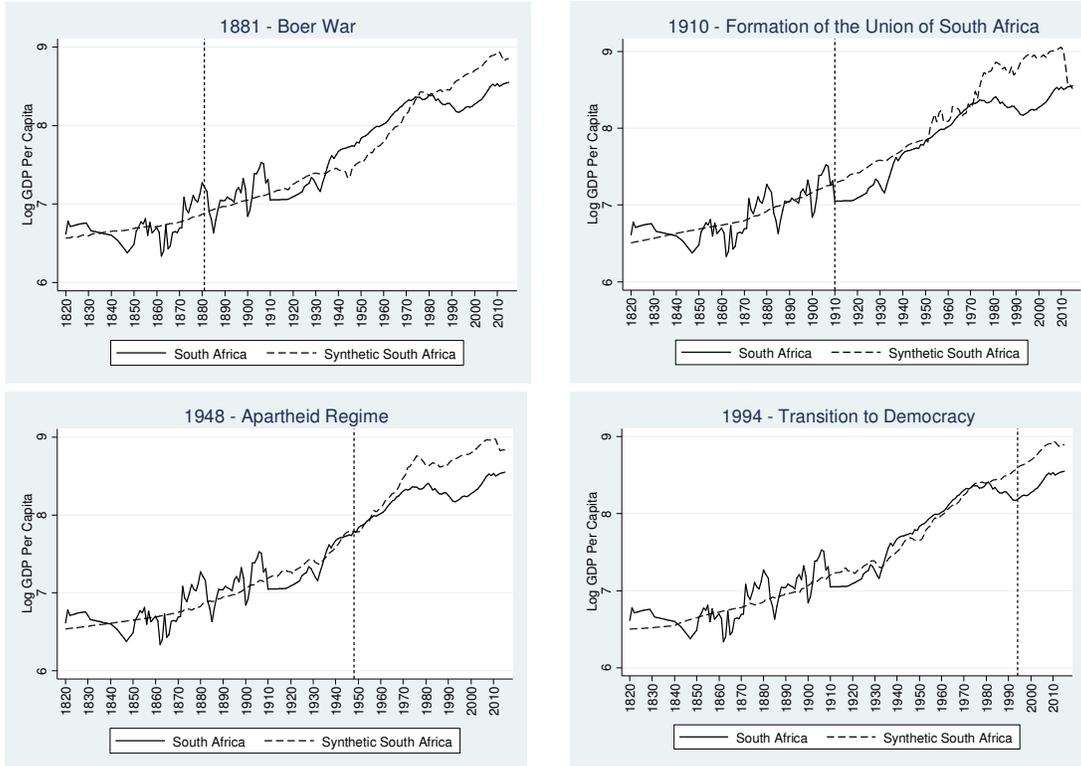


Figure A8: Institutional Breakdowns: The Case of Philippines, 1820-2015

