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Favoritism by the Governing Elite

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Abstract

In this paper, we study the extent to which ministers engage in regional favoritism. We are the first to provide a comprehensive analysis of a larger set of the governing elite, not just focusing on the primary leader. We hand-collect birthplaces of this governing elite globally. Combining this information with extended night-time luminosity and novel population data over the period from 1992 to 2016, we utilize a staggered difference-in-differences estimator and find that birthplaces of ministers globally emit on average roughly 7% more nightlight. We do not find evidence that this is driven by, or induces migration to their home regions. The size of our data set lets us investigate heterogeneities along a number of dimensions: political power, ministerial portfolio, and the institutional setting.

Keywords: Favoritism, elite capture, spatiality, luminosity, population, democracy.

JEL codes: D72, H72, H77, R11.

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1 Introduction

The fundamental starting point of public choice theory is to caution of the government as a self-interested actor. One way in which this theoretical concept has been shown to manifest itself, is in political leaders favoring some regions in the allocation of public resources over others. Indeed, empirical studies have documented this phenomenon termed regional favoritism across the world, and in diverse institutional settings (Hodler and Raschky, 2014). One question that is under-investigated however, is how widespread this regional favoritism occurs at the level of governing elites right below the primary leader. We look to fill this gap by studying regional favoritism of ministers.

We approach this study with a number of research questions in mind: First, we want to understand if ministers engage in favoritism, and if so quantify the extent of it. While ministers might be less powerful than the primary leader of the country, they are at the same time under less public scrutiny. Therefore it is ex ante not clear what effect size to expect, and it will be informative to compare our empirical results to those for primary leaders of the prior literature.

A second set of questions we have, revolves around the determinants of favoritism at the minister level. Do factors such as a minister's portfolio and the prestige associated with it influence the effects we measure? Furthermore, we look to explore the mediating character of different institutional settings. While stronger democracies might restrain politicians ability to divert resources, they at the same time might provide stronger incentives, or even necessitate engaging in regional favoritism to secure electoral support.

To address these questions, we compile a worldwide sample of hand-collected and geo-referenced data on birthplaces of the governing elite. We combine data on recently published night lights, extending the possible scope of analysis up to 2021 (DMSP Extension Series), a new data set on population numbers at the pixel level (WorldPop, 2000-2020), and individual-level information on cabinet members (WhoGov) and their birthplaces. To the best of our knowledge, our sample is the largest data set hitherto used in the favoritism literature with regard to the time dimension (1992-2016) and with respect to information on birth places of the governing elite (around 9,000 unique cabinet members with birth place coordinates).

Our empirical strategy exploits the different timing of ministers coming into power, and the geographical spread of their birthplaces. We compare nightlight intensity and population numbers of small geographical units (0.5 x 0.5 degree pixels, where 0.5 degrees correspond to about 55km at the equator) before and after a minister comes into power, where those regions never being the home of a minister serve as the control group. The staggered nature of this setting requires us to implement a difference-in-difference estimator capable of addressing the short-comings of traditional two-way fixed effects regressions. We employ the estimator proposed by Callaway and Sant'Anna (2021).

Although the drawbacks of the traditional two-way-fixed designs in settings with multiple treatment timings and groups have been highlighted extensively in recent literature, and new estimators to avoid these issues have been developed, we are aware of only a handful of papers in the field that adopted the new methodology. Hence, our contribution is also to provide an applied example using the new staggered difference-in-difference methodology.

Our core finding is an aggregate increase of nighttime light intensity of roughly 7% for minister pixels, indicating regional favoritism effects of ministers even exceeding those of primary leaders from the prior literature. A sub-sample analysis of the African, European, Asian, and American continent reveals that these effects are most prominent in Africa and Asia, less strong in Europe, and not detectable in Americas. We further document, that the minister pixels do not experience any migration inflows. We rather find a decrease of between 1% to 2% in total population in the global sample. We interpret this as a preliminary finding, indicating potential migration patterns due to increased out-group tensions between favored and disfavored groups. Our auxiliary results suggest that larger political power as measured by the prestige of a minister's portfolio, is associated with stronger effects. A deeper dive into the highest prestige category shows that specifically finance and foreign ministers exude the favoritism effect. These results suggest that favoritism increases with political power, and point to portfolios with easy access to domestic and foreign financial capital playing a major role in allocating resources towards favored regions. Lastly, we investigate effect heterogeneity by institutional setting. We find that our baseline results are driven by autocracies. In more democratic settings ministers seem to be restricted to perform redistribution to the same extent as their autocratic counter-parts.

Our paper is primarily related to the evolving literature on regional favoritism. The seminal paper by Hodler and Raschky (2014) suggests that regions connected to a national leader exhibit more economic activity, as proxied by nighttime luminosity. Hodler and Raschky (2014) also show that favoritism does not seem to have a persistent effect once the connected leader steps down from power. Asatryan et al. (2021a) document that firms located in favored regions are larger in size and more productive. However sectors are affected differentially, and the induced allocation towards service sector firms leads to aggregate output losses in the economy, due to diminishing marginal returns. A series of papers investigates favoritism specifically on the African continent. Dreher et al. (2021) show that the allocation of Chinese aid is subject to favoritism, and that favored regions appear to benefit in terms of local economic development, again measured by nighttime luminosity. However, the results do not hold for aggregate World Bank aid. Asatryan et al. (2021b) study the economic implications of mine openings and find that leaders' birth regions benefit unlike other non-mining region, but only in autocratic regimes. Furthermore, Asatryan et al. (2021c) on the one hand show that males exposed to regional favoritism during their adolescence have higher human capital later in life potentially leading to more stable employment. On the other hand, they do not find similar results for women, except for those females belonging to the same ethnic group as their national leader. Specifically analyzing favoritism by ministers in 36 African countries, Widmer and Zurlinden (2022) find decreased neonates' and infants' mortality especially for children of rural-based or uneducated mothers, when the current health minister originates from their region. They argue that better healthcare access at birth presumably explains part of the mortality-decreasing effects.

A closely related literature focuses on the mechanisms through which favoritism might manifest, but often these studies are limited the context of a single country. For example, Burgess et al. (2015) show that Kenyan regions inhabited by co-ethnics of the president receive more road spending than other regions during periods of autocracy. During periods of democracy, favoritism appears to be enacted by less visible strategies, for example educational transfers. Similar evidence on the importance of regional favoritism is available for a diverse set of countries such as Germany (Baskaran and da Fonseca, 2021), Vietnam (Do et al., 2017), Italy (Carozzi and Repetto, 2016), as well as across regions of Europe (Asatryan and Havlik, 2020). Bandyopadhyay and Green (2019) on the other hand find that connected leaders provide poorer quality roads to their home regions. Based on qualitative evidence, they argue that leaders channel resources to elites in their home regions at the expense of non-elites. Focusing on chief ministers of Indian state governments, Khalil et al. (2021) find that constituencies represented by a sitting chief minister have an about 13 % increase in luminosity compared to all other constituencies. They suggest that the main mechanism is likely to be political expediency rather than in-group favoritism.

Our paper is also connected to the literature on accountability of politicians, as well as the literature on political selection (Barro, 1973; Besley and Coate, 2003; Besley, 2005; Maskin and Tirole, 2004; Alesina and Tabellini, 2007, 2008). Analyzing power sharing among ministers in Africa, the results of Francois et al. (2015) suggest that that the governing elite might strive for forming inclusive coalitions and insuring against revolutions and coups by distributing patronage to elites proportionally to population shares across ethnic groups.

Finally, our paper connects with the literature on the spatial implications of distributive politics. Neoclassical models of distributive politics derive that office-motivated politicians have strong incentives to allocate disproportionate public resources to electorally important geographies (Weingast et al., 1981), such as core, swing, or politically aligned districts. (Cox and McCubbins, 1986; Cox, 2010; Albouy, 2013; Baskaran and Hessami, 2017).

The remainder of this paper is structured as follows: Section 2 introduces the data, while Section 3 explains the empirical strategy. In Section 4 we present the results. Section 5 concludes.

2 Data

2.1 Grid-level Data

We overlay a grid of 0.5 $\times$ 0.5 degree cells (0.5 degrees correspond to about 55km at the equator) over the World. We then intersect this grid with a map of country borders to identify within which country a particular cell is located. We then drop from this grid all border cells that are located in more than one country. The final sample consists of 1,189,560 cells over the period 1992-2016. We plot all remaining data presented below on this grid.

2.2 Ministers Data

We receive information on governing elites from the WhoGoV database covering 177 countries and the years 1966 to 2016. To the best of our knowledge, this is the largest global data set on ministers and cabinets. In summary, the data set includes information on 50,197 cabinet members. The original and publicly available data set contains variables documenting the years ministers were in power, official position, name, years of birth and death, party, portfolio, and several other information.

We use this data and extend it by a geographic dimension. In particular, we identify the birthplaces and birth regions of cabinet members, resulting in a geocoded dataset of 9,415 unique birthplaces of ministers in 120 countries (Table A.2).

Split up by continent, the coverage rates for birthplace information of cabinets at the country level for our sample of 120 countries over the period 1992-2016 are as follows: 50.96 % Africa, 43.38 % Europe, 31.39 % Asia, 21.02 % Americas (41.12 % World). We project the latitude and longitude coordinates of these ministers onto a worldwide map (Figure 2).

2.3 Luminosity Data

We use nighttime luminosity as a proxy for economic development at the local level (Alesina et al., 2016; Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2016; Bruederle and

Hodler, 2018; Martínez, 2022). These data are based on images of the earth at night obtained by satellites of the US Air Force (USAF) Defense Meteorological Satellite Program Operational Linesman System (DMSP-OLS). The original imagery is processed by the National Oceanic and Atmospheric Agency (NOAA) and released to the public as raster datasets. We use the annual composites collected from satellites F10, F12, F14, F15, F16, and F18 in which ephemeral lights, e.g. fires and flaring, are removed. The processing also excludes (at the pixel level) images for nights affected by clouds, moonlight, sunlight, and other glare. The images are available at a resolution of 30 arc-seconds (about 0.86 square kilometer at the equator) for all years after 1992. Each pixel of the dataset stores a 6-bit digital value ranging from 0 to 63 indicating the amount of average light of an area covering 30 arc-seconds. Higher values imply that a pixel emanates more light (Henderson et al., 2012).

The initial release of stable lights data time-series ended in 2013, but it has recently been extended with data collected from satellites F15 and F16 for 2014-2021. At the beginning of 2014, the F18 satellite was no longer capturing usable nighttime data. As a consequence, the interest had moved to processing global nighttime images from Visible Infrared Imaging Radiometer Suite (VIIRS) Day/Night Band (DNB) data. However, later it was discovered that the satellite F15 had started collecting pre-dawn nighttime light data beginning in 2012. Satellite F16 may have also collected usable nighttime data in the pre-dawn hours. Based on this new information, EOG (Earth Observation Group) has extended the annual nighttime lights time series by enhancing the established algorithms of the previous years to process DMSP-OLS data from 2013 on (Ghosh et al., 2021).

To obtain cell-level measure of economic development, we overlay the grid of cells over the raster datasets and calculate the area mean of the digital values of each cell with size 30 arc-seconds that falls within the boundaries of each of the 0.5×0.5 degree cells (see Figure 3).

The time of night that light output is measured differs between the original and the extended series. The initial series of images was collected around 7:30 p.m. by satellites F10, F12, F14, F16, and F18. The extended series was captured around 4:30 a.m. by satellites F15 and F16. Moreover, there may also be differences regarding the sensors, as F15 has been in orbit for longer. Presumably, more deterioration may have affected the equipment of the satellite. Our estimation design controls for year-by-year differences by utilizing time fixed effects. These fixed effects will account for satellite-specific measurement differences.

2.4 Population Data

We retrieve publicly available population data from the WorldPop Project.[1] For the years 2000-2020 the data set captures annual gridded population as raster files. The population values per pixel of the WorldPop data set are based on recent official census population data and various other input data sources, such as location and extent of settlements, roads, land cover, building maps, satellite nightlights, vegetation, topography, health facility locations, and refugee camps. Stevens et al. (2015) shows methodological details regarding the random forest regression tree-based mapping approach that is used to generate gridded pixel data at a spatial resolutions of 1 km and 100 m.[2]

We use the raster data sets with 1 km resolution to estimate population sums at the grid level. To receive cell-level measure of population development, we overlay the grid of cells over the yearly raster data sets. We then compute the area sum of the digital values of each cell with size 30 arc-seconds that falls within the boundaries of each of the 0.5 x 0.5 degree cells (see Figure 4).

2.5 Further Data Sources

To capture the extent of democracy and civil rights in a country, we use data from Freedom House (House, 2019).

[1] https://www.worldpop.org

[2] https://www.worldpop.org/methods/

3 Empirical Design

We conduct a series of staggered difference-in-differences estimations and plot event studies to observe the potential aggregate effects of favoritism. First, by comparing birth places of ministers with all other regions with regard to night light output, we study on a broader level to which extent ministerial favoritism might affect local economic development. Second, we apply a series of heterogeneity checks with respect to ministerial positions and their prestige level. Third, we analyze the broader institutional context by splitting our sample in democratic and authoritarian regime types.

3.1 Staggered Difference-in-Differences

A recent series of papers analyzes the inference question when treatment is staggered across units over time and has discovered that the two-way fixed effects estimator (TWFE) may not be an unbiased estimator of the ATT when treatment effects occur at different point in time and are heterogeneous. Many authors suggest alternative estimators and provide diagnostic tools to reveal potential bias (Baker et al., 2022; Borusyak et al., 2022; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021).

The canonical Difference-in-Differences models involve two periods and two groups. The untreated group never participates in the treatment, and the treated group becomes treated in the second period. However, using the canonical models in cases where there are more than two time periods and where different units can become treated at different times, already treated units may serve as control group for later treated units because their treatment status is constant over time. An important finding is that every group acts as a control group at some point in time. If treatment effects vary over time, the estimated coefficients may be biased. Goodman-Bacon (2021) proves that the usual fixed effects estimator yields a weighted average of all possible pairs of the underlying TWFE estimator. In particular, the Goodman-Bacon Decomposition shows that when treatment effects are not homogeneous, some of these weights may be negative.

In other words, the TWFE is not robust to treatment effect heterogeneity, as relatively comparing newly treated units to already treated units adjusts the path of outcomes for newly treated units by the path of outcomes for already treated units. However, this is not the path of untreated potential outcomes, it includes treatment effect dynamics. As a result, these dynamics appear in the coefficient of the treatment dummy, making it difficult to give a convincing causal interpretation. Callaway and Sant'Anna (2021) show in simulations that examples exist where the effect of participating in the treatment is positive for all units in all time periods, but the TWFE estimation results indicate a negative effect of participating in the treatment (Callaway and Sant'Anna, 2021).

With multiple treatment timings (appointments to ministerial positions) across units (cells in countries) and potentially heterogeneous treatment effects, as countries are heterogeneous in size and cabinets are heterogeneous regarding political power, our research design is a prime example for a staggered design. To overcome the previously described pitfalls of the canonical models, we therefore use the dynamic estimator proposed by Callaway and Sant'Anna (2021) as our main specification.

3.2 Specification

In our main specification, the treatment group are pixels that are birthplaces of minister in power as well as pixels that are birth places of cabinet members that have stepped down from power in the period of investigation (1990-2016). The control group are all remaining pixels of our sample. Callaway and Sant'Anna (2021) propose numerous ways to aggregate group-time average treatment effects. We use the aggregation methods simple and aggregated as defined in the *did* R package.[3] Both procedures are outlined in the following.

The ATT in setups with multiple treatment groups and multiple time periods can be formalized by:

[3] https://cran.r-project.org/web/packages/did/did.pdf

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_g = 1]. \quad (1)$$

The $ATT(g,t)$ represents the average treatment effect for pixels that are members of a particular group g[4] at a particular time period t.

Consider the average effect of receiving treatment, separately for each group. This can be denoted as:

$$\theta_S(g) = \frac{1}{T-g+1} \sum_{t=g}^{T} \mathbf{1}\{g \leq t\} ATT(g,t). \quad (2)$$

$\theta_S(g)$ is the average effect of receiving the treatment among units in group g, across their post-treatment periods. There are T total time periods, where t in our setting is yearly $t = 1,...,T$. The parameter $\theta_S(g)$ allows to emphasize treatment effect heterogeneity with respect to treatment adoption time. Furthermore, it is fairly straightforward to further aggregate $\theta_S(g)$ to receive an overall effect parameter that is easy to interpret:

$$\theta_S^O = \sum_{g \in G}^{T} \theta_S(g) P(G = g|G \leq T). \quad (3)$$

θ_S^O is the average effect of receiving the treatment for units (pixels) in group g as defined in equation 2. θ_S^O first calculates the average effect for each group (across all time periods). Then it averages these effects together across groups to summarize the total average effect of receiving the treatment. Hence, θ_S^O is the average effect of participating in the treatment for all units that ever received treatment. In this regard, its interpretation is the same as the ATT in the traditional DiD setup with two periods and two groups.

As shown, the simple aggregation method is an intuitive approach. It yields a weighted average of all group-time average treatment effects with weights proportional to group size. This type of aggregation circumvents the negative weights problem that might occur in two-way

[4]Groups are defined by treatment timing. For example, a pixel that is a birth place of cabinet member that came into power in the year 1996 belongs to $g = 1996$.

fixed effects regressions. Therefore, it is a straightforward summary statistic of the overall effect of receiving the treatment in the context of multiple time periods and variation in treatment timing. However, this simple aggregation has the tendency to overestimate the effect of early-treated groups simply because more of them exist during post-treatment periods. Therefore, we also implement a dynamic approach, as outlines next.

In our application, there is a large number of groups and time periods and we are interested in understanding treatment effect dynamics. A common approach to analyze these dynamics is to aggregate group-time effects into an event study plot. We do this by computing average effects across different lengths of exposure to the treatment and plot the results.

Let e be event-time, i.e., $e \cdot t - g$ captures the years passed since treatment was adopted. A way to aggregate the group-time average treatment effect $ATT(g,t)$ to highlight treatment effect dynamics with respect to e is given by:

$$\theta_D(e) = \sum_{g \in G}^{T} \mathbf{1}\{g + e \leq T\} P(G = g | G + e \leq T) ATT(g, g + e). \tag{4}$$

$\theta_D(e)$ is the aggregated parameter of interest for our event study. It captures the average effect of a pixel having a birthplaces of a ministers e years after the treatment was adopted across all pixels that are ever observed to have birthplace of a minister for specifically e years. In this specification, the "on impact" average effect of receiving the treatment appears at $e = 0$. This aggregation avoids the drawbacks associated with the dynamic TWFE specification discussed in the previous section. The overall effect is then calculated by averaging the effect of the treatment across all positive lengths of exposure.

An obvious methodological challenge is that regions or pixels that are connected to the governing elite might be systematically different than other polygons. For instance, ministers might be more likely to originate from more urbanized parts of their respective countries. As such, comparing pixels that were connected to a cabinet member with all other (not yet treated) pixels might lead to biased estimates. To address this concern, we incorporate covariates in our event study estimations. In particular, we utilize a matrix of covariates including country dummies and controls for leader birthplaces by passing it in the DiD estimator. We use the default doubly robust approach of the did R command to compute group-time average treatment effects. This procedure allows us to verify if the results hold after conditioning on these pre-treatment covariates.[5]

Given that the properties of the staggered adoption does not allow status switches of treatment, we assume that once a pixel is indicated as treated, it remains treated in all subsequent periods. However, it is plausible to assume that after a political leader stepped down from power, persistent network effects might be at place that might affect his home region in the long run. Furthermore, the estimator does not account for treatment intensity. In our sample, pixels exist that are birth places of more than one minister. We only use the first treatment in any particular pixel for the estimation. Therefore, we analyze the potential impact on a pixel level of having ever been the birth place of a minister during our sample period.

4 Results

4.1 Baseline Results

4.1.1 Luminosity in Minister Pixels

We start out by presenting results from our baseline specification, which utilizes the Callaway and Sant'Anna estimator. In Table 1 we present the aggregate effect of being a ministers' birthplace on the intensity of nightlight a pixel emits. The aggregation of the group-time specific effects follows the two procedures outlined in Section 3.2. In column (1) we show the aggregate effect for our full sample which spans countries around the world. Both aggregation methods result in sizeable significant effects, suggesting aggregate increases of around 7% of nighttime light intensity after ministers come into power. In their seminal paper Hodler and

[5]The *did* package requires that covariates are time-invariant. For time varying variables, the did package sets the value of the covariate to be equal to the value of the covariates in the base period. In the post-treatment periods the base period is the period immediately before observations in a particular group receive the treatment, and in pre-treatment periods the base period is the period immediately before the current period.

Raschky (2014) estimate a baseline effect of 3.8% increased nighttime light intensity in leaders birthplace pixels.

There are a number of potential reasons for the almost doubled effect size we measure: First, the sample compositions have a large overlap but are not identical. This is true for the countries included, but particularly for the time periods. As Hodler and Raschky show a strong interaction effect with leader tenure, e.g. in their paper in figure III effects start becoming statistically different from zero only in year 14, our longer study period might capture more long tenures. This type of sample composition effect is potentially exacerbated by the fact that ministers typically don't have formalized tenure restrictions. Second, the last years have brought large changes and refinements to the difference-in-difference estimation technique, especially in the case of staggered treatments. In countries with multiple primary rulers over the sample period, potentially harmful comparisons of treated and already-treated pixels might arise in a standard difference-in-difference design. The updated methods in our paper promise to address this problem, however it also is clearly a larger issue given the many more treatments we observe with ministers. Third, our results imply large effects for minister birth pixels. If minister cabinet changes typically coincide with changes of the primary ruler, then not controlling for minister birth pixels dilutes the control group, and downward biases the estimate. Fourth, ministers might be more strongly incentivized and better able to exert favoritism towards their birthplaces. For example they might rely more on regional political support, while at the same time being under less public scrutiny.

In the columns (2) to (5) of the table, we present the results for sub-samples of individual continents. We observe strong heterogeneity of effects between the continents. We find that African and Asian countries seem to drive the results in the worlds sample, as Europe and Americas have small estimates that are not significantly different from zero. Part of these differences might be driven by the fact that nightlights as a measure will behave differently across the continents in our sample. For example already very strongly electrified countries in Europe have a different potential to become lighter, given that a linear relationship between economic

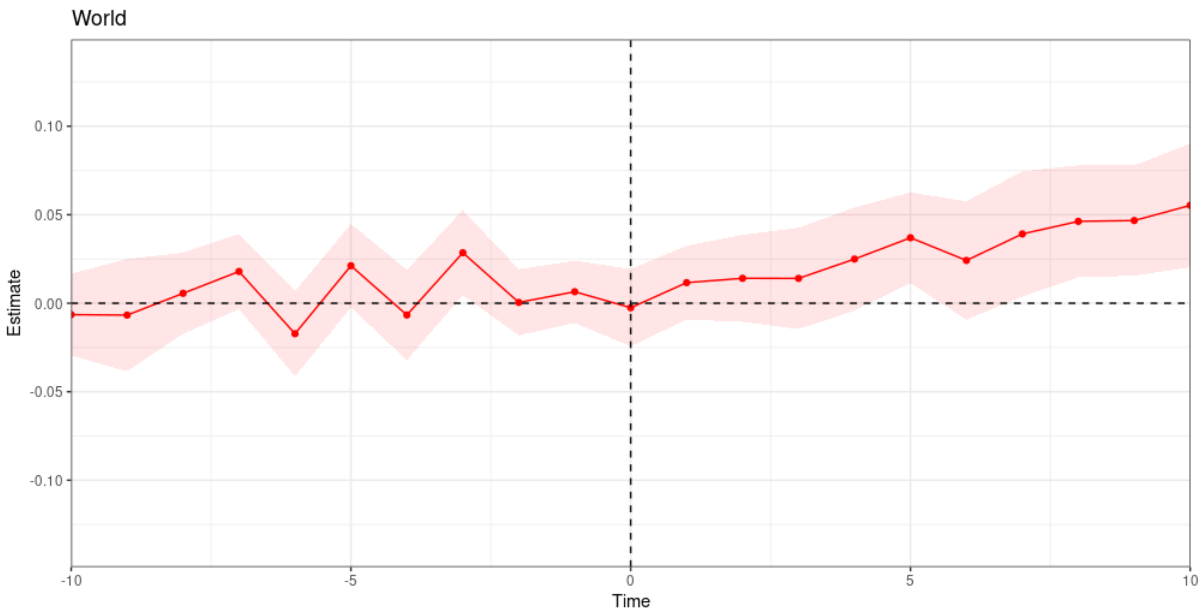
Table 1: Treatment effect in minister birth pixels

	Dependent Variable: **Nightlights**				
Aggregation Method	(1) World	(2) Africa	(3) Europe	(4) Asia	(5) Americas
simple	0.066***	0.130***	0.039	0.071**	-0.004
	(0.016)	(0.029)	(0.029)	(0.027)	(0.020)
dynamic	0.074***	0.141***	0.035	0.087***	-0.007
	(0.016)	(0.030)	(0.029)	(0.031)	(0.025)
Observations	1,153,230	252,390	389,940	302,280	208,620

Notes : The dependent variable is specified in logarithmic form. The method of aggregation *simple* is given by Equation 3 and *dynamic* by Equation 4. We restrict the *dynamic* aggregation to 20 post treatment periods, to restrict the duration for which there is a plausible treatment effect on the outcome we study. All estimations include covariates identifying the birth pixels of countries' primary rulers and a country-specific factor variable. * (**) (***) indicates significance at the 10 (5) (1) percent level.

activity and nightlight intensity is unlikely. Furthermore it is likely that the institutional setting mediates the size of the effect. We turn to this aspect in Section 4.2.2.

Figure 1: Dynamic Treatment Effects in Minister Birth Pixels.



We are also interested in the time dynamics of the effects we measure, as tenure showed to be an important factor in Hodler and Raschky (2014). To this end we plot the group aggregates by distance to treatment start in an event study type plot in Figure 1. We observe a slowly increasing effect over the first ten years after a minster comes into power for the global sample, which again is driven by the African and Asian sub-sample (see Figure A.1). For Europe and Americas the line plotting the aggregated coefficients stays fairly flat and statistically insignificant. The steady increase over the years is in line with the notion that ministers are diverting resources and differentially benefit their home regions more, the longer they stay in power. The figures let us also investigate the existence of pre-trends. If minister pixels were substantially different from non-minister pixels, or if ministers coming into power could be anticipated and elicit a change of nightlights, this should lead to significant effects in the time periods prior to them getting into office. None of the samples in Figure A.1 displays a pattern that is consistent with this narrative.

4.1.2 Population in Minister Pixels

Nightlight intensity is by design a very broad measure, and naturally raises the question: What is actually happening on the ground? In this section we turn to another measure that lets us keep the large scale nature of our study, but sheds some light on this question. As we lay out in Section 2.4 we build a measure of pixel-year-level population sums from data by the WorldPop Project. We run our baseline specification employing this measure as the outcome variable.

Table 2 presents the results. For the world sample we observe small negative effects that are statistically significant. Our results suggest an aggregate population decline between 1% and 2% in the minister birth pixels compared to the control group. For Africa and Europe we find small effects that are statistically not precisely differentiable from zero. There is a smaller negative effect in the Asia sub-sample and a surprisingly large negative effect for the Americas sub-sample, that drives the worlds result. There are some plausible explanations for such a negative effect. First, our empirical design estimates changes in relation to the control group. In some control regions comparatively lower economic growth can be correlated with higher fertility. The reversed effect could occur in the treated areas. Fertility however is a long-term concept, but the dynamic effects plotted in Figure A.2 for the Americas sub-sample suggest

Table 2: Treatment Effect in Minister Birth Pixels

	Dependent Variable: **Population sum**				
Aggregation Method	(1) World	(2) Africa	(3) Europe	(4) Asia	(5) Americas
simple	-0.013**	-0.010	0.011	-0.025*	-0.065***
	(0.005)	(0.008)	(0.009)	(0.010)	(0.008)
dynamic	-0.025**	-0.019	0.011	-0.031	-0.097***
	(0.008)	(0.013)	(0.011)	(0.032)	(0.014)
Observations	812,921	172,893	283,668	210,672	145,677

Notes : The dependent variable is specified in logarithmic form. The method of aggregation *simple* is given by Equation 3 and *dynamic* by Equation 4. We restrict the *dynamic* aggregation to 20 post treatment periods, to restrict the duration for which there is a plausible treatment effect on the outcome we study. All estimations include covariates identifying the birth pixels of countries' primary rulers and a country-specific factor variable. * (**) (***) indicates significance at the 10 (5) (1) percent level.

effects already in the short-term. Conceptually such a pattern could be observed, if nomination of a minister and subsequent favoring of one ethnic or political group increases out-group tensions leading to migration responses of the disfavored group. In the case of the Americas sample it has to be noted, that in Figure A.2 we observe that the minister pixels appear to have been on a slightly declining population trend before the ministers come to power. Hence we are careful in giving this effect a causal interpretation.

Overall our interpretation of the results on population is that the regional favoritism effect we estimate in the nightlights measure appears to not induce systematic migration responses, and in the same vein is not explained by such relocation patterns. Increased nightlight intensity thus must rather be the result of increased investment or spending activity.

4.2 Extensions / Mechanisms

4.2.1 Prestige Levels and Portfolios

Given the rich nature of the cabinet member data set, we can explore a range of attributes of ministerial positions and their potential impact on favoritism. After harmonizing ministerial positions across countries into 42 categories, Nyrup and Bramwell (2020) identify prestige levels of ministers using a three-fold typology similar to the approach developed by Krook and O'Brien (2012).

Table A.1 in the appendix presents a list of portfolios and their associated prestige levels (high, medium, and low) that we use in the following estimations. We redefine the treatment variables of our main specification according to the three prestige levels, i.e. we estimate the potential effect on a pixel level of having ever been the birth place of a high prestige minister compared to all other pixels, including birth places of ministers in the medium and low prestige categories. Accordingly, setting medium prestige ministers as treatment category, we assign all other pixels including birth places of high and low prestige ministers to the control group (for the low prestige category, the control group are all other pixels and birth places of high and medium prestige ministers). In each estimation of this subsection, we further modify our specification by adding dummies to our covariates matrix accounting for birth place pixels of all other minister categories.

The results in Table 3 indicate that portfolios assigned to the high and medium prestige categories drive the results. For our worldwide sample we observe an ATT of 8.6 % for birth places of high prestigious and an ATT of 5.8 % for pixels linked to medium prestige ministers (dynamic, column 1). While these potential effects are strong in magnitude and statistically significant, the low prestige estimates are statistically insignificant.

In line with our baseline results of the dynamic aggregated approach, we observe particular large estimates for Africa (ATT: 13.7 %) and Asia (ATT: 10.8 %) for the high prestige portfolios (dynamic, columns 2 and 4). For the medium prestige portfolios we identify an aggregated estimate for Africa of 11.6 %.

It is notable that we overall find the largest estimates in the high prestigious category. These results imply that the split into three prestige groups represents an arguably sufficient measure of political power. It is likely to occur that ministers holding a more prestigious office, such as the finance, budget, or treasury ministry have more political power to allocate resources than for example the ministry of energy or the ministry of children & family.

Table 3: Treatment Effects in Minister Birth Pixels by Prestige Level

		Dependent Variable: **Nightlights**				
Aggregation Method	Prestige	World	Africa	Europe	Asia	Americas
simple	High	0.083***	0.107***	0.049	0.130***	-0.027
		(0.021)	(0.040)	(0.031)	(0.043)	(0.031)
	Medium	0.053***	0.098***	-0.011	0.031	0.009
		(0.018)	(0.040)	(0.037)	(0.029)	(0.019)
	Low	0.020	0.033	0.094	-0.002	-0.034
		(0.028)	(0.047)	(0.057)	(0.049)	(0.036)
dynamic	High	0.086***	0.137***	0.020	0.108**	-0.015
		(0.023)	(0.039)	(0.075)	(0.054)	(0.031)
	Medium	0.058***	0.116***	-0.016	0.035	-0.052*
		(0.019)	(0.031)	(0.039)	(0.048)	(0.030)
	Low	0.028	0.034	0.099	0.023	-0.023
		(0.034)	(0.046)	(0.063)	(0.079)	(0.040)
Observations	high	1,116,780	233,700	390,990	283,020	209,070
	medium	1,110,780	229,080	390,150	282,780	208,770
	low	1,122,570	238,920	391,320	283,140	209,190

Notes : The dependent variable is specified in logarithmic form. The method of aggregation *simple* is given by Equation 3 and *dynamic* by Equation 4. We restrict the *dynamic* aggregation to 20 pre and post treatment periods to account for the duration for which there is a plausible treatment effect on the outcome we study. All estimations include covariates identifying the birth pixels of countries' primary rulers, other minister categories and a country-specific factor variable. * (**) (***) indicates significance at the 10 (5) (1) percent level. Dynamic treatment effects are displayed in Figures A.3, A.4 and A.5.

While this piece of evidence is interesting in its own right, we still have little information on which portfolios might be particularly successful in channeling resources towards their home regions. To address this question, we link the treatment status to the four high prestige portfolios: "Defense, Military & National Security", "Foreign Relations", "Finance, Budget & Treasury", and "Government, Interior & Home Affairs". In doing so, we use the same specification properties as for the prestige analysis to estimate treatment effects by portfolios.

Among our preliminary results for the high prestige portfolios (Table 4), for the African sub-sample we identify that the birth pixels of ministers that hold or held the finance portfolio

Table 4: Treatment Effects in Minister Birth Pixels of High Prestige Portfolios

		Dependent Variable: **Nightlights**				
Aggregation Method	Portfolio	World	Africa	Europe	Asia	Americas
simple	Defense	0.088**	0.104	0.136	0.1047	0.002
		(0.041)	(0.090)	(0.109)	(0.064)	(0.034)
	Foreign	0.087***	0.069	0.081	0.074	-0.012
		(0.031)	(0.057)	(0.067)	(0.065)	(0.037)
	Finance	0.053	0.125**	0.086	0.064	-0.075*
		(0.041)	(0.059)	(0.086)	(0.084)	(0.042)
	Interior	0.040	0.040	0.116	0.158	-0.006
		(0.045)	(0.046)	(0.099)	(0.115)	(0.058)
dynamic	Defense	0.074	0.084	0.129	0.186	0.043
		(0.076)	(0.070)	(0.103)	(0.197)	(0.027)
	Foreign	0.125***	0.128**	0.098	0.181**	-0.035
		(0.040)	(0.065)	(0.070)	(0.094)	(0.022)
	Finance	0.060	0.141**	0.080	0.021	-0.071
		(0.045)	(0.061)	(0.083)	(0.079)	(0.044)
	Interior	0.055	0.065	0.117	0.236	-0.014
		(0.051)	(0.050)	(0.115)	(0.159)	(0.065)
Observations	Defense	1,168,980	265,800	391,200	302,760	209,220
	Foreign	1,168,320	265,110	391,230	302,760	209,160
	Finance	1,168,560	265,410	391,260	302,730	209,220
	Interior	1,168,230	265,080	391,410	302,790	208,950

Notes : The dependent variable is specified in logarithmic form. The method of aggregation *simple* is given by Equation 3 and *dynamic* by Equation 4. We restrict the *dynamic* aggregation to 20 pre and post treatment periods to account for the duration for which there is a plausible treatment effect on the outcome we study. All estimations include covariates identifying the birth pixels of countries' primary rulers, other minister categories and a country-specific factor variable. * (**) (***) indicates significance at the 10 (5) (1) percent level. Dynamic treatment effects are displayed in Figures A.6, A.7 and A.8.

have sizable estimates that are also statistically significant (dynamics, columns 1 and 2). These results indicate that the ministries at the heart of financial resources seem to be specifically prone to regional favoritism. Furthermore, we observe an aggregated estimate of 12.8 % for the African and 18.1 % for the Asian foreign ministries (column 4). The channel through which favoritism by foreign ministers might take place seems to be more indirect. As international trade often falls within the responsibilities of the foreign ministry, we hypothesize that favoritism in Africa and Asia might often be expressed by foreign (direct) investments or aid flows in the home regions of foreign ministers.[6] To some extent, this interpretation of the results fits into the greater narrative of the findings of Dreher et al. (2021): similar to Chinese aid engagements in Africa, foreign investments in Africa and Asia might be subject to political capture, allowing (foreign) ministers of recipient countries to use it for their own political purposes. Whether or not this type of favoritism is a threat to the effectiveness of foreign engagements remains an open question.

4.2.2 Democracy versus Autocracy

Next up we investigate whether the institutional context mediates the effects we measured in the baseline specification. We achieve this by manually interacting the treatment variables of our main specification with a dummy indicating democratic and autocratic country-years according to the Freedom House classification. The treatment then occurs when the first autocratic (democratic) minister in our sample comes into office, while adding a dummy that indicates the existence of a democratic (autocratic) minister at any other time. We thus estimate the effect of having ever been the birth place of a minister in an autocratic or democratic regime on the nightlight intensity emitted by a pixel, compared to the other pixels. An alternative approach would be to split the sample into autocratic and democratic country-years. Directly testing an interaction is currently not easily implementable in the staggered DID design. When

[6] As of 2021, Asia is still the largest recipient of FDI worldwide with an inflow of $619 billion followed by Latin America and the Caribbean with an inflow of $134 billion and Africa with an inflow of $83 billion (United Nations Conference on Trade and Development, 2022).

comparing the two options, we choose the one that preserves the largest sample, as sample size reductions, and specifically the imbalance they introduce to the panel structure, impose additional restrictions on the estimator.

Table 5: Treatment Effects in Minister Birth Pixels by Institutional Setting

		Dependent Variable: **Nightlights**				
Aggregation Method		World	Africa	Europe	Asia	Americas
simple	Autocracy	0.084***	0.090***	0.056	0.108***	-0.037
		(0.023)	(0.032)	(0.046)	(0.037)	(0.036)
	Democracy	0.012	0.063	0.017	-0.051	0.014
		(0.017)	(0.048)	(0.032)	(0.035)	(0.022)
dynamic	Autocracy	0.092***	0.108***	0.049	0.130***	-0.048
		(0.025)	(0.035)	(0.047)	(0.046)	(0.036)
	Democracy	0.026	0.082	0.028	-0.026	0.020
		(0.021)	(0.053)	(0.035)	(0.047)	(0.029)
Observations	Autocracy	1,156,470	254,100	390,810	302,490	209,070
	Democracy	1,168,110	266,130	390,600	302,580	208,800

Notes : The dependent variable is specified in logarithmic form. The method of aggregation *simple* is given by Equation 3 and *dynamic* by Equation 4. We restrict the *dynamic* aggregation to 20 post treatment periods, to restrict the duration for which there is a plausible treatment effect on the outcome we study. All estimations include covariates identifying the birth pixels of countries' primary rulers and a country-specific factor variable. Dynamic treatment effects are displayed in Figure A.9 and Figure A.10. * (**) (***) indicates significance at the 10 (5) (1) percent level.

Table 5 shows the results. We take note of two findings: For autocratic settings we measure large positive effects. The effects are statistically significant for the world, African, and Asian sub-sample. For democratic settings we observe a close to zero result for the full sample. While the Asian sub-sample displays negative effects, there is tentative evidence for sizeable positive effects in the African sub-sample, however both come with large standard errors attached to them, and are statistically insignificant.

Conceptually it is not clear which institutional setting should come up with the larger effects. We think of the institutional context as a mediator that affects both the possibility to engage in regional favoritism, as well as the incentives to do so. While autocratic ministers might be less constrained to engage in favoritism than their democratic counterparts, they might face a lower incentive to share rents broadly, as they face less electoral competition. Our results in this section then suggest that the restrictive features of some democracies in our samples dominate these electoral incentives, giving rise to the stronger observable effects in autocratic settings.

4.2.3 Rural vs. Urban Ministers

We employ our grid level population numbers again, to construct a measure of urbanization. Specifically we build a dummy variable that indicates rural and urban areas according to the quartile of the population sum distribution the pixel is in. The most populated quartile of pixels gets classified as an urban area.

There are theoretical arguments for both urban and rural areas being more strongly subjected to regional favoritism by ministers. Channeling resources to birthplaces that are urban areas might be easier, as more public investment opportunities exist. Furthermore, targeting more densely populated urban areas affects a larger share of the population, and thus might hold larger electoral benefits. On the other hand, diverting resources to more sparsely populated rural areas might enable the minister to win over a larger share of the population in these places, facilitating a stronghold of support for them.

ongoing

4.2.4 Left-wing vs. Right-wing / Populist vs. Non-populist Parties

Next we exploit the party affiliation information of ministers from the WhoGov data set, to study differences in the favoritism effects depending on the party affiliation of the ministers. We are interested in analysing two dimensions: First, we look to study whether there are systematic differences along the right-left-scale of politics. Ministers coming from left or right aligned parties will typically exhibit different preferences for re-distribution and taxes. Second, we investigate whether ministers belonging to more populist parties, or parties that engage more in clientelism, redistribute more or less resources to their birthplaces.

To study these dimensions, we link minister parties to the V-Party data set, which offers indicators capturing these features on the party level. We utilize a fuzzy match to link the English party names provided in both data sets, and manually check the non-perfect matches up until a similarity score of 0.9. In this way we are able to obtain scores on populism, clientilism and the political spectrum for roughly 24,500 minister-years.

ongoing

4.2.5 Women minister

The WhoGov data set indicates the gender of the politicians, which allows us to study heterogeneous effects along the dimension of gender. From the prior literature we know that a policy makers gender can interact in various ways with the outcome of their governance, for a comprehensive review on this see Hessami and da Fonseca (2020). We want to highlight two findings from this literature that we look to test within our setting: First, in developing countries the increase in female political representation has caused improved provision of public goods, especially in education and health. Second, higher female representation has improved institutional quality by reducing corruption and rent-extraction by those in power. We look to investigate this relation by studying countries that transition from men dominated ministerial cabinets to above median shares of women participation in ministerial positions.

ongoing

4.2.6 Military and academic ministers

We exploit information on the titles of ministers to construct subsets of ministers with academic and military backgrounds.

ongoing

4.3 Robustness Tests

4.3.1 First and Secondary School Pixels

One common concern of utilizing the birthplace as defining the treatment location is that ministers might have been raised somewhere else, and might have closer ties for this or other reasons to a different place, which they then choose to favor over their birthplace. If this is the case on a larger scale, we would wrongfully identify many birth place pixels as treated and many control pixels as untreated, which would lead to our estimates representing a lower bound. In this section we perform a robustness check, where we re-define treatment to be the location of first or secondary schooling of the minister. This information is hand collected and, as it is often hard to come by, suffers from attrition.

ongoing

4.3.2 Randomization Inference

In this section we randomly perturb our definition of treatment across space and time, and estimate the resulting treatment effects. We repeat this process 5,000 times and plot the resulting distribution. This allows us to assess whether the estimates we receive under the true treatment allocation are statistically speaking an extreme case in a non-parametric significance test.

ongoing

5 Conclusion

Our paper documents that ministers have the ability to, and do strongly engage in regional favoritism. To quantify: Utilizing the correlation of 0.3 suggested by Henderson et al. (2012) translates the nighttime light intensity increases of between 7% and up to 14.1% in the African sub-sample, into average local GDP growth of 2.1% to 4.2%. Our heterogeneity checks reveal that predominantly the most powerful of ministers, and especially those with very direct power to affect budgets, drive the effects. Our results on the institutional setting suggest, that these ministers can be constraint under more democratic institutions.

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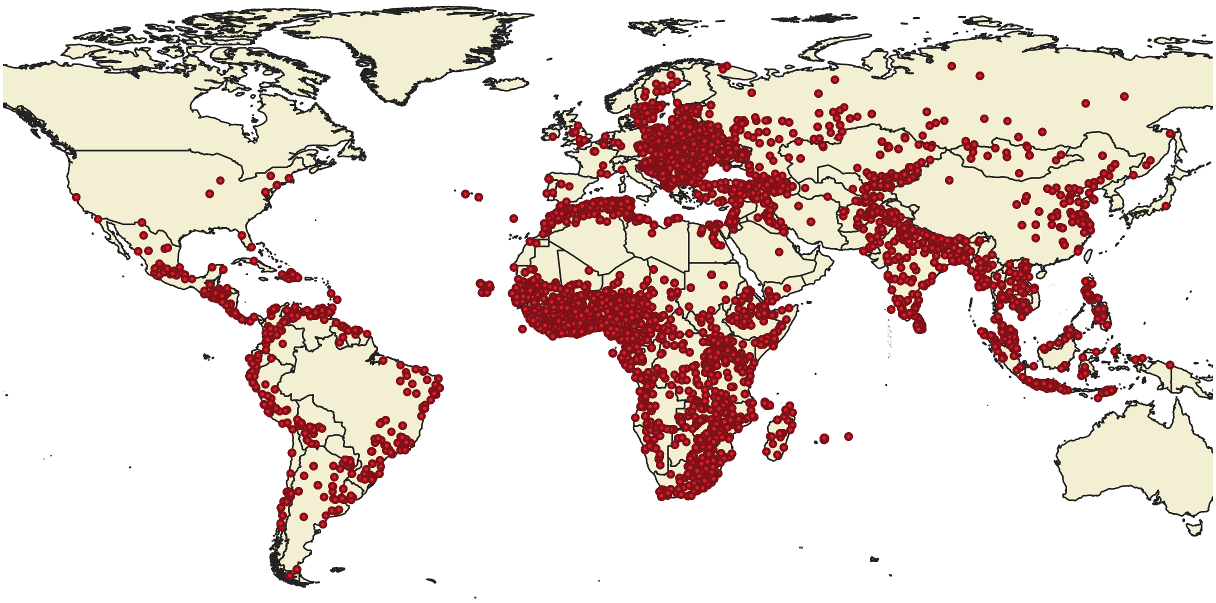


Figure 2: Birth places of cabinet members. The dots in this figure represent the location of collected birth places of cabinet members in our sample of the world.

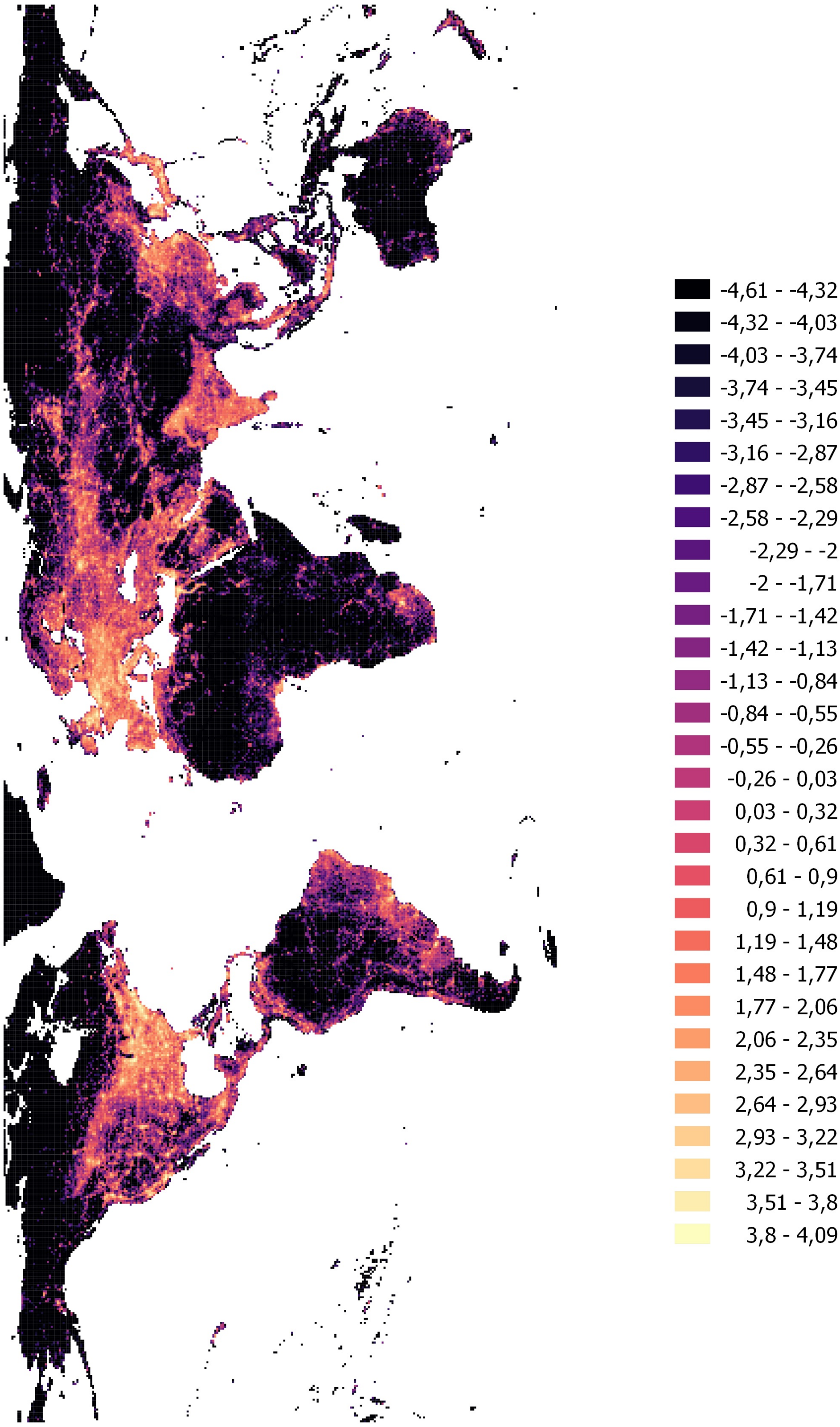


Figure 3: Grid of the World displaying mean night light intensity. This figure shows (the logarithm) of mean night light output for the period of our sample (1990-2016). The values for the pixels were computed by extracting information from the night light raster files based on the grid of the World utilized in our empirical analysis. For this process we used the *exactextractr* R package. Brighter cells indicate higher nighttime light intensity. The corresponding values are tabulated in the legend.

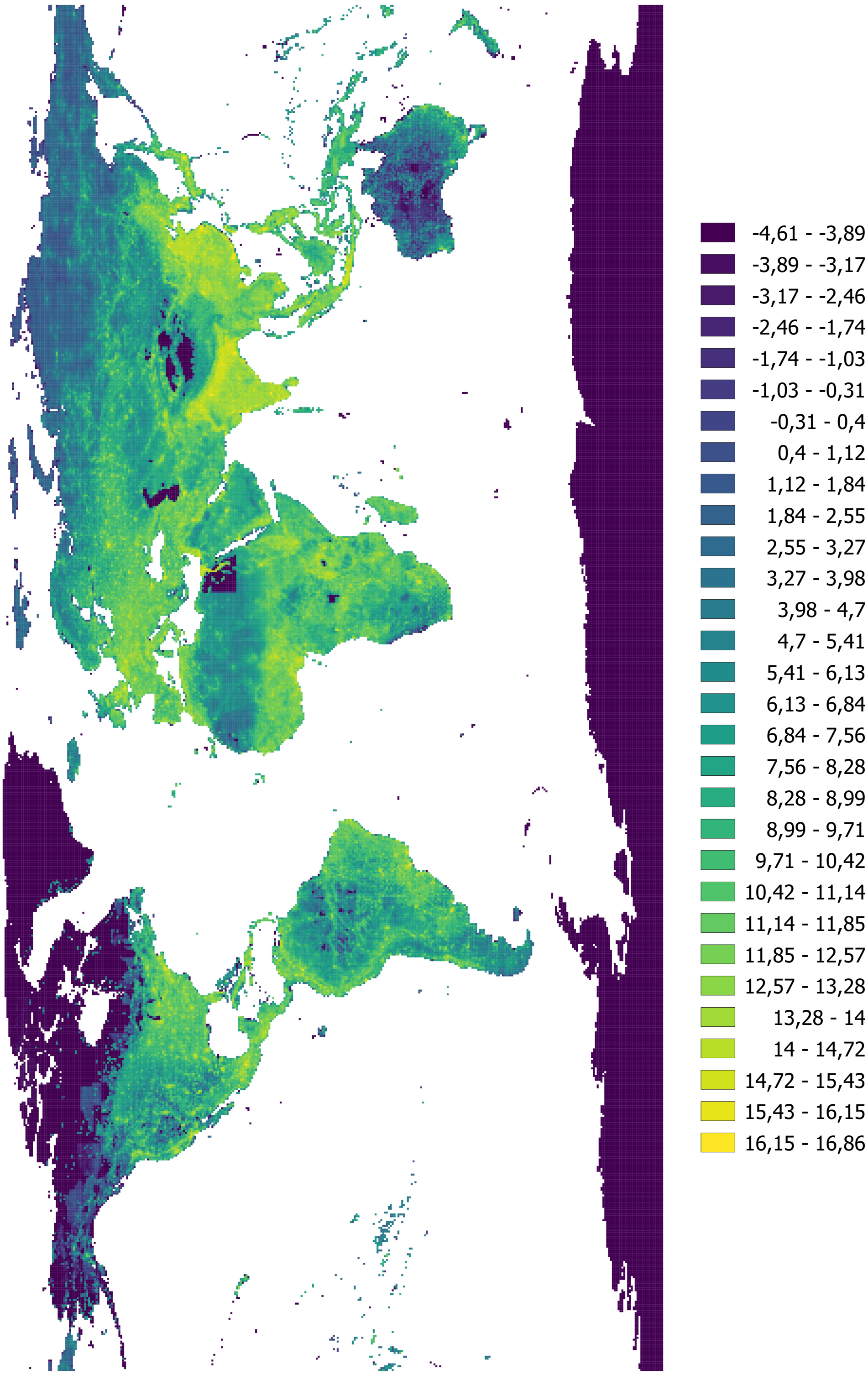


Figure 4: Grid of the World displaying mean population. This figure shows (the logarithm of) sum population for the period (2000-2016). The values for the pixels were computed by extracting information from the population raster files based on the grid of the World utilized in our empirical analysis. For this process we used the *exactextractr* R package. Brighter cells indicate higher population numbers. The corresponding values are tabulated in the legend.

Appendix

A.1 Baseline Results

Figure A.1: Dynamic Treatment Effects in Minister Birth pixels: Nightlights.

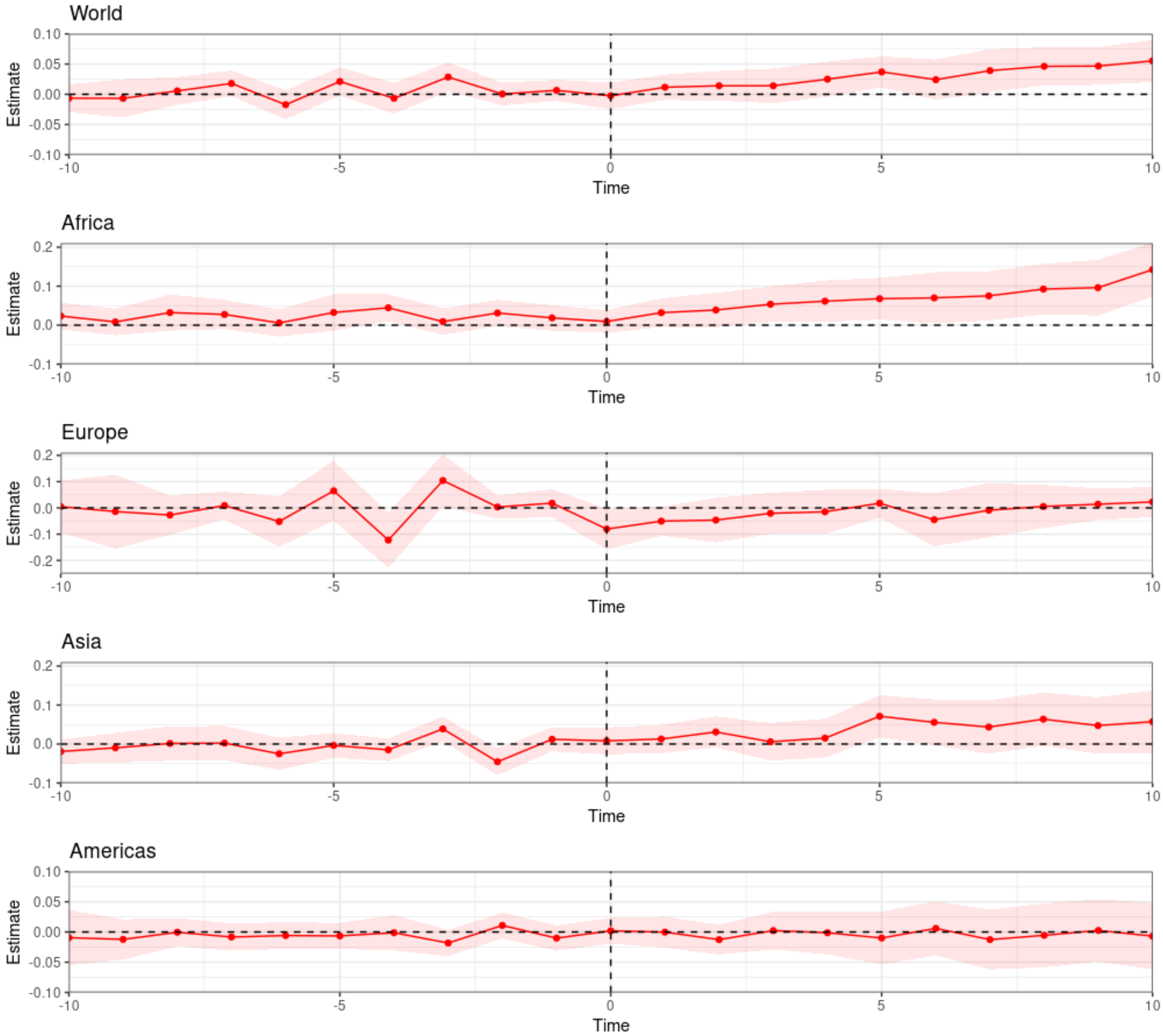
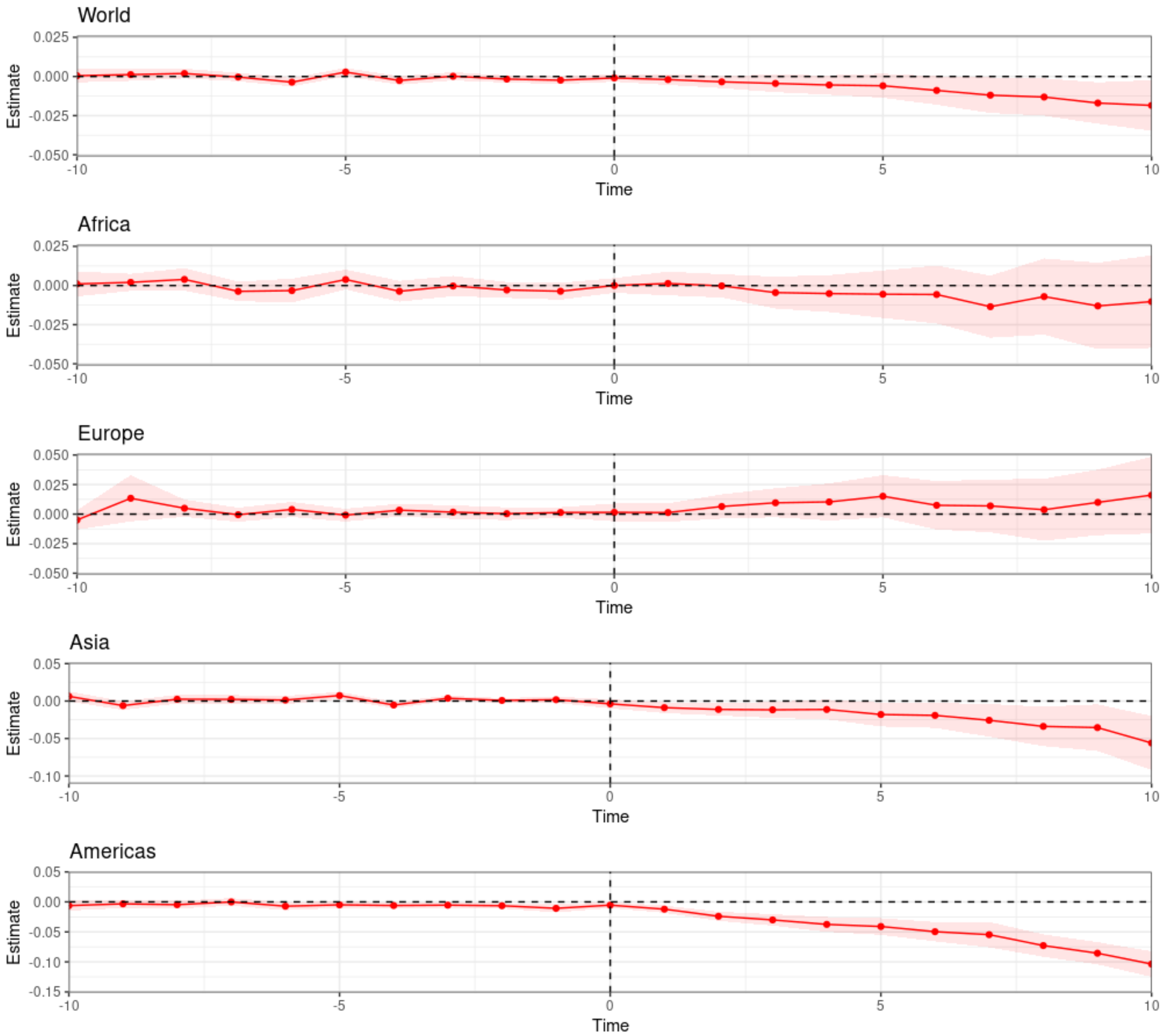


Figure A.2: Dynamic Treatment Effects in Minister Birth pixels: Population.



A.2 Extension and Mechanisms

A.2.1 Prestige Levels and Portfolios

Table A.1: Portfolios and Prestige Level Categories

Portfolio	Prestige
Defense, Military & National Security	High
Foreign Relations	High
Government, Interior & Home Affairs	High
Finance, Budget & Treasury	High
Agriculture, Food, Fisheries & Livestock	Medium
Audit, Oversight & Internal Affairs	Medium
Civil Service	Medium
Communications & Information	Medium
Construction & Public Works	Medium
Correctional Services & Police	Medium
Culture & Heritage	Medium
Education, Training & Skills	Medium
Energy	Medium
Enterprises, Companies & Business	Medium
Environment	Medium
Executive & Legislative Relations	Medium
Foreign Economic Relations	Medium
General Economic Affairs	Medium
Health & Social Welfare	Medium
Housing	Medium
Industry & Commerce	Medium
Justice & Legal Affairs	Medium
Labor, Employment & Social Security	Medium
Medium Local Government	Medium
Planning & Development	Medium
Political Reform	Medium
Properties & Buildings	Medium
Religion	Medium
Regional	Medium
Tax, Revenue & Fiscal Policy	Medium
Transport	Medium
Ageing & Elderly	Low
Children & Family	Low
Immigration & Emigration	Low
Minorities	Low
Science, Technology & Research	Low
Sports	Low
Tourism	Low
Veterans	Low
Without Portfolio	Low
Women	Low
Youth	Low

Source : Nyrup and Bramwell (2020)

Figure A.3: Dynamic Treatment Effects in High Prestige Minister Birth Pixels .

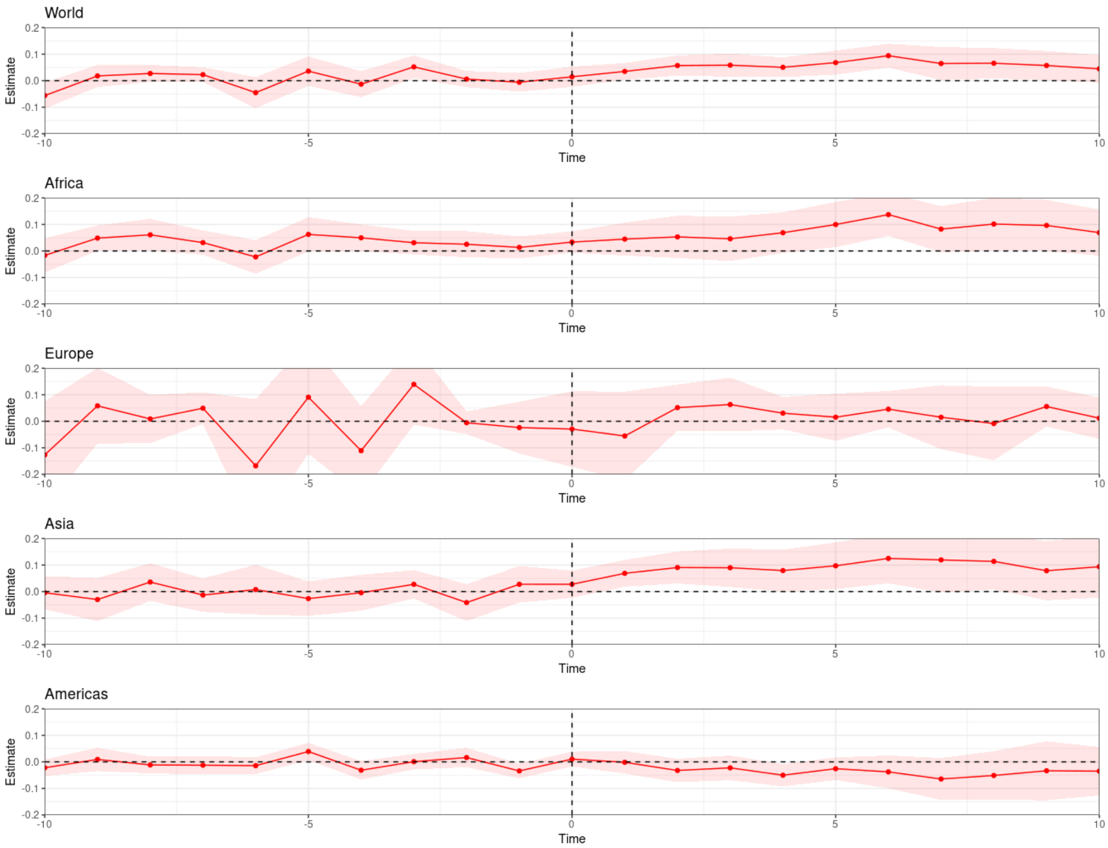


Figure A.4: Dynamic Treatment Effects in Medium Prestige Minister Birth Pixels .

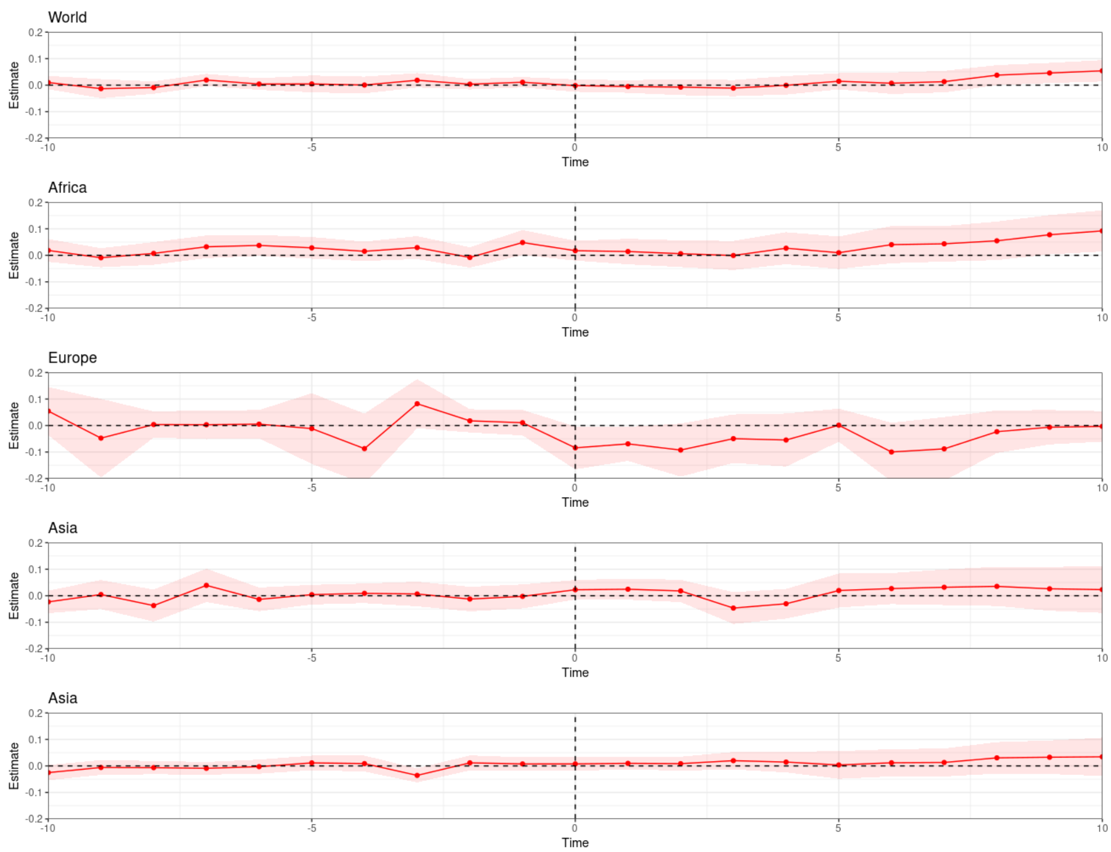


Figure A.5: Dynamic Treatment Effects in Low Prestige Minister Birth Pixels .

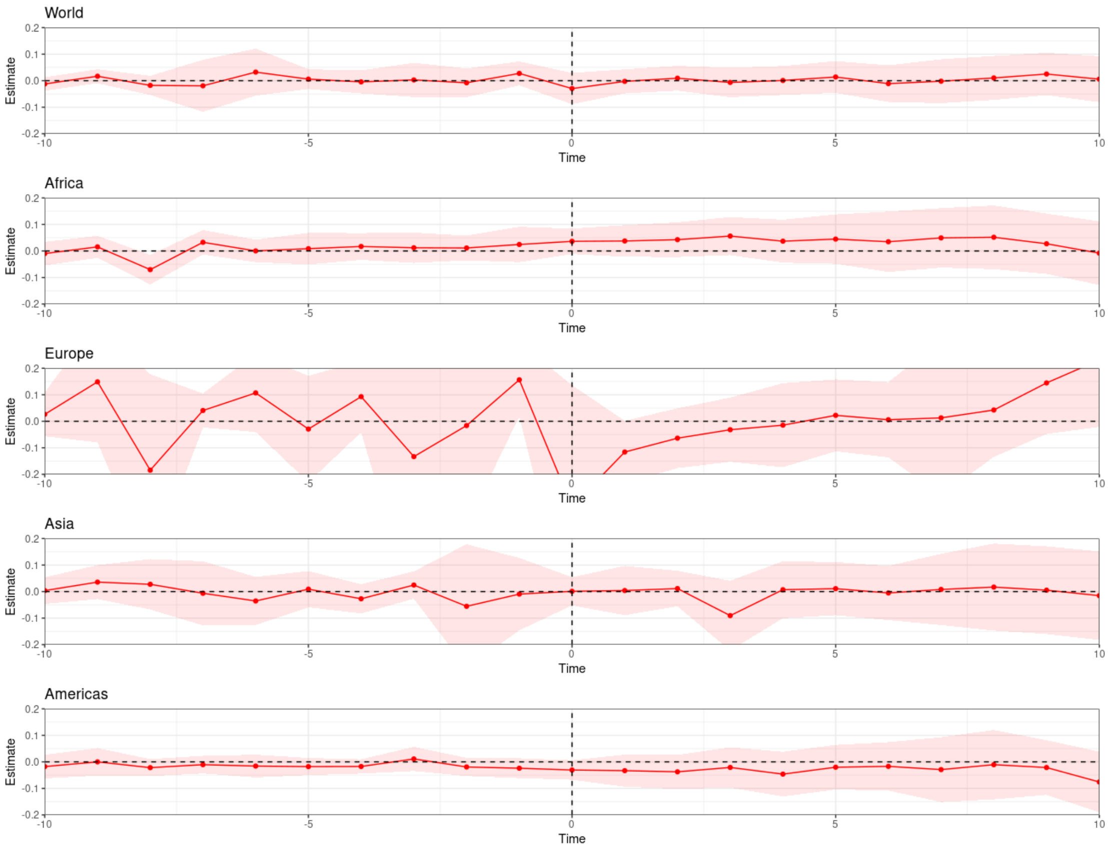


Figure A.6: Dynamic Treatment Effects in Defense Minister Birth Pixels .

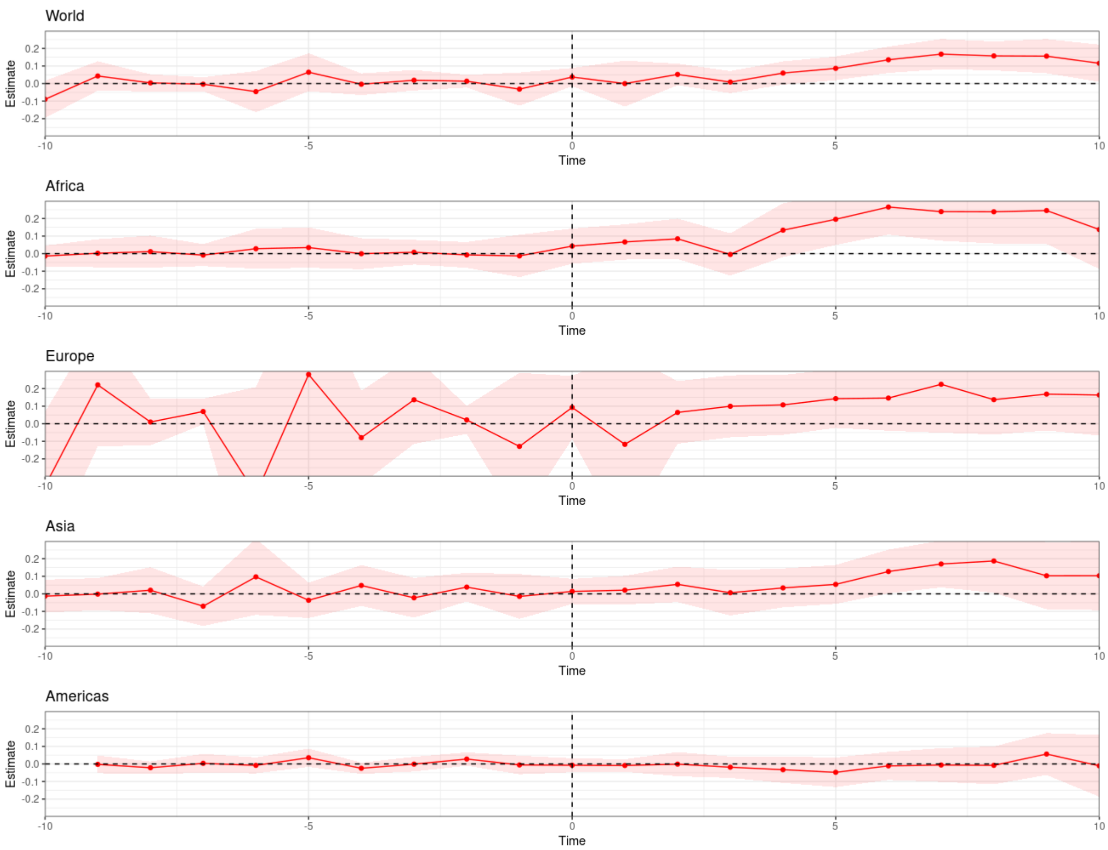


Figure A.7: Dynamic Treatment Effects in Foreign Minister Birth Pixels .

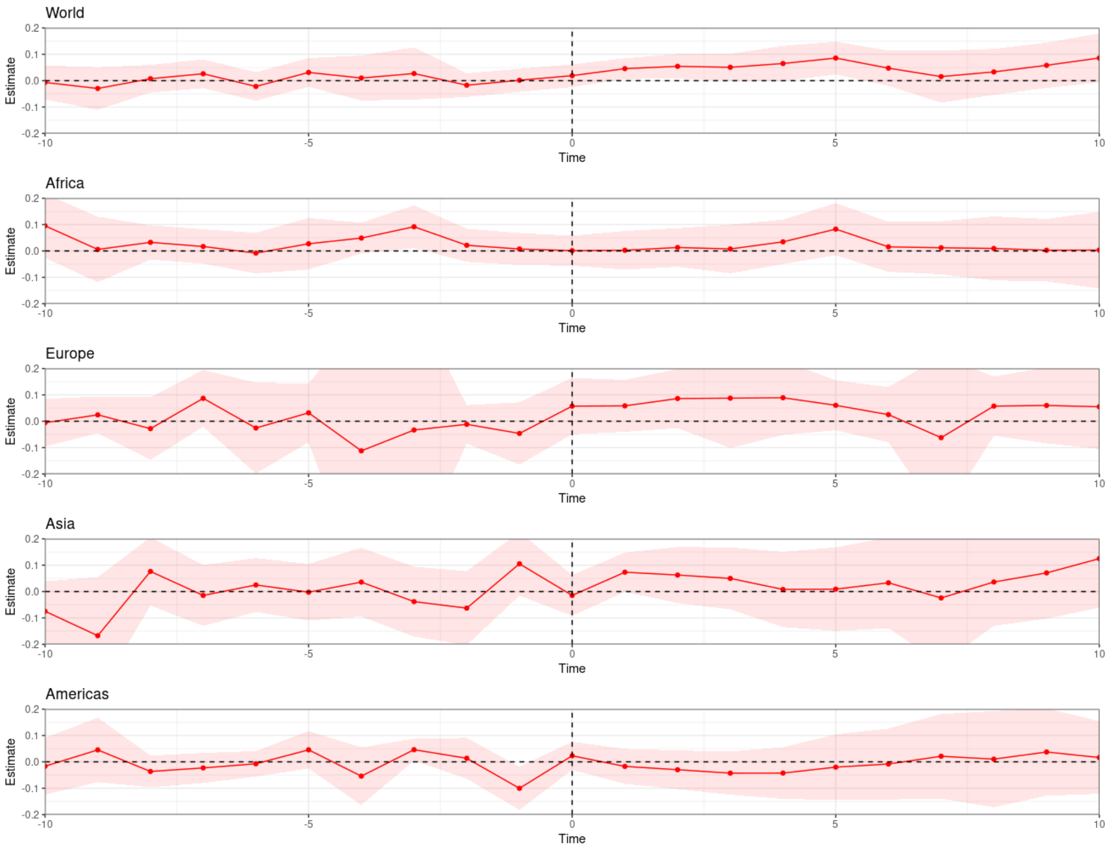
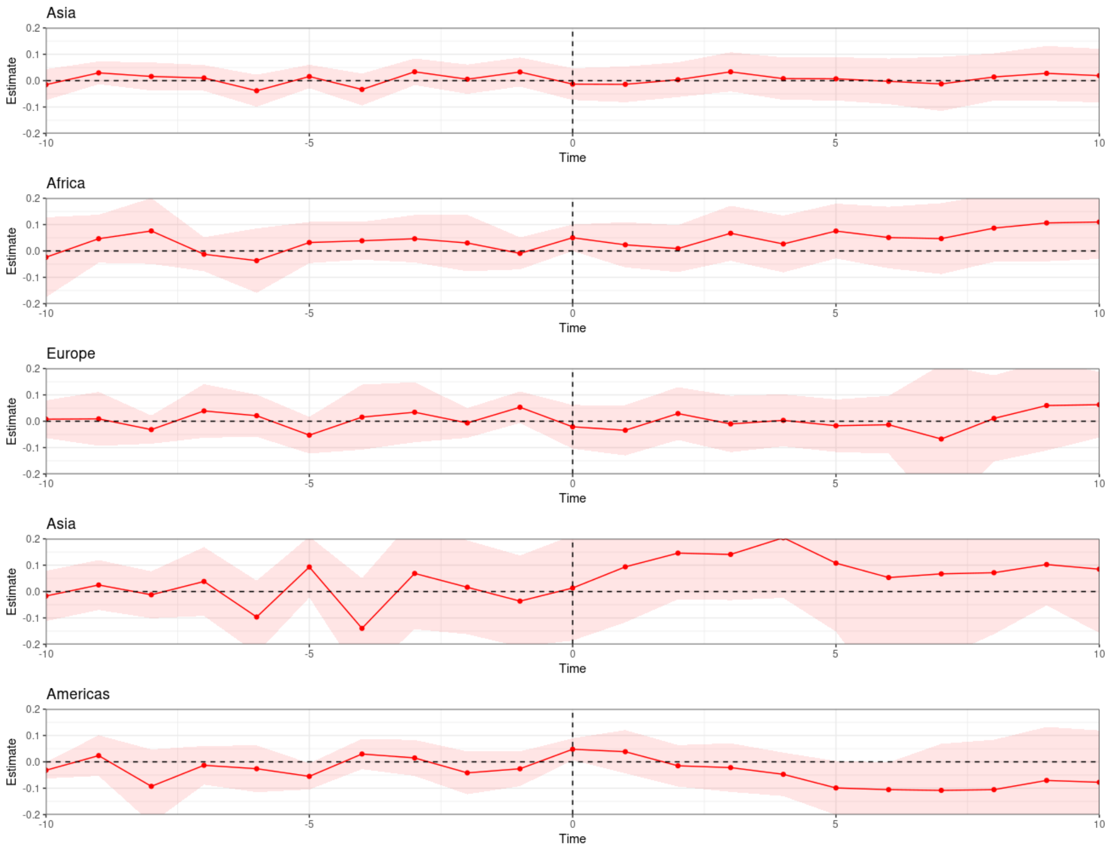


Figure A.8: Dynamic Treatment Effects in Finance Minister Birth Pixels .



A.2.2 Institutions

Figure A.9: Dynamic Treatment Effects: Ministers in Autocracies .

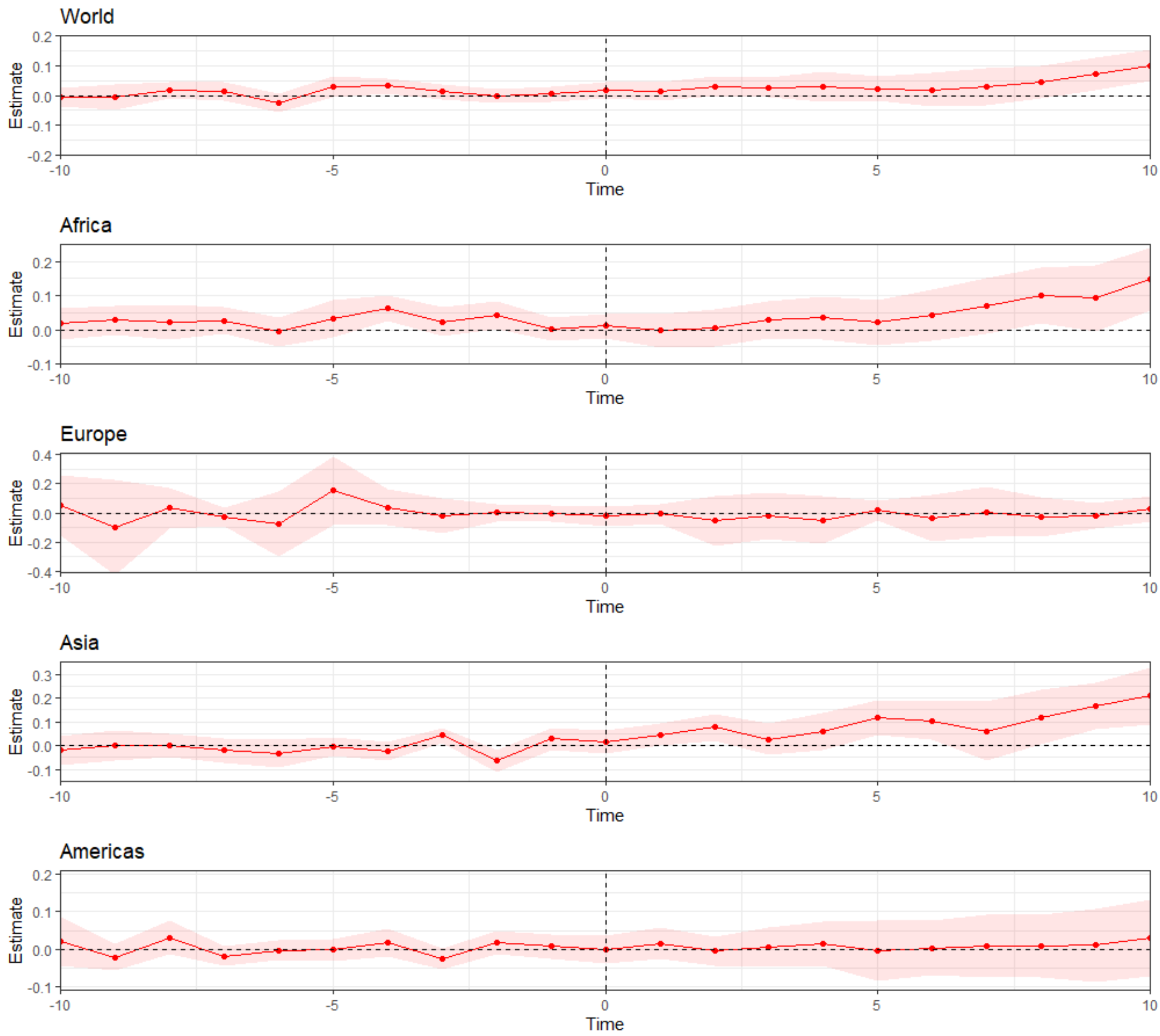
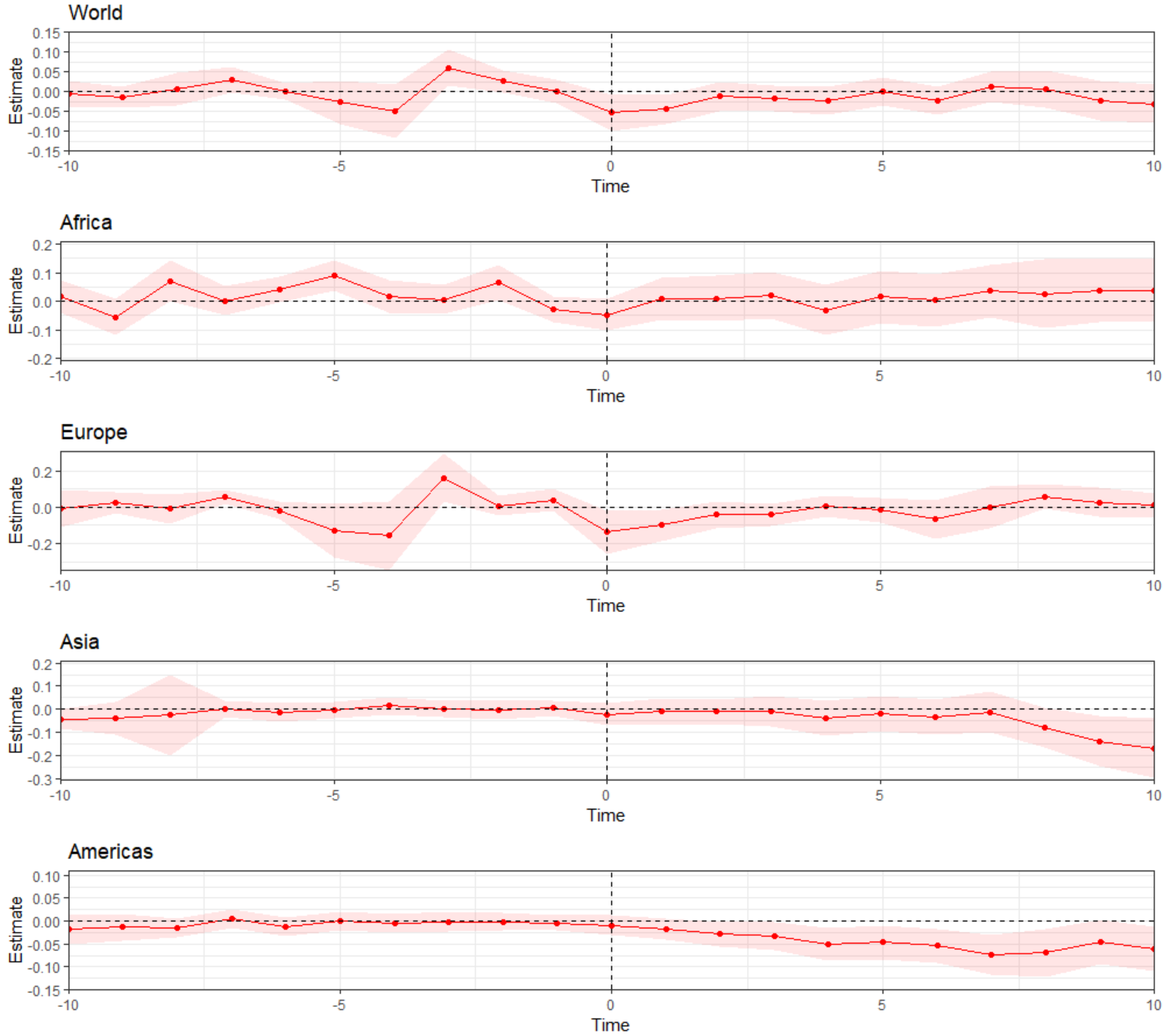


Figure A.10: Dynamic Treatment Effects: Ministers in Democracies .



A.3 Data

Table A.2: Countries and Years of Collected Birth Places

Country	Continent	Years
Algeria	Africa	1990-2016
Angola	Africa	1990-2016
Benin	Africa	1990-2016
Botswana	Africa	1990-2016
Burkina Faso	Africa	1990-2016
Burundi	Africa	1990-2016
Cameroon	Africa	1990-2016
Cape Verde	Africa	1990-2016
Central African Republic	Africa	1990-2016
Chad	Africa	1990-2016
Comoros	Africa	1990-2016
Djibouti	Africa	1990-2016
Egypt	Africa	1990-2016
Equatorial Guinea	Africa	1990-2016
Eritrea	Africa	1990-2016
Ethiopia	Africa	1990-2016
Gabon	Africa	1990-2016
Gambia	Africa	1990-2016
Ghana	Africa	1990-2016
Guinea	Africa	1990-2016
Côte d'Ivoire	Africa	1990-2016
Kenya	Africa	1990-2016
Lesotho	Africa	1990-2016
Liberia	Africa	1990-2016

Libya	Africa	1990-2016
Madagascar	Africa	1990-2016
Malawi	Africa	1990-2016
Mali	Africa	1990-2016
Mauritania	Africa	1990-2016
Mauritius	Africa	1990-2016
Morocco	Africa	1990-2016
Mozambique	Africa	1990-2016
Namibia	Africa	1990-2016
Niger	Africa	1990-2016
Nigeria	Africa	1990-2016
Congo	Africa	1990-2016
Rwanda	Africa	1990-2016
São Tomé Príncipe	Africa	1990-2016
Senegal	Africa	1990-2016
Sierra Leone	Africa	1990-2016
Somalia	Africa	1990-2016
South Africa	Africa	1990-2016
South Sudan	Africa	1990-2016
Sudan	Africa	1990-2016
Eswatini	Africa	1990-2016
Tanzania	Africa	1990-2016
Togo	Africa	1990-2016
Tunisia	Africa	1990-2016
Uganda	Africa	1990-2016
Zambia	Africa	1990-2016
Zimbabwe	Africa	1990-2016
Afghanistan	Asia	1990-2016

Armenia	Asia	1990-2016
Azerbaijan	Asia	1990-2016
Bangladesh	Asia	1972-2016
Bhutan	Asia	1973-2016
Cambodia	Asia	1967-2016
China	Asia	1982-2016
Georgia	Asia	1990-2016
India	Asia	1980-2016
Indonesia	Asia	1998-2016
Iraq	Asia	2004-2016
Israel	Asia	1970-2016
Jordan	Asia	1975-2016
Kazakhstan	Asia	1992-2016
Kyrgyz Republic	Asia	1992-2016
LaoPDR	Asia	2006-2016
Lebanon	Asia	2006-2016
Malaysia	Asia	2006-2016
Mongolia	Asia	2006-2016
Myanmar	Asia	2006-2016
Nepal	Asia	2006-2016
Pakistan	Asia	2006-2016
Philippines	Asia	2006-2016
SriLanka	Asia	2006-2016
Tajikistan	Asia	2006-2016
Thailand	Asia	2006-2016
Timor-Leste	Asia	2006-2016
Turkey	Asia	2006-2016
Uzbekistan	Asia	2006-2016

Vietnam	Asia	2006-2016
Yemen	Asia	2006-2016
Albania	Europe	1990-2016
Belarus	Europe	1990-2016
Bulgaria	Europe	1990-2016
Croatia	Europe	1990-2016
Czech Republic	Europe	1990-2016
Estonia	Europe	1990-2016
Hungary	Europe	1990-2016
Lithuania	Europe	1990-2016
Moldova	Europe	1990-2016
Montenegro	Europe	1990-2016
North Macedonia	Europe	1990-2016
Poland	Europe	1990-2016
Romania	Europe	1990-2016
Russia	Europe	1990-2016
Slovak Republic	Europe	1990-2016
Slovenia	Europe	1990-2016
Sweden	Europe	1990-2016
Ukraine	Europe	1990-2016
DominicanRepublic	North America	2005-2018
ElSalvador	North America	2005-2018
Guatemala	North America	2005-2016
Honduras	North America	2005-2016
Mexico	North America	2007-2018
Nicaragua	North America	2007-2016
Panama	North America	2005-2016
Argentina	South America	2001-2016

Bolivia	South America	2006-2016
Brazil	South America	1996-2016
Chile	South America	2000-2016
Colombia	South America	2000-2018
Ecuador	South America	2000-2018
Guyana	South America	2003-2016
Paraguay	South America	2000-2016
Peru	South America	2007-2016
Suriname	South America	2000-2018
TrinidadandTobago	South America	1998-2018
Uruguay	South America	2005-2016
Venezuela	South America	1999-2016

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