Closing the Gender Gap in Climate

Can Electing More Women into Office Narrow the Climate Divide?

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Abstract

I seek to identify the causal effect that electing higher proportions of women in national legislatures in African and Arab nations has on yearly per capita CO_2 emissions. I utilize a two-stage difference-in-difference approach on 64 different countries throughout Africa and the Middle East from 1998-2022. To eliminate the potential endogeneity in using the proportion of women in national legislatures as a treatment on per capita CO_2 emissions, I use years since women were granted suffrage in each respective country as an instrumental variable. One nominal paper in the literature has shown evidence that increased proportions of women in parliaments are more than likely causally related to stricter climate policies (Mavisakalyan & Tarverdi, 2019). However, this paper does not take into account how carbon emissions and women in government have evolved. To my knowledge, little research has been done to show an intertemporal causal link between women in parliaments and its effects on climate. I expand on this research by showing how carbon emissions have changed over time to elicit a local average treatment effect between changes in gender compositions at the national legislature level. When the average proportion of women in national legislatures in African and Arab countries increases by 1%, I find a significant effect that total yearly (gigatons of) CO_2 emissions per capita decrease by 1.08 (-1.64, -0.51), on average. To enforce the credibility of significance, I run a placebo regression, using the number of natural disasters[1] that occur within a country in a particular year as an effective placebo.

[1]As classified and recorded by the Centre for Research on the Epidemiology of Disasters (CRED).

Introduction

The current climate and economic research today overwhelmingly support the thesis that women—especially in developing countries—are disproportionately affected by the negative externalities of climate change (A1). If women are disproportionately affected by climate externalities, one would expect that female policymakers would enact effective climate policy at higher rates relative to other policymakers, assuming they have the means and power to do so. Thus, the causal question of interest reasonably follows as to whether increased proportions of women decrease CO_2 emissions. As we will see, the nature of causality of this question is rather difficult to elicit due to intricate endogeneities that persist throughout this problem. However, the focus of this paper falls on two important motives: (1) The necessity and obligation of answering the question at hand considering its implications on climate policy, and (2) The econometric implications of my identification strategy with its attempt to reasonably arrive at a causal effect[2]. The outline of this paper will proceed as follows: I will first introduce the data I collected for this study and assess its reliability. I will then discuss the identification strategy I use to estimate the local average treatment effect. I support this result with a placebo analysis, and then I will finish by discussing the results of my econometric analysis and its implications, and then conclude.

Data

The data used for this analysis was strategically gathered using publicly available sources[3]. The Inter-Parliamentary Union (IPU) maintains a consistent archive of records that monitors the number of women elected into the national legislature for each country. Not every government maintains the same structure of government, however, the IPU distinguishes between women elected into the lower house and upper house levels of government throughout multiple periods. I take the average

[2]As I will show in this paper, the causal effect that I identify is a local average treatment effect with regards to the instrumental variable that I invoke.

[3]Data was collected using ethical web-scraping methods or obtained via direct download. Further documentation and the scripts used to obtain the data used for this analysis can be found on this project's Github repository https://github.com/SamLeeBYU/Elections.

proportion of women holding office in the national legislature overall in a given year by taking a weighted average of the two houses[4][5]. I obtained emissions data, including total yearly (gigatons of) CO_2 emissions per capita per country, from the Emissions Database for Global Atmospheric Research (EDGAR) as maintained by the European Commission[6]. I obtained country-year-level specific covariates, such as population and GDP, from the World Bank[7]. I obtained the database linking each country's year they passed a right of universal suffrage from "A Lexical Index of Electoral Democracy" on the Harvard Dataverse (Skaaning et. al, 2015). I checked country-specific anomalies using research from the Pew Research Center[8] (Schaeffer, 2020). Finally, I used the U.S. Department of State's regional definitions to define which countries I define as either African or Arab.

By adjusting for time, country, and other specific endogenous effects on the proportion of women in government in a country (c) at year (t), I hope to show that effectively when the proportions of women in government increase, CO_2 emissions decrease. However, one potential problem with data is that I do not account for country-specific government intricacies that could prevent women or a legislature from passing effective climate policy—such as not accounting for government corruption or other unique anomalies (wars between countries, coups, laws that are discriminatory against women, etc, that prevent a government from acting efficiently)—at every year[9].

In my sample, the United Arab Emirates (UAE) and Saudia Arabia are excluded because they don't give full suffrage to women: The UAE held its first national elections in 2006, but only a select

[4]Note that econometrically, this assumes that on average, the power that women are given in each house of national legislature is equal to the 'weight' of women in that chamber of legislature: In other words, assuming a fair political system, I assume no chamber can systemically dominate the other chamber.

[5]A graphical illustration of the average proportion of women in the national legislatures for each country in my sample can be seen in Figure 1 of the Appendix.

[6]A graphical illustration of the yearly CO_2 emissions over time for each country in my sample can be seen in Figure 2 of the Appendix.

[7]Statistics for these key variables are summarized in Table 1 of the Appendix.

[8]Not all countries in Africa and the Middle East treat women equally. Even when suffrage is granted, it is not granted universally. Thus, the instrument may not influence the proportion of women in government the same in every country. This does not discredit my econometric analysis, however, so long as the monotonicity assumption of my instrument is met (which will be discussed hereafter). However, I exclude obvious anomalies in my analytic sample. See Table 2 in the Appendix for summary statistics for each country in my sample.

[9]To assert consistency in my estimates, econometrically, I assume that on average, these factors in the idiosyncratic term of my econometric model are 0 for any given country at year t. (See equation (3)).

amount of men and women were eligible to participate (Schaeffer, 2020). Saudi Arabia doesn't hold national elections[10]. This leaves us with a total of 64 different countries in the analytic sample. Merging the suffrage data with all the IPU available data and the CO_2 emissions data from EDGAR, I was left with data spanning 1997-2022[11].

Identification

To estimate the causal effect that electing more women in national legislatures in African and Arab countries have on reducing per capita CO_2 emissions, I employ a two-stage difference-in-difference identification strategy. In regards to curbing the negative externalities of climate change in the context of this paper, I use per capita CO_2 emissions as a response variable. Let Y_{ct} represent the total (gigatons of) carbon emissions per capita for a country (c) in a year (t). I seek to identify consistently and interpret the local average treatment effect of δ, the effect of higher proportion of women in national legislatures in African and Arab countries on national CO_2 emissions per capita. Research from Mirziyoyeva and Salahodjaev (2023) suggests that women in government promote economic growth. This could pose potential problems of reverse causality: We need to be certain that any effect we see on women's representation causes a reduction (or increase) in carbon emissions. If carbon emissions are highly correlated with economic growth and industrialization then an OLS regression (that is, using a typical difference-in-differences approach) on Y_{ct} to estimate δ would almost certainly be biased and inconsistent. To first eliminate the endogeneity for reverse causality—the supposition that perhaps lower carbon emissions cause a higher proportion of women to become elected during the same year; or rather, perhaps higher carbon emissions are correlated with other variables such as economic growth that are linked to higher proportions of women becoming elected—I lag the treatment variable of interest, W_{ct}, the average proportion of

[10]Women are, however, enfranchised at the local level government levels.

[11]Notably, the data is left as an unbalanced panel data set (See Table 2 in the Appendix). The World Bank does not have GDP or population data for every single year of every single country in the data, and critically, the IPU also notes that it cannot reliably retrieve the number of women in national legislatures for every year. These years for those specific countries are naturally excluded. I assume that the probability at which data for a specific year-country combination will be missing is essentially random. This assumption may be violated if countries that are systemically difficult to obtain data for (such as Somalia or the Central African Republic) is also correlated with other covariates and or CO_2 emissions.

women in national legislature for country c during year t one year. This implies that any change in Y_{ct} will inherently come *after* a change in W_{ct}. While I do not focus on specific regulatory practices within this paper, effective climate policy would consist of enacting particular policies and regulations that would reduce the CO_2 emissions by economic constraint. Hence, as a matter of the causal chain of inference, it could be that increased proportions of women in government could (causally) affect economic performance within a country at time t. Thus, to establish the causal chain of inference, I infer:

$$\sum_{p=2}^{P} X_{ct-p} \rightarrow W_{ct-1} \rightarrow Y_{ct}$$

Where X_{ct-p} is a matrix of controls related to economic growth. For the same reason the W_{ct} is lagged one year to ensure that a change in Y_{ct} comes after a change in W_{ct}, I include a series of autoregressive lags in the economic controls (starting at lag 2) to ensure that changes in W_{ct-1} occur *after* changes in economic development. A priori, I set $P = 5$ in my econometric model to control for economic lags in a country. For this two-stage regression, X_{ct-p} consists of logged GDP and logged population for a country at year t[12].

In the two-stage design, I further suspect that there exists some endogeneity in W_{ct}[13]. Both CO_2 emissions and women's representation in government are highly correlated with industrialization and development. While X_{ct} can eliminate some of the endogeneity of W_{ct} by including economic controls, these covariates alone are not enough to explain a country's development and how it interacts with how women are elected into national legislatures. The rate at which a country develops, both industrially and politically, in regards to the countries in this study may be highly related to which European power originally occupied the territory and thus, how the power left the state when the occupation ceased, including the political institution(s) established. Endogeneity may also arise in what natural resources are available to the country at year t. Countries with a greater propensity to use carbon-intensive resources will thus have, on average, higher CO_2 emissions. If this also

[12]A further analysis could (and should) include further economic controls that may affect W_{ct-1} such as fertility rate, etc.
[13]Mathematically, with endogeneity in the treatment variable, I assume $\exists \quad c_0, t_0 \quad s.t. \quad \mathbb{E}[W_{c_0 t_0 - 1} \epsilon_{c_0 t_0}] \neq 0$.

impacts women disproportionately such that women in these countries are more compelled to run for office and enact effective climate policies, then the estimand for δ will be biased in the negative direction.

To eliminate this endogeneity I invoke a two-stage difference-in-difference design, using an instrumental variable for W_{ct-1}. A common instrument in the literature for women's representation in parliament has been to use the number of years since the country granted suffrage (Mavisakalyan & Tarverdi, 2019). I use a similar instrumental variable design here. Let Z_{ct-1} be the number of years since a country c has granted suffrage (measured at year t, and lagged one year to align with W_{ct}). In the difference-in-difference design, I use 62 country-fixed effects (omitting the Qatar and Iraq dummies due to colinearity). After lagging W_{ct} one year, I am left with data spanning 1998-2022, thus, including year-fixed effects, I also omit 1998 for colinearity. I utilize the following two-stage estimation approach:

$$(1) \quad W_{ct-1} = \pi_0 + \lambda Z_{ct-1} + \sum_{p=2}^{P} X'_{ct-p}\Psi_p +$$

$$\sum_{k=\text{Angola } k\neq\text{Qatar, Iraq}}^{Zimbabwe} \mu_k \mathbb{1}(\text{Country}_c = k) + \sum_{j=1999}^{2022} \eta_j \mathbb{1}(\text{Year}_t = j) + v_{ct}$$

$$(2) \quad Y_{ct} = \beta_0 + \delta W_{ct-1} + \sum_{p=2}^{P} X'_{ct-p}\Omega_p +$$

$$\sum_{k=\text{Angola; } k\neq\text{Qatar, Iraq}}^{Zimbabwe} \beta_k \mathbb{1}(\text{Country}_c = k) + \sum_{j=1999}^{2022} \gamma_j \mathbb{1}(\text{Year}_t = j) + \epsilon_{ct}$$

The Validity of Years Since Suffrage as an Instrument

Mavisakalyan and Tarverdi, although make use of a cross-sectional data set, to their credit, report that, "an IV estimation can be potentially more convincing in a dataset that varies over time" (161). This is precisely what I employ here. I will expand on the research that uses years since suffrage as an instrument (Grier and Maldonado, 2015; Hicks et al., 2016). Relevance of Z_{ct-1} is convincing with the idea that women in countries where suffrage is granted will see more involvement in government over time as women's rights expand in politics. Mavisakalyan and Tarverdi note that "a country's history of suffrage should be highly relevant for female exposure to politics" (160). By partialling-out Z_{ct-1} on the fixed-effects and control variables[14], the regression coefficient is significantly positive: 0.0045 (0.00343, 0.00556). Hence, we have significant evidence to believe in the relevance condition of using (lagged) years since suffrage as an instrument. Regardless of when a country granted suffrage, I assert that the potential carbon emissions per capita as a function of the proportion of women in the national legislature in a country c are only dependent upon W_{ct-1} after fixed effects are accounted for, and not on Z_{ct-1}. Mathematically, $Z_{ct-1} \perp Y_{ct}(W_{ct-1}) | X_{ct-1}, \text{Country}_c, \text{Year}_t$. However, as Mavisakalyan & Tarverdi note, "[our instrument] would be questionable if there is the possibility that the history of suffrage affects climate change policy outcomes through mechanisms other than the Female in Parliament." To satisfy exclusion, I assume,

$$(3) \quad \mathbb{E}[Z_{ct-1} v_{ct} | \mathbb{X}_{ct}] = 0 \quad \forall c, t$$

Where $\mathbb{X}_{ct}$ is the matrix of covariates (including country and yearly fixed effects) I include in equations (1) and (2). This could be violated if countries whose governments are established after colonial rule have (i) strong dependencies on the formal colonial power in terms of institutional structure and (ii) as a function of that institutional structure relies on carbon-intensive resources to kick start its economy in post-colonial development. In other words, the instrument will be invalid if there are interactions between time, country, and carbon emissions. More research needs to be done to see if this is the case, however, in countries that granted suffrage to women immediately after occupation typically took place in the late 1900s, if it was less than random which countries these were, then the economic development—in relation to carbon-intensive resources—is also most likely non-random as well and thus would cause endogeneity in the instrument[15]. In this paper, I assume away this endogeneity by assuming there is no distinct correlation between when

[14]This is the coefficient and 95% C.I. on λ from the first stage regression. See Figures 3 and 4 in the Appendix to visually assess the relevance condition of Z_{ct-1}.

[15]To assert this, one would have to see a distinct pattern between the periods of when women were granted suffrage and other metrics associated with carbon-intensive resource use such as oil exports.

a country granted suffrage and the types of resources the country typically consumes. Critically, I assume monotonicity: the longer a country has passed from the year it granted suffrage, the more, on average, women are elected into national legislatures. I assume that there are no countries for which this trend reverses—though there may be countries that always elect women at high rates (always-takers) or always elect women at low rates (never-takers) regardless of when suffrage was granted; the fact that the instrument may have a weak effect for some countries is tolerable. The monotonicity assumption may be violated if women become less interested in politics over time for certain countries such that their participation decreases and hence the average proportion of women in national legislatures decrease with respect to years since suffrage but would otherwise increase when a country first grants suffrage[16]. I assume that trends that reverse in a given year are due to country-specific, year-specific fixed effects and or economic-related included covariates, and not because of the instrument[17].

Assumptions for Difference-in-Differences

In the absence of more (or less) women being elected conditioned on the proportion of women elected being influenced by the instrument of years since suffrage was granted in the country, I assume the total CO_2 emissions per capita for each country would have followed the same trend over time as the other countries that did not elect more (or less) women in national legislatures at that specific time. Since my treatment variable (W_{ct-1}) is continuous, the parallel trends assumption is hard to verify or even visually asses. However, I assume no other events or factors are influencing CO_2 emissions per capita differently across different countries over time. I assume no unobserved heterogeneity; my estimate is robust to omitted covariates—this assumption is satisfied if we believe in the exclusion restriction of our instrumental variable (equation (3)). Following Bertrand et.

[16]Additionally, monotonicity may be violated if obstacles to women's ability to participate in democratic elections become more severe over time.

[17]It is also possible that even after controlling for country and year fixed effects along with economic covariates, there could exist a non-parametric (a non-linear and non-monotonic) relationship between our instrument and treatment variable: i.e. when suffrage is granted in a country, where voting interest is initially high, but overtime that decreases, but for some reason, it reverses course with some quadratic or non-parametric relationship and increases again. Equation (1) would not capture such a relationship and the monotonicity assumption would fail.

al (2004), I cluster standard errors by countries, where I assume standard errors across countries are independent from each other. This is most appropriate given that within each country, CO_2 emissions are most likely correlated with each other given that the economic and industrial structure mostly remains consistent within each country from year to year (yet different across country). This assumption could be violated if groups of countries form specific treaties and agreements to reduce CO_2 emissions or make other agreements pertaining regarding the usage of carbon-intensive resources.

Results

The difference-in-difference parameter, δ, was estimated using a two-stage design and assumed to be linear[18]. If my assumptions hold, then the economic and statistical implications are relatively straightforward (interpreted on a 1% scale): On average, for every 1% the average proportion of women in national legislatures in African and Arab countries increase by, the total yearly (gigatons of) CO_2 emissions per capita decrease by 1.08 (-1.64, -0.51) (significant at the 5% level). Interestingly, the only significant economic indicator is the first lag of GDP (lagged 2 years).

Placebo Regression

To assess the significance of my results, I run a placebo regression by first theorizing a priori a new vector in place of CO_2 emissions per capita that in theory, wouldn't be affected by the proportion of women in the national legislature in a given country in a given year[19]. I used data from the Centre for Research on the Epidemiology of Disasters (CRED) to measure the number of natural disasters that occur within a country within a given year. Although there is significant evidence, particularly from the IPCC, to suggest that the rate and severity of natural disasters have been increasing over

[18] See Table 3 in the Appendix for the regression results and 95% confidence intervals.

[19] This is a non-trivial task from a social science perspective. If women elected within these countries during this period are given the power to enact change (and this paper takes the stance that women are given this opportunity and power if elected), then nationally elected officials who are women may potentially affect *everything* within an economy and government.

time, using my identification strategy (equations (1) and (2)), year-fixed effects should account for any trends in time and country-fixed effects should account for countries particularly vulnerable to natural disasters[20]. Although, intrinsically, the number of natural disasters that occur within a country at year t seems Poisson-distributed[21], however, upon partialling out the regressand for fixed effects, the response variable could be reasonably modeled as a Normal random vector[22]. The goal of this placebo regression is to reason that there is a signal in the treatment on per capita CO_2 emissions. By using my identification strategy outlined previously, I hope to show that mechanically, there is something within the (local) causal chain that influences CO_2 emissions to decrease when the average proportion of women in national legislatures increase. If the gender composition of national legislatures isn't influencing the number of natural disasters that occur within a country, this supports the theory that I did not obtain the regression results in the original regression by random chance. Indeed, after running my regression, the placebo regression results support this. See Table 4 in the Appendix.

Economic Implications for Climate Policy

My estimation strategy identifies a local average treatment effect on how increases in average proportions of women in national legislatures in African and Arab countries affect yearly per capita CO_2 emissions. In this empirical example, the local average treatment effect applies to countries that see greater political engagement[23] (and thus, a greater proportion of women in national leg-

[20] Reasonably, one could also make the argument that with the increased rate and severity of natural disasters, the propensity for women to enact effective climate policy may increase, and thus, if women within a particular country are able to pass effective climate policies to reduce the negative externalities seen from climate change, one would expect either (i) a negative coefficient for estimation on δ, or conversely (ii) reverse causality, where increased disasters may increase the average proportion of women in national legislatures, if women (and their constituents) are driven by this issue. However, as discussed in the identification portion of this paper, I hope to eliminate most of the endogeneity (including the potential for reverse causality) with my identification strategy. Using a two-stage estimation approach with lags implies that any change in Y_{ct} must come *after* a change in W_{ct} by construction.

[21] See Figures 5 and 6 in the Appendix. The placebo regression was also estimated using an iterative generalized method of moments (GMM) framework by specifying $\mathbb{E}[Z_{ct-1}(Y_{ct-1} - exp\{\mathbb{X}'_{ct}[\beta_0|\delta|\Omega_1|\ldots|\Omega_4|\beta_{\text{Angola}}|\cdots|\beta_{\text{Zimbabwe}}|\gamma_{1999}|\cdots|\gamma_{2022}]'\})] = 0$ as the relevant moment condition for instrumental variables Poisson regression. Upon estimation, however, The estimated Jacobian matrix was highly singular so the coefficients and standard errors did not converge.

[22] Regardless, the GMM asymptotic variance (used for two-stage least squares) converges in distribution to a Normal distribution even upon clustering (I cluster by country in this regression as well).

[23] Notably, since the instrument is continuous, more heterogeneity in the treatment variable is tolerable.

islatures) among women the more progressed they are after suffrage is granted. Given that $\hat{\delta}$ is significantly negative, we have reason to believe that within countries (and years) that elect more women into government as a function of being more politically developed (that is, indicated by years since suffrage was given), per capita CO_2 emissions significantly fall at the end of the following year. Among these implications, it is also interesting that the GDP from two years prior (or equivalently, one year prior to when women are elected into office), is significantly positive, indicating that either (i) economic growth facilitates higher probabilities of women being elected into office (supporting my thesis for a causal chain of inference), or (ii) use of carbon-intensive resources may promote economic growth in terms of GDP, and, if externalities placed on women are significant, encourage women to run for office to alleviate the very same externalities that facilitated the economic growth in the first place. Although if equation (3) holds, then the instrumental variable may successfully eliminate entirely the plausibility of reverse causality. Alternatively, contrary to my initial assumption, successful climate policies may not hinder economic growth at all. It could be that that that the policies that are being passed to lower CO_2 emissions are also catalysts of promotions for substitute goods such as subsidies for alternative energy sources. These projects could lower CO_2 emissions while boosting a country's GDP. While this paper cannot investigate specific interactions between a country's economy, gender composition in the national government, and CO_2 emissions, it seems clear (with high confidence), when more women enter the national legislature, we see lower per capita CO_2 emissions.

Conclusion

This paper reaffirms the effect that was outlined by Mavisakalyan and Tarverdi. Utilizing a two-stage difference-in-difference econometric framework, as national legislatures in African and Arab nations witness higher concentrations of women we see significantly lower per capita CO_2 emissions. While the exact mechanism through which women's presence influences climate policy remains open for further investigation, these findings hold significant policy implications. By encouraging women to run for office and dismantling barriers to their political participation, governments can potentially achieve environmental benefits alongside broader societal advancements. While this study focused on the local average treatment effect across countries, future research should dive deeper into specific economic policy interactions and their implications on women in government and climate change. Furthermore, investigating the specific policies enacted by legislatures with higher female representation can shed light on the mechanisms at play. This study underscores the importance of considering gender equality within climate change mitigation strategies. Understanding how women's leadership shapes environmental policy is crucial for developing more effective strategies to achieve a sustainable future.

Appendix

A1 - Existing Research on Disproportionate Impacts of Climate Change on Women

During periods of increased droughts, which are exacerbated due to the effects of climate change, women often make long trips to meet agricultural, hygiene, and family needs. This has been linked to increased violence against women (UNDP, 2019). Increased risks of poverty and food insecurities have also been shown to increase as a result of increased rates of natural disasters and variable weather patterns. One article reports that "when a family is faced with the impact of the climate crisis, girls' education is one of the first things families drop" (Medlicott, 2021). Another meta-analysis consisting of 53 studies report that two-thirds of those studies find that women are more often the victims of death or injury in the case of extreme weather events (Seller, 2016).

Tables & Figures

Table 1: Summary Statistics of Key Variables

Variable	Mean	SD	Min	Max
CO2 Emissions Per Capita	3.224	7.345	0.021	57.16
Proportion of Women in Gvt	0.155	0.108	0	0.575
ln(GDP)	23	1.7	18	27.2
ln(Population)	15.851	1.545	11.256	19.202
Suffrage	1968.109	13.363	1947	2006

Figure 1 − Average Proportion of Women in National Legislatures Over Time

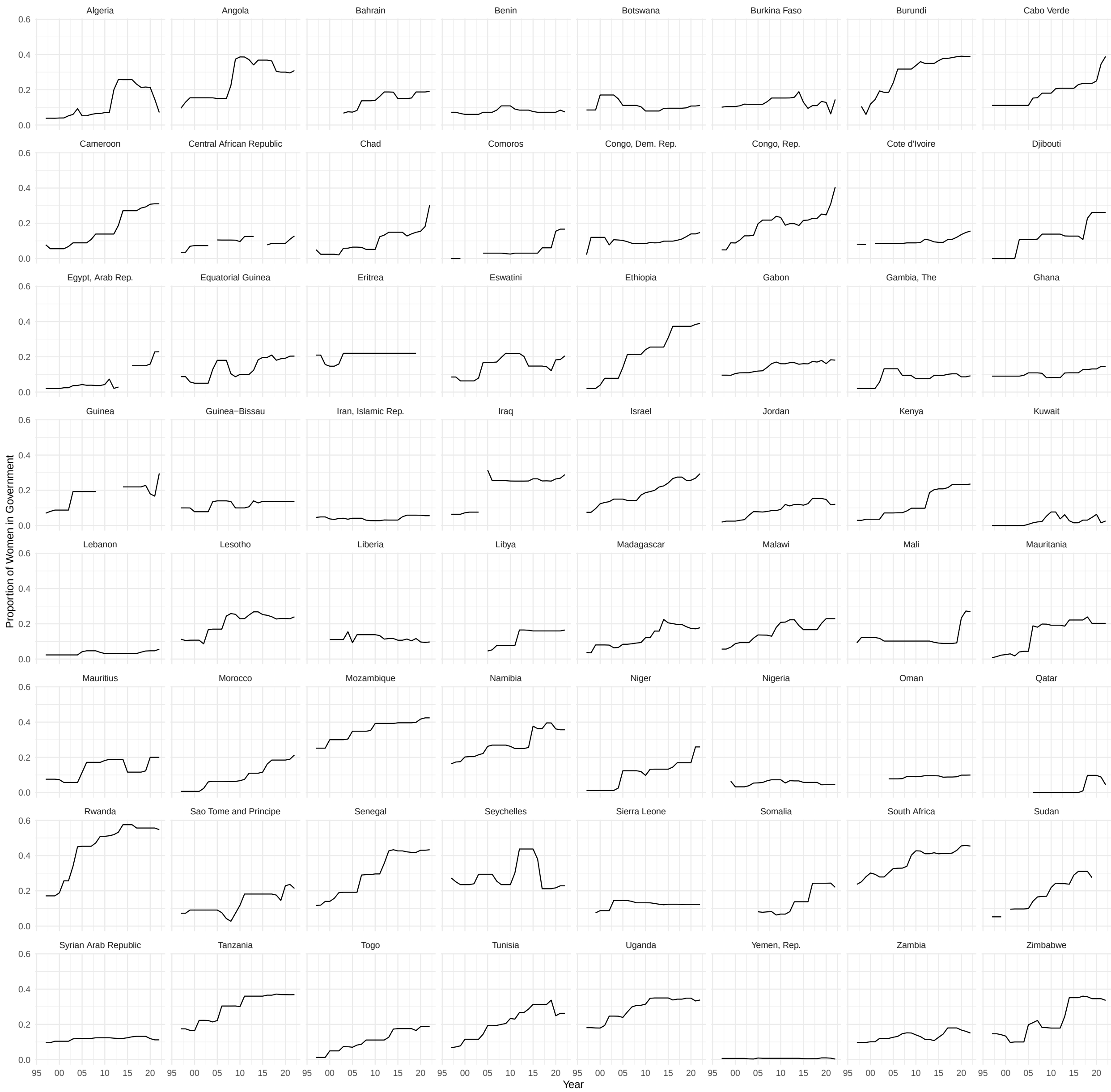


Figure 2 − Per Capita Carbon Emissions Over Time

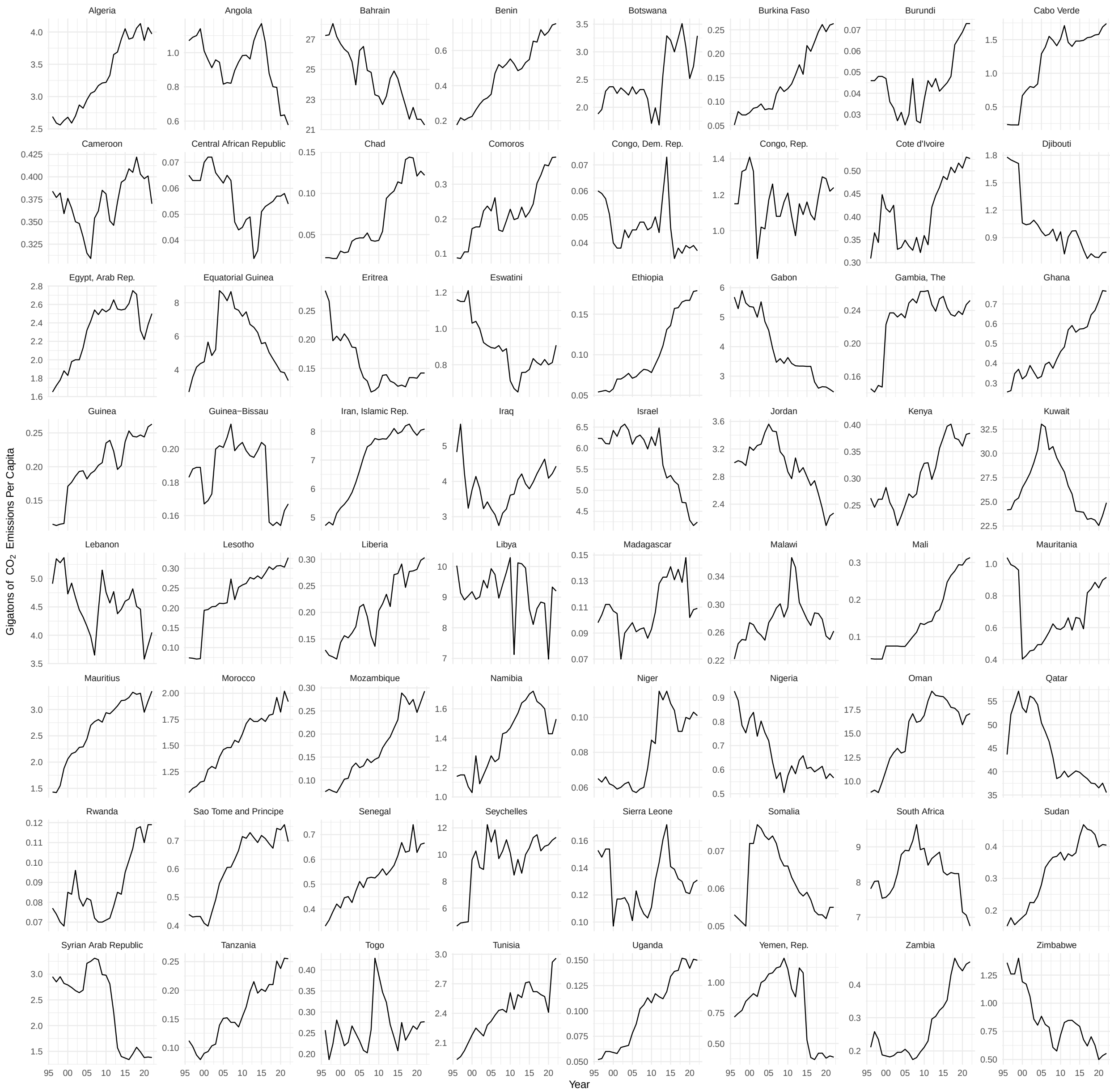
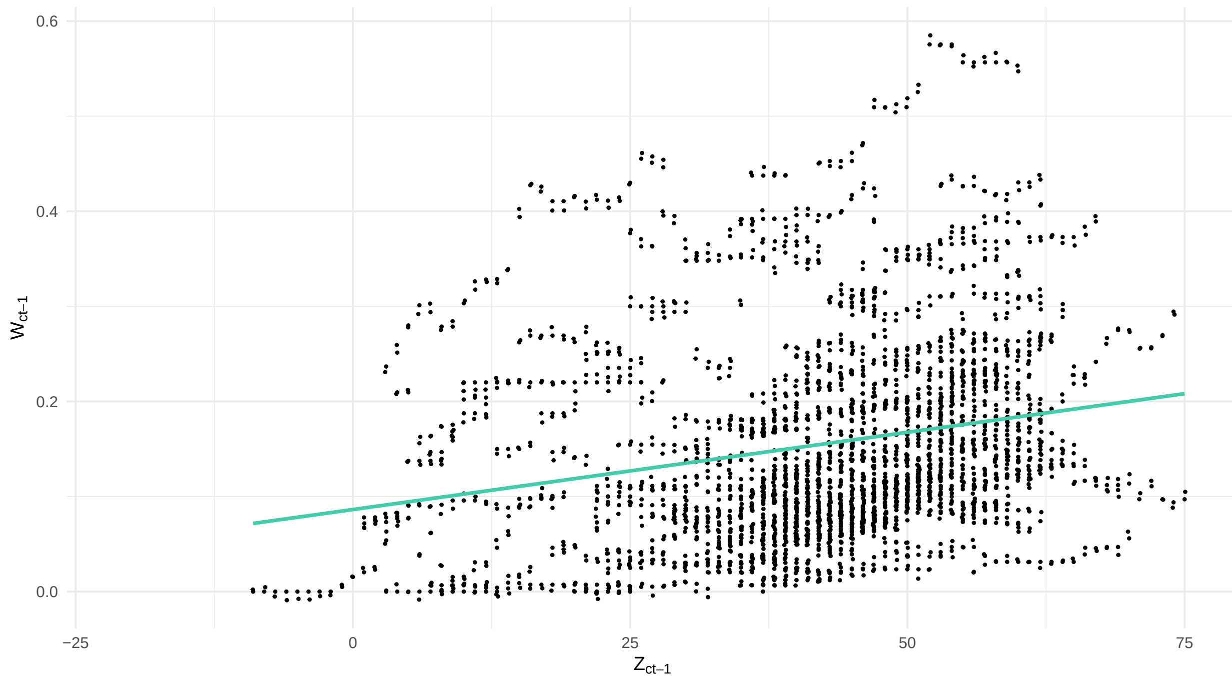


Table 2: Summary Statistics by Country

Country	N	Mean CO2 Emissions Per Capita	Mean Proportion of Women in Gvt	Mean GDP	Mean Population	Suffrage
Algeria	26	3.33192	0.12144	133413556539	36256149.08	1962
Angola	26	0.92681	0.25617	61136510425	23819407.27	1975
Bahrain	20	24.41692	0.14652	23866885356	1110339.23	2002
Benin	26	0.48000	0.07758	9427907948	9513753.81	1960
Botswana	26	2.51423	0.10956	11679307166	2093686.46	1966
Burkina Faso	26	0.14869	0.12574	9879179362	16222351.12	1960
Burundi	25	0.04481	0.29223	1858691481	9013842.27	1962
Cabo Verde	26	1.22450	0.18505	1483184743	517811.46	1975
Cameroon	26	0.37169	0.16573	26614285194	20110188.96	1960
Central African Republic	23	0.05631	0.09069	1717825115	4516393.23	1960
Chad	26	0.07246	0.09761	8092429967	12009809.12	1960
Comoros	22	0.22754	0.04802	839948182	658580.65	1975
Congo, Dem. Rep.	26	0.04538	0.10226	26137691195	67798246.61	1967
Congo, Rep.	26	1.16296	0.19112	9890831710	4342198.92	1960
Cote d'Ivoire	25	0.41704	0.09852	37241947845	21228611.50	1960
Djibouti	26	0.98342	0.12020	1568798898	908429.23	1977
Egypt, Arab Rep.	24	2.32654	0.07382	210728516057	87759979.69	1956
Equatorial Guinea	26	5.84538	0.13341	10680310300	1098129.11	1968
Eritrea	23	0.15304	0.20727	1120484041	3025058.73	1993
Eswatini	26	0.88365	0.14917	3443873221	1098973.65	1968
Ethiopia	26	0.10427	0.21588	43426199788	89604476.46	1955
Gabon	26	3.89038	0.14328	12441574754	1731276.23	1960
Gambia, The	26	0.23292	0.08174	1276629190	1948392.35	1965
Ghana	26	0.48408	0.10488	35446770393	25480258.61	1957
Guinea	21	0.20485	0.17269	7558680162	10412517.15	1958
Guinea-Bissau	26	0.18654	0.11840	874252813	1580784.61	1974
Iran, Islamic Rep.	26	7.04769	0.04200	323334435131	75438778.31	1979
Iraq	25	3.84692	0.20797	128507863985	32491105.15	1958
Israel	26	5.72923	0.18622	250706702966	7614703.85	1948
Jordan	26	2.93385	0.09033	25772832748	7476728.62	1974
Kenya	26	0.30900	0.12401	49487400802	40920573.00	1963
Kuwait	26	26.68577	0.02475	102805583557	3032646.92	2006
Lebanon	26	4.53423	0.03430	32455205151	5123114.12	1952
Lesotho	26	0.23662	0.19993	1813006912	2071184.42	1966
Liberia	24	0.20858	0.11397	2268958422	3925143.88	1947
Libya	18	9.11577	0.12542	52387031513	5970720.88	1963
Madagascar	26	0.10985	0.12502	9465270164	21695233.31	1960
Malawi	26	0.27958	0.15362	7514886577	14841171.27	1964
Mali	26	0.15212	0.12074	10274167660	15662439.38	1960
Mauritania	26	0.67550	0.14266	5071728438	3476739.54	1960
Mauritius	26	2.70000	0.13275	9303361634	1234473.96	1968
Morocco	26	1.56115	0.09184	91087307855	32367924.31	1962
Mozambique	26	0.17431	0.35431	11612120970	23558217.61	1975
Namibia	26	1.39731	0.27434	9018993290	2113443.23	1990
Niger	26	0.08123	0.10870	7682887769	17080118.61	1960
Nigeria	24	0.66092	0.05481	313024619441	161517413.54	1977
Oman	19	15.47154	0.08991	56775810363	3281428.62	2003
Qatar	17	44.70962	0.02556	108516614223	1637385.77	2003
Rwanda	26	0.08800	0.44140	5978302715	10348245.77	1962
Sao Tome and Principe	26	0.61331	0.13043	255006484	179928.11	1975
Senegal	26	0.53638	0.29679	15447790277	12665052.88	1960
Seychelles	26	9.72346	0.28266	1080544320	89129.92	1976
Sierra Leone	25	0.12796	0.12040	2694746567	6391405.35	1961
Somalia	18	0.06238	0.14398	8216255642	12121495.23	1964
South Africa	26	8.27500	0.36477	308320233630	52097664.77	1994
Sudan	22	0.32985	0.18022	47869298018	34068352.38	1964
Syrian Arab Republic	26	2.33077	0.11750	84109722132	19402662.58	1953
Tanzania	26	0.16458	0.29519	34798787505	46143525.54	1963
Togo	26	0.26342	0.10986	4458357376	6566135.35	1960
Tunisia	26	2.44115	0.20983	37604341839	10925330.88	1959
Uganda	26	0.10385	0.28969	21152773534	32832378.15	1962
Yemen, Rep.	26	0.81108	0.00665	22320277039	24799160.46	1990
Zambia	26	0.28300	0.13306	15927797878	13906848.08	1964
Zimbabwe	26	0.83592	0.22631	13726241713	13273904.50	1979

Figure 3 − Relevance of Instrumental Variable



Both the instrument and response variable are graphed using the actual raw vectors; this is useful for intuition, but not necessarily a good estimation for λ

Figure 4 − Relevance of Instrumental Variable Using Partialled−out Regressor and Regressand

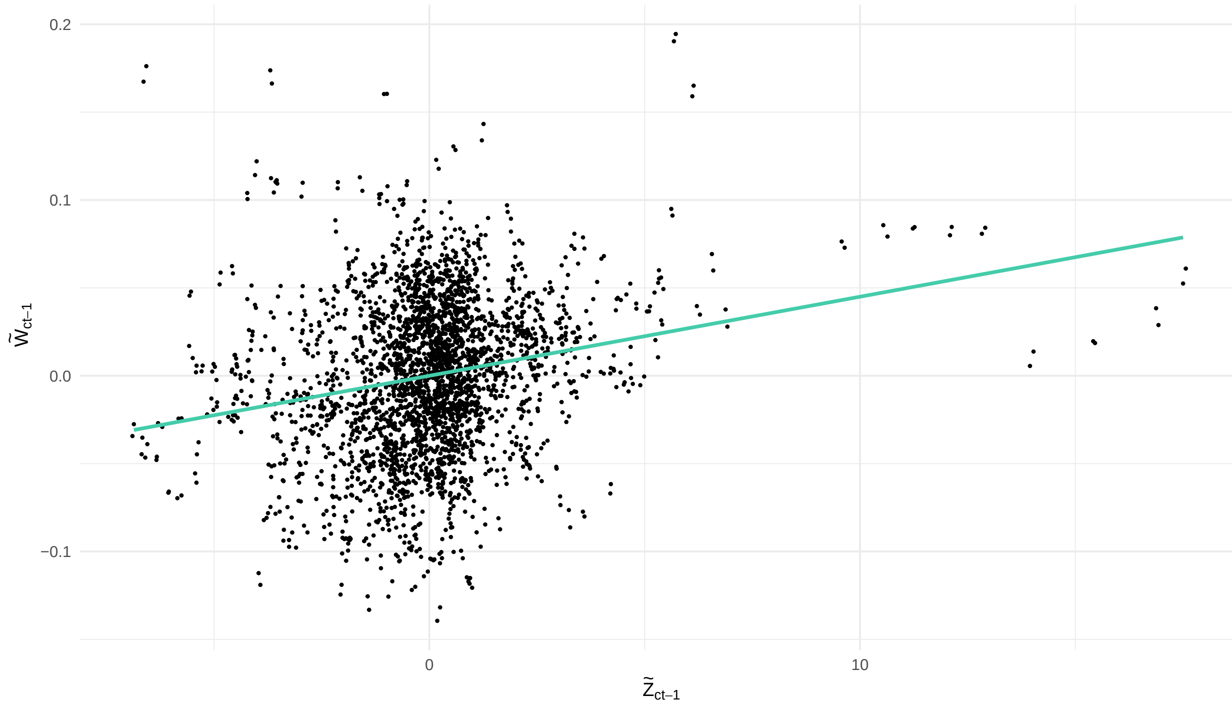


Table 3: Regression Results: Two-Stage Difference-in-Difference Regression Results (year and country-fixed effects omitted)

	CO_2 Emissions (Gigatons per Capita)
δ	-107.8**(***)**
	[-164.3,-51.43]
(GDP_{ct-2}) Ω_{11}	2.925**(*)**
	[0.305,5.545]
(GDP_{ct-3}) Ω_{21}	-0.653
	[-2.293,0.987]
(GDP_{ct-4}) Ω_{31}	0.328
	[-0.908,1.564]
(GDP_{ct-5}) Ω_{41}	0.770
	[-0.912,2.452]
$(\text{Population}_{ct-2})$ Ω_{12}	-32.77
	[-72.56,7.017]
$(\text{Population}_{ct-3})$ Ω_{22}	29.63
	[-7.546,66.80]
$(\text{Population}_{ct-4})$ Ω_{32}	-0.869
	[-24.47,22.73]
$(\text{Population}_{ct-5})$ Ω_{42}	-6.992
	[-25.07,11.08]
N	1499

95% confidence intervals in brackets

(*)$p < 0.05$, **(**)**$p < 0.01$, **(***)**$p < 0.001$

Figure 5 − Distribution of Placebo Random Vector

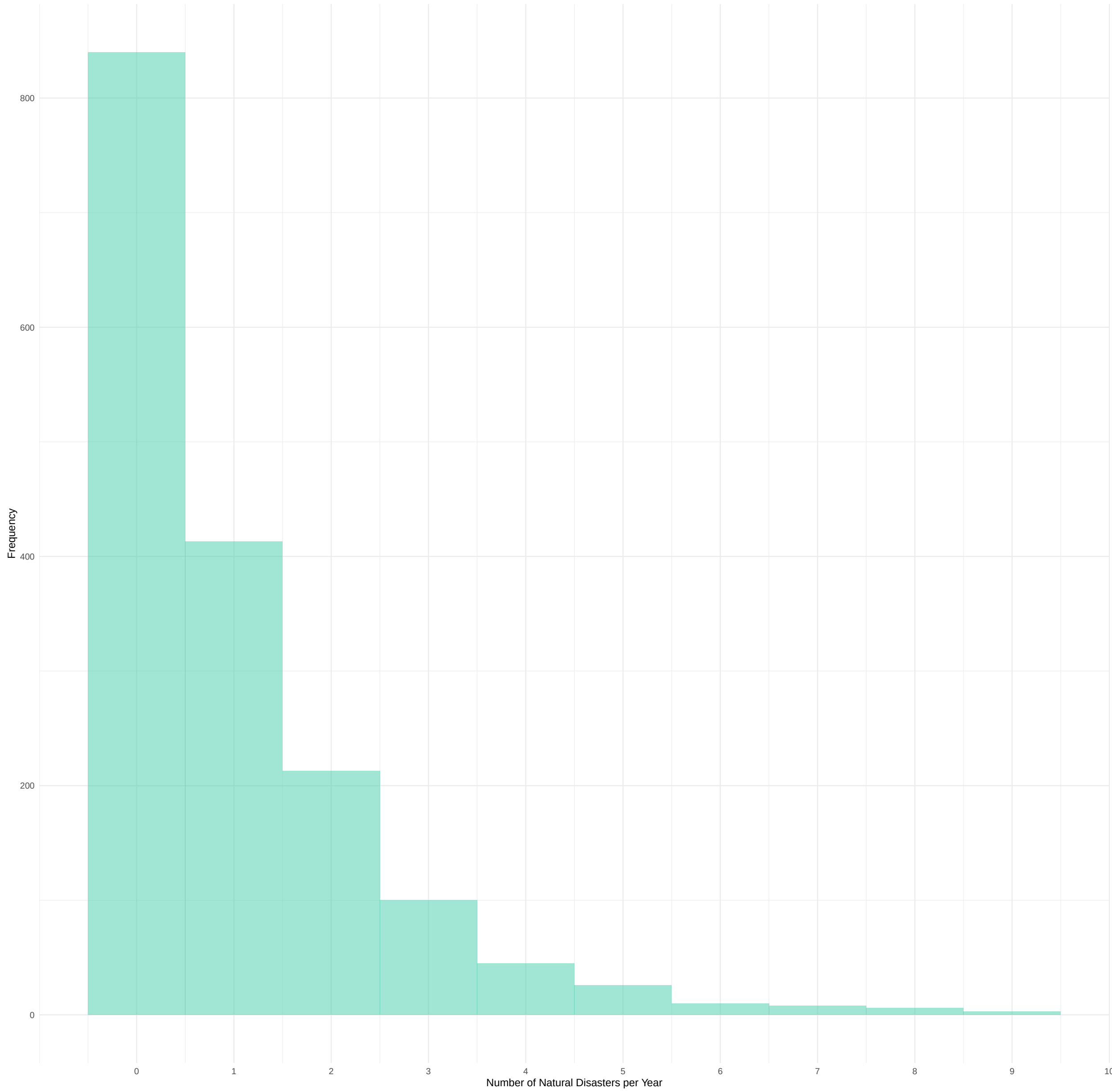


Figure 6 − Number of Natural Disasters Over Time

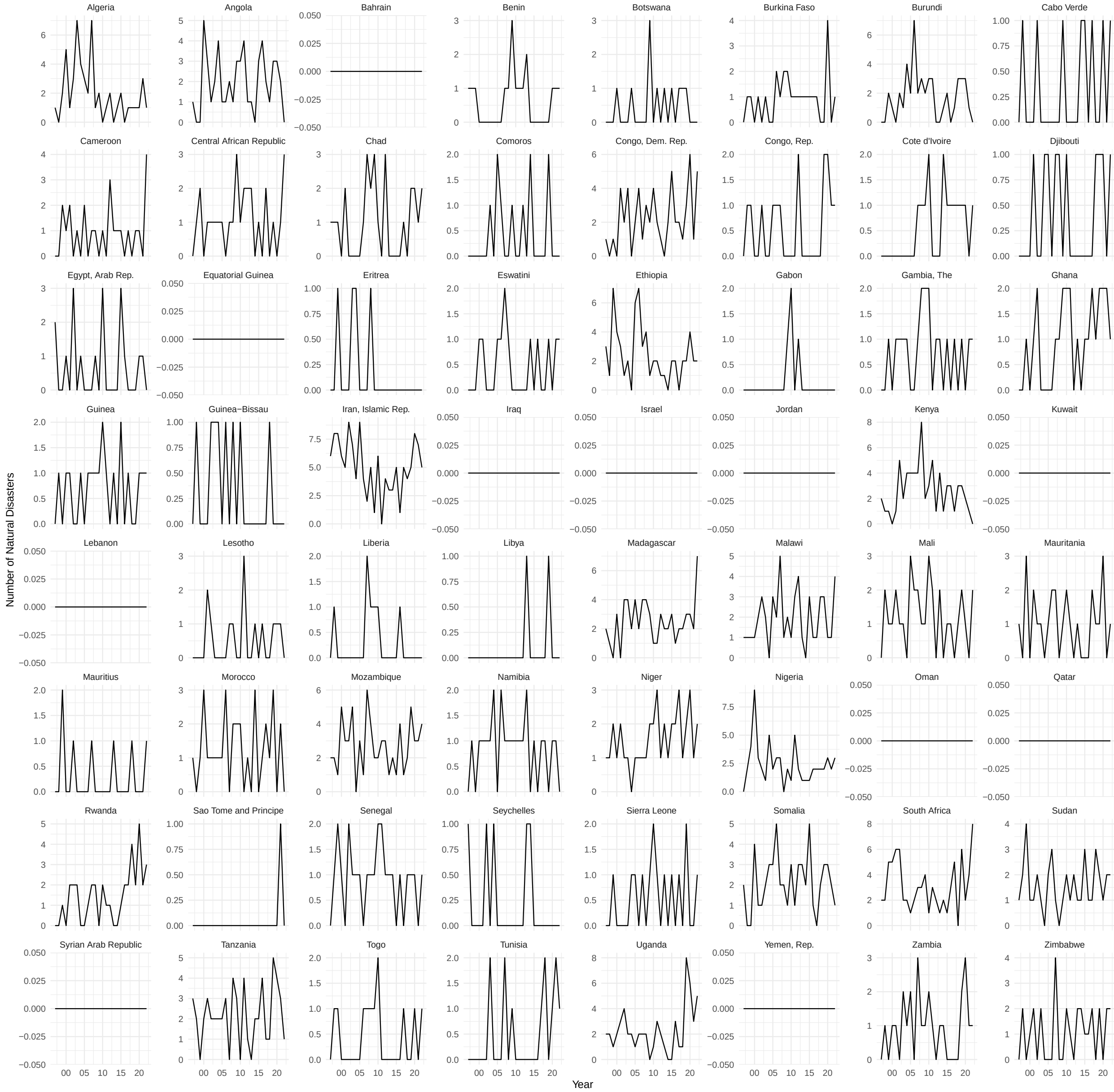


Table 4: Placebo Regression Results: Two-Stage Difference-in-Difference Regression Results

	Yearly Number of Natural Disasters
δ	-2.824
	[-8.282,2.634]
$(\text{GDP}_{ct-2})\quad \Omega_{11}$	-0.205
	[-0.520,0.109]
$(\text{GDP}_{ct-3})\quad \Omega_{21}$	0.149
	[-0.429,0.726]
$(\text{GDP}_{ct-4})\quad \Omega_{31}$	0.314
	[-0.176,0.804]
$(\text{GDP}_{ct-5})\quad \Omega_{41}$	-0.280
	[-0.576,0.0162]
$(\text{Population}_{ct-2})\quad \Omega_{12}$	0.856
	[-3.615,5.327]
$(\text{Population}_{ct-3})\quad \Omega_{22}$	-0.679
	[-6.232,4.874]
$(\text{Population}_{ct-4})\quad \Omega_{32}$	1.689
	[-2.111,5.488]
$(\text{Population}_{ct-5})\quad \Omega_{42}$	-1.415
	[-3.535,0.704]
N	1499

95% confidence intervals in brackets

(*)$p < 0.05$, **(**)**$p < 0.01$, **(***)**$p < 0.001$

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