# American Exceptionalism and the Benefits of Statehood: An Analysis of the Growth Effects of Joining the United States of America*

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**Abstract**

*This paper quantifies the economic benefits of joining the United States. Adapting extant static synthetic control models into a dynamic model similar to Arellano and Bond (1991), we are able to construct the counterfactual growth paths of Texas, California, Arizona, New Mexico, Colorado, Utah, Wyoming and Nevada had they not joined the USA. We show that the real growth path outperforms the counterfactuals substantially in all cases. In the same way, we construct counterfactual growth paths of Puerto Rico, Cuba, the Philippines and Greenland in the scenario where they joined the USA at times in history where this might have been a (remote) possibility. We find counterfactual growth to be substantially higher than the actual growth. Having established the positive economic effects of US membership, we subsequently assess the sources of this added growth, distinguishing between a class of explanations related to internal market access and a class of explanations related to institutional quality. Using a large number of determinants of institutional quality, we find that the institutional quality of the USA as a whole matches the quality predicted for New England most closely. This suggests that upon accession, states imported the institutional quality of New England, which was typically superior to what they would have likely developed by themselves. We show that this institutional bonus accounts for the bulk of the growth benefits of US accession.*

**Keywords**: statehood, United States, economic growth
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## 1 Introduction

The 19th century witnessed the transformation of the United States from peripheral economy to global economic leader. This gave birth to the idea of American exceptionalism, stating that certain aspects of the American experience gave it a unique edge (De Tocqueville 2003 [1840]). Most notably, the unique political institutions and culture of the US were singled out as setting the country apart and contributing to its upward economic trajectory (Lipset 1996). Thus far, it has been hard to assess the economic benefits of 'being the US' in quantitative terms.

This paper aims to address that. We do so in two ways. First, we exploit another part of the economic story of the 19th century US, which is the continuous westward expansion of the country. The sequential addition of territories to the United States acts as a natural experiment of exposure to American institutions, markets and culture. To exploit this, we adapt extant static synthetic control models into a dynamic model similar to Arellano and Bond (1991). We are thus able to construct the counterfactual growth paths of Texas, California, Arizona, New Mexico, Colorado, Utah, Wyoming and Nevada had they not joined the USA. The American Exceptionalism thesis suggests that territories joining the United States would experience a bonus in terms of economic performance.

The second way in which we address the question is by focusing on territories that at one point in time were (remotely) considered candidates for joining the United States. Specifically, we construct counterfactual growth paths of Puerto Rico, Cuba, the Philippines, and Greenland in the scenario where they joined the USA as states at times in history where this might have been a possibility. If America is indeed exceptional, the real growth of these countries lags behind their counterfactual growth paths as US states.

Both analyses point at large economic performance premiums of being part of the USA, which increase over time. A follow-up question we address is what drives these growth premia. Next to institutional quality, the main contender for driving the performance effect is the access to the large domestic market that states gained upon joining the US (Alston and Smith 2019; Donaldson & Hornbeck 2016; Fogel 1962). To assess this alternative explanation, we construct counterfactual growth paths for US states using the connection to the railroad network rather than accession as alternative starting point. By comparing the trajectories, we are able to spot which event made the more sizable difference. We find that for most states, railroad access has a less sizable impact than accession.

To get a better sense of the institutional element in the accession effect, we use the same dynamic synthetic control approach to calculate counterfactual institutional quality US states had they not joined the federation. As institutional quality indicators, we use V-Dem scores as these are the only ones that are available for this time in history (Coppedge et. al. 2019). We are thus able to show that institutional quality of the USA as a whole resembles the quality one would expect to arise in New England, which is significantly higher than the predicted, counterfactual quality of the new states. This strongly suggests that upon joining, new states experienced an institutional quality boost.

Finally, we ask the question to what extent this institutional quality boost is responsible for the growth premium. We estimate the counterfactual growth paths of all countries in the world had they joined the USA. We subsequently regress the gap between factual and counterfactual economic performance on the gap between US institutional quality and actual country-level institutional quality. Doing so, we show that the institutional quality gap explains about 30% of the economic performance gap. We conclude that upon joining the United States, territories typically gain in institutional quality which leads to a long-run premium in economic performance.

We make several contributions. First, we contribute to the large literature on American development in the 19$^{th}$ and early 20$^{th}$ century by tracking quantitatively the economic performance effect of being part of the US. We are thus able to shed a novel light on the theory of American exceptionalism. Second, we contribute to the literature on institutions and economic development, by assessing the large role of American federal political institutions in its 19$^{th}$ century economic development. Econometrically, we make a third set of contributions. We apply the relatively novel literature using synthetic control methods as a way to construct counterfactuals to answer long-running historical questions. We extend this approach by adapting it to a dynamic model in the spirit of Arellano and Bond (1991). Also, by combining the dynamic synthetic control setup with a difference-in-difference approach, we are able to assess the significance of treatment effects.

The set-up of the remainder of this paper is as follows. Section II discusses the economic benefits of statehood. We start by explaining our empirical strategy. Then we move on to quantify the economic effects of accession for territories that joined the American federation after the US-Mexican war. This is followed by our estimation of the hypothetical effects of statehood for countries not joining the US. After thus having established the GDP per capita effects of statehood, we explore the drivers of this growth premium in Section III. Section IV concludes.

## 2 Quantifying the benefits of statehood

### *2.1 General approach*

We aim to quantify the benefits of US membership via two opposite strategies. First, we construct synthetic controls for territories that joined the United States. This allows us to create their counterfactual development paths in the scenario where they had stayed outside of the federation. We focus on the states entering the United States as part of the so-called Mexican Cession. This includes Texas, which broke away from Mexico and was annexed by the US in 1845, which triggered the Mexican-American War. This war resulted in the Treaty of Guadalupe (1848), followed by the Gadsden Purchase

(1854), in which the US acquired territories in what is now Texas, California, Nevada, New Mexico, Utah, Arizona, Colorado and Wyoming. We compare the trajectories of these states outside and within the United States to assess the benefits of US membership.

Second, we follow the same synthetic control approach to create counterfactual paths for states not entering the United States at a moment in history where this was a (remote) possibility. The first three states we consider are the Philippines, Cuba and Puerto Rico, all of which came under US rule after the end of the Spanish-American war in 1898. In Cuba, intense discussions about the future status of the country followed, in which annexation was one of the options. The outcome of this process was that Cuba gained formal independence in 1902. In the Philippines, similar discussions led to the installation of the Taft Commission in 1900, which set out to devise a plan for the future status of the country. As a result, the Philippines came under indirect colonial rule in 1902, ostensibly in preparation for full independence. Puerto Rico has remained US territory until today but fell short of statehood in spite of repeated discussions and plebiscites about such a status. We focus on 1973 as a potential point of statehood, had the 1967 referendum turned out differently. Finally, we consider Greenland, which has almost continuously come up as potential US territory since 1867, most recently in 2019. We use 1979 as potential accession year. While this date is mainly chosen for reasons of data availability, the possibility of Greenland's purchase was flouted shortly this date before by Vice-President Rockefeller (Persico 1982).

### *2.2 Methodology*

Our aim is to examine the contribution of joining the United States to long-run growth and development consistently. To this end, we first exploit the trends in growth and development between territories under pre-1848 Mexican control and the rest of the world, and compute the missing counterfactual long-run development scenario in the absence of US statehood by using the synthetic control estimator proposed by Abadie et. al. (2010, 2015). More specifically, we consider a simple canonical panel with $i = 1,2,...N$ countries observed for $t = 1,2,...T$ time periods. Suppose $J \in \{i = 1,2,...N\}$ denotes the set of states from Mexican cession as a treatment group, and let $Z$ be an indicator that j-th state is directly affected by the cession. Hence, we assume that the full set of J states are treated by the cession that takes place at time $T_0 < T$ whereas others are excluded from the treatment, $Z_i = 0$. In total, we have $N_1 = \sum_{j=1}^{J} W_j$ treated states and $N_0 = N_1 - N_0$ control countries in the donor pool.

Our outcome of interest is per capita income. To this end, we use the potential outcomes framework proposed by Rubin (1974) and assume a well-defined treatment that excludes interference between states. The potential per capita income in the ceded states and interference-adjusted control samples is $y_{i,t}\left(0\right)$ and $y_{j,t}\left(1\right)$, which leads to the following observed outcomes:

$$y_{i,t} = \begin{cases} y_{i,t}\left(0\right) & \text{if } Z_i = 0 \text{ or } t \le T_0 \\ y_{j,t}\left(1\right) & \text{if } Z_j = 1 \text{ and } t > T_0 \end{cases} \quad (1)$$

Let $\mathbf{X}$ represent pre-cession per capita income and covariates associated with long-run growth and development that corresponds to $N_0 \times T_0$ matrix where $\mathbf{X}_0^{Control}$ denotes the full set of covariates for the countries in the control sample and $\mathbf{X}_{j,1}^{Ceded}$ is the set of pre-cession outcomes and covariates for $j$-th ceded state. The treatment effect of the statehood is given by:

$$\lambda = \lambda_{1T} = y_1\left(Z_j = 1\right) - y_1\left(Z_i = 0\right) = y_1 - y_0 \quad (2)$$

We construct the counterfactual growth and development trajectory by imputing the missing potential outcome for the treated state as a weighted average of the outcomes in the control sample. By following Abadie and Gardeazabal (2003) and Abadie et. al. (2010), the set of weights used to construct the counterfactual outcome is defined as a solution to the constrained optimization problem:

$$\min_{\mu} \sum_{t=1}^{T_0} \left(\mathbf{X}_{j,1}^{Ceded} - \mathbf{X}_{i \notin j,0}^{Control}\mu\right)^2 \quad (3)$$

where $\sum_{i=1}^{n} \mu_i = 1$ and $\mu_1 \ge 0$ with $i = 1,2,\ldots N$ and the minimization constraints set the limit of $\mu$ to the $N_0$ simplex such that $\mu \in \Delta^{N_0}$. The weights from Eq. (3) minimize the imbalance in covariates and pre-treatment outcomes between the ceded states and their synthetic control group. If the exact balance can be achieved such that $\sum_{t=1}^{T_0}\left(\mathbf{X}_{j,1}^{Ceded} - \mathbf{X}_{i \notin j,0}^{Control}\mu\right) = 0$, the synthetic control estimator for the treatment effect of interest is asymptotically unbiased. Country-level weights that yield the exact balance may be achieved if the treated country is inside the convex hull of the control sample.

We also assume that there are $K$ latent time-varying covariates $\gamma_t = \{\gamma_{kt}\}$, $k = 1,2,...K$ with $\max_{kt} |\gamma_{kt}| < L$, where $K$ will be small relative to $N$. Each country has a vector of factor loading denoted by $\theta_i \in \mathbb{R}^K$. This implies that the per capita income in the control group is a weighted average of these factors plus the additive error term:

$$y_{it}(0) = \sum_{k=1}^{K} \theta_{ik}\gamma_{kt} + u_{it} \qquad (4)$$

where $u$ denotes the random error term, and $\gamma$ represents the full set of pre-cession outcomes and covariates that may be collected into the matrix $\gamma \in \mathbb{R}^{T_0 \times K}$. By invoking temporal and cross-sectional independence in the random error term as a sub-Gaussian random variable with a certain scale parameter, we further assume that the latent time-varying factors are orthogonal and that treatment assignment variable $Z$ is ignorable given the country-level factor loadings which implies that:

$$\mathrm{E}_{u_T}\left[y_i(0), \theta_i, Z_i\right] = \mathrm{E}_{u_T}\left[y_i(0), \theta_i\right] \qquad (5)$$

where under the linearity in parameters assumption and ignorable treatment assignment for given $\theta_i$, the synthetic control estimator that balances the pre-cession outcomes and covariates between the ceded states and the country-level control group, yields a reasonably unbiased estimate of $y_1(0)$ if the ex-ante observed differences in outcomes are covariates between the treatment and control samples are sufficiently small to rule out large approximation errors. The bias of the weighting estimator with the series of non-negative and additive weights is (Ben Michael et. al. 2018):

$$E_{\varepsilon_T}\left[y_1(0)-\sum_{Z_i=0}\mu_i y_i\right]=\left(\theta_1-\sum_{W_i=0}\mu_i\theta_i\right)\cdot\gamma_T=$$
$$=\frac{1}{T_0}\left(\gamma'\mathbf{X}_{j,1}^{Ceded}-\sum_{Z_i=0}\mu_i\gamma'\mathbf{X}_{i\notin j}^{Control}\right)\cdot\gamma_T-\frac{1}{T_0}\left(\gamma' u_1-\sum_{Z_i=0}\mu_i\gamma' u_i\right)\cdot\gamma_T \qquad (6)$$

Where the first term captures the imbalance in covariates, and the second term captures the approximation error. Notice that the approximation error is typically small if the pre-treatment period $T_0$ is sufficiently large. Following Dube and Zippner (2015), Firpo and Possebom (2018), Botosaru and Ferman (2019), and Aldhikari and Alm (2016), we avoid using the average of pre-treatment outcomes in our specification (Ferman et. al. 2017, Ferman and Pinto 2018, Ferman 2019). Instead, we further minimize the covariate imbalance by deploying the two lags of the outcomes into our key synthetic control specification, allowing us to fully exploit the time-series dynamics in per capita output to further unravel the long-term treatment effect of US accession.

We follow the same procedure to quantify the costs of not joining the US. Here, we exploit the trends in growth and development between countries under consideration for accession and the US states, and compute the missing counterfactual long-run development scenario in the case of US statehood by using synthetic control estimator.

### *2.3 Data and Samples*

Our dependent variable is per capita GDP capita at the constant prices adjusted for purchasing power parity in the period 1840-2016. We reconstruct the per capita GDP by combining several existing measures of per capita income or product in several steps. In the first step, we collect the annual state-level data on per capita income for 48 states since 1929 by relying on Bureau of Economic Analysis (1986) series of Current Business Surveys. Given the lack of useful measures of price level for each state, we deflate state-level series by the national index of consumer prices. We link the 1929-1963 per capita income series to the state-level series on per capita gross state product for the period 1963 and onwards and use the aggregate state-level gross product deflator to convert nominal figures into real ones (Reenshaw et. al, 1988). The historical estimates of state personal income per capita without transfer payments provided by Easterlin (1960a, 1960b) for the benchmark years 1840, 1880, 1900, and 1920, and used by Barro and Sala-i-Martin (1992) to study convergence across US states. Hence, the combined series is linked to these estimates of per capita income. The level of per capita income missing period between the benchmark years is proxied by a weighted index of state-level population density (U.S Census Bureau Historical Statistics of the United States, 2006) and aggregate growth of per capita GDP of the US as whole (Sutch 2006, Bolt and Van Zanden 2014), or Mexico for the states that belong to the territory of Alta California prior to the Mexican cession. The weighted index is used in state-specific proportions that best link the levels of per capita income between the benchmark years.

We separately construct the historical per capita GDP series for Hawaii by linking post-1929 series of GDP per capita from Bureau of Economic Analysis and pre-1929 per capita income proxies reported by Schmitt (1973) in Historical Statistics of Hawaii. Our country-level per capita GDP data is from Bolt and Van Zanden (2014) for the same period where we adjust both state-level and country-level series for PPP differences to make them comparable. Taken together our sample yields a strongly balanced panel of 50 states[2], and 57 countries[3] for the period 1840-2016 with a total of 18,939 observations.

### *2.4. Covariates*

The set of covariates used in our synthetic control analysis consists of three distinctive blocks of variables. The first block consists of exogenous geographic characteristics unaffected by the admission of the states from the Mexican cession to the United States. By relying on the U.S Geological Survey's Geographic Information System (GIS), we extract the vector of easily observable state-level geographic variables such as mean annual temperature (in F°), annual precipitation (in inches), sunshine duration (in hours), humidity (in terms of percentage), latitude and longitude coordinates, annual snowfall (in inches), indicators for desert area, sea access, and island, and size of the land mass area (in sq. miles). The second block of variables consists of pre-statehood per capita GDP dynamics which we capture by including the level of per capita GDP in the year 1840 (i.e. initial year), first lag and second lag of per capita GDP in the main synthetic control specification. The third block of variables consists of the legal origin indicators (La Porta et. al. 1998) and captures the role of common legal and institutional history in long-run growth and development. For the sake of convenience, we use the

[2] Alabama, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming.

[3] Algeria, Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Burma, Canada, Chile, China, Colombia, Cuba, Czechia, 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, Uruguay, Venezuela, Vietnam

British common law indicator and civil law indicator rather than indicators for separate legal families within the civil law tradition. The key descriptive statistics for the dependent variable and covariates is reported in greater detail in Table 1.

TABLE 1 [INSERT HERE]

# 3 Results

## *3.1 Baseline results*

Can the pre-accession growth trajectory of the ceded states be plausibly reproduced as a combination of other countries' growth and development characteristics? Table 2 presents the covariate-level balanced between the ceded states and their respective synthetic control groups. Notice that the covariate values of the synthetic control group are obtained as a linear combination of the covariate-level growth and development characteristics of the countries that fall within the convex hull of the transposed matrix but did not experience the cession to the United States. The evidence suggests a reasonably strong degree of similarity between the ceded states and other countries on pre-cession GDP per capita dynamics, initial level of per capita GDP and its level in pre-admission year, and pre-determined geographic characteristics.

TABLE 2 [INSERT HERE]

Table 3 presents the composition of the synthetic control groups for each ceded state and indicated marked disparities in the set of countries that plausibly reproduce the growth trajectory prior to the statehood. For instance, the growth pattern of pre-statehood California is best reproduced by growth and development covariate values of Canada (49%), South Africa (19%), New Zealand (15%), Egypt (10%), Norway (5%), and United Kingdom (3%), respectively. On the other hand, the synthetic control group for Arizona consists of Egypt (70%), Jamaica (18%), United Kingdom (11%), and Greece (1%), respectively. The key question pertains to the quality of the pre-statehood fit of the growth trajectory of the admitted states compared to their synthetic control groups. In all eight cases, our evidence indicates reasonably strong fit and very little discrepancy in the estimated outcome model prior to the statehood. More specifically, we estimate the lowest mean square prediction error (MSPE) for Texas, Utah and Colorado where the trajectory of pre-statehood growth can be reproduced by their respective synthetic control groups with an almost non-existing discrepancy of about 1 percent pre-treatment error margin. In a similar vein, the growth trajectories of Arizona, Wyoming and

California can be reproduced by their synthetic control groups with a pre-treatment error margin of between 2 percent and 3 percent. For Nevada and New Mexico, the growth trajectories are more difficult to capture by their control groups. For instance, Nevada's pre-1861 growth trajectory can be best approximated by New Zealand (58 percent) and Australia (28 percent) while New Mexico's growth path is most adequately predicted by the growth dynamics of Canada (64 percent) and Cuba (25 percent). At the bottom line, the low size of the pre-statehood prediction error suggests that other countries provide a meaningful donor pool for the admitted states that is able to capture the pre-statehood growth variation reasonably well but did not undergo the same institutional shock as the treated US states.

TABLE 3 [INSERT HERE]

Figure 1 presents our baseline synthetic control estimate of the long-run growth effect of statehood. The evidence clearly suggests large and pervasive growth gains in response to the admission to the United States. The counterfactual scenario, in which the eight states would not join the U.S., demonstrates remarkably slower path of long-run growth. We estimate the largest statehood-induced long-run growth gap for Arizona, Utah and Texas. For instance, the estimated statehood-induced per capita GDP gap between Arizona and its synthetic control group amounts to 1.91 log points down to the present day. This implies that a hypothetical Arizona without the admission to the U.S would have 86 percent lower per capita income than the real Arizona after the admission to the U.S. Similarly, the estimated per capita income gaps for Utah and Texas are 81 percent and 79 percent, suggesting a substantially lower per capita income in the absence of US statehood. The corresponding gaps are somewhat smaller in terms of magnitude for California (68 percent), Colorado (71 percent), Nevada (48 percent), New Mexico (54 percent), and Wyoming (69 percent). Our baseline estimates convey a couple of implications. First, admission to the US is associated with significant and sizeable improvement of the long-run economic growth. A possible reason for this may be that the US institutional framework appears to be superior compared to the set of institutions that these states could develop by themselves. The outline of the hypothetical institutional framework adopted in the absence of statehood is partly reflected in the composition of the synthetic control group for each ceded state. We delve more into this possibility in Section III of the paper. Second, the growth premium of accession tends to increase over time for some states (Arizona, Texas, Utah) while it remains broadly stable for others. This indicates that statehood was especially beneficial for those states that had less favorable geographical characteristics and were more isolated from the US domestic market and international markets. Thus, it should be no surprise that the estimated growth effect of statehood is smaller for California and quite large for Arizona or Utah. Third, the admission of the ceded states to the US is reminiscent of the form of structural institutional break-up which permanently improved the long-run growth path rather than having a temporary impact on growth. This implies that the admission of these territories to the US posits an institutional shock that altered the long-run growth equilibrium on a higher level compared to the counterfactual scenario.

FIGURE 1 [INSERT HERE]

### *3.2 Inference on the effects of statehood*

The empirical evidence so far advocates large and pervasive growth premium of US statehood. The question that remains unanswered is whether the long-run growth effect of statehood is statistically significant. To assess the statistical significance of the growth effect of statehood, we rely on the inferential techniques proposed by Abadie and Gardeazabal (2003), Abadie et. al. (2010, 2015), and further extended by Dube and Zipperer (2015), Adhikari and Alm (2015) and Firpo et. al. (2018). More specifically, we falsely assign the statehood to all the other countries that never joined and were never admitted to the United States, and reproduce the counterfactual scenario in the event they were admitted to the US. This procedure is similar to the permutation-based inference where the intervention of interest is assigned to the non-intervention groups. In a similar vein, we apply the synthetic control estimator to every potential control country in our donor pool. This allows us to gauge the size of the statehood-related growth effect compared to the countries, and construct the entire distribution of the estimated effect of placebo interventions. In our specific setting, we examine whether the estimated growth effect of statehood is large relative to the distribution of placebos estimated for the countries not exposed to the Mexican cession and subsequent admission to the US.

To evaluate the significance of our estimates, we ask whether our baseline estimates are driven by chance or by accession to the US. To this end, we run a series of placebo tests where the de facto date of statehood is applied to every country in the donor pool. This effectively shifts the affected state from the treatment sample to the donor pool. By applying the synthetic control estimator to the unaffected countries, we are able to construct gaps in per capita GDP. If the placebo runs create gaps of similar magnitude to the one estimated for each affected state, then the analysis does not provide evidence that could support the significant effect of admission to the US on long-run growth. On the contrary, if the placebo gaps indicate an unusually large per capita GDP gap for the affected state relative to the gaps in the control group, the significant effect of statehood on long-run growth becomes more plausible.

FIGURE 2 [INSERT HERE]

Figure 2 presents the placebo distributions of per capita GDP per capita for each treated state. The figure indicates that our model is able to provide a good fit for the path of economic growth prior to the admission to the US while the path of growth cannot be well reproduced for most countries. Post-cession per capita GDP gap appears to be unusually large for most ceded states except Nevada, New Mexico, and to some extent, Wyoming. If the synthetic control groups for the ceded states had a poor fit of per capita GDP level prior to the statehood, much of the post-statehood gap were created by the lack of fit rather than by the effect of joining the US. In this respect, placebos with poor fit prior to the admission to the US do not provide enough information to measure the rarity of estimating a large post-statehood growth premium for a country that was reasonably well fitted prior to the admission to the US. Following Abadie et. al. (2010) and several others, we address the relative rarity of obtaining large but implausible post-admission effects by excluding all countries from the donor pool that have pre-admission MSPE of more than twice the MSPE of the affected states. Hence, we only consider those countries that can fit almost as well as the ceded states in terms of their pre-admission growth path. Against the distribution of these gaps, the per capita GDP gap for the admitted states appears to be large and usual with a marked discrepancy compared to the other countries, which confirms the large growth effects emanating from the admission of the ceded states to the US.

In Figure 3, we compute the p-values associated with the growth effect of the admission to the US. The p-values are obtained non-parametrically and denote the fraction of countries in the donor pool with the estimated post-admission growth effect as large as the effect obtained for the admitted states. The p-values roughly indicate the probability of obtaining the effect by chance under a random assignment of intervention to the unexposed countries. The evidence suggests that with the exception of Nevada and New Mexico, the effect of joining the United States on long-run growth appears to be significant. The computed p-values in the last post-treatment year are in the range between 0.01 (for Arizona) and 0.05 (for California), which is within the conventional 5% significance level. Taken together, the estimated p-values suggest that states such as Arizona, California, Colorado, Texas, and Utah were readily and strongly affected by the admission to the US where the underlying effect is easily perceptible. By contrast, the –values on the growth effect of admission to the United States are 0.17 and 0.19 for Nevada and New Mexico, respectively, which implies that the statehood-related growth effect in these two states is not as easily perceptible as for the other states.

The estimated placebo gaps indicate unusually large post-treatment effect of joining the US for the affected states compared to the countries in the control group. The remaining question is whether the estimated post-admission long-run growth gap is specific to the affected state or not. To assess whether the effect is specific to the affected states or equally perceptible across other countries, we examine the effect of joining the United States on the behavior of gap in the post-admission period by estimating a simple difference-in-differences model specification using the full set of placebo gaps and the estimated gap for each affected state as a dependent variable, and the interaction between state indicator and post-admission indicator as the underlying post-treatment variable of interest. We add the full set of country-fixed effects and common time-fixed effects to each specification to partially account for the unobserved heterogeneity.

FIGURE 3 [INSERT HERE]

Table 4 reports the parametric difference-in-differences (DD) estimate of the statehood effect on long-run growth. Panel A reports the percentile ranks for the estimated growth effect of statehood in the treated states. Panel B reports the difference-in-differences effect of joining the United States for each ceded state. The estimates arguably indicate strong and pervasive effects of the admission on long-run growth. The underlying post-admission gap coefficients are both large, positive and statistically significant at 1%, respectively, and readily suggest that the effect of joining the US appears to be specific to the treated states. The gap coefficients do not seem to be confounded by country-specific heterogeneity bias or common time-varying shocks. Panel C reports cluster-robust gap coefficients and the corresponding standard errors and confirms the significant and positive post-admission gap between the real and synthetic GDP per capita. In the presence of fixed effects, the distribution of the placebo DD coefficients is very similar to the corresponding distribution of the synthetic control estimates, and largely confirms our theoretical notions. Figure 4 displays the fixed-effects placebo DD coefficient for each affected state.

TABLE 4 [INSERT HERE]
FIGURE 4 [INSERT HERE]

### *3.3 Estimating the cost of non-statehood*

### *3.3.1 Constructing control group*

The results of our first analysis indicate large and pervasive effects of joining the United States on the long-run economic growth of the states ceded to the U.S from Mexico. The underlying growth effects of statehood appear to be statistically significant at conventional levels, tend to be specific to the ceded states, and highly distinct from the placebo distributions of the effect in unexposed countries. If the growth premium of joining the United States is large and apparent, a next question arises. How different would the long-run growth path have been for countries that once had the remote possibility to join the United States but did not do so, where they admitted to the US? To estimate this, we apply the synthetic control estimator to the four countries that had a remote possibility of joining the US. The first three of these are Cuba (in 1901), the Philippines (in 1900), and Puerto Rico (in 1973), all of which came to be occupied by the US after the end of the Spanish-American war in 1898. In the light of recent public debate, we also add Greenland and use the year 1979 as a hypothetical date of admission. To determine whether the effect of joining the United States is significant or not, we use the US states as a donor pool to capture pre-quasi-admission growth and development trends in the these countries, and construct the counterfactual scenario of the hypothetical statehood. Table 5 reports covariate balance scores between each country and state-level synthetic control groups, and suggests that the synthetic control groups are able to capture the pre-quasi-admission growth trajectories reasonably well with little discrepancy in the fit of the underlying outcome model.

TABLE 5 [INSERT HERE]

Which US states are best able to reproduce the growth trajectory of the four countries that could hypothetically join the US prior to the hypothetical date of admission? Table 6 presents the composition of synthetic control groups for Cuba, Philippines, Puerto Rico and Greenland. The sets of states that capture pre-hypothetical admission growth and development trends tend to exhibit both similarities and marked contrasts between the affected states. For instance, a synthetic Cuba prior to 1901 can be best reproduced as a linear combination of growth and development characteristics of Hawaii (56 percent), North Dakota (31 percent), and California (14 percent). The growth trajectory of synthetic Philippines prior to the US occupation in 1901 can be best synthesized by Hawaii (89 percent) and Tennessee (11 percent), respectively. In a similar vein, a synthetic pre-1973 Puerto Rico comprises the growth and development characteristics of Hawaii (59 percent) and Florida (41 percent) while synthetic Greenland's growth performance as a quasi-US state can be best reproduced as a linear combination of Hawaii (53 percent), West Virginia (27 percent), and Michigan (20 percent). The similarities and contrasts in the composition of synthetic control groups denote the set of states with the pre-existing GDP per capita dynamics, and physical geographic covariates most similar to the quasi-treated countries that could join the US when the admission was a remote possibility. In all four cases, we find that each country can be matched reasonably well with the US states prior to the hypothetical year of the admission to the US. The resulting gap in per capita income between the synthetic version of these countries as US state and the actual country versions is large and tends to persist down to the present day.

TABLE 6 [INSERT HERE]

### *3.3.2 Baseline results*

Having constructed the synthetic control groups, we can address the question of the costs of non-accession. Our estimates suggest that the effects of the potential statehood on long-run growth are very large with a notable variation across the treated countries. The counterfactual growth trajectory that captures the long-run growth effect of the statehood is constructed as a weighted average of US states' growth and development characteristics that capture pre-shock trends to match to treated country with the US states. In all five countries, the counterfactual growth trajectory is substantially higher than its real counterpart which suggests that if these countries joined the US, their long-run growth would most likely improve substantially.

Figure 6 presents the effects of the hypothetical admission of the four countries to the United States. To no surprise, we find that less developed countries would have gained most from the hypothetical statehood in terms of improved long-run growth. Specifically, the counterfactual growth trajectory of Cuba and Philippines exhibits a sizeable gap compared to their actual performance where the magnitude of the gap tends to increase over time. For Cuba, our estimates indicate a slight improvement in the path of growth following the establishment of the US military government in the aftermath of the Spanish-American war when Spain ceded Cuba to the United States. After early 1940s, the two series diverge rapidly. By 2015, the synthetic Cuba as a hypothetical US states would be seven times richer than its real counterpart. For Philippines, our estimates uncover a similar pattern of the growth effect of statehood except that in the early years of US occupation, the real Philippines' growth path moves in tandem with its synthetic peer whereas the series tend to diverge markedly after 1940. This suggests that in the hypothetical event of the statehood, post-1940 institutional ruptures associated with Marcos dictatorship would most likely be avoided. By 2015, the difference between the synthetic Philippines as a US state, and the real Philippines is about a factor 10. The evidence for Cuba and the Philippines unequivocally suggests that the adoption of the US institutional framework in the hypothetical event of joining the United States would yield superior long-run economic effects compared to the set of institutions these countries developed by themselves.

We find substantially smaller, but still positive long-run effects for Puerto Rico and Greenland. For Puerto Rico, we uncover persistently slower growth in response to the failure to become a US state. By matching Puerto Rico's pre-1973 growth and development trends on the full set of US states, we find that a synthetic Puerto Rico as a US state would perform considerably better than the real Puerto Rico. The difference between synthetic Puerto Rico and its real version appears to be fairly stable over time. Our estimates imply that down to the present day, synthetic Puerto Rico as a US state would be 51 percent richer than the real non-state Puerto Rico. Our evidence for Greenland indicates a slightly different pattern of growth effects. We find that if Greenland joined the United States in 1979, its per capita income would have improved substantially with an important caveat. In the short run, the growth effect of hypothetically joining the United States appears to be large. In the long run, especially after mid-1990s, the real Greenland's growth performance tends to converge with its synthetic control group although the growth premium of US statehood is still substantial. In terms of magnitude, the gap between synthetic Greenland as a US state and its real version as a Danish territory is 32 percent, respectively. The decidedly lower growth premium for Greenland suggests something about the relative importance of the institutional channel. Since institutional quality in Denmark is at least as high as that in the USA (Kaufmann et. al. 2011; Fukuyama 2011), Greenland does not enjoy a boost in institutional quality upon accession. Hence, it is likely that the reason that the Greenlandish growth premium is so much lower than that of other candidate states is that it is driven by non-institutional channels alone.

FIGURE 6 [INSERT HERE]

### *3.3.3 Inference on the cost of non-statehood*

To assess whether the effects of the hypothetical admission to the US on long-run growth are statistically significant, we construct a series of placebo distributions to determine whether the estimated per capita GDP gap appears to be unusually large for the treated countries. In a similar vein as in the case of US states, we assign the admission to the US to the full set of donor states that did not undergo such an institutional shock as the five treated countries, and iteratively apply the synthetic control estimator to each unexposed state. We address the relative rarity of estimating an extremely large placebo by excluding those donor states with RMSE twice the size of the treated country's RMSE. If the effect of the potential statehood is unusually large for the treated country relative to the placebo gaps of the unexposed states, our interpretation is that this provides evidence of the significant growth impact of adopting the US institutional framework in the event of the statehood.

Figure 7 presents the per capita GDP placebo gap against the estimated gap for each treated country alongside the corresponding randomization-based p-values for the full post-treatment period. The estimated in-space placebo gaps confirm our theoretical expectations. The estimated per capita GDP gap is particularly large and highly unusual for Cuba and the Philippines. The corresponding gap for Puerto Rico and Greenland is slightly narrower though still unusually large compared to the placebo gaps for US states. The left column exhibits the randomization-based p-values indicating whether the growth effect of hypothetically joining the United States is obtained by chance. For Cuba and the Philippines, the effect of US military government appears to be somewhat strong in the initial years while briefly disappearing afterwards. After the $40^{\text{th}}$ post-treatment year, the effect of statehood gains both in terms of strength and significance. In particular, the estimated potential statehood-related GDP per capita gap is statistically significant within 5 percent threshold. In the last year of the post-treatment period, the simulated p-value = 0.000 in both cases, respectively. The randomization-based p-values for Puerto Rico readily suggest that the implied per capita GDP gap between Puerto Rico and its state-level synthetic control group is statistically significant at 5% throughout the entire post-1973 period, indicating the potential statehood as a source of improved growth. In a similar fashion, the simulated p-values on the estimated per capita GDP gap between Greenland and its state-level synthetic control group are consistently low and within the 5 percent bound. A noteworthy feature of Greenland's distribution of p-values is the consistency of low p-values in spite of the narrowing per capita GDP gap between Greenland and its synthetic control group in the post-1990 period. Taken together, our evidence provides estimates indicating the hypothetical admission of countries with relatively lower institutional quality to the US as a source of discernable improvement in long-run growth.

FIGURE 7 [INSERT HERE]

Table 7 reports the parametric difference-in-differences (DD) test of the long-run growth impact by assessing the gaps in per capita GDP for each treated country and full set of placebos against the country-level post-treatment indicator for each affected country while controlling for country-level heterogeneity bias and common unobserved time-varying shocks. The Dube-Zipperer percentile ranks of the estimated per capita GDP gap (Panel A) indicate that the incidence of placebo gaps as large as the ones of the treated countries is quite rare. In particular, the placebo gaps of Cuba and the Philippines are ranked in the upper percentile. For Puerto Rico and Greenland, they are ranked in the $2^{nd}$ percentile and $4^{th}$ percentile of the placebo distribution. The DD post-statehood gap/placebo coefficients are very large and significant at 0.1 percent for Cuba and the Philippines, but are considerably smaller for Puerto Rico and Greenland. In the latter cases, however, the estimated post-statehood DD coefficient is statistically significant at 1 percent. This implies that the estimated per capita GDP gap is specific to these countries, and does not seem to be reflective of the common trend indicated by the state-level placebos. The fixed-effects, DD-estimated long-run growth impacts of hypothetical statehood are summarized further in Figure 8.

TABLE 7 [INSERT HERE]
FIGURE 8 [INSERT HERE]

## 4 What Drives the Performance Premium?

American Exceptionalism not only suggests that there is a unique bonus of 'being the US', but also that this bonus has its roots in the unique American political institutions. As argued above, the decidedly lower growth premium of Danish Greenland compared to the other candidate states possibly reflects this. An alternative explanation for the growth success of the United States since the 19th century is the large domestic market (Alston and Smith 2019; Fogel 1962). The economic integration of the continent spurred by the expansion of the railroad network throughout the 19th century gave firms access to large markets, triggered human capital mobility and stimulated continental knowledge flows. In this account, the main effect of US membership does not arise from membership itself but from access to the continental transportation networks.

We check this possibility by re-running our analyses for the Mexican Cession states, replacing the date of entry with a proxy for the date of economic integration. Our proxy variable for the access to internal market is the date of first transcontinental railway connection that linked the ceded states to the major railway hubs in the United States. This implies that we consider the year 1869 to designate the date of access to internal market for the states where the First Transcontinental Railroad (i.e. Pacific Railroad) was built, namely, California, Colorado, Nevada, Utah and Wyoming. In a similar way, we consider the year 1878 as a proxy date of access to internal market for Arizona, New Mexico and Texas since Southern Pacific Railroad connected these three states with the rest of the United States.

We assess the contribution of internal market and institutional quality to the improved growth in three distinctive steps. In the first step, we match the treated states from the Mexican cession on the full set of pre-cession per capita income dynamics and geographic characteristics. In the second step, we apply the synthetic control estimator to each ceded state using the date of access to transcontinental railway network as the treatment year to construct the counterfactual scenario. In the third step, we compare the treatment effects of statehood and of the internal market in order to ascertain the effect balance. Notice that for Arizona and New Mexico, access to the internal market precedes the official statehood status, which was attained in 1912. In spite of the overlap, we are able to compare both treatment effects, and determine how much of the post-statehood the growth gap is attributable to internal market access.

### *4.1 Results*

Table 8 compares summarizes the effects of internal market access on long-run growth. To compute a plausible treatment effect of internal market access on growth, we exclude poorly fitting placebos by splitting off those countries where the pre-access RMSPE is twice the size of the original RMSPE or greater to ensure that the relative rarity of obtaining an extreme effect does not contaminate the underlying treatment effect. The estimated per capita income gap between the ceded states and their country-level synthetic control group is large and strongly positive across the full set of specifications. The highest estimated gap is prevalent for Texas, New Mexico and Arizona whereas Colorado, Nevada and California portray a gap that is considerably lower but still positive. The treatment effect of internal market access does not appear to be as powerful as the overall effect of US membership in terms of statistical significance. For instance, the growth effect emanating from internal market access only comes close to conventional 5 percent levels of significance for New Mexico and Texas. For Arizona and Wyoming, the respective p-values are 0.087 and 0.086 whereas for California, the p-value associated with the internal market effect size is 0.109. For Nevada, the effect of internal market is statistically insignificant even when the threshold is pushed up to 15 percent level.

With this caveat, we compare the estimated effects of internal market access with the overall long-run growth effects of statehood by looking at the ratio of both effects in Figure 9. The evidence is particularly telling and suggests that the ratio varies substantially across the ceded states. For New Mexico, we find that the effect of internal market access is crucial in accounting for the long-run growth bonus of joining the United States. The internal market effect exceeds the overall statehood effect by a factor of 1.7. For Wyoming and Texas, the estimated internal market/statehood effects ratio is almost evenly split, which is not surprising given the close temporal proximity of both treatments. For other ceded states we find that the size of the statehood effect exceeds the size of the internal market effect. For Nevada, the internal market effect represents about 79 percent of the size of the overall statehood effect. The corresponding ratios for Utah, Arizona and Colorado are 0.65, 0.61 and 0.56. The ratio for California appears to be the lowest among all ceded states, and implies that internal market access accounts for about 9 percent of the statehood-related growth premium. The full distribution of p-values on whether the transmission effect of internal market access is driven by chance alone is presented in Figure 10. We conclude that while there is tentative support for sizable domestic market access effects, the growth premium not driven by this channel is still substantial.

TABLE 8 [INSERT HERE]

FIGURES 9 AND 10 [INSERT HERE]

*4.2 Statehood and the institutional quality premium*

If not market access (alone), what are the drivers of the growth bonus of statehood? In the following, we consider to what extent the performance premia we observed are caused by the uniquely superior institutions ascribed to America. To this end, we first assess the institutional quality in the US, comparing it to the institutional quality predicted for each of its states individually on basis of their characteristics.

Our strategy is as follows. We start out by collecting data on the several existing indices of institutional quality from the Worldwide Governance Indicators (Kaufmann et. al. 2011). We extract the first principal component from WGI indices in order to get a synthetic measure of institutional quality with the most powerful common variation behind the indicators. In the second step, the first principal component of the WGI indicators is regressed on the full set of covariates reported in Table 1. We then calculate predicted institutional quality values for individual US states as if they were independent countries. Finally, we calculate the gap between overall US institutional quality and the predicted institutional quality of individual states. Our conjecture is that the states with the lowest residuals would have developed institutional quality similar to the US even as non-members. In contrast, states exhibiting large gaps would have ended up with a substantially different institutional quality on their own than as part of the US.

Figure 11 reports the institutional quality residuals across the states. The lowest residuals are to be found in the Northeast (New Hampshire, New York, Massachusetts, Connecticut, Rhode Island). This suggests that the overall institutional quality of the US matches the one predicted for New England states. Perhaps not surprisingly, the highest residuals are found in Arizona, Texas, Florida, New Mexico and Utah—the Mexican Cession states. Intuitively, this result makes historical sense. The new nation and its unique political institutions largely originated in the Northeastern states. From there on, the institutional framework of the US was transmitted all over the continent. The end result is that the Mexican Cession states joining the US entered a political unit in which institutions were largely determined by Northeastern states already. The quality of those institutions was not hugely exceptional for the states in which they originated. However, when rolled out over territories that would otherwise have been expected to develop decidedly lower quality institutions, this process gave rise to a substantial institutional quality bonus for most of the country. By joining the United States, Southern and Southwestern states imported the institutional quality rooted in New England, which they would otherwise probably never have developed endogenously. American institutional exceptionalism thus seems to have its roots in the particular pattern of territorial expansion of the federation.

FIGURE 11 [INSERT HERE]

### *4.3 The Impact of Institutional Quality Premium*

Second, we consider to what extent this institutional premium is driving the economic performance premium. To this end, we first calculate the hypothetical economic performance premia for all countries in the world had they joined the USA in 1900. We do so following the same approach as we used for the potential candidate states in Section II (i.e. The Philippines, Cuba, etc), using US states as donors.

Second, we regress the resulting performance gap on the difference in institutional quality between the US and the respective country. Institutional quality data comes from Coppedge et al. (2019) in the form of the V-DEM country-indices of electoral democracy (i.e. polyarchy) and liberal democracy. The resulting estimates represent the extent to which the performance gaps between the within- and outside US scenarios for all countries are explained by the institutional quality gap between these countries and the

US. This allows us to infer the relative share of the statehood-induced growth premium that emanates from improved institutional quality.

Results are presented in Table 9. The key post-treatment variables of interest are the two interaction terms between V-DEM indices of electoral and liberal democracy and post-1900 temporal variable. Accross specifications, we control for unobserved country-level heterogeneity bias and common time-varying shocks perceptible across all states and countries. Columns (1) through (3) report the estimated DD coefficients of interest for the effect of electoral democracy. The evidence suggests that post-1900 per capita GDP gap between the quasi-treated countries and their state-level synthetic control groups tends to increase significantly over time. The coefficient on the interaction term between the level of electoral democracy and post-1900 temporal indicator is both large, negative and statistically significant at 1 percent, respective. The estimated DD coefficient on the interaction term implies that improving the level of electoral democracy of non-US states to the US level reduces the per capita GDP gap between real and counterfactual scenarios by between 0.5 and 0.6 basis points. In other words, adoption of the electoral institutions of the US level accounts for almost half of the GDP/capita premium of statehood. Columns (4) through (6) report the corresponding estimated DD coefficients of interest for the effect of liberal democracy. The estimated DD coefficient on the interaction term between the index of liberal democracy and post-1900 indicator is even larger, and in the range between -0.6 and -0.7. It is statistically significant at 1%. This implies that the impact of levelling the liberal democracy in the quasi-treated countries to the US threshold accounts for a large part of the GDP gap between real and counterfactual statehood scenarios.

On balance, our estimates indicate that the joint temporal and spatial variation in the level of electoral democracy can explain up to 41 percent of the statehood-induced growth premium. The corresponding variation in the level of liberal democracy accounts for up to 56 percent of the long-run growth benefits stemming from improved institutional quality upon the admission to the United States. The institutional quality bonus received by joining the US apparently drives a large part of the performance boost. This lends support to the second leg of the American Exceptionalism thesis, which is that it is America's political institutions that gave the US its unique advantage.

TABLE 9 [INSERT HERE]

## 5 Conclusion

This paper has quantified the economic benefits of being part of the United States. Using a dynamic synthetic control set-up to construct counterfactuals, we are able to show that US membership for the states entering upon the Mexican Cession came with a boost in GDP/capita of between 170-350 percent in the long run. Following the same method, we show that gains from US statehood for territories such as Cuba and The Philippines after the Spanish-American War would have been similar if not higher. The same applies to a lesser extent for Puerto Rico and Greenland.

Next, we show that the accession premium in terms of GDP/capita is to a large extent driven by the boost in institutional quality. As the political institutions of the United States originally developed in the Northeast of the country, the characteristics of these states left their imprint on the institutional quality of the federation. When the country expanded South- and Westwards and the institutions born in New England were rolled out over the continent, they increasingly covered territories that based on their own characteristics would not have been expected to develop high-quality political institutions. As a result, new states entering the US typically entered an institutional setting superior to their own. We show that this institutional boost is responsible for about 40% to 60% of the gap between performance within and outside the US.

Our results come with various caveats. First of all, our calculations of the hypothetical benefits of joining the US for countries outside the union ignore the potentially quite sizable transition costs. It is not clear, for example, that the Philippines just by virtue of statehood would immediately enjoy *de facto* US level institutional quality. More likely, this will take a period of potentially painful institutional reform and adaptation. The initially lower performance premia we found for the Mexican Cession states may reflect these transition costs. Since in their case we compare the real trajectory which includes transition costs to a counterfactual without adaptation, we would expect their income bonus to be smaller in the short-run. Second, although we are able to match pre-treatment real and synthetic control trajectories rather closely, there obviously may be important characteristics of the countries we consider that we failed to include. For instance, the presence of a predominantly indigenous population with a strong degree of cultural and institutional autonomy in the case of Greenland may make institutional quality effects less profound. Similarly, potential feelings of local resentment against domination by a larger political entity may easily erode any benefits of US statehood for some potential states. Finally, a related point is that US accession may be endogenous. If Texas and California joined the United States and Cuba and the Philippines did not, this may reflect the better contemporary growth prospects of the former due to some variable we fail to observe. If so, such omitted variable bias inflates our results. However, we note that our pre-treatment real and synthetic control trajectories match each other quite closely. Since we would expect any omitted variables driving post-accession growth premia to make their influence felt before accession already, we consider it unlikely that omitted variable bias is very important.

Our results thus provide a solid empirical foundation for the idea of American Exceptionalism, stating that the United States in the 19th and early 20th century enjoyed extraordinary prosperity due to its superior political institutions. At the same time, our analysis suggests that the key to this exceptionalism is the geographical expansion of the new nation over the course of the 19th century. Institutional quality was not that exceptional by the standards of the Northeastern states most responsible for creating the institutional framework. It was only when these institutions were rolled out over the rest of the continent that they entered territories where they can be considered exceptional.

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**Table 1:** Descriptive statistics

| | Mean | | StD | | Min | | Max | |
|---|---|---|---|---|---|---|---|---|
| | US Sample | World Sample | US Sample | World Sample | US Sample | World Sample | US Sample | World Sample |
| Panel A: Dependent Variable | | | | | | | | |
| log GDP per capita | 8.64 | 7.56 | 1.27 | 1.12 | 5.51 | 5.55 | 11.12 | 10.46 |
| Panel B: Past GDP Dynamics Covariates | | | | | | | | |
| Log GDP per capita in 1840 | 6.89 | 6.44 | 0.381 | 0.44 | 6.15 | 5.55 | 8.00 | 7.74 |
| Log GDP per capita(t-1) | 8.63 | 7.55 | 1.26 | 1.11 | 5.51 | 5.55 | 11.11 | 10.43 |
| Log GDP per capita(t-2) | 8.62 | 7.54 | 1.26 | 1.11 | 5.51 | 5.56 | 11.07 | 10.42 |
| Panel C: Physical Geography Covariates | | | | | | | | |
| Temperature | 52.48 | 61.93 | 7.87 | 11.40 | 40.4 | 42.6 | 70.7 | 83.5 |
| Rainfall | 37.52 | 37.51 | 14.40 | 24.61 | 9.5 | 0.09 | 63.7 | 109.57 |
| Sunshine | 2725 | 2330 | 347.88 | 552.13 | 2112 | 1328 | 3806 | 3451 |
| Humidity | 52.38 | 69.56 | 8.33 | 10.04 | 25 | 42 | 64 | 83 |
| Latitude | 39.07 | 26.28 | 5.36 | 27.41 | 19.89 | -41.80 | 47.75 | 64.48 |
| Longitude | -91.51 | 24.16 | 18.28 | 65.23 | -155.66 | -102.53 | -45.6 | 171.47 |
| Snowfall | 20.59 | 5.93 | 17.62 | 11.09 | 0 | 0 | 60.8 | 47.83 |
| Desert | 0.22 | 0.24 | 0.41 | 0.43 | 0 | 0 | 1 | 1 |
| Size of the area | 68802.91 | 447550 | 73500.37 | 880582 | 68.34 | 1063 | 473162 | 3900000 |
| Island | 0.02 | 0.15 | 0.14 | 0.36 | 0 | 0 | 1 | 1 |
| Landlocked | 0.42 | 0.12 | 0.49 | 0.32 | 0 | 0 | 1 | 1 |
| Panel C: Legal History Covariates | | | | | | | | |
| British common law | | 0.25 | | 0.43 | 0 | 0 | 1 | 1 |
| Civil law | | 0.75 | | 0.43 | 0 | 0 | 1 | 1 |

**Table 2**: Covariate Balance

| | Arizona (1912) | | California | | Colorado | | Nevada | | New Mexico | | Texas | | Utah | | Wyoming | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic |
| Log GDP per capita in 1840 | 6.45 | 6.41 | 6.43 | 6.44 | 6.82 | 6.83 | 6.42 | 6.07 | 6.45 | 6.56 | 6.46 | 6.47 | 6.76 | 6.77 | 6.89 | 6.89 |
| Log GDP per capita(t-1) | 6.45 | 6.45 | 6.53 | 6.53 | 7.05 | 7.06 | 6.79 | 6.79 | 7.05 | 7.05 | 6.48 | 6.49 | 6.78 | 6.78 | 7.04 | 7.04 |
| Log GDP per capita(t-2) | 6.45 | 6.45 | 6.53 | 6.52 | 7.04 | 7.05 | 6.75 | 6.76 | 7.04 | 7.04 | 6.48 | 6.49 | 6.78 | 6.78 | 7.03 | 7.03 |
| Log GDP per capita in pre-admission year | 6.53 | 6.50 | 6.66 | 6.63 | 7.29 | 7.29 | 7.72 | 7.40 | 7.02 | 7.03 | 6.48 | 6.49 | 6.77 | 6.80 | 7.22 | 7.22 |
| Temperature | 60.3 | 70.9 | 59.4 | 55.4 | 45.1 | 62.28 | 49.9 | 61.98 | 53.4 | 57.61 | 64.8 | 65.5 | 48.6 | 67.3 | 42 | 58.04 |
| Rainfall | 13.6 | 8.73 | 22.2 | 30.1 | 15.9 | 22.5 | 9.5 | 43.58 | 14.6 | 35.69 | 28.9 | 18.3 | 12.2 | 19.9 | 12.9 | 25.31 |
| Sunshine | | | 3055 | 2375 | 3204 | 2510 | 3646 | 2300 | 3415 | 2375 | 2850 | 2863 | 3029 | 2597 | 3073 | 2394 |
| Humidity | 25 | 59.94 | 62 | 73.6 | 35 | 64.81 | 32 | 71.6 | 29 | 77.15 | 49 | 65.4 | 43 | 66 | 43 | 67.4 |
| Latitude | 34.04 | 28.18 | 36.77 | 26.04 | 39.55 | 18.76 | 38.8 | -27.71 | 34.97 | 41.48 | 31.96 | 30.98 | 39.32 | 34.28 | 43.07 | 29.11 |
| Longitude | -111.09 | 6.40 | -119.31 | -14.42 | -105.78 | 53.87 | -116.41 | 139.98 | -105.63 | -78.72 | -99.9 | -22.65 | -111.89 | -18.03 | -107.29 | 0.93 |
| Desert | 1 | 0.69 | 1 | 0.28 | 1 | 0.54 | 1 | 0.30 | 1 | 0 | 1 | 0.55 | 1 | 0.28 | 1 | 0.43 |
| Island | 0 | 0.18 | 0 | 0.14 | 0 | 0.29 | 0 | 0.85 | 0 | 0.26 | 0 | 0.14 | 0 | 0.33 | 0 | 0.12 |
| Landlocked | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 1 | 0.01 |
| Log land area | 11.63 | 11.86 | 13.06 | 13.73 | 11.55 | 12.61 | 11.61 | 12.43 | 11.70 | 13.80 | 12.50 | 12.90 | 11.34 | 10.82 | 11.49 | 13.20 |
| Common Law | 1 | 0.99 | 1 | 0.95 | 1 | 1 | 1 | 0.89 | 1 | 0.75 | 1 | 0.97 | 1 | 0.96 | 1 | 0.99 |
| Civil Law | 0 | 0.01 | 0 | 0.05 | 1 | 0 | 0 | 0.11 | 0 | 0.25 | 0 | 0.03 | 0 | 0.04 | 0 | 0.01 |

**Figure 1**: Effects of Statehood on Long-Run Growth

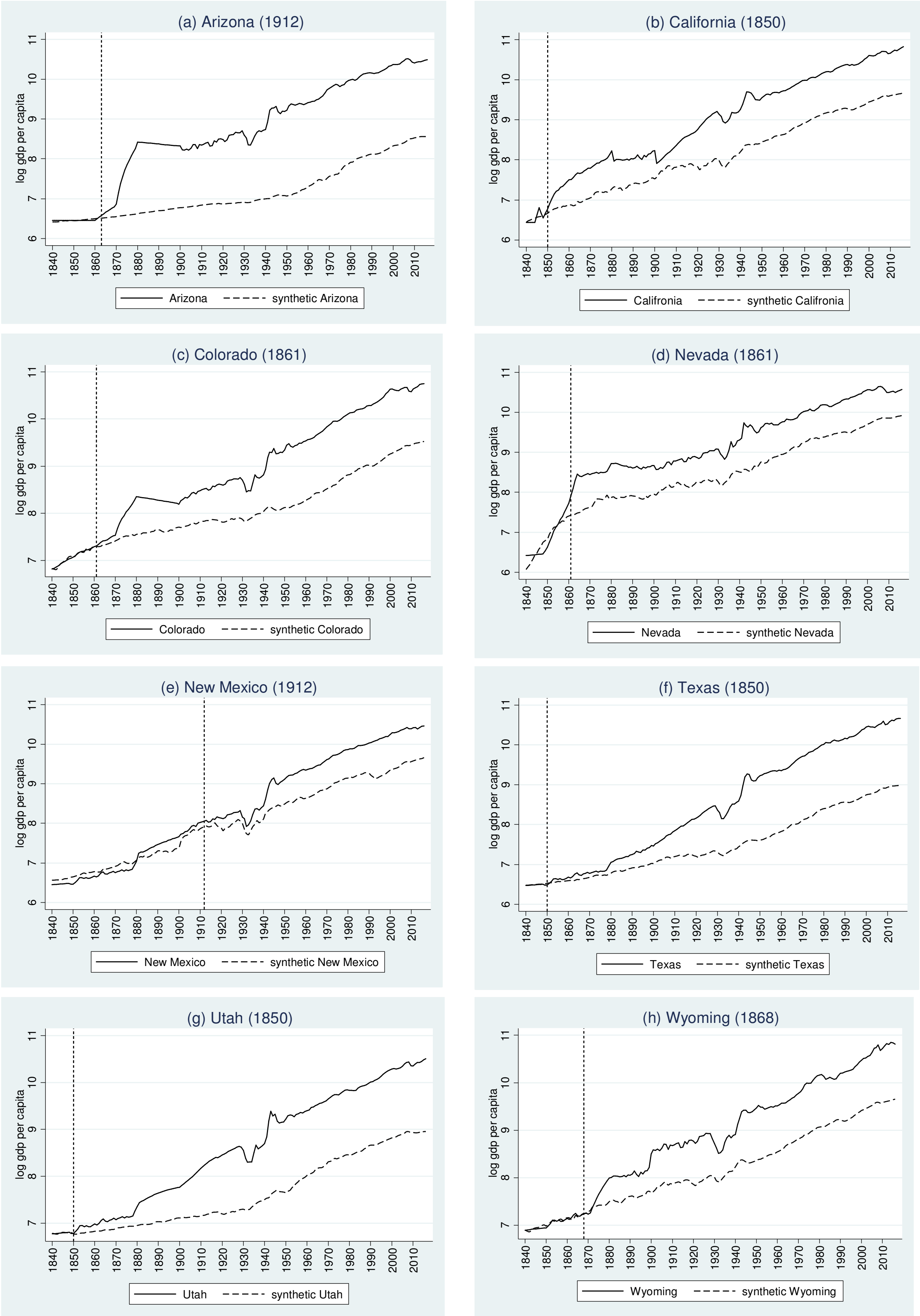

**Table 3**: Composition of Synthetic Control Groups

| | Arizona (1912) | California (1850) | Colorado (1861) | Nevada (1861) | New Mexico (1912) | Texas (1850) | Utah (1850) | Wyoming (1868) |
|---|---|---|---|---|---|---|---|---|
| Root MSPE | 0.024 | 0.097 | 0.022 | 0.195 | 0.163 | 0.011 | 0.012 | 0.034 |
| Algeria | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Argentina | 0 | 0 | 0 | 0 | 0 | 0.03 | 0 | 0 |
| Australia | 0 | 0 | 0.19 | 0.28 | 0 | 0 | 0 | 0.12 |
| Austria | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Belgium | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Bolivia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Brazil | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Burma | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Canada | 0 | 0.49 | 0 | 0 | 0.64 | 0.27 | 0 | 0.23 |
| Chile | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| China | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Colombia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Cuba | 0 | 0 | 0 | 0 | 0.25 | 0 | 0 | 0 |
| Czechia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Denmark | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Egypt | 0.70 | 0.10 | 0.35 | 0 | 0 | 0.43 | 0.29 | 0.16 |
| Finland | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| France | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Germany | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Greece | 0.01 | 0 | 0 | 0.12 | 0 | 0 | 0 | 0 |
| Hong Kong | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Hungary | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| India | 0 | 0 | 0 | 0 | 0 | 0.04 | 0 | 0 |
| Indonesia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Iran | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Iraq | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Ireland | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Italy | 0 | 0 | 0 | 0 | 0 | 0 | 0.01 | 0 |
| Jamaica | 0.18 | 0 | 0 | 0 | 0 | 0.14 | 0.34 | 0 |
| Japan | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Jordan | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Lebanon | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Malaysia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Mexico | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Morocco | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Nepal | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Netherlands | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| New Zealand | 0 | 0.15 | 0.11 | 0.58 | 0.01 | 0 | 0 | 0 |
| Norway | 0 | 0.05 | 0 | 0 | 0 | 0 | 0.03 | 0 |
| Philippines | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Poland | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Portugal | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Slovenia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| South Africa | 0 | 0.19 | 0 | 0.03 | 0.11 | 0.05 | 0 | 0.16 |
| South Korea | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Spain | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Sri Lanka | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Sweden | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Switzerland | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Syria | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Thailand | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Tunisia | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Turkey | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| United Kingdom | 0.11 | 0.03 | 0.35 | 0 | 0 | 0.04 | 0.34 | 0.33 |
| Uruguay | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Venezuela | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Vietnam | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

**Figure 2**: US Statehood Placebo Gaps

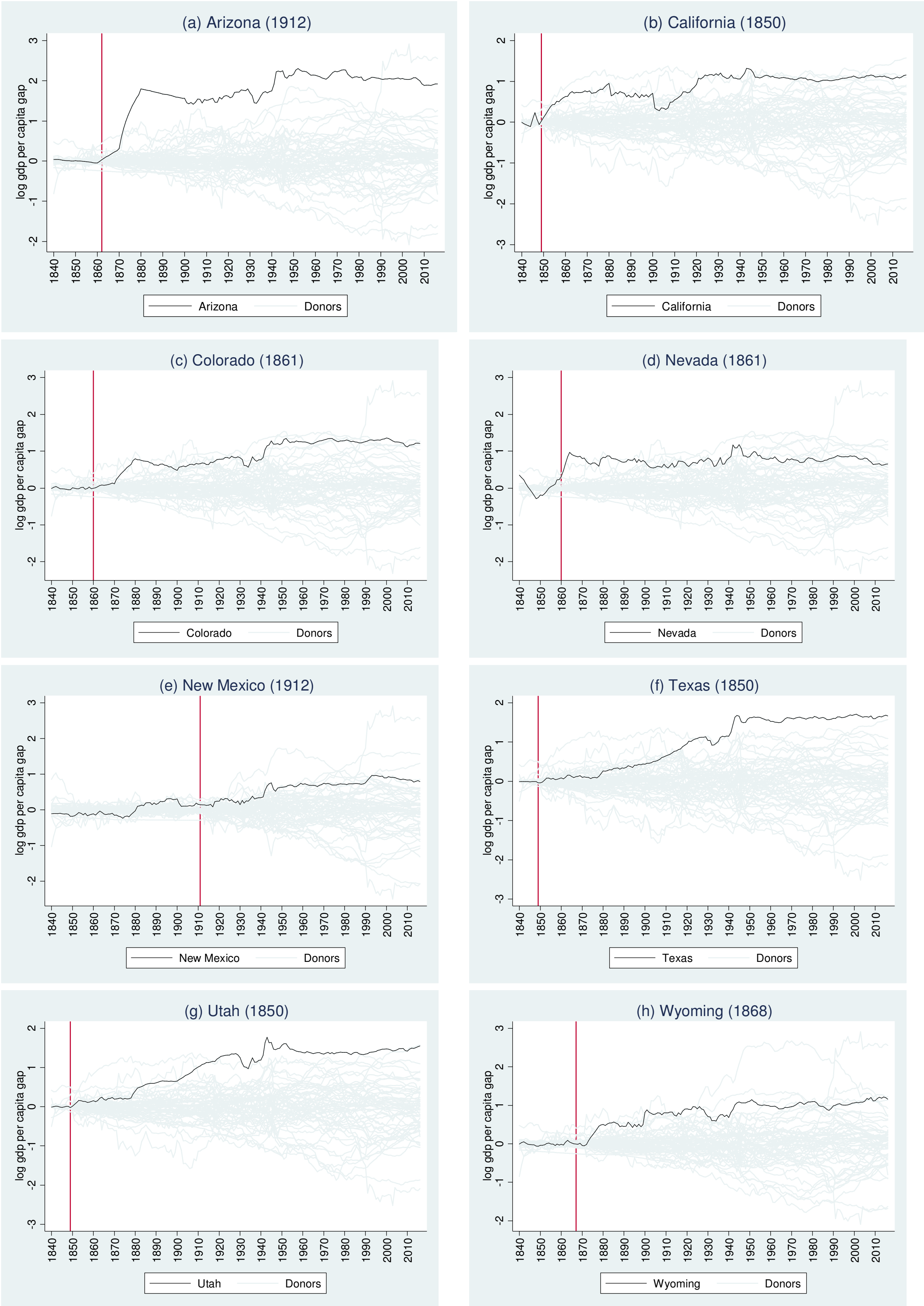

**Figure 3**: Randomization-Based Inference on Statehood Effects

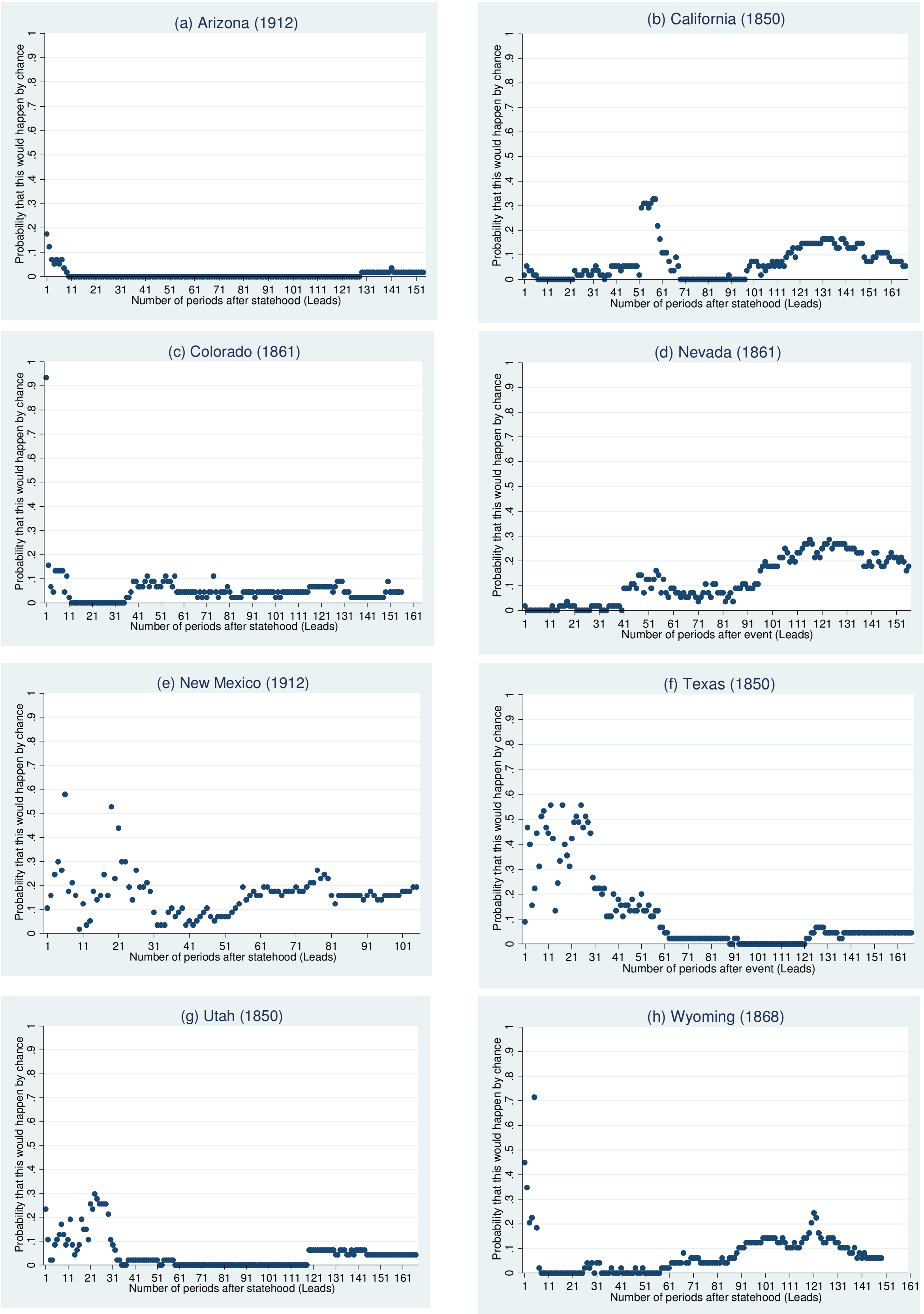

**Table 4**: Parametric State-Level Difference-in-Differences Inference on the Per Capita GDP Gap

| | Arizona | California | Colorado | Nevada | New Mexico | Texas | Utah | Wyoming |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A: synthetic control estimates with Dube-Zipperer percentile ranks | | | | | | | | |
| Synthetic Post-Statehood DD gap coefficient | 1.922 | 1.142 | 1.214 | 0.668 | 0.793 | 1.672 | 1.539 | 1.159 |
| | (0.017) | (0.054) | (0.044) | (0.178) | (0.124) | (0.044) | (0.040) | (0.061) |
| Panel B: difference-in-differences estimate with Huber-White standard errors | | | | | | | | |
| Post-Statehood DD gap coefficient | 1.065*** | .910*** | .910*** | .714*** | .607*** | 1.077*** | 1.052*** | .831*** |
| | (.057) | (.047) | (.048) | (.048) | (.051) | (.045) | (.047) | (.052) |
| Panel C: difference-in-differences estimate with Cameron-Gelbach-Miller multi-way clustered standard errors | | | | | | | | |
| Post-Statehood DD gap coefficient | 1.065*** | .885*** | .910*** | .714*** | .609*** | 1.077*** | 1.052*** | .831*** |
| | (.055) | (.002) | (.047) | (.047) | (.022) | (.047) | (.046) | (.051) |
| Country/State-Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| Time-Fixed Effects | YES | YES | YES | YES | YES | YES | YES | YES |
| # Obs | 10,266 | 10,266 | 10,266 | 10,266 | 10,208 | 10,266 | 10,266 | 10,208 |
| R2 | 0.15 | 0.05 | 0.05 | 0.03 | 0.02 | 0.06 | 0.06 | 0.04 |

Notes: the table presents difference-in-differences coefficient on post-statehood per capita GDP gap using the full set of country-level placebo gaps as a control sample. Standard errors are adjusted for arbitrary heteroscedasticity and serially correlated stochastic disturbances, and denoted in the parentheses. Asterisks denote statistically significant difference-in-differences gap coefficients at 10% (*), 5% (**), and 1% (***), respectively.

**Figure 4**: Overview of Growth Effect of Statehood

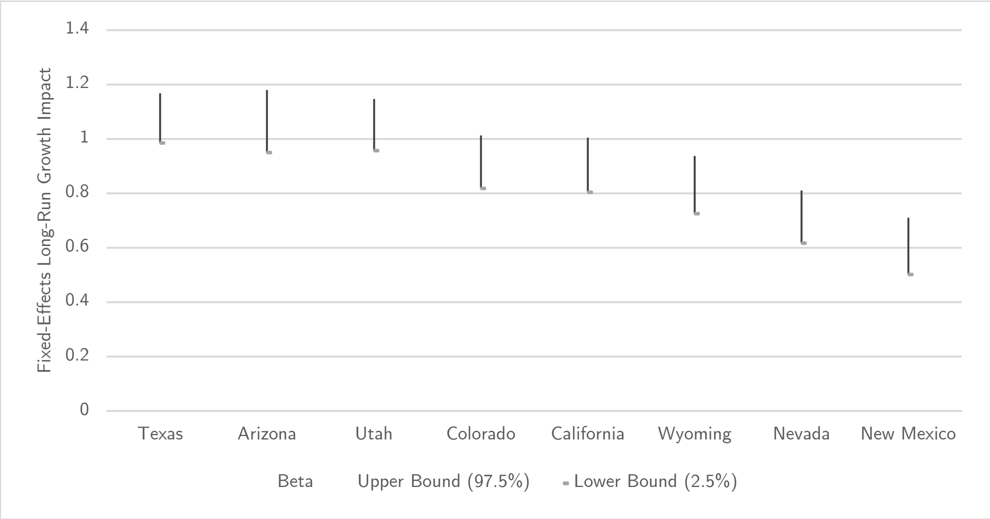

**Table 5**: Effects of Potential Statehood on Long-Run Growth – Covariate Balance

| | Cuba (1901) | | Philippines (1900) | | Puerto Rico (1973) | | Greenland (1970) | |
|---|---|---|---|---|---|---|---|---|
| | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic | Treated | Synthetic |
| Log GDP per capita in the initial year | 6.18 | 6.28 | 6.12 | 6.21 | 8.15 | 8.16 | 9.19 | 9.20 |
| Log GDP per capita(t-1) | 6.38 | 6.39 | 6.91 | 7.55 | 8.69 | 8.68 | 9.43 | 9.43 |
| Log GDP per capita(t-2) | 6.76 | 6.72 | 6.90 | 7.42 | 8.66 | 8.66 | 9.40 | 9.40 |
| Log GDP per capita in pre-admission year | 77 | 60 | 6.21 | 6.09 | 9.28 | 9.22 | 9.70 | 9.69 |
| Temperature | 46.8 | 44.3 | 83.1 | 68.6 | 81 | 70.28 | 25 | 59.09 |
| Rainfall | 46.8 | 44.3 | 82.8 | 62.6 | 56.4 | 59.90 | 30 | 52.60 |
| Sunshine | 2831 | 3031 | 2105 | 3101.8 | 2963 | 3071 | 1369 | 2732 |
| Humidity | 76 | 55.5 | 74 | 55.68 | 74 | 56.4 | 81 | 56.3 |
| Latitude | 23.13 | 30.29 | 11.73 | 21.54 | 18.4 | 23.09 | 64.17 | 29.75 |
| Longitude | -82.35 | -134.64 | 122.86 | -148.33 | -66.06 | -125.11 | -51.73 | -113.63 |
| Desert | 0 | 0.16 | 0 | 0 | 0 | 0 | 0 | 0 |
| Island | 1 | 0.56 | 1 | 0.89 | 1 | 0.59 | 1 | 0 |
| Landlocked | 0 | 0 | 0 | 0.11 | 0 | 0 | 0 | 0.53 |
| Log land area | 10.65 | 10.41 | 11.66 | 9.44 | 8.16 | 10.03 | 14.60 | 9.94 |

**Figure 6**: Effects of Hypothetical US Statehood on Long-Run Growth of Affected Countries

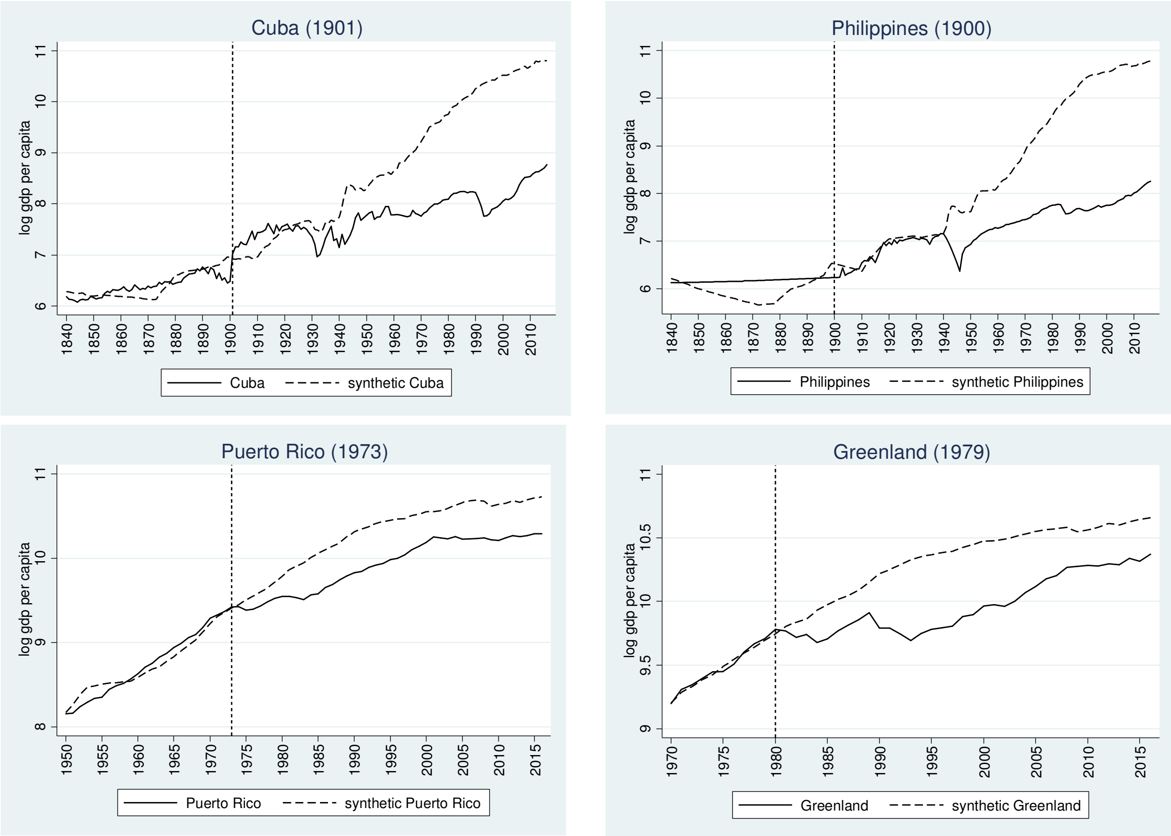

**Table 6**: Composition of Synthetic Control Groups – Cuba, Philippines etc.

| | Cuba (1901) | Philippines (1900) | Puerto Rico (1973) | Greenland (1979) |
|---|---|---|---|---|
| Root MSPE | 0.174 | 0.296 | 0.091 | 0.022 |
| Alabama | 0 | 0 | 0 | 0 |
| Arizona | 0 | 0 | 0 | 0 |
| Arkansas | 0 | 0 | 0 | 0 |
| California | 0.14 | 0 | 0 | 0 |
| Colorado | 0 | 0 | 0 | 0 |
| Connecitcut | 0 | 0 | 0 | 0 |
| Delaware | 0 | 0 | 0 | 0 |
| District of Columbia | 0 | 0 | 0 | 0 |
| Florida | 0 | 0 | 0.41 | 0 |
| Georgia | 0 | 0 | 0 | 0 |
| Hawaii | 0.56 | 0.89 | 0.59 | 0.53 |
| Idaho | 0 | 0 | 0 | 0 |
| Illinois | 0 | 0 | 0 | 0 |
| Indiana | 0 | 0 | 0 | 0 |
| Iowa | 0 | 0 | 0 | 0 |
| Kansas | 0 | 0 | 0 | 0 |
| Kentucky | 0 | 0 | 0 | 0 |
| Louisiana | 0 | 0 | 0 | 0 |
| Maine | 0 | 0 | 0 | 0 |
| Maryland | 0 | 0 | 0 | 0 |
| Massachusettes | 0 | 0 | 0 | 0 |
| Michigan | 0 | 0 | 0 | 0.20 |
| Minnesota | 0 | 0 | 0 | 0 |
| Mississippi | 0 | 0 | 0 | 0 |
| Missouri | 0 | 0 | 0 | 0 |
| Montana | 0 | 0 | 0 | 0 |
| Nebraska | 0 | 0 | 0 | 0 |
| Nevada | 0 | 0 | 0 | 0 |
| New Hampshire | 0 | 0 | 0 | 0 |
| New Jersey | 0 | 0 | 0 | 0 |
| New Mexico | 0 | 0 | 0 | 0 |
| New York | 0 | 0 | 0 | 0 |
| North Carolina | 0 | 0 | 0 | 0 |
| North Dakota | 0.31 | 0 | 0 | 0 |
| Ohio | 0 | 0 | 0 | 00 |
| Oklahoma | 0 | 0 | 0 | 0 |
| Oregon | 0 | 0 | 0 | 0 |
| Pennsylvania | 0 | 0 | 0 | 0 |
| Rhode Island | 0 | 0 | 0 | 0 |
| South Carolina | 0 | 0 | 0 | 0 |
| South Dakota | 0 | 0 | 0 | 0 |
| Tennessee | 0 | 0.11 | 0 | 0 |
| Texas | 0 | 0 | 0 | 0 |
| Utah | 0 | 0 | 0 | 00 |
| Vermont | 0 | 0 | 0 | 0 |
| Virginia | 0 | 0 | 0 | 0 |
| Washington | 0 | 0 | 0 | 0 |
| West Virginia | 0 | 0 | 0 | 0.27 |
| Wisconsin | 0 | 0 | 0 | 0 |
| Wyoming | 0 | 0 | 0 | 0 |

**Figure 7**: Placebo Gaps for Hypothetical Statehood

**Table 7**: Parametric State-Level Difference-in-Differences Inference on the Per Capita GDP Gap

| | Cuba | Philippines | Puerto Rico | Greenland |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Panel A: synthetic control estimates with Dube-Zipperer percentile ranks | | | | |
| Post-Statehood DD gap | -2.039 | -2.502 | -0.437 | -0.284 |
| coefficient | (0.000) | (0.000) | (0.021) | (0.039) |
| Panel B: difference-in-differences estimate with Huber-White standard errors | | | | |
| Post-Statehood DD gap | -1.082*** | -1.454*** | -.396*** | -.356*** |
| coefficient | (.020) | (.024) | (.024) | (.016) |
| Panel C: difference-in-differences estimate with Cameron-Gelbach-Miller multi-way clustered standard errors | | | | |
| Post-Statehood DD gap | -1.082*** | -1.454*** | -.396*** | -.356*** |
| coefficient | (.015) | (.021) | (.023) | (.016) |
| Country/State-Fixed Effects | YES | YES | YES | YES |
| Time-Fixed Effects | YES | YES | YES | YES |
| # Obs | 8,976 | 8,976 | 3,366 | 2,392 |
| R2 | 0.21 | 0.26 | 0.13 | 0.18 |

Notes: the table presents difference-in-differences coefficient on post-statehood per capita GDP gap using the full set of state-level placebo gaps as a control sample. Standard errors are adjusted for arbitrary heteroscedasticity and serially correlated stochastic disturbances, and denoted in the parentheses. Asterisks denote statistically significant difference-in-differences gap coefficients at 10% (*), 5% (**), and 1% (***), respectively.

**Figure 9**: Long-Run Impact of Not Joining the United States and Attempted Statehood

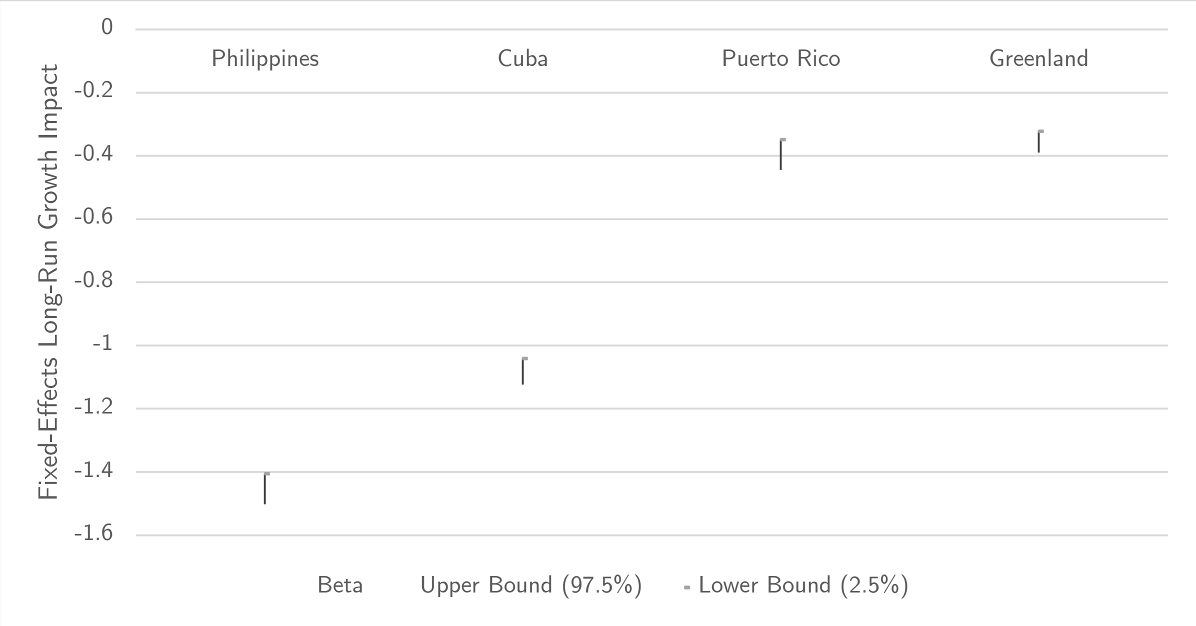

**Table 8**: Effect of Internal Market Access on Long-Run Growth

| | Arizona | California | Colorado | Nevada | New Mexico | Texas | Utah | Wyoming |
|---|---|---|---|---|---|---|---|---|
| Initial Year | 1840 | 1840 | 1840 | 1840 | 1840 | 1840 | 1840 | 1840 |
| Year of Internal Market Access | 1878 | 1869 | 1869 | 1869 | 1878 | 1878 | 1869 | 1869 |
| RMSPE | 0.300 | 0.138 | 0.070 | 0.387 | 0.035 | 0.035 | 0.034 | 0.033 |
| Donor Pool | 57 | 57 | 57 | 57 | 57 | 57 | 57 | 57 |
| Actual Donors | 3 | 5 | 4 | 2 | 7 | 8 | 6 | 6 |
| Main Donor (weight share) | South Africa (0.499) | Australia (0.508) | Australia (0.469) | New Zealand (0.593) | Egypt (0.594) | Egypt (0.506) | Canada (0.265) | United Kingdom |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Treatment Effect | 1.186 | 0.932 | 0.684 | 0.515 | 1.392 | 1.568 | 1.019 | 1.129 |
| (p-value) | (0.087) | (0.109) | (0.173) | (0.280) | (0.052) | (0.052) | (0.087) | (0.086) |
| # observations | 10,266 | 10,266 | 10,266 | 10,266 | 10,266 | 10,266 | 10,266 | 10,266 |

**Figure 9**: Statehood and Internal Market Effect

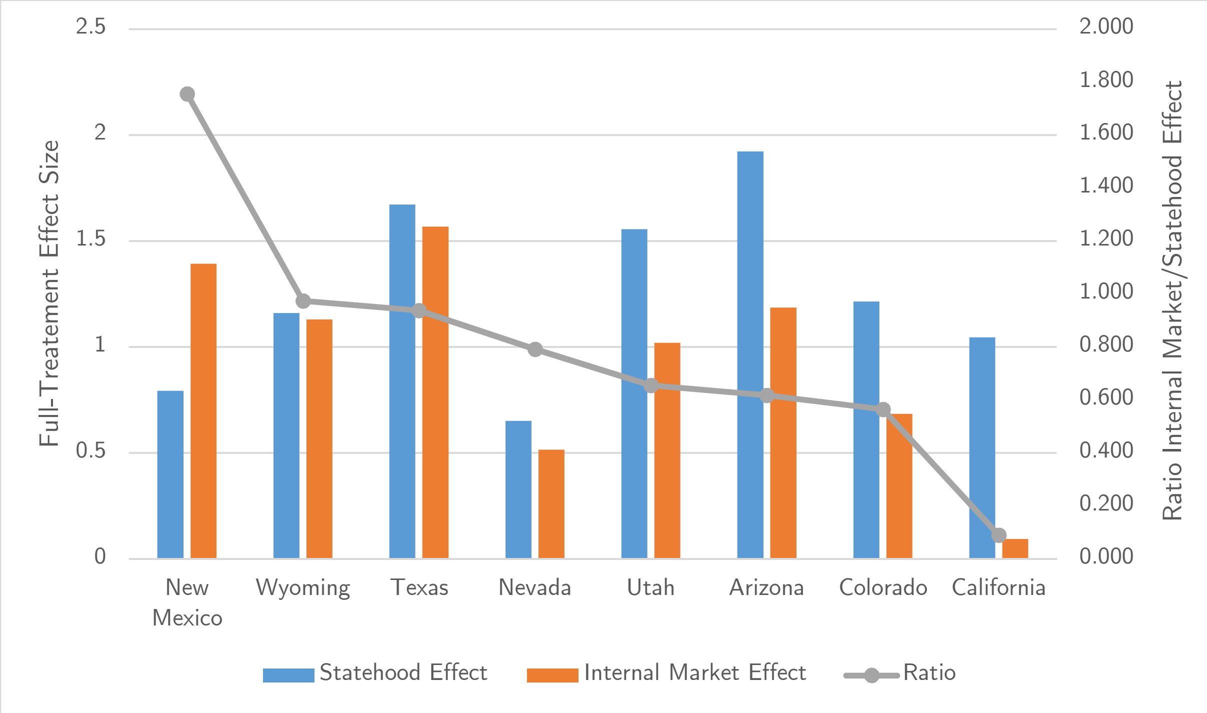

**Figure 10**: Randomization Inference on Indirect Effect of Internal Market Access on Long-Run Growth

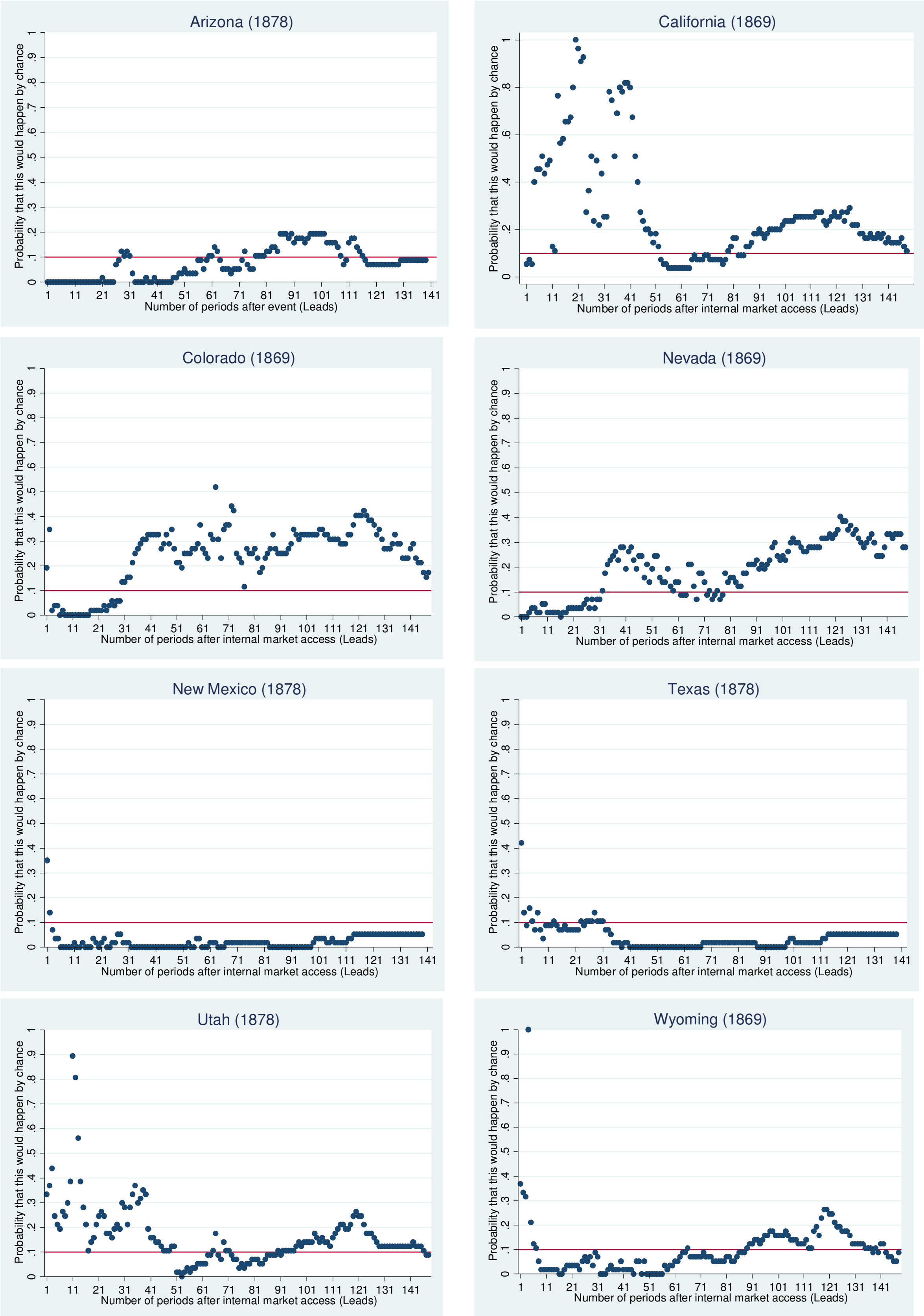

**Figure 11:** Institutional Quality Residuals Across US States

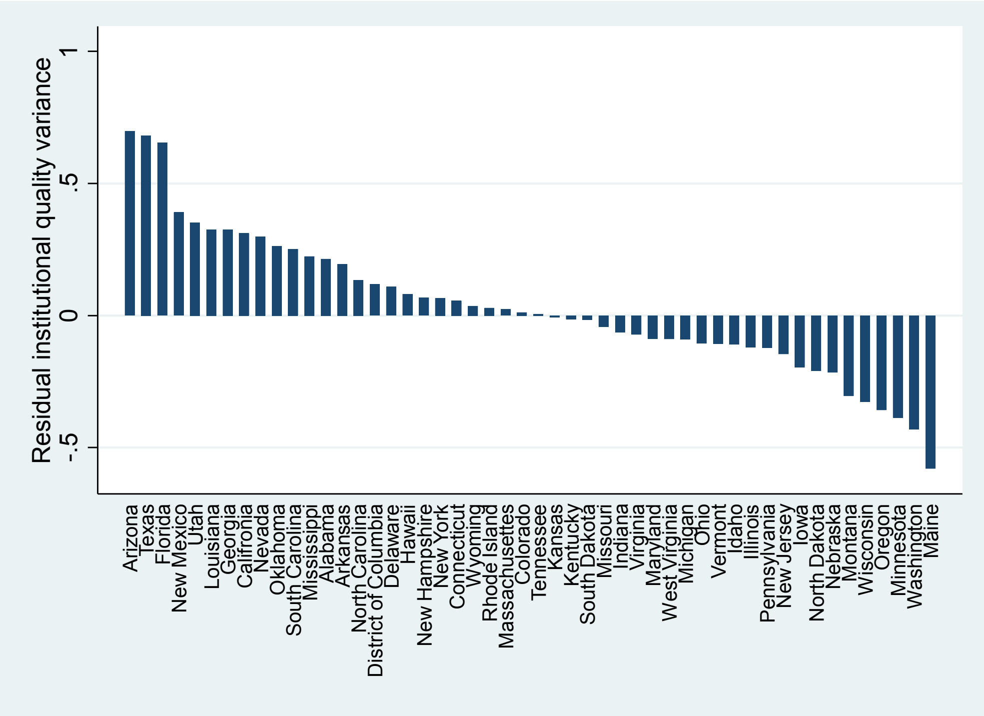

**Table 9**: The Contribution of Institutional Quality to Post-Statehood Growth

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Cameron-Gelbach-Miller country-year paired standard errors | | | Huber-White robust standard errors | | |
| Electoral Democracy | -.005 | -.008 | .689*** | | | |
| | (.018) | (.019) | (.149) | | | |
| Liberal Democracy | | | | -.008 | .634*** | .598*** |
| | | | | (.019) | (.155) | (.127) |
| Post-Quasi Statehood Indicator | 1.280*** | 1.301*** | 1.222*** | 1.301*** | 1.038*** | 2.040*** |
| | (.118) | (.113) | (.086) | (.113) | (.084) | (.143) |
| Electoral Democracy × Post-Statehood | -.535*** | -.604*** | -.564*** | | | |
| | (.112) | (.102) | (.092) | | | |
| Liberal Democracy × Post-Statehood | | | | -.604*** | -.687*** | -.698*** |
| | | | | (.102) | (.088) | (.091) |
| Country-Fixed Effects | NO | YES | YES | NO | YES | YES |
| (p-value) | | (0.000) | (0.000) | | (0.000) | (0.000) |
| Time-Fixed Effects | NO | NO | NO | NO | NO | YES |
| (p-value) | | | | | | (0.000) |
| # country clusters | 51 | 51 | 51 | 51 | 51 | 51 |
| # year clusters | 177 | | | 177 | | |
| # observations | 9,027 | 9,026 | 9,027 | 9,026 | 9,026 | 9,026 |
| R2 | 0.38 | 0.41 | 0.25 | 0.41 | 0.30 | 0.56 |

Notes: the table reports difference-in-differences estimated effect of the electoral and liberal democracy in the hypothetical event of joining of the United States. The dependent variable is the per capita GDP between the quasi-treated country and state-level weighted average of each country's synthetic control group. Standard errors are adjusted for arbitrary heteroscedasticity and serially correlated stochastic disturbances, and denoted in the parentheses. Asterisks denote statistically significant coefficients at 10% (*), 5% (**), and 1% (***), respectively.