RUHR ECONOMIC PAPERS

Zareh Asatryan
Thushyanthan Baskaran
Carlo Birkholz
Patrick Hufschmidt

Favoritism by the Governing Elite

#1029

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Editors

Editorial Office

Ruhr Economic Papers #1029

Responsible Editor: Thomas Bauer

ISSN 1864-4872 (online) – ISBN 978-3-96973-198-7

Ruhr Economic Papers #1029

Zareh Asatryan, Thushyanthan Baskaran,
Carlo Birkholz and Patrick Hufschmidt

Favoritism by the Governing Elite

Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at **http://dnb.dnb.de**

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

http://dx.doi.org/10.4419/96973198
ISSN 1864-4872 (online)
ISBN 978-3-96973-198-7

Zareh Asatryan, Thushyanthan Baskaran, Carlo Birkholz and Patrick Hufschmidt[1]

Favoritism by the Governing Elite

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 manually collect birthplaces of this governing elite globally. Combining this information with extended nighttime 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 9 % more nightlight. This result is predominantly attributable to the African sub-sample. We find no evidence that the measured effect is driven by, or induces, migration to the home regions of ministers. The size of our data set lets us investigate heterogeneities along a number of dimensions: political power, ministerial portfolio, and the institutional setting.

JEL-Codes: D72, H72, H77, R11

Keywords: Favoritism; elite capture; spatiality; luminosity; population; democracy

July 2023

1 Zareh Asatryan, ZEW Mannheim; Thushyanthan Baskaran, Ruhr University Bochum and ZEW Mannheim; Carlo Birkholz, ZEW Mannheim, University of Mannheim; Patrick Hufschmidt, Ruhr University Bochum. – *We gratefully acknowledge financial support from the German Research Foundation (DFG) within the Project "Regional Favoritism and Development" (Grant no. 423358188 / BA 496716-1). The usual disclaimer applies.* *– All correspondence to: Thushyanthan Baskaran, Department of Economics, Ruhr University Bochum, Universitaetsstrasse 150, 44801 Bochum, Germany; Email: thushyanthan.baskaran@ruhr-uni-bochum.de.*

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), 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 literature with regard to the time dimension (1992-2016) and with respect to information on birth places of the governing elite (around 12,300 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 shortcomings of traditional two-way fixed effects regressions. We employ the estimator proposed by Callaway and Sant'Anna (2021).

Our core finding is an aggregate increase of nighttime light intensity of roughly 9% 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. Furthermore, 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. We measure regional favoritism only in the context of more corrupt and less industrialized settings, looking specifically at increases in nightlight in the birthplaces of ruling cabinets. Finally, our findings indicate that women ministers appear not to engage in regional favoritism.

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; Francois et al., 2015).

Finally, our paper connects with the literature on the spatial implications of distributive politics. Neoclassical models of distributive politics propose 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). Our paper is distinct from this literature because we focus on geographical distortions in the allocation of public resources due to leaders' intrinsic preference for their birthtowns, rather than due to opportunistic electoral considerations.

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

We overlay a grid of 0.5×0.5 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 Minister 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 geo-coded dataset of 12,337 birthplaces of ministers in 141 countries (Table A.1).

Split up by continent, the coverage rates for birthplace information of cabinets at the country level for our sample over the period 1992-2016 are as follows: 52.04% Africa, 61.62% Europe, 47.67% Asia, 55,92% Americas. We project the latitude and longitude coordinates of these ministers onto a worldwide map (Figure A.1).

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 A.2).

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 World-Pop 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]

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

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

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 A.3).

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). Positions of capital cities are obtained from (Mayer and Zignago, 2011). To measure corruption, we utilize the corruption perception index (CPI) from Transparency International.[3]

3 Empirical strategy

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 average treatment effect on the treated (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).

[3] https://www.transparency.org/en/cpi/2022

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 dynamic as defined in the *did* R package.[4] Both procedures are outlined in the following.

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

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

The $ATT(g,t)$ represents the average treatment effect for pixels that are members of a particular group g[5] at a particular time period t.

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

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

$\theta_S(g)$ 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:

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

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.

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

[5]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 $g = 1996$.

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 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 outlined 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.[6]

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

[6]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.

might be at place that might affect his home region in the long run. Furthermore, the estimator does not account for treatment intensity. Hence, 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 between 5% and 9% of nighttime light intensity after ministers come into power. In their seminal paper Hodler and 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 larger effect sizes that 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 unit of study in our estimations is on the pixel level, and thereby more granular than the region level employed by Hodler and Raschky. Third, 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. Fourth, 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. Fifth, 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 the effect is driven by African countries, as the other subsamples 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 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.

Table 1: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: NIGHTLIGHTS

	Dependent variable: luminosity				
Aggregation method	(1) World	(2) Africa	(3) Europe	(4) Asia	(5) Americas
simple	0.054***	0.144***	-0.006	-0.010	-0.017
	(0.015)	(0.032)	(0.044)	(0.036)	(0.029)
dynamic	0.094***	0.187***	0.009	0.027	-0.07
	(0.022)	(0.043)	(0.051)	(0.052)	(0.047)
Observations	957,350	209,900	324,825	250,550	172,075

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses.

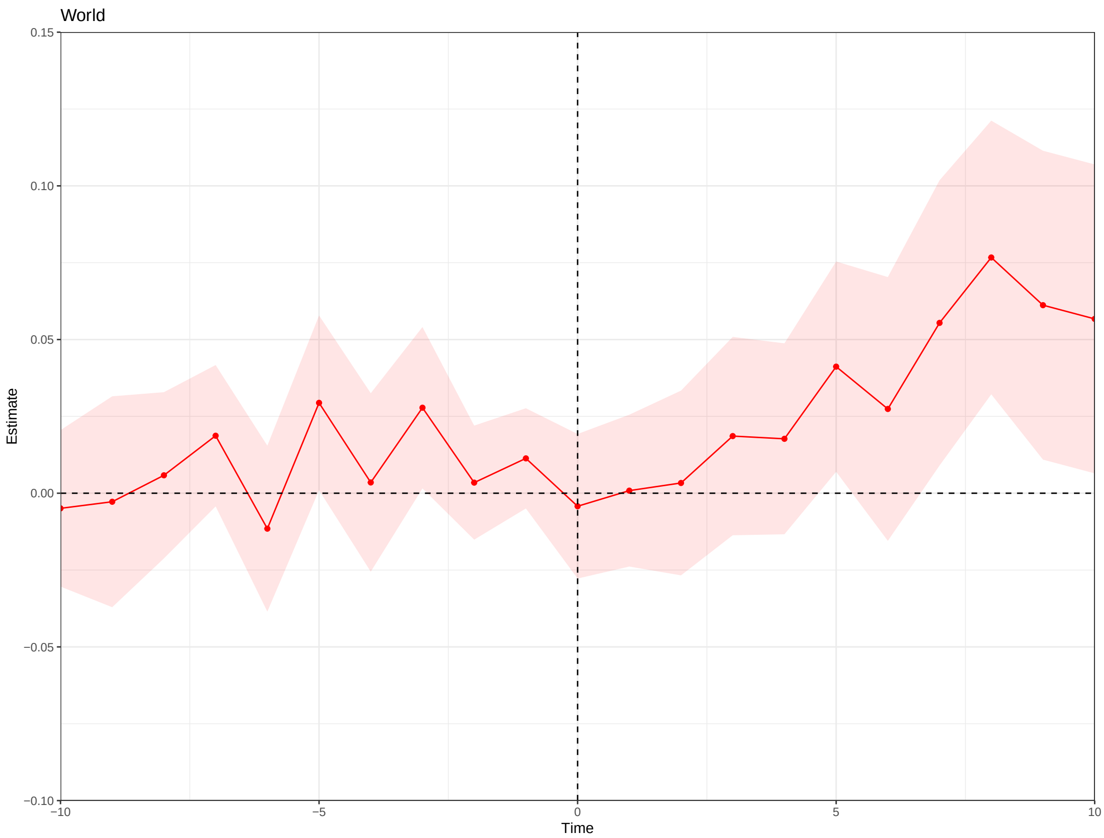


Figure 1: DYNAMIC TREATMENT EFFECTS IN MINISTER BIRTH PIXELS. This figure shows an event-study based on the Callaway and Sant'Anna (2021) difference-in-differences estimator, relating birth places of ministers in power to luminosity at the grid-level. The red shaded areas on the plot represent 95 percent confidence intervals, with standard errors clustered at the cell level.

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 sub-sample (see Figure A.4). For the other continents, 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.4 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 no effects. 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.

Table 2: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: POPULATION

	Dependent variable: population				
Aggregation method	(1) World	(2) Africa	(3) Europe	(4) Asia	(5) Americas
simple	-0.014***	-0.008	0.009	-0.013*	-0.053***
	(0.004)	(0.007)	(0.008)	(0.007)	(0.007)
dynamic	-0.028***	-0.016	0.008	-0.039***	-0.074***
	(0.007)	(0.011)	(0.010)	(0.013)	(0.013)
Observations	664,343	148,291	229,857	169,609	116,586

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses.

Conceptually, there are some explanations for this finding. 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.5 for the Americas sub-sample suggest effects already in a shorter time period. These patterns 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. Finally, the negative estimates could be a result of the not-switching treatment status of the estimator. That is out-migration is driven by places that are no longer home of an active minister. As they no longer receive the benefits from being home to a high ranking public official, firms and people relocate leading to a differential population decline compared to the control group. Since treatment units will over time eventually loose their active ministers, the persistent effect measured by the estimator displays a continuous decline for periods further away from the start of treatment.

Indeed, in Section 4.2.1 we find evidence in support of this last point. When we estimate the average treatment effect based only on units with active treatment status, we find positive effects on population for ministers with long tenure. This implies that the negative effects of the estimator that measures persistent effects are driven by places where ministers loose their office.

Overall, our interpretation of the results on population is that the regional favoritism effect we estimate in the nightlights appears to not induce systematic migration responses. There is also no evidence for persistent growth of the local population. Our results from the alternative estimator suggest positive effects on the population number for ministers with the longest tenures.

4.2 Extensions and mechanisms

4.2.1 Treatment status switches

In this section we test the robustness of our results to the use of alternative estimators (Figures 2 and 3). In particular, we benchmark our baseline results from the (Callaway and Sant'Anna, 2021) estimator against the canonical TWFE estimator and the estimator proposed by (de Chaisemartin and D'Haultfœuille, 2020). Because the latter allows for treatment status switches, we can speak more to the persistence of the treatment effect, as well as the role that minister tenure plays by comparing its results to our baseline, that captures the persistent effect of ever having been the birthplace of a minister.

There are two core findings we want to highlight. First, the classical TWFE displays significant pre-trends for both our baseline analyses. Estimates from the TWFE are very likely to be biased in our setting with a strongly staggered and heterogeneous treatment. Second, the estimates from the de Chaisemartin and D'Haultfœuille (2020) estimator are consistently more positive and they come with larger standard errors attached the farther away from treatment. Both findings are in line with expectations, as the treatment effect in these specifications will be estimated only against observations with active and ongoing treatment. That means that for the late dynamic treatment effects the number of still treated observations goes down as ministers drop out of office, and naturally the precision of the estimates decreases. For the same reason it is sensible that the measured effect are more positive vis-à-vis our baseline estimator, that estimates the persistent effect of ever having been treated and as such combines the effects of still treated and not anymore treated observations.

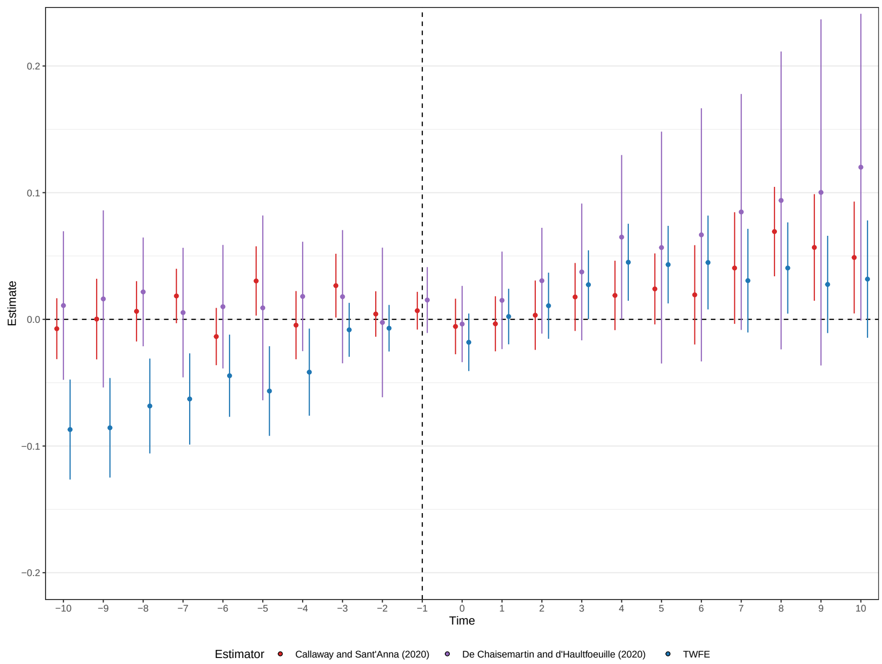


Figure 2: ROBUSTNESS OF DYNAMIC TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: NIGHT-LIGHTS. This figure displays event-studies that examine the relationship between the birthplaces of ministers in power and the (logarithm of) nightlight output at the grid-level. The estimators utilized include the dynamic version of the TWFE model (blue), Callaway and Sant'Anna (2021) (red), and De Chaisemartin and d'Haultfoeuille (2020) (purple). These estimates were computed using the *did2s* R package. Cell-level covariates include the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Comparison groups were defined by the default settings: not-yet treated and never-treated entities (cells). The x-axis represents time, measured in years, with the vertical reference line indicating the reference period. The bars on the plot represent 95 percent confidence intervals, with standard errors clustered at the cell level.

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.2 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, defining medium prestige ministers as treatment, 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 medium and high prestige categories drive the results. For our worldwide sample we observe an ATT of 7.4% 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 for the World sample are statistically insignificant.

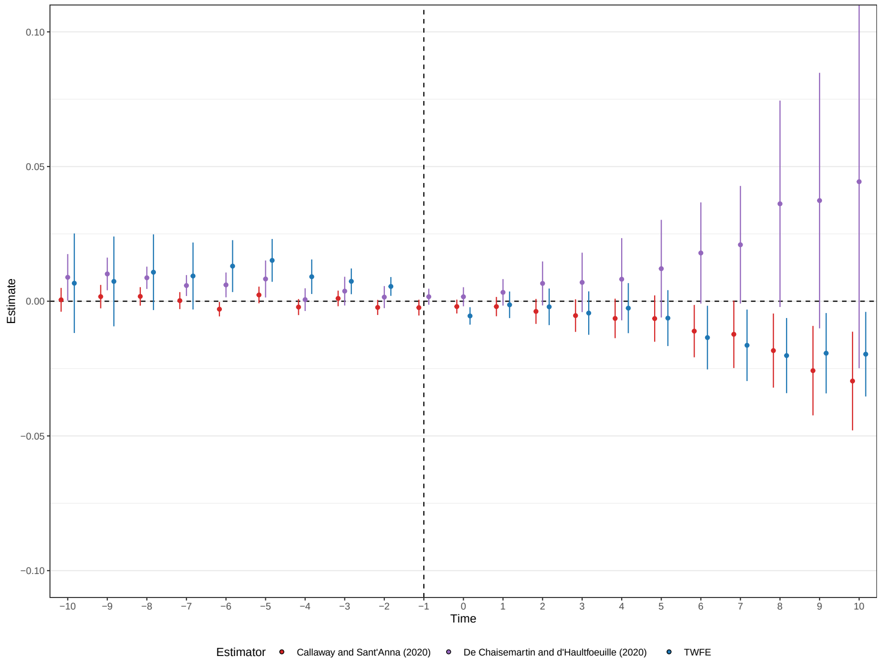


Figure 3: ROBUSTNESS OF DYNAMIC TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: POPULATION. This figure displays event-studies that examine the relationship between the birthplaces of ministers in power and the (logarithm of) population at the grid-level. The estimators utilized include the dynamic version of the TWFE model (blue), Callaway and Sant'Anna (2021) (red), and De Chaisemartin and d'Haultfoeuille (2020) (purple). These estimates were computed using the *did2s* R package. Cell-level covariates include the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Comparison groups were defined by the default settings: not-yet treated and never-treated entities (cells). The x-axis represents time, measured in years, with the vertical reference line indicating the reference period. The bars on the plot represent 95 percent confidence intervals, with standard errors clustered at the cell level.

In line with our baseline results of the dynamic aggregated approach, we observe particular large estimates for Africa (ATT: 14.6%) for the high prestige portfolios (dynamic, column 2). For the medium prestige portfolios we identify an aggregated estimate for Africa of 14.9%.

For Asia, the results however suggest that treatment effects are particularly negative in birth places of ministers associated with less prestigious positions (dynamic and simple, columns 3 and 4). The pretrends of the corresponding event study plot (Figure A.8) nevertheless imply that these results should be taken with a pinch of salt.

It is notable that we overall find the largest positive estimates in the high and medium prestige categories. 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.

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.

Table 3: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS BY PRESTIGE LEVEL

		Dependent variable: luminosity				
Aggregation method	Prestige	World	Africa	Europe	Asia	Americas
simple	High	0.057	0.119***	-0.109	0.048	-0.018
		(0.206)	(0.041)	(0.162)	(0.054)	(0.025)
	Medium	0.039**	0.117***	-0.079	0.013	-0.012
		(0.017)	(0.034)	(0.062)	(0.037)	(0.029)
	Low	-0.041	0.022	0.024	-0.224**	-0.105***
		(0.053)	(0.085)	(0.071)	(0.112)	(0.037)
dynamic	High	0.067	0.146***	-0.076	0.067	-0.031
		(0.310)	(0.051)	(0.140)	(0.105)	(0.031)
	Medium	0.074***	0.149***	-0.053	0.042	-0.027
		(0.024)	(0.039)	(0.060)	(0.074)	(0.033)
	Low	-0.029	0.018	0.093	-0.212**	-0.143***
		(0.053)	(0.083)	(0.092)	(0.099)	(0.051)
Observations	High	967,000	216,300	325,750	251,550	173,400
	Medium	960,675	212,225	324,925	250,875	172,650
	Low	973,625	221,150	326,100	252,125	174,250

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses. Dynamic treatment effects are displayed in Figures A.6, A.7, and A.8.

On a global scale, we find that defense and foreign ministers seem to be subjected to favoritism (dynamic, column 1), with the results for defense being particularly driven by the Asian subsample (dynamic, column 4).

Among our 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 have sizable estimates that are also statistically significant (dynamic, column 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 13.3% for the African and 19.3% 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.[7] 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.

[7] 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).

Table 4: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS OF HIGH PRESTIGE PORTFOLIOS

		Dependent variable: luminosity				
Aggregation method	Portfolio	World	Africa	Europe	Asia	Americas
simple	Defense	0.093*	0.086	0.051	0.171***	-0.073
		(0.051)	(0.102)	(0.064)	(0.064)	(0.068)
	Foreign	0.086**	0.080	-0.005	0.075	0.071
		(0.038)	(0.062)	(0.094)	(0.067)	(0.246)
	Finance	0.056	0.112*	0.119	0.007	-0.060
		(0.073)	(0.059)	(0.135)	(0.084)	(0.057)
	Interior	0.030	0.040	0.116	0.158	-0.006
		(0.043)	(0.046)	(0.099)	(0.115)	(0.058)
dynamic	Defense	0.116*	0.074	0.119	0.290***	-0.137
		(0.058)	(0.163)	(0.090)	(0.105)	(0.094)
	Foreign	0.102**	0.133*	0.098	0.193**	0.243
		(0.044)	(0.076)	(0.070)	(0.085)	(0.190)
	Finance	0.083	0.153***	0.143	-0.058	-0.070
		(0.093)	(0.069)	(0.155)	(0.100)	(0.063)
	Interior	0.042	0.065	0.117	0.236	-0.014
		(0.048)	(0.050)	(0.115)	(0.159)	(0.065)
Observations	Defense	973,550	221,400	326,000	252,050	174,100
	Foreign	972,775	220,800	326,000	252,100	173,875
	Finance	973,350	221,075	326,050	252,150	174,075
	Interior	973,300	220,900	326,175	252,200	174,025

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses. Dynamic treatment effects are displayed in Figures A.9, A.10 and A.11.

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. When 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: luminosity				
Aggregation method		World	Africa	Europe	Asia	Americas
simple	Autocracy	0.085***	0.106***	0.056	0.019	-0.022
		(0.027)	(0.035)	(0.044)	(0.038)	(0.037)
	Democracy	-0.034	0.079	-0.036	0.011	-0.024
		(0.024)	(0.061)	(0.041)	(0.054)	(0.024)
dynamic	Autocracy	0.116***	0.141***	0.085*	0.045	-0.035
		(0.031)	(0.051)	(0.048)	(0.052)	(0.038)
	Democracy	-0.011	0.229*	-0.022	0.190	-0.026
		(0.047)	(0.131)	(0.068)	(0.116)	(0.036)
Observations	Autocracy	961,350	211,325	325,600	250,850	173,575
	Democracy	971,875	221,750	325,500	251,850	172,775

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses. Dynamic treatment effects are displayed in Figure A.12 and Figure A.13.

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, and the African and European sub-sample. For democratic settings we observe a close to zero result for the full sample. The sub-sample analysis reveals some tentative evidence for sizeable positive effects in democratic countries of the African and Asian continent, however both come with large standard errors attached to them.

Conceptually it is not unambiguous 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 OECD versus non-OECD

Membership in the Organisation for Economic Co-operation and Development (OECD) serves as an indication of a country's economic, political, and social status. Typically, OECD members are high-income economies with a high Human Development Index. These nations are typically democratic with market-based economies, regulated by international standards and norms set by the OECD. Thus, an OECD membership not only indicates economic prosperity but also reflects a country's commitment to democratic principles, free market practices, and global policy cooperation.

Table 6: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS BY OECD MEMBERSHIP

	Dependent variable: luminosity	
Aggregation method	OECD	Non-OECD
simple	-0.092	0.062***
	(0.061)	(0.018)
dynamic	-0.054	0.093***
	(0.059)	(0.024)
Observations	60,350	897,000

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses.

We divide our sample of countries into OECD and non-OECD members and reestimate the baseline equations. Table 6 reports the results. We detect evidence for regional favoritism as indicated by nightlight intensity only in non-OECD countries. This finding is a further puzzle piece pointing to the role robust institutions play in constraining the ability of politicians to redistribute resources to their birth places. OECD countries generally have stronger institutions and governance structures, as well as higher levels of transparency, all of which can help deter regional favoritism.

It is important to note that nighttime light luminosity may not serve as an effective measure of economic development in industrialized countries, such as OECD-countries. This observation does not dismiss the validity of nighttime light luminosity as a global indicator; rather, it suggests that its interpretative power may be limited in the context of highly developed economies.

Using luminosity as an indicator of economic activity, is a method particularly useful in developing countries. However, in industrialized nations, it might not serve as an accurate measure due to several reasons. First, these countries typically have widespread and uniformly high illumination, making it challenging to spot differences in economic activity based on nightlight data alone. This is due to the fact that nightlight data in dense urban areas is top coded. Second, energy efficiency measures and regulations against light pollution can further reduce the perceived nightlight output. Third, significant service and digital sectors in these countries may not correlate with high nightlight output. Therefore, while nightlight output may be useful in certain contexts, it may not accurately represent economic development in industrialized countries (Gibson et al., 2021).

4.2.4 Corruption

To further infer information from the institutional setting, we employ the corruption perception index (CPI) developed by Transparency International. The CPI is designed to measure the perceived levels of public sector corruption in countries worldwide.

In essence, it is a composite index, i.e. made up of various other indices from different sources. These sources might include surveys of business people or assessments by country experts, all of whom are asked to rate countries on their perceived levels of corruption.

Scores on the CPI range from 0 to 100, where 0 means that a country is perceived as highly corrupt, and 100 means that a country is perceived as very clean, or having very low levels of corruption.[8]

We divide our sample into less corrupt and more corrupt countries, using an index value of 50 as the threshold. The results are summarized in Table 7. Our analysis reveals that regions with higher perceived corruption are more likely to exhibit regional favoritism (dynamic, column 2). This evidence suggests that corruption may considerably influence resource allocation among the ruling elite, particularly in environments with less robust institutions. The coefficients in column 1 suggest that less corrupt countries may experience reverse favoritism. However, all regions coded as less corrupt fall within industrialized countries, where nighttime light as a measure for economic development carries certain limitations, as explained earlier. Consequently, this result should be interpreted with caution.

4.2.5 Women ministers

The WhoGov dataset 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). Hence, we redefine the treatment variables in our main specification based on the gender of the ministers, i.e., we estimate the potential impact on a pixel of having ever been the birthplace of a woman minister compared to all other pixels, including birthplaces of male ministers.

[8] It is important to note that the CPI measures perceptions of corruption, rather than actual levels of corruption. The rationale behind this is that corruption generally happens behind closed doors and is therefore difficult to measure directly. Perceptions, on the other hand, can be gauged through surveys and expert assessments and can provide valuable insights into the relative levels of corruption in different countries.

Table 7: TREATMENT EFFECTS IN MINISTER BIRTH PIXELS BY CORRUPTION

	Dependent variable: luminosity	
Aggregation method	Less corrupt	More corrupt
simple	-0.198	0.059***
	(0.149)	(0.015)
dynamic	-0.296***	0.099***
	(0.115)	(0.023)
Observations	24,900	932,450

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses.

In each estimation within this subsection, we further adjust our specification by adding dummies to our covariates matrix that account for birthplace pixels of male ministers.

Table 8: TREATMENT EFFECTS IN FEMALE MINISTER BIRTH PIXELS

	Dependent variable: luminosity				
Aggregation method	(1) World	(2) Africa	(3) Europe	(4) Asia	(5) Americas
simple	-0.050	-0.003	-0.141	-0.148**	0.004
	(0.033)	(0.038)	(0.123)	(0.065)	(0.033)
dynamic	-0.074	-0.015	-0.155	-0.174*	-0.047
	(0.043)	(0.052)	(0.160)	(0.077)	(0.039)
Observations	974,050	221,875	325,925	252,150	174,100

The dependent variable is specified in logarithmic form. The method of aggregation *simple* is defined by Equation 3, and the *dynamic* aggregation is defined by Equation 4. To limit the duration of a plausible treatment effect on the outcome, we constrain the *dynamic* aggregation to 20 post-treatment periods. All estimations include covariates identifying the birth pixels of a country's primary rulers, capital cities, and a country-specific factor variable. Stars denote significance levels at 10%(*), 5%(**), and 1%(***). Heteroscedasticity-robust standard errors, which are clustered at the pixel level, are presented in parentheses.

The results in Table 8 suggest that women ministers do not engage in regional favoritism. This finding aligns with literature positing that greater representation of women enhances institutional quality by reducing corruption (Hessami and Da Fonseca, 2020). However, there may be a potential limitation related to statistical power, given that only around 10% of cabinet members in our sample are women. Despite this, the variation and size of our sample lend credibility to our findings.

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 9.4% and up to 18.7% in the African sub-sample, into average local GDP growth of 2.8% to 5.6%. 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 may be constrained under more democratic institutions.

References

Albouy, D. (2013). Partisan Representation in Congress and the Geographic Distribution of Federal Funds. *Review of Economics and Statistics*, 95(1):127–141.

Alesina, A., Michalopoulos, S., and Papaioannou, E. (2016). Ethnic inequality. *Journal of Political Economy*, 124:428–488.

Alesina, A. and Tabellini, G. (2007). Bureaucrats or Politicians? Part I: A Single Policy Task. *American Economic Review*, 97(1):169–179.

Alesina, A. and Tabellini, G. (2008). Bureaucrats or politicians? Part II: Multiple policy tasks. *Journal of Public Economics*, 92(3):426–447.

Asatryan, Z., Baskaran, T., Birkholz, C., and Gomtsyan, D. (2021a). Favoritism and Firms: Micro evidence and macro implications. Published: ZEW Discussion Paper 21-031.

Asatryan, Z., Baskaran, T., Birkholz, C., and Hufschmidt, P. (2021b). Regional Redistribution of Mineral Resource Wealth in Africa. SSRN Scholarly Paper ID 3820533, Social Science Research Network, Rochester, NY.

Asatryan, Z., Baskaran, T., Hufschmidt, P., and Stöcker, A. (2021c). Regional favoritism and human capital accumulation in Africa. Published: ZEW Discussion Paper 21-030.

Asatryan, Z. and Havlik, A. (2020). The political economy of multilateral lending to European regions. *The Review of International Organizations*, 15(3):707–740. Publisher: Springer.

Baker, A. C., Larcker, D. F., and Wang, C. C. Y. (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, 144(2):370–395.

Bandyopadhyay, S. and Green, E. (2019). Roads and regional favoritism in sub-Saharan Africa. Published: LSE International Development Working Paper 19-195.

Barro, R. J. (1973). The control of politicians: an economic model. *Public Choice*, 14:19–42.

Baskaran, T. and da Fonseca, M. L. (2021). Appointed public officals and local favoritism: evidence from the German States. *Journal of Urban Economics*, 124:103354.

Baskaran, T. and Hessami, Z. (2017). Political alignment and intergovernmental transfers in parliamentary systems: evidence from Germany. *Public Choice*, 171:75–98.

Besley, T. (2005). Political Selection. *Journal of Economic Perspectives*, 19:43–60.

Besley, T. and Coate, S. (2003). Elected versus Appointed Regulators: Theory and Evidence. *Journal of the European Economic Association*, 1(5):1176–1206.

Borusyak, K., Jaravel, X., and Spiess, J. (2022). Revisiting Event Study Designs: Robust and Efficient Estimation.

Bruederle, A. and Hodler, R. (2018). Nighttime lights as a proxy for human development at the local level. *PloS one*, 13(9):e0202231. Publisher: Public Library of Science.

Burgess, R., Jedwab, R., Miguel, E., Morjaria, A., and Padró i Miquel, G. (2015). The Value of Democracy: Evidence from Road Building in Kenya. *American Economic Review*, 105(6):1817–1851.

Callaway, B. and Sant'Anna, P. H. C. (2021). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Carozzi, F. and Repetto, L. (2016). Sending the pork home: Birth town bias in transfers to Italian municipalities. *Journal of Public Economics*, 134:42–52.

Cox, G. W. (2010). Swing voters, core voters, and distributive politics. In Kirshner, A. S., Wood, E. J., Shapiro, I., and Stokes, S. C., editors, *Political Representation*, pages 342–357. Cambridge University Press, Cambridge.

Cox, G. W. and McCubbins, M. D. (1986). Electoral Politics as a Redistributive Game. *The Journal of Politics*, 48(2):370–389. Publisher: The University of Chicago Press.

de Chaisemartin, C. and D'Haultfœuille, X. (2020). Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review*, 110(9):2964–2996.

Do, Q.-A., Nguyen, K.-T., and Tran, A. (2017). One mandarin benefits the whole clan: hometown favoritism in an authoritarian regime. *American Economic Journal: Applied Economics*, 9:1–29.

Dreher, A., Fuchs, A., Hodler, R., Parks, B. C., Raschky, P. A., and Tierney, M. J. (2021). Is favoritism a threat to chinese aid effectiveness? A subnational analysis of chinese development projects. *World Development*, 139:105291.

Francois, P., Rainer, I., and Trebbi, F. (2015). How Is Power Shared in Africa? *Econometrica*, 83(2):465–503. _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA11237.

Ghosh, T., Baugh, K. E., Elvidge, C. D., Zhizhin, M., Poyda, A., and Hsu, F.-C. (2021). Extending the DMSP Nighttime Lights Time Series beyond 2013. *Remote Sensing*, 13(24):5004. Number: 24 Publisher: Multidisciplinary Digital Publishing Institute.

Gibson, J., Olivia, S., Boe-Gibson, G., and Li, C. (2021). Which night lights data should we use in economics, and where? *Journal of Development Economics*, 149:102602.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Henderson, J. V., Storeygard, A., and Weil, D. N. (2012). Measuring economic growth from outer space. *American Economic Review*, 102:994–1028.

Hessami, Z. and Da Fonseca, M. L. (2020). Female political representation and substantive effects on policies: A literature review. *European Journal of Political Economy*, 63:101896.

Hodler, R. and Raschky, P. A. (2014). Regional favoritism. *The Quarterly Journal of Economics*, 129(2):995–1033.

House, F. (2019). Freedom in the world 2019. Published: www.freedomhouse.org.

Khalil, U., Oak, M., and Ponnusamy, S. (2021). Political favoritism by powerful politicians: Evidence from chief ministers in India. *European Journal of Political Economy*, 66:101949.

Krook, M. L. and O'Brien, D. Z. (2012). All the President's Men? The Appointment of Female Cabinet Ministers Worldwide. *The Journal of Politics*, 74(3):840–855.

Martínez, L. R. (2022). How much should we trust the dictator's GDP growth estimates? *Journal of Political Economy*, forthcoming.

Maskin, E. and Tirole, J. (2004). The Politician and the Judge: Accountability in Government. *American Economic Review*, 94(4):1034–1054.

Mayer, T. and Zignago, S. (2011). Notes on CEPII's distances measures: The GeoDist database. Published: CEPII WP No. 2011 – 25.

Michalopoulos, S. and Papaioannou, E. (2016). The long-run effects of the scramble for Africa. *American Economic Review*, 106:1802–1848.

Nyrup, J. and Bramwell, S. (2020). Who Governs? A New Global Dataset on Members of Cabinets. *American Political Science Review*, 114(4):1366–1374. Publisher: Cambridge University Press.

Stevens, F. R., Gaughan, A. E., Linard, C., and Tatem, A. J. (2015). Disaggregating Census Data for Population Mapping Using Random Forests with Remotely-Sensed and Ancillary Data. *PLOS ONE*, 10(2):e0107042. Publisher: Public Library of Science.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

United Nations Conference on Trade and Development (2022). *World Investment Report 2022: International Tax Reforms and Sustainable Investment*. United Nations Conference on Trade and Development (UNCTAD) World Investment Report (WIR). United Nations.

Weingast, B., Shepsle, K., and Johnsen, C. (1981). The political economy of costs and benefits: a neoclassical approach to distributive politics. *Journal of Political Economy*, 89(4):642–664.

Widmer, P. and Zurlinden, N. (2022). Ministers Engage in Favoritism Too. *Journal of Public Economics*, 213:104707.

Appendix

A.1 Data

Table A.1: 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	1990-2016
Kyrgyz Republic	Asia	1990-2016
Lao PDR	Asia	1990-2016
Lebanon	Asia	1990-2016
Malaysia	Asia	1990-2016
Mongolia	Asia	1990-2016
Myanmar	Asia	1990-2016
Nepal	Asia	2006-2016
Pakistan	Asia	2006-2016
Philippines	Asia	2006-2016

Sri Lanka	Asia	2006-2016
Tajikistan	Asia	1990-2016
Thailand	Asia	2006-2016
Timor-Leste	Asia	2006-2016
Turkey	Asia	2006-2016
Uzbekistan	Asia	1990-2016
Vietnam	Asia	2006-2016
Yemen	Asia	2006-2016
Albania	Europe	1990-2016
Austria	Europe	1990-2016
Belarus	Europe	1990-2016
Belgium	Europe	1990-2016
Bosnia and Herzegovina	Europe	1990-2016
Bulgaria	Europe	1990-2016
Croatia	Europe	1990-2016
Czech Republic	Europe	1990-2016
Denmark	Europe	1990-2016
Estonia	Europe	1990-2016
Finland	Europe	1990-2016
France	Europe	1990-2016
Germany	Europe	1990-2016
Greece	Europe	1990-2016
Hungary	Europe	1990-2016
Italy	Europe	1990-2016
Lithuania	Europe	1990-2016
Moldova	Europe	1990-2016
Montenegro	Europe	1990-2016
North Macedonia	Europe	1990-2016
Norway	Europe	1990-2016
Netherlands	Europe	1990-2016
Poland	Europe	1990-2016
Portugal	Europe	1990-2016
Romania	Europe	1990-2016
Russia	Europe	1990-2016
Slovak Republic	Europe	1990-2016
Slovenia	Europe	1990-2016
Spain	Europe	1990-2016
Sweden	Europe	1990-2016
Ukraine	Europe	1990-2016
United Kingdom	Europe	1990-2016

Canada	North America	1990-2016
Costa Rica	North America	1990-2016
Dominican Republic	North America	2005-2018
El Salvador	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
United States	North America	1990-2016
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	1990-2018
Guyana	South America	1990-2016
Paraguay	South America	1990-2016
Peru	South America	1990-2016
Suriname	South America	1990-2018
Trinidad and Tobago	South America	1990-2018
Uruguay	South America	1990-2016
Venezuela	South America	1990-2016
Australia	Oceania	1990-2016
Fiji	Oceania	1990-2016
New Zealand	Oceania	1990-2016
Solomon Islands	Oceania	1990-2016

A.2 Descriptive statistics

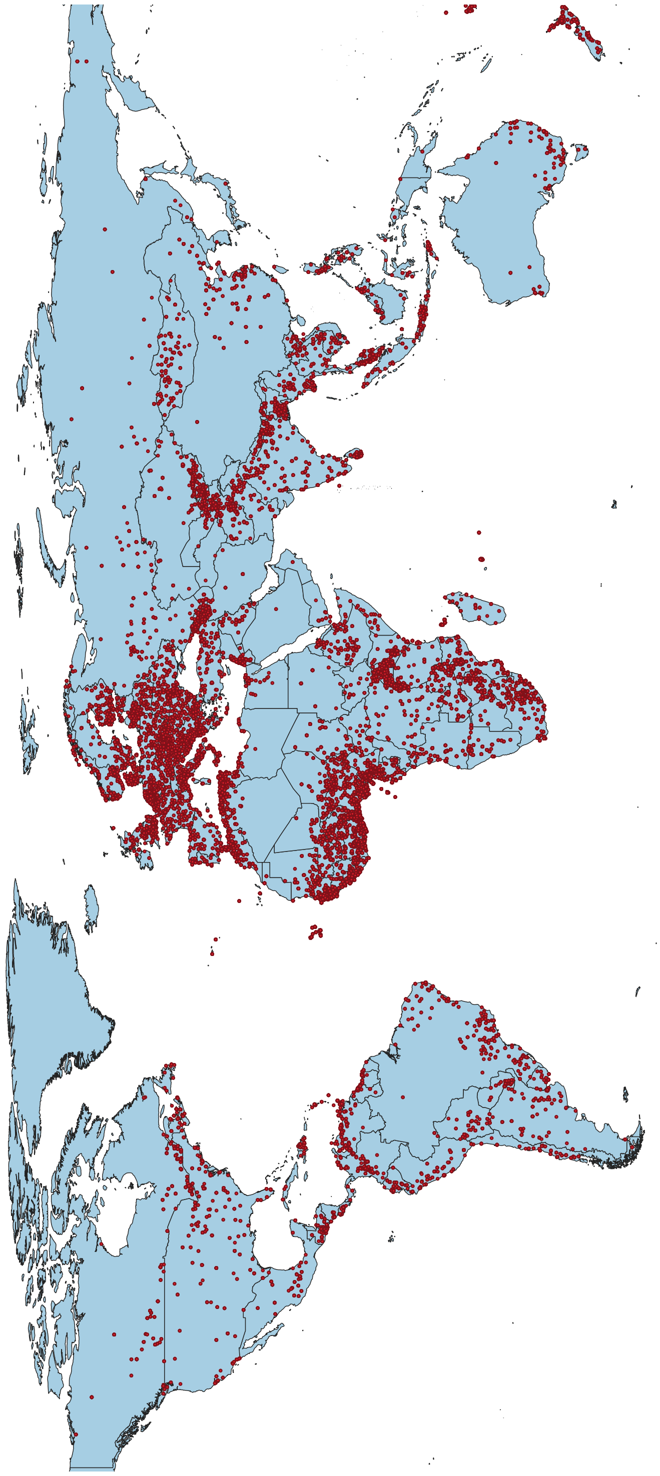


Figure A.1: BIRTH PLACES OF CABINET MEMBERS. The dots in this figure represent the location of collected birth places of cabinet members in our sample of the world.

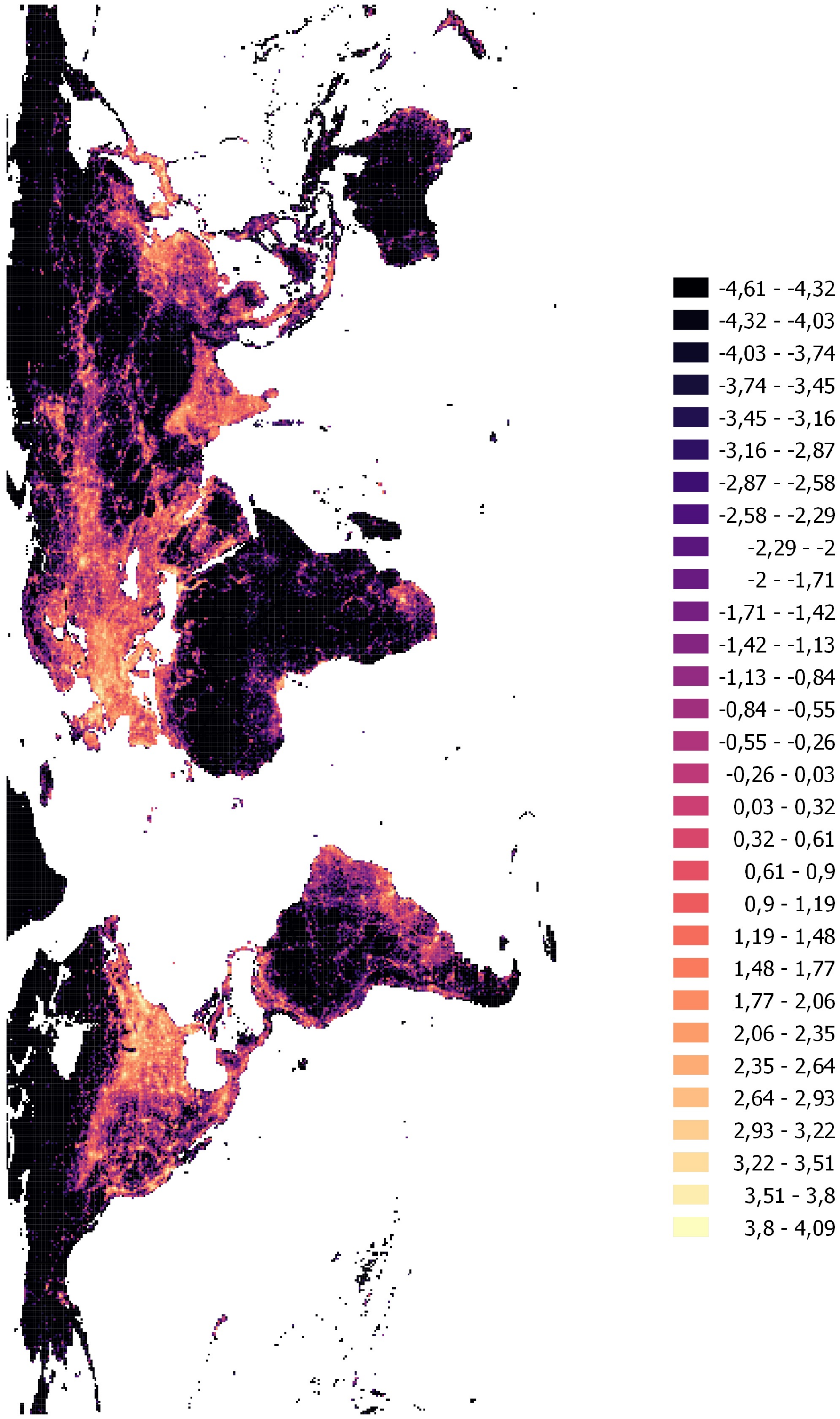


Figure A.2: GRID OF THE WORLD DISPLAYING MEAN NIGHT LIGHT INTENSITY. This figure shows (the logarithm) of mean night light output for the period of our sample (1990-2016). The values for the pixels were computed by extracting information from the night light raster files based on the grid of the World 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.

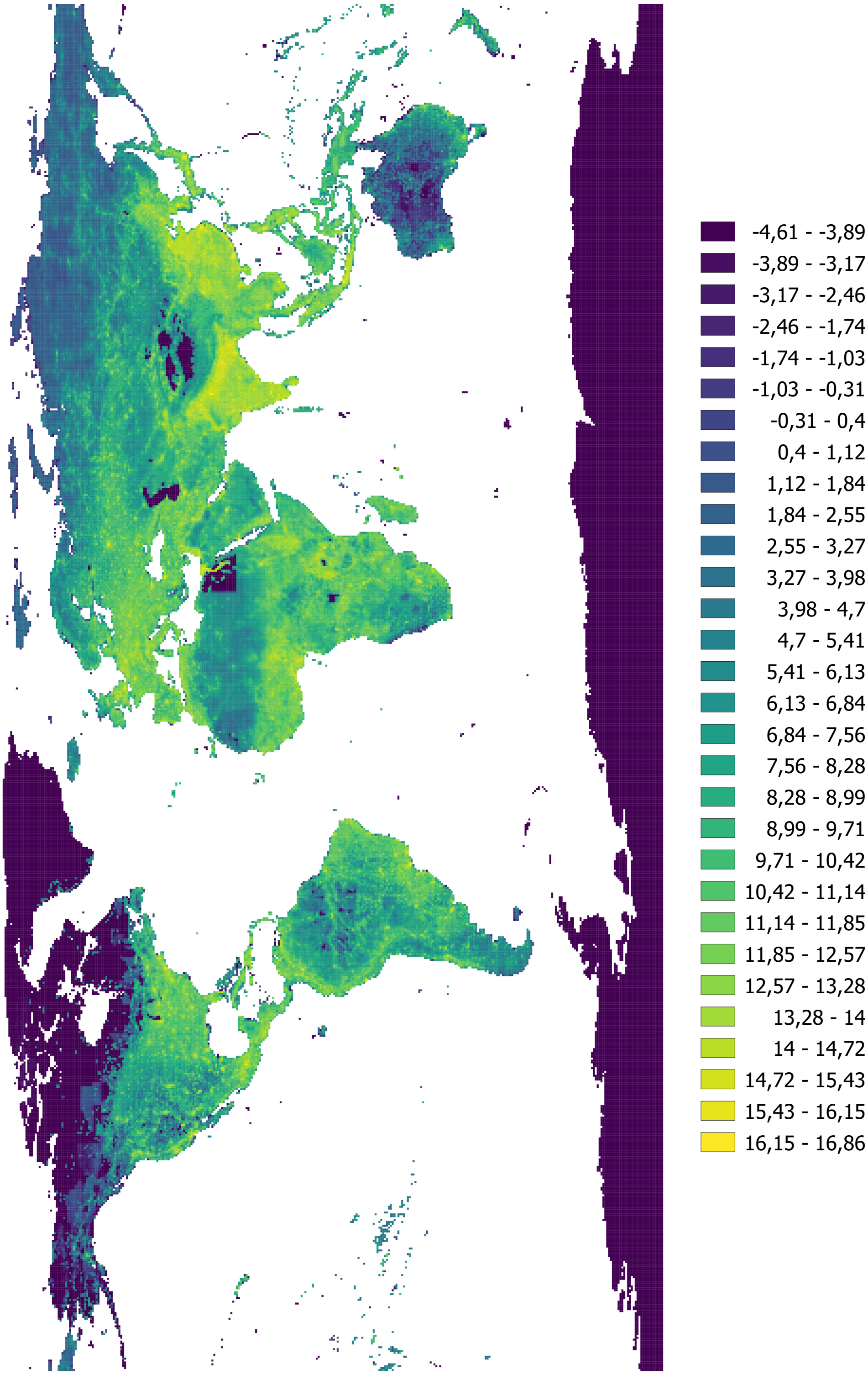


Figure A.3: GRID OF THE WORLD DISPLAYING MEAN POPULATION. This figure shows (the logarithm of) sum population for the period (2000-2016). The values for the pixels were computed by extracting information from the population raster files based on the grid of the World utilized in our empirical analysis. For this process we used the *exactextractr* R package. Brighter cells indicate higher population numbers. The corresponding values are tabulated in the legend.

A.3 Baseline results

Figure A.4: DYNAMIC TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: NIGHTLIGHTS.

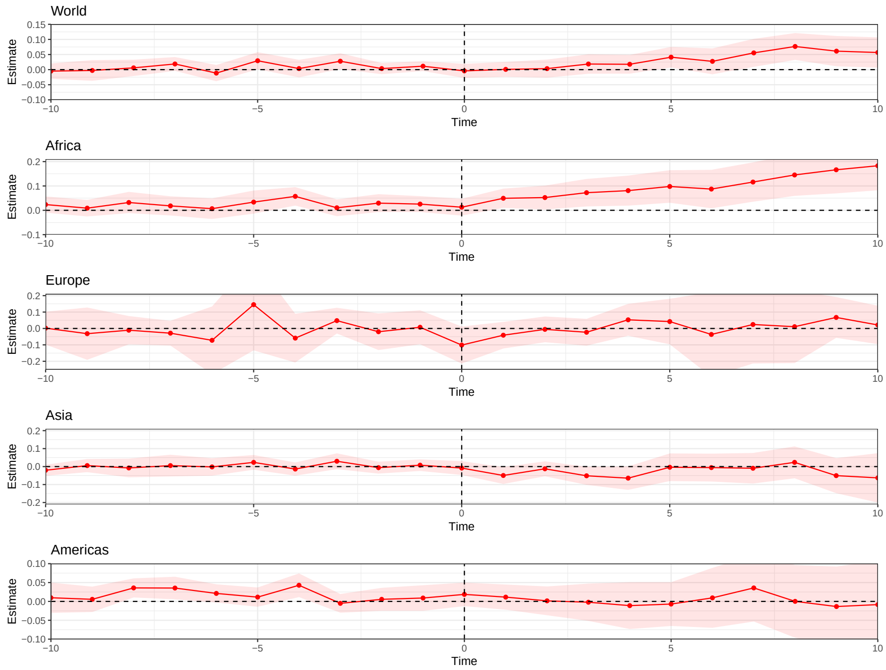
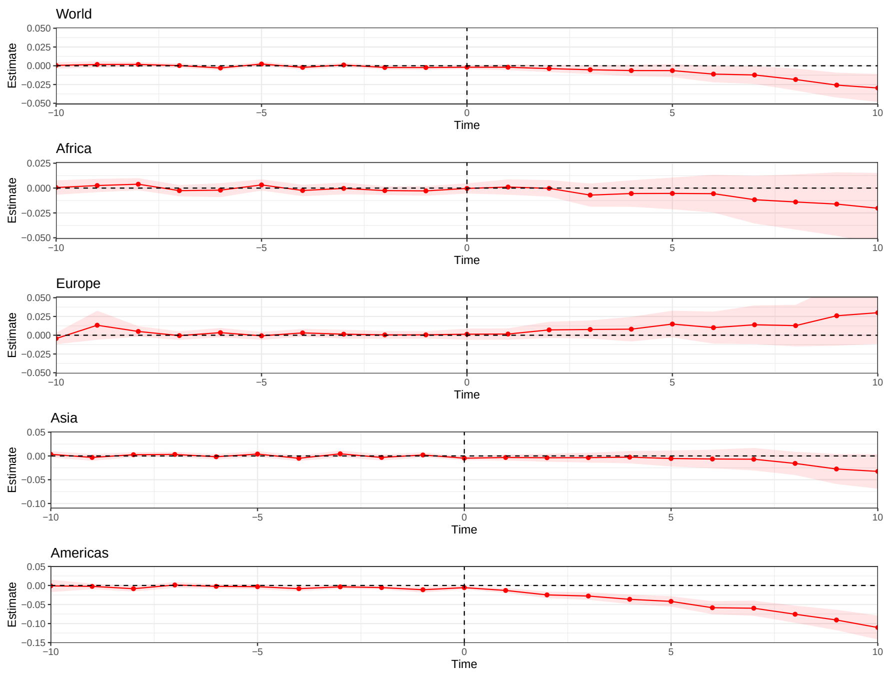


Figure A.5: DYNAMIC TREATMENT EFFECTS IN MINISTER BIRTH PIXELS: POPULATION.



A.4 Extension and mechanisms

A.4.1 Prestige levels and portfolios

Table A.2: 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.6: DYNAMIC TREATMENT EFFECTS IN HIGH PRESTIGE MINISTER BIRTH PIXELS.

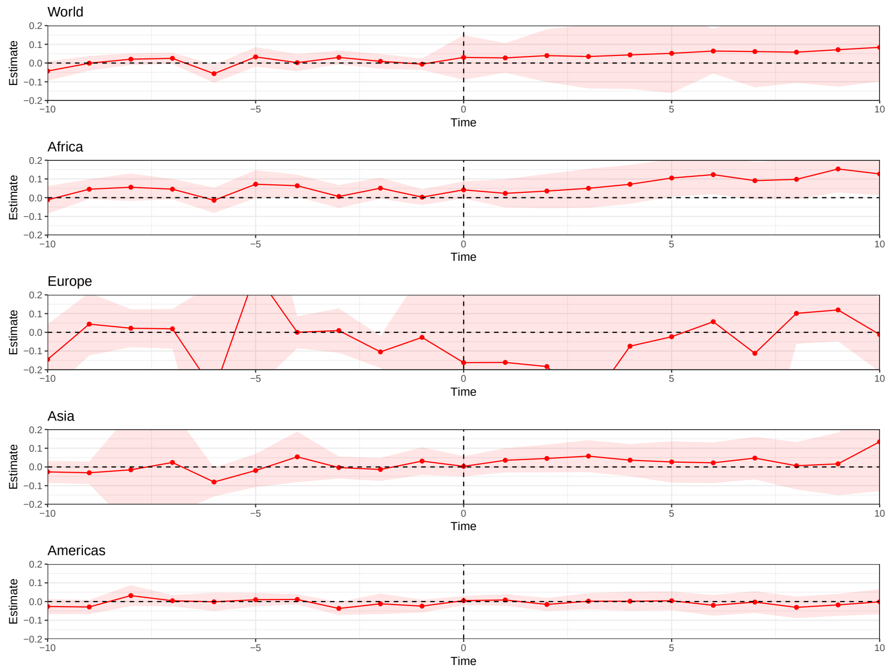


Figure A.7: DYNAMIC TREATMENT EFFECTS IN MEDIUM PRESTIGE MINISTER BIRTH PIXELS.

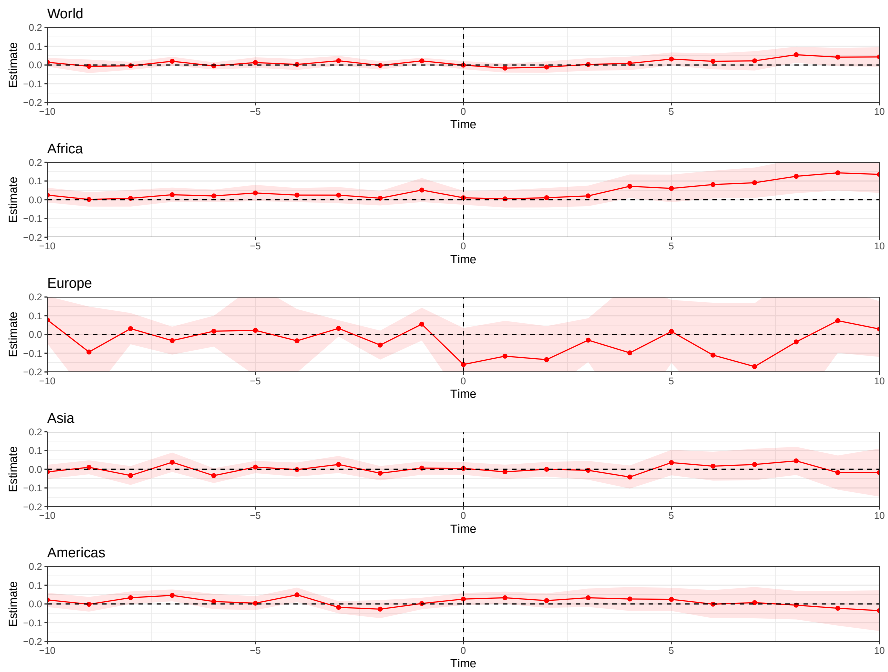


Figure A.8: DYNAMIC TREATMENT EFFECTS IN LOW PRESTIGE MINISTER BIRTH PIXELS.

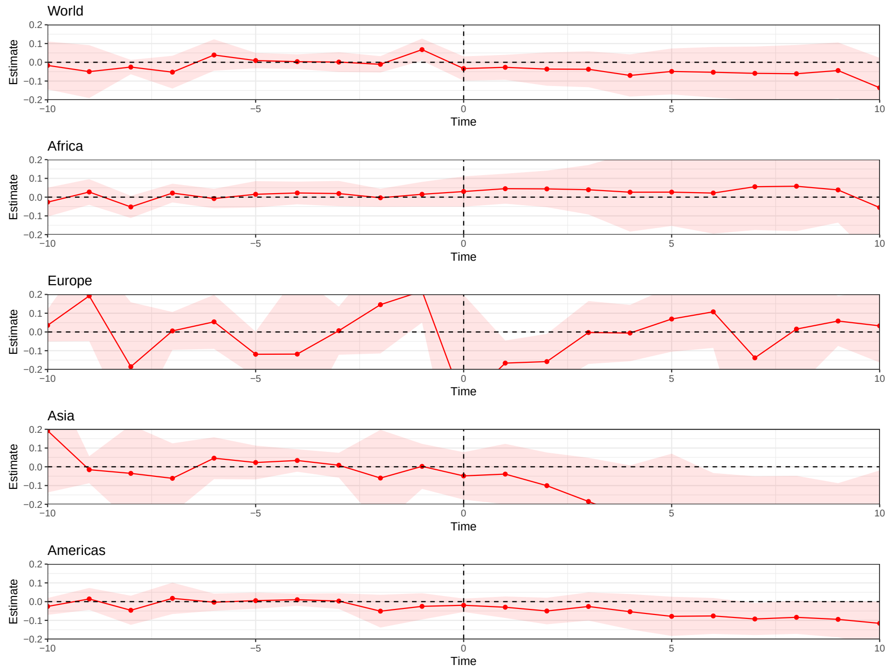


Figure A.9: DYNAMIC TREATMENT EFFECTS IN DEFENSE MINISTER BIRTH PIXELS.

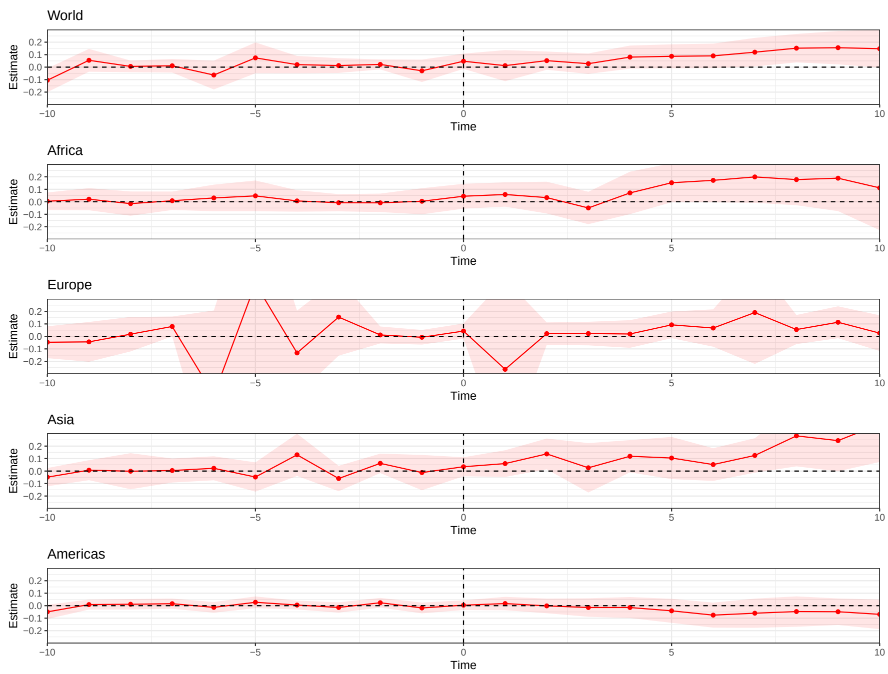


Figure A.10: Dynamic treatment effects in foreign minister birth pixels.

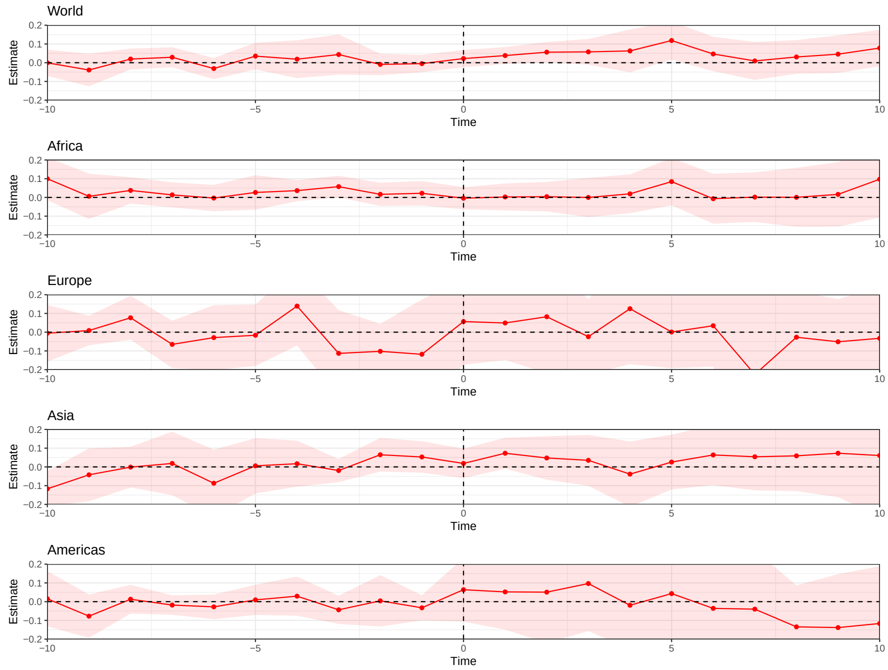
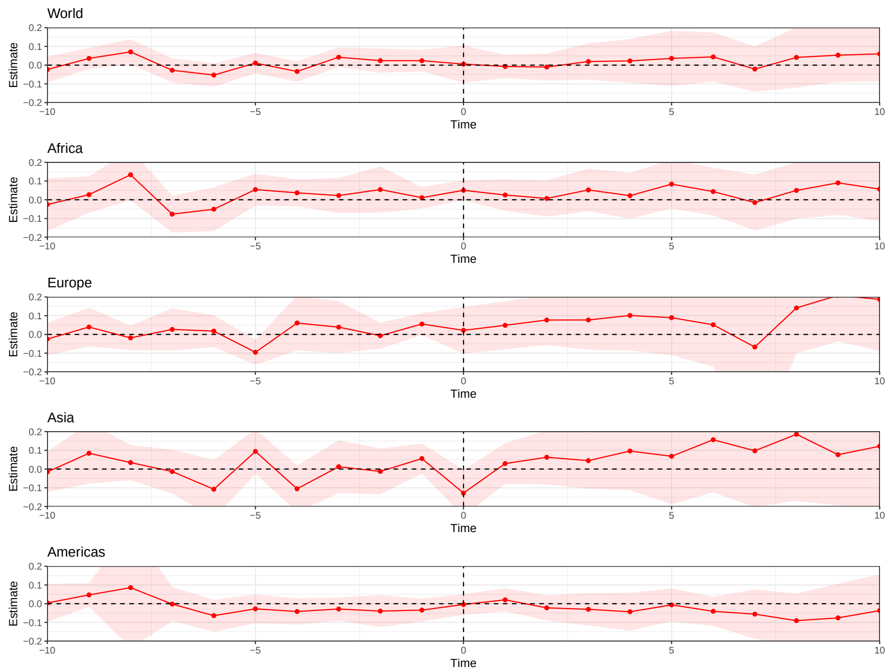


Figure A.11: Dynamic treatment effects in finance minister birth pixels.



A.4.2 Institutions

Figure A.12: DYNAMIC TREATMENT EFFECTS: MINISTERS IN AUTOCRACIES.

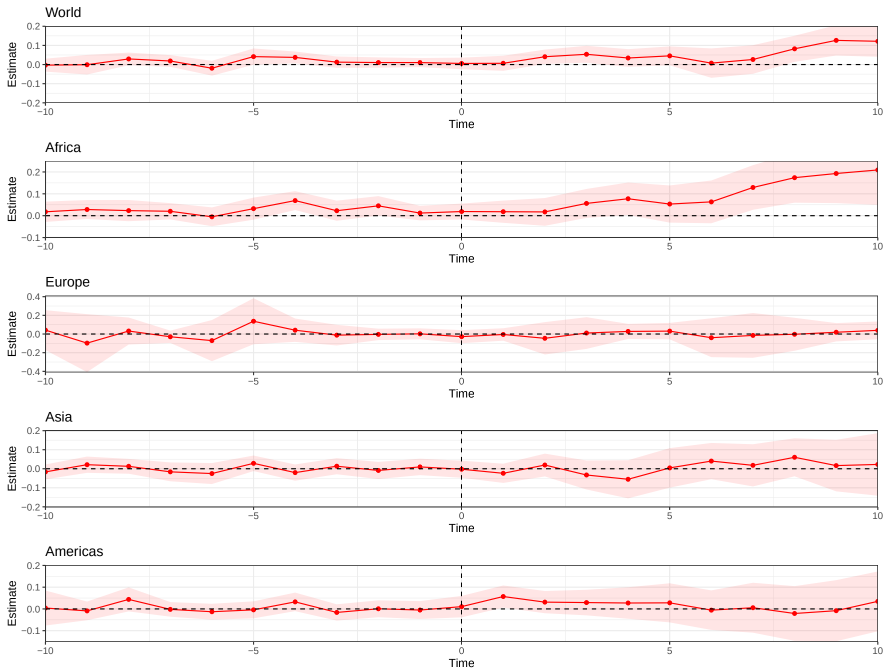


Figure A.13: DYNAMIC TREATMENT EFFECTS: MINISTERS IN DEMOCRACIES.