An Analysis of the Economic Impact of the Belt and Road Initiative on 53 African Countries

April, 2023

Department of Economics: University of California, Berkeley Undergraduate Honors Thesis

Julia Verdickt

Advised by Dr. Benjamin Faber

Abstract:

This paper is an analysis of the economic impacts of the Chinese Belt and Road Initiative (BRI) on African countries. This BRI is a Chinese infrastructure investment project aimed at increasing the ease of trade around the globe. I conduct a difference in differences analysis to see if signing an official memorandum of understanding (MOU) on the Belt and Road with China impacts an African country's purchasing power parity GDP per capita. I used fixed effects regression models to conduct my analysis and found little to no impact of the BRI on African countries' purchasing power parity GDP per capita. My results provide a contradiction to many existing expectations about the benefits of Chinese infrastructure investment in Africa. Based on this, I argue that the benefits of the BRI should be touted with caution. However, I encourage future research on this topic in order to investigate the possibility of a lagged effect of the BRI.

Acknowledgments:

Special thanks to Dr. Benjamin Faber for advising me throughout this project.

I. Introduction

In 2013, President Xi Jinping of the People's Republic of China (PRC) launched an extremely ambitious infrastructure project titled the Belt and Road Initiative (BRI) or One Belt One Road Initiative (OBOR). This project was originally focused on investing in infrastructure to improve trade connections between East Asia and Europe, but it has expanded to include countries in Africa, Oceania, and Latin America. The BRI is sometimes referred to as a plan to create a "New Silk Road," which is a reference to the historical trade routes that connected Asia and Europe. In practice, the BRI consists of investment in railways, highways, maritime ports, and other infrastructure that improves ease of trade (McBride et al., 2023).

Projects like the BRI have the potential to improve the economic situations of participating countries around the world by increasing trade accessibility and domestic infrastructure. Thus, of particular interest in regards to the BRI are the possible benefits of the BRI for lower and middle-income countries. If the BRI is able to provide a good path for economic development, it might be something worth encouraging countries to participate in it.

While research has been conducted on the possible or existent impacts of the BRI on participating countries, much of this research has been concentrated on the BRI's impacts in Asia or at the global level. I argue that attention should also be paid to the effects of the BRI in Africa. After Asia, Africa is the continent with the most countries participating in the BRI (Sow, 2022). Africa is also home to many low and middle-income countries that have the potential to benefit from a project like the BRI[1]. If an effect is observed, it can be compared to similar research on Asia or at the global level to see if results from previous studies are externally generalizable to Africa. Understanding how the BRI impacts Africa can provide more insight into the nature of Sino-African relations. There are some voices that express concerns about the economic

[1] List of low and middle income countries found here: https://data.worldbank.org/?locations=ZG-XO

influence and political intentions of China in Africa (Sow, 2022; Nolan & Leutert, 2022). My research can shed some light on what the impacts of Chinese economic influence might be.

When investigating the impacts of the BRI in Africa, I specifically asked the question: does the BRI have an effect on economic growth in African countries, and if so what is this effect? To answer this question, I conducted a difference-in-differences analysis to see if joining the BRI impacts a country's purchasing power parity GDP per capita. For my difference-in-differences analysis, I ran a series of fixed effects regressions inspired in part by similar research conducted at the global level (Ma, 2022). The treatment effect in my analysis is the signing of a Memorandum of Understanding (MOU) or cooperation agreement between a country and China. An MOU can be thought of as an official written statement about a country's joining of the BRI. It often includes provisions about the specific projects that China will invest in in the newly joined country (Nolan & Leutert, 2022).

I originally hypothesized that the results of my analysis would show a positive effect from the BRI. Trade infrastructure investments from China would improve African countries' trade flows and thus increase purchasing power parity GDP per capita. Instead, my findings point to there being very little impact of the BRI in Africa as of 2020. However, I also note that infrastructure projects take time to complete, meaning future research on the same topic might show different results. Thus, while my findings do not indicate any clear benefit of the BRI, I also suggest returning to this question at a later date, which might uncover a lagged BRI effect.

The rest of this paper is as follows. (II) I present a literature review; (III) then I include a description of my empirical approach. (IV) I then discuss the data I used before (V) presenting my results. I end with a (VI) discussion of my results and (VII) some robustness checks before (VIII) my conclusion.

II. Literature Review

A. Key Inspiration

The main inspiration for this project is the research findings of Ma (2022). Ma (2022) provided inspiration for both my topic and main methodological approach. This paper explores the growth effects of the Belt and Road Initiative (BRI) using a difference-in-differences (DiD) estimation. Ma's findings suggest that the effect of participating in the Belt and Road Initiative is positive from an economic growth perspective. Like Ma (2020), I adopt a fixed effects regression model for my analysis. However, my focus is more specific. Ma observes multiple countries across different continents, while I focus solely on Africa. Furthermore, Ma does not analyze all countries that joined the BRI, but a random selection of treated and untreated countries. Despite Ma (2022) finding an overall positive effect of the Belt and Road, no mention is given to his sampling method of the treated countries in the study, so it remains unclear how the difference in Ma's selection of countries and mine impact our results. Ma's results briefly addressed regional differences in the effect of the treatment. His findings show that the effect of the BRI is significantly reduced and insignificant in Africa. It will be interesting if this result is mirrored in my findings. If this is the case, then it might indicate that his overall findings about the benefits of the BRI are subject to overgeneralization and should be taken with caution.

B. The BRI: Economic Expectations

Beyond Ma's research, many other papers focus on the economic effects and benefits of the BRI. While not all existing literature focuses on the impacts of the BRI on GDP in particular, they do shed light on a similar topic and context and demonstrate a general sentiment toward the economic effects the BRI will and does have.

The impact of the BRI on trade, for example, has been of considerable interest in recent literature, as the expressed goal of the BRI is to facilitate trade between China and countries along a “New Silk Road.” Much research has been published with predictions and expectations about the impact of the BRI. de Soyres et al. (2020) presents a general equilibrium model and framework that could be used to quantify the expected impact of the BRI on reducing transport costs and advancing trade. Their findings point to the expectation that the BRI would have a generally positive effect for the majority of countries, but these effects will not necessarily be evenly distributed across countries. They recommend appropriate policy transformations if countries hope to take full advantage of the BRI.

On a similar note, a series of simulations were performed by Herrero and Xu (2017) to investigate the possible trade gains Europe might experience from the BRI’s advancement. They concluded Europe could majorly benefit from reduced transportation costs if China were not to create exclusive trade agreements with BRI participating countries. Other research about the effects of the BRI on the Europe-China trading corridors was conducted in Wen et al. (2019). They note the expected positive effects of the BRI on Europe-China trading corridors rely on the local political stability of countries along the trade corridor. Thus, it seems de Soyres et al. (2020), Herrero and Xu (2017), and Wen et al. (2019) all consider how political contexts can hinder or promote the BRI’s benefits. Lall and Lebrand (2020) also present a general equilibrium model that addresses the trade impacts of the BRI but with a focus on economic geography within countries. They only focus on Central Asia and China and argue that the BRI will have positive economic effects, but the effects will be concentrated around urban hubs, border regions, and port regions. Yang et al. (2020) also conduct a simulation analysis with a focus on Asia only.

Their focus is on how the BRI will increase trade infrastructure, which they expect promotes economic growth across all countries and regions to different degrees.

Existing literatures' expectations about the BRI's facilitation of trade in Asia was somewhat supported by Yu et al. (2020). This paper focussed on the export potential of China to Asian BRI participants. Using a DiD estimation, the paper found that export potential rapidly increased in Central Asia, West Asia, and Southeast Asia. However, this effect was not observed for other regions such as South Asia.

Beyond benefits to trade, there is some existing literature on the BRI that focuses on the BRI's impact on economic growth. Similar to the findings of Ma (2022), Luo et al. (2021) find that the BRI will help promote economic growth in BRI countries. Luo et al. (2021) specifically argue that increased trade with China will promote industrialization and urbanization in BRI economies. This is similar to the increased urban concentration mentioned by Lall and Lebrand (2020). Luo et al. (2021) ultimately argue that increased industrialization and urbanization in BRI countries will reduce income disparities in BRI countries. Thus, it becomes clear that for the most part, existing literature about the BRI expects positive benefits from the BRI.

C. The BRI and Africa

It is also valuable to briefly note that there is existing research on the BRI that specifically covers Africa. Most of the research about how the BRI impacts African countries is focussed on the impacts of the BRI on trade in Africa. Dumor et al. (2021), for example, conduct a network analysis to understand how Africa is situated in Chinese trading networks. They conclude that trade network connectivity has increased between Africa and China due to the BRI. Also noted is the fact that trade network density is not uniform across Africa, with Eastern Africa having a more tightly connected trade network. However, the authors also account for the fact that increased trade connectivity does not necessarily imply economic growth outcomes. This will only be the case if African countries can optimize BRI trade partnerships, which requires a level of political stability and government action.

Githaiga et al. (2019) expand on a similar topic, arguing that security risks and corruption, in particular, need to be addressed if African countries are to benefit from the BRI. While Githatiga et al. (2019) note the BRI provides a platform for China-Africa cooperation and economic growth and Africa, African countries need appropriate policies in order for this cooperation to be beneficial. Similar arguments are mirrored in Kalu et al. (2020) but for intra-regional trade in Africa. They note that the BRI could be a great contributor to economic development in Africa as well as intra-regional trade if countries in Africa are able to appropriately harness BRI-related infrastructure. Kalu et al. (2020) specifically argue that African countries should focus on transitioning from raw resource economies to economies with high-value services and manufacturing if they are to effectively take advantage of the BRI. However, for this economic development to be accessible African countries must clearly define their development goals and plan of action to achieve those goals.

Overall, existing literature—that focuses on the impacts of the BRI in Africa—shares the belief that the benefits of the BRI cannot be realized in Africa if other policy changes are not made.

Based on the above literature, I ultimately developed initial anticipations about the direction my research might go. First, while much existing literature supports a "generally" positive effect of the BRI at the global level and in Asia, I find it is currently not clear whether this will be the case in Africa. While Ma (2022) briefly touched on this, no in-depth analysis was done for Africa in particular, and much of the current literature on Africa is apprehensive about whether the full potential benefits of the BRI will be realized. My empirical analysis will shed more light on the question of the BRI's success in aiding economic growth in African countries with a focus on changes in purchasing power parity GDP per capita (PPP GDPpc) as a measure of economic growth.

III. Main Methodology

A. Empirical Model: OLS

This paper uses a DiD estimation to find the effect of the BRI on log PPP GDPpc. The treatment effect is a country's signing of an official Memorandum of Understanding (MOU) on the BRI, which represents a country's joining of the BRI. Since the BRI is generally associated with major infrastructure and trade investments in China, much of the existing literature expects overall positive economic effects from BRI investments and increased trade. Chinese investment in Africa had been ongoing before the start of the BRI. Thus, I am not simply measuring the effects of Chinese investment in general. Rather, I want to see if the signing of a BRI-specific MOU has led to significant consequences and changes from the previous patterns of Chinese investment in Africa that in turn have led to notable economic changes in African countries.

With "joining the BRI" as a treatment effect, I approached my analysis of the BRI's impacts like a natural experiment. In order to measure the effects of the BRI, I started with a very simple model: an ordinary least squares regression:

$$(1)\ Y_{it} = \beta_0 + \beta_1 post$$

In this simple model, Y_{it} is equal to the log of PPP GDP per capita for country *i* in year *t*. I regress Y_{it} onto a constant and the *post* variable. Each observation in the data set is indexed by country and year. The *post* variable is a dummy variable that is 1 when a particular country-year observation is after the treatment year of said country, and 0 when a particular country-year observation is before the treatment year of said country. Note that the treatment is staggered, so there is no single treatment year. Each country has its own treatment year.

B. Empirical Model: Fixed Effects

i. Key Assumption: Parallel Trends

Before delving into my main DiD fixed effects model, it is important to note an important assumption I make before conducting my analysis. A key assumption in any DiD analysis is the parallel trends assumption. The basic parallel trends assumption is the assumption that the “path” of outcomes over time—that those in a treatment group would have experienced if they had not been treated—is the same as the path of outcomes experienced by a control or “untreated” group. This is equivalent to the statement:

$$E[Y_t(0) - Y_{t-1}(0) \mid D = 1] = E[Y_t(0) - Y_{t-1}(0) \mid D = 0]$$

In this statement, $Y_t(0)$ is the untreated potential outcome and D is an indicator of group, with D = 1 being the case for units in the treated group and D = 0 being true for units in the untreated group. In other words, we treat the control group as a “counterfactual” scenario that is observable, which allows us to isolate the causal effect of a treatment on the treatment group. I go into my analysis with this assumption.

ii. Alternative Hypothesis:

Also, taking into account the findings in Ma (2022), my initial hypothesis (H_A) before conducting my analysis was that the coefficient on *post* would be positive. The BRI would increase or strengthen China-Africa economic ties, in terms of trade and investment, and thus lead to economic growth in African countries.

iii. The Fixed Effects Models

Since I am working with panel data and comparing observations across countries and years, I also decided to expand my model into a fixed-effects model. Naturally, when dealing with multiple countries over multiple years, it is unreasonable to assume that I can easily consistently estimate β_1 without any omitted variable bias (OVB) in our ordinary least squares regression. There will be other unobservable factors that impact Y_{it} and cause OVB. OVB due to unobserved heterogeneity can be addressed with a fixed-effects regression model when said heterogeneity is constant over time. In short, I add the fixed effects to the model in order to control for any individual-specific attributes that do not vary across time.

The initial model with all included fixed effects that I use is as follows:

$$(2)\ Y_{it} = \beta_1 post + countryFE + yearFE + regyearFE$$

In this model, *countryFE* are the country fixed effects, *yearFE* are the year fixed effects, and *regyearFE* is a region-year fixed effect. In the fully-specified model, I account for country fixed effects, which essentially means I account for static differences across countries. By including year fixed effects, I account for static differences across years. The *regyearFE* is an interaction term for region and year.

I include *regyearFE* in the model to allow for more specific comparison between countries within a particular region in each year. A particular agglomeration of countries may have certain static characteristics that impact its log PPP GDPpc. Within each region, countries may have their own specific, static country characteristics, however, they may also share some regional static characteristics. For example, Dumor et al. (2021) mentioned that countries in East Africa have a more tightly connected trade network. Within the East African countries, there may still be country-level differences, which is why *countryFE* is included. Yet it is still important to account for the regional differences that may be at play as well beyond just the country fixed effects.

My use of fixed effects regression is better suited for causal inference than the simple OLS model. Because I use a fixed effects model, I can better control for endogeneity bias caused by unobserved, static, confounding characteristics, which means my estimate for the coefficient on *post* will not be as biased. If the parallel trends assumptions hold, I can essentially better estimate the true effect of *post* on log PPP GDPpc by including fixed effects in my models.

Besides the initial OLS regression, my analysis ultimately involves an individual country fixed effects model, an individual year fixed effects model, a two-way fixed-effects model with both country and year fixed effects, and lastly the model with all fixed effects included.

C. Quality of Governance:

Even if I include fixed effects in my model, there are still other unaccounted for variables that may cause omitted variable bias in my model. These are non-static variables that change from year to year and may be simultaneously impacting the treatment effect as well as the outcome variable. My concerns over this possible bias stem from the findings of Kalu et al. (2019) and Githaiga et al. (2019) who note that the ability of African countries to harness the benefits of the BRI might be hindered by poor governance quality. Thus, the effect of the treatment might differ based on countries' differing levels of governance quality from year to year. Controlling for quality of governance then, can help address possible endogeneity bias experienced by the *post* variable, whose measure of the effect of the treatment may be biased due to the way in which governance quality correlates with the treatment effect and the outcome variable.

In order to control for governance quality, I have opted to include measures of these qualities in my fixed effects regression as covariates. Specifically, I intend to include a measure of Government Effectiveness for each country-year observation in the dataset. This measure is taken from the World Governance Indicators (WGI) database provided by the World Bank. The measure is recorded on an indexed scale of -2.5 to 2.5. Including this covariate in my analysis should provide insights into the role that government institutions play in the effect of the treatment. More detailed description of this covariate and WGI data is provided in the *Overview of the Data* portion of this paper.

D. Empirical Model: Additional Covariates:

When adding this covariate to my fixed effects model, I will add it in a staggered manner, similar to how the fixed effects were added. The fully specified model is as follows:

$$(3)\ Y_{it} = \beta_1 post + countryFE + yearFE + regyearFE + \beta_2 GE$$

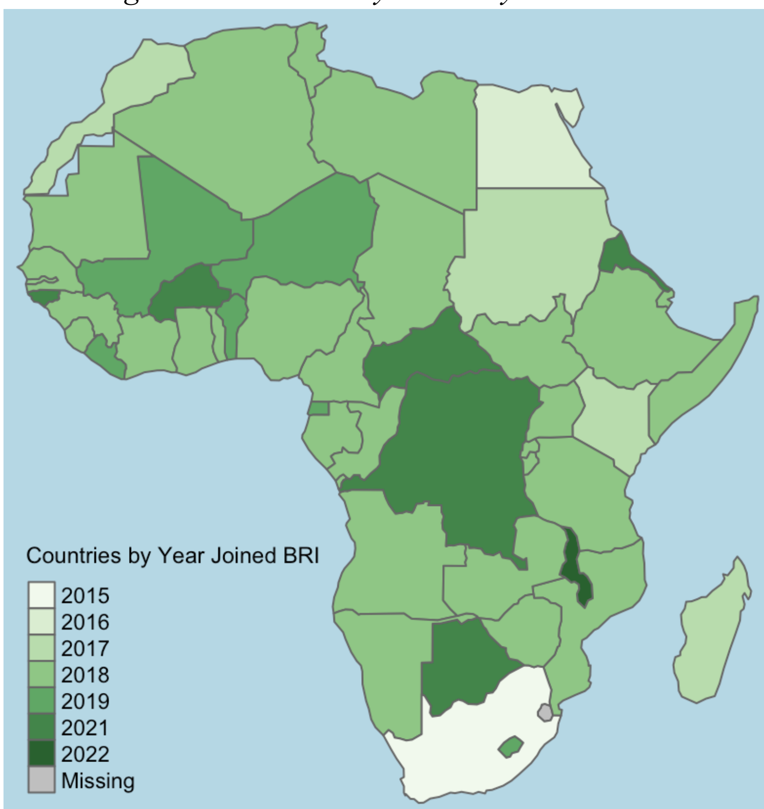
There will be a model with *countryFE, GE,* and *post*, as well as a model with *yearFE*, *GE*, and *post*. Lastly, there will be a two-way fixed effects model with *GE* and *post*, where *GE* is a measure of government effectiveness. Lastly, I will add *regyearFE,* which should result in model (3). Ideally, the inclusion of the *GE* indicator should account for the concerns discussed by Githaiga et al. (2019) and Kalu et al. (2019).

IV. Overview of the Data

For my analysis, I focused on 53 African countries, which are listed in the appendix (Table 1A). Eritrea was excluded from my analysis as real PPP GDPpc data was not available in my years of focus. Figure 1 maps most of the countries by the year that they joined the BRI with lighter colors symbolizing earlier treatment years. We can see that a high concentration of countries joined in 2018, which is likely due to the mass signing of MOUs during the 2018

Beijing Summit of the Forum on China-Africa relations. Figure 1 shows that as of 2023, the vast majority of African countries have already signed onto the BRI. One country—Eswatini—stands out as likely the only country by 2023 in Africa that has not signed an MOU, as the country still retains ties with the Republic of China over the People's Republic of China.

Figure 1: Countries by Year they Join the BRI



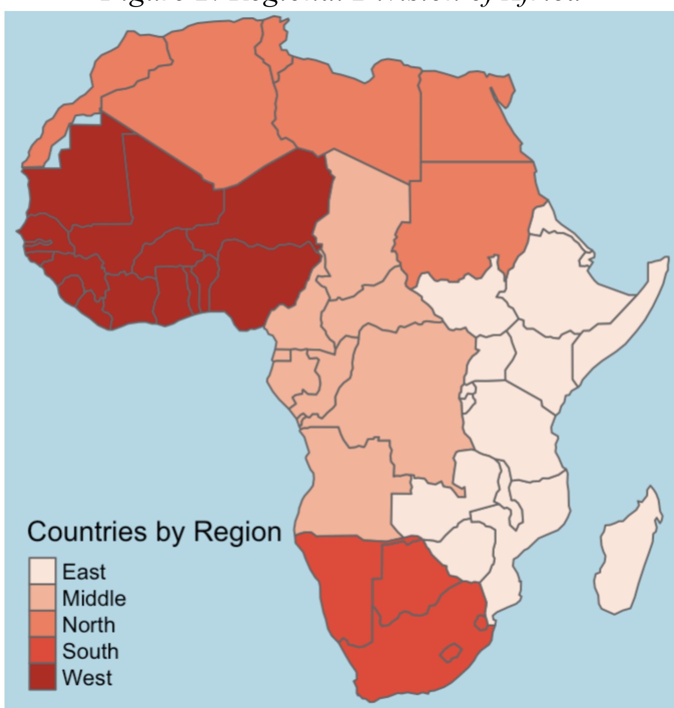
Note: Not all countries are included in this map. See Appendix Table 1A for the full list of countries and dates

I determined the year a country was treated by synthesizing multiple sources. The main sources were the official Chinese government BRI portal[2] and the Xinhua News Silk Road BRI portal[3]. Since Xinhua News is the official state news agency of the government of the People's Republic of China, I found both of these sources a credible way to determine the year a country "officially" signed on to the Belt and Road. I supplemented my findings on these websites by cross-referencing with Ma (2022).

[2] https://www.yidaiyilu.gov.cn/xwzx/roll/77298.htm
[3] https://en.imsilkroad.com/

The yearly PPP GDP per capita data for each country was taken from the World Bank World Development Indicators database. This variable is measured in constant 2017 international dollars. The World Bank clarifies, “An international dollar has the same purchasing power over GDP as the U.S. dollar has in the United States.” By choosing to work with real units instead of nominal ones, my analysis does not have to account for some year-on-year changes like inflation. Also, note that I choose to work with log PPP GDPpc because it condenses my data. The use of log also allows me to interpret my regression results in terms of a unitless percentage change in the outcome variable. This is useful for an analysis of the growth impacts of the BRI.

Figure 2: Regional Division of Africa



Note: The regional division here are based on those provided by the United Nations Statistical Commission[4]

The regional designations that I used to create the *regyearFE* fixed effects come from the United Nations geoscheme. Figure 2 depicts these regional divisions.

In the dataset I ultimately constructed, there are 477 observations spanning from 2012 to 2020 with 9 observations per country. I chose to start my analysis in the year 2012 to avoid

[4] https://unstats.un.org/unsd/methodology/m49/

complications in the data caused by the Sudanese civil war in 2011. Table 1 provides summaries of the key variables of interest: log PPP GDPpc and *post*. *Treat* is a dummy variable that is 1 when a country is treated before 2020 and 0 otherwise.

Table 1: Summary of Variables

Year Joined BRI	log PPP GDPpc	Treat	Post
Min. :2015	Min. : 6.567	Min. :0.0000	Min. :0.0000
1st Qu.:2018	1st Qu.: 7.529	1st Qu.:1.0000	1st Qu.:0.0000
Median :2018	Median : 8.116	Median :1.0000	Median :0.0000
Mean :2018	Mean : 8.222	Mean :0.8302	Mean :0.1887
3rd Qu.:2019	3rd Qu.: 8.937	3rd Qu.:1.0000	3rd Qu.:0.0000
Max. :2022	Max. :10.380	Max. :1.0000	Max. :1.0000
NA's :18	NA's :11	NA	NA

From this, we see that about 83% of observations are treated before 2020, and only 18% of observations are post-treatment. The small percentage of post-treatment observations is largely due to the fact that treatment only begins in 2015 and many countries are treated later than 2015.

The data for my covariate comes from the World Governance Indicators database where 6 governance indicators are calculated using a methodology created by Daniel Kaufmann and Aart Kraay[5]. Government Effectiveness is specifically defined as the quality of public services and the civil service as well as these institutions' freedom from political pressures.

I have opted to include the index measures of these indicators with values between -2.5 and 2.5 in my model. Having a more positive score will indicate a better government effectiveness. This index can show how a country's governance may impact its ability to harness the BRI. One component in the computation of WGI indicators is public perception data from a multitude of sources. The full methodology is provided by Kaufmann and Kraay on the WGI database website. If a country has relatively low governance quality, this might make said

[5] https://info.worldbank.org/governance/wgi/

country a less desirable trading or business partner. Since the main benefit of the BRI is building strong trading and business relationships, it seems reasonable that the relative quality of a country's governance factors into its ability to form successful economic relationships with other countries. Thus, having a higher index rating might imply that a country is better at harnessing the benefits of the BRI.

V. Modeling Results

A. The OLS Model:

Preliminary visualization of the simple OLS model seen in Figure 3 makes a strong treatment effect seem unlikely. The results of the first OLS regression model results are shown in Table 2. The results appear to be in agreement with Figure 3. In the OLS model, *post* is not significant with a p-value greater than 0.1.

Figure 3: Plot of Post Against log PPP GDPpc data With Fitted OLS Model

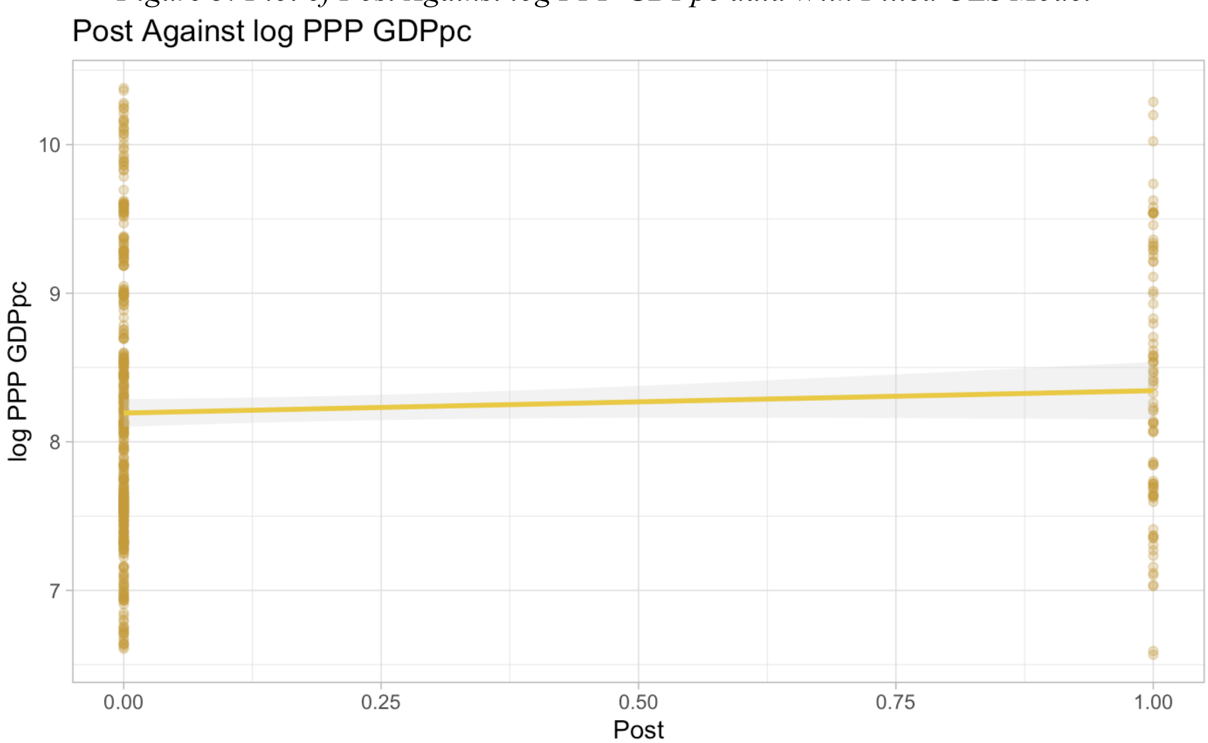


Table 2: OLS Model Results

	Dependent variable:
	log PPP GDPpc
post	0.150
	(0.109)
Constant	8.194***
	(0.047)
Observations	466
R^2	0.004
Adjusted R^2	0.002
Residual Std. Error	0.923 (df = 464)
F Statistic	1.895 (df = 1; 464)
Note:	*p<0.1; **p<0.05; ***p<0.01

B. The Fixed Effects Models:

Table 3 shows the results of the fixed effects regression models. In line with the OLS model, the coefficient on *post* does not appear significant in the model with only country fixed effects. However, the coefficient on *post* is also significant at the 5% level when year fixed effects are included. Then, when both year and country fixed effects are included, the coefficient is no longer significant and grows closer to 0. Furthermore, the standard error of the estimate grows smaller once both country and year fixed effects are included. When I include the region-year fixed effects, the coefficient on *post* becomes significant at the 10% level. There is also a rather large change in the actual magnitude of the coefficient between models 3 and 4, as the coefficient in model 4 grows more negative. The standard error remains the same.

C. The GE-inclusive Model

The models with the *GE* covariate included can be found in Table 4. When the measure of government effectiveness is included in the model, there is interestingly little change in the coefficient on *post* when compared to the results in Table 3. There is a very slight change, but the coefficient on post remains mostly the same with the same standard error. One distinction here is that the Adjusted R^2 of model (4) in Table 3 is higher than in model (4) of Table 4. Note that the Adjusted R^2 is negative for models (1),(2), and (3) in both Tables 3 and 4. The Adjusted R^2 is only positive in model (4) of Tables 3 and 4, but it is higher for model (4) of Table 3. Still, for both models, the R^2 is relatively low.

Table 3: Fixed Effects Model Results

	Dependent variable:			
	log PPP GDPpc			
	(1)	(2)	(3)	(4)
post	0.012	0.377**	−0.005	−0.032*
	(0.011)	(0.180)	(0.018)	(0.018)
Country FE	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	Yes
Region-Year FE	No	No	No	Yes
Observations	466	466	466	466
R^2	0.003	0.010	0.0002	0.246
Adjusted R^2	−0.123	−0.010	−0.148	0.061
F Statistic	1.239 (df = 1; 413)	4.399** (df = 1; 456)	0.085 (df = 1; 405)	3.696*** (df = 33; 373)
Note:				*p<0.1; **p<0.05; ***p<0.01

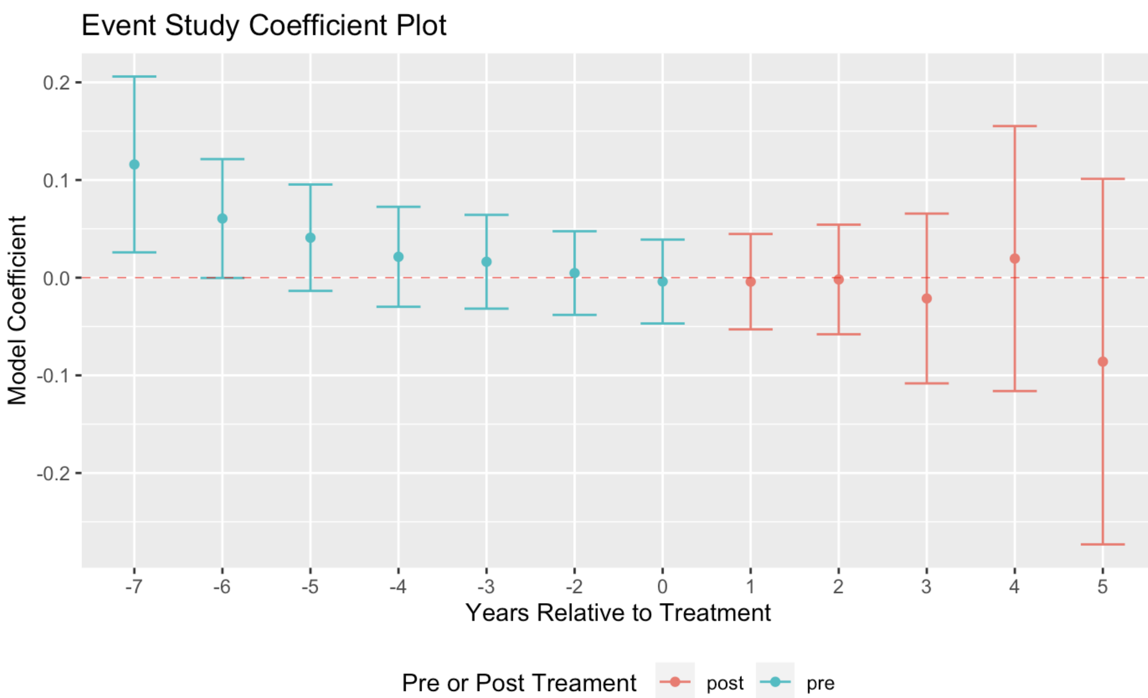
Table 4: Fixed Effects Model Results: Governance Quality Covariate

	Dependent variable:			
	log PPP GDPpc			
	(1)	(2)	(3)	(4)
post	0.007	0.370**	−0.005	−0.033*
	(0.011)	(0.180)	(0.018)	(0.018)
GE	0.012	−0.116	0.004	−0.002
	(0.008)	(0.083)	(0.009)	(0.009)
Country FE	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	Yes
Region-Year FE	No	No	No	Yes
Observations	466	466	466	466
R^2	0.009	0.014	0.001	0.247
Adjusted R^2	−0.119	−0.008	−0.150	0.058
F Statistic	1.792 (df = 2; 412)	3.168** (df = 2; 455)	0.163 (df = 2; 404)	3.580*** (df = 34; 372)
Note:				*p<0.1; **p<0.05; ***p<0.01

D. Verifying the Parallel Trends Assumption

Before discussing the results of the key models in this paper, I also want to take note of an event study fixed effects model that I used to verify the parallel trends assumption. I initially assumed parallel trends when conducting this analysis, however, this may not truly be the case for the observed data I have. I attempt to verify the parallel trends assumptions using similar modeling techniques to those in my main analysis. If I am able to verify parallel trends, the grounds for causal inference in my analysis are stronger.

Figure 4: Even Study Coefficient Plot for Parallel Trends Test



Using a fixed effects model, I regressed 12 dummy variables on log PPP GDPpc alongside country and year fixed effects. The dummy variables each represent whether or not a country-year observation is *i* years from said country's year of treatment effect, where *i* = *-7,-6,....5*. A negative *i* means an observation is pre-treatment while a positive *i* means an observation is post-treatment. Omitted from the model is a dummy for *i* = *-1* in order to avoid collinearity issues. The full results of this regression are in Appendix Table 2A. Figure 4 shows a coefficient plot of the results from this model. The final outcome shows that there are moderately strong parallel trends pre-treatment. Except for the coefficient on the *i* = *-7* dummy variable, the 95 percent confidence intervals for the other pre-treatment estimates all include 0, and are relatively close to 0. Thus, a parallel trends assumption, in this case, is not completely unrealistic but also not extremely robust either. This is taken into account in my discussion of the causal interpretation of my results.

VI. Discussion of Results

A. Discussion of Regression Results

At first glance, my results seem to imply that there is no significant effect of the treatment on log PPP GDPpc. In the OLS model and the two-way fixed effects model, the coefficient on *post* is insignificant. In the two-way fixed effects model, the coefficient is also close to 0. Even though results in the OLS model and two-way fixed effects model show the coefficient on *post* is not significant at the 5 percent level, this changes in the fully specified fixed effects model. This implies that when accounting for static differences across countries and across years, there is no significant change in log PPP GDPpc following the treatment. However, when accounting as well for static differences across regions in each year, there is a significant change in the log PPP GDPpc following the treatment. Model (4) of Table 3 seems to imply that after a country signs onto the BRI, its PPP GDPpc is expected to decrease by about 3.2%[6]. Still, note that this result is only significant at the 10 percent level, so if I were to hold a stronger threshold for significance, the results in Table 3 only weakly imply the BRI has negative impacts. Since the results in Table 3 weakly imply a negative impact of the BRI, it is too rash to immediately assume it is truly the case that the BRI has a negative effect.

Production of the covariate models resulted in some unexpected findings. In both Tables 3 and 4, it appears that without accounting for region-year fixed effects, the coefficient on post in the two-way fixed effects model remains close to zero and insignificant. This falls in line with the conclusions of OVB that I drew after observing the fixed effect models. Only after including region-year fixed effects does the coefficient on *post* become significant. However, this is not

[6] Using log-linear models allows me to interpret my coefficients as percentage changes.

necessarily the case with the inclusion of the *GE* covariate. After adding the *GE* covariate, there is little to no change in the coefficient on *post* and the Adjusted R^2 of the model decreases.

This implies that the inclusion of the *GE* covariate does not improve the fit of the model, and it likely does not address endogeneity in *post* as much as I originally hypothesized. However, this also does not mean that the fully-specified model I used in Table 3 is completely free of OVB and endogeneity bias. The inclusion of the *regyearFE* fixed effects addressed some of the OVB in the model, however, there might be some other variables at play that are biasing the coefficient on *post* toward the negative; *GE* is simply not one of those variables. Considering other possible variables that might be biasing *post* towards a negative value is a topic of interest that might be explored with further research.

B. Interpreting the Coefficient:

While the coefficient of the *post* variable is significant in the fully-specified models of Tables 3 and 4, the coefficient is negative, which goes against my initial hypothesis. This might simply imply that the BRI has a negative impact on the log of PPP GDPpc. However, this assumption should be taken with caution. Especially since the coefficient on *post* is only significant at the 10 percent level in these models, making strong claims about the BRI's effects should be avoided due to a higher potential of a type two error. It still may be that there are other omitted variables that I have not included in my model that have resulted in a negative coefficient. Furthermore, my parallel trends test ultimately showed only moderately parallel trends pre-treatment, which means causal interpretation of the results should be taken with caution regardless of the low p-value in my results. Overall, it is rash to immediately strongly conclude upon any causal effect.

Even if I were to draw a causal conclusion, it is important to understand what a negative effect of the BRI treatment might really mean. In the case that the coefficient on *post* truly is negative, this might not fully imply that investment from the BRI causes countries to suffer economically. Note that before the BRI, African countries were already receiving investment from China. Thus, a negative "effect" of the BRI might also simply mean that Chinese investment had not disappeared but possibly decreased after the signing of an MOU. This then means that Chinese investment is not necessarily inherently having a negative impact on PPP GDPpc in African countries. Rather, it might also be that reduction or change in investment following the BRI is what caused a negative impact on PPP GDPpc.

It is quite difficult to reconcile the idea that more investment and economic support from China leads to a decrease in GDPpc despite economic partnership being the goal of the BRI. Another reason for observing this treatment effect might be an overall misinterpretation of what exactly the treatment is. It might be that the expectations of the BRI benefits have simply not come to fruition for many countries, which has cost African countries. Note that the treatment effect I use is the signing of an MOU. The signing of an MOU does not necessarily guarantee the successful implementation of all that is set forth in such a memorandum. Thus, an MOU is more an intended course of action than something completely guaranteed. If this is the case, this may be why a negative effect of the BRI is observed in Tables 3 and 4, thus it is too presumptuous to claim a negative causal effect if any effect.

C. Implications of Results:

Overall, the results of my analysis mean that I cannot support my initial hypothesis and conclude that the BRI has had a uniquely positive effect on the economic growth of African countries. I can more strongly conclude that the effects of the BRI are uncertain or insignificant and minimal. However, I can also weakly conclude that the effects of the BRI may be negative. This interpretation is weaker, as it is difficult to make a definite causal claim considering there are only moderate pre-treatment parallel trends and the low significance levels of the coefficient on *post* in the models I produced. I have proposed some possible explanations for the results observed above, but also find future research into other possible causes of OVB might lead to clearer results.

What I can say, however, is that my results do not support the general conclusion of Ma (2022) and other literature discussed in this paper. Much of the existing literature I have discussed held generally high expectations of the benefits of the BRI including Ma (2022). However, the reality is that a broadly "positive" effect of the BRI was not externally generalizable to all African countries. Thus, it is important to promote the benefits of the BRI with some reservation, as my results show that signing an MOU does not guarantee the actualization of economic benefit

VII. Robustness Checks

The robustness checks in this portion of my paper should be seen as tangential to my main analysis. I include this section in my paper to discuss limitations in my research as well as possible solutions to these limitations. However, I do not go into as much depth or detail in this section due to its supplementary nature.

A. Limitation of the Model and Data:

There is one main limitation of the model and data I use that should be addressed. Namely, it is not guaranteed that the treatment is homogeneous for all countries. While a homogeneous treatment may be somewhat plausible in a very general sense, it is quite unrealistic at a more microscopic level. As discussed above, the plans set forth in MOU agreements are not necessarily completely identical even if the general intentions of an MOU are similar. Furthermore, the benefits of signing an MOU are not truly "guaranteed" for all countries until the actual BRI-related plans are complete. Chaisemartin and D'Haultfœuille (2020) argue that running a fixed effects model with heterogeneous treatments may negatively bias treatment effect estimates. They argue that fixed effects models estimate weighted sums of the average treatment effects in each group and period, and these weights may be negative. This may result in a negative aggregate treatment effect even when the average treatment effects for each group and period are not actually negative. Thus, the existence of heterogeneous treatments in my data may have led to the limitation discussed by Chaisemartin and D'Haultfœuille (2020), which might explain the results of my fixed effects models.

C. Possible Alternative Specifications:

In response to the limitation I have discussed, I conducted an additional robustness check using an alternative specification discussed in Callaway and Sant'Anna (2021) and Marcus and Sant'Anna (2021). This is an additional method for measuring a treatment effect that relies more strongly on the parallel trends assumption to draw causal claims and generalizes the parameter of interest to a group-time average treatment effect. The method of estimating treatment effects also accounts for heterogeneous treatment effects across time, as it uses a separate method for calculating the treatment effect that is proposed in Sant'Anna and Zhao (2020).

In general, this method calculates the average effect of participating in the treatment for units in a particular "group" at a particular period, where "group" is the year that a unit is treated. The effect that is calculated is called the group-time average treatment effect on the treated (ATT). The general definition of ATT is as follows:

$$ATT(g, t,) = E[Y_t(g) - Y_t(0) | G = g]$$

This is the average difference in the potential outcomes for each time period t between the treatment group and control group given the treatment time *g*. For example, in time *t* with *g* being 2015, this is the average difference between units treated in 2015 and those not yet treated as of period *t* across all periods *t* included in the study.

Callaway and Sant'Anna (2021) propose multiple parallel trends assumptions that can be applied to their method and provide a statistical computing package[7] that I used to calculate the treatment effect. The parallel trends assumption I apply is an assumption based on "not-yet-treated" units. Because almost every country in my sample is eventually treated, having a control group with only countries that were "never-treated" would lead to a large imbalance between the number of units in the treatment and control groups, which can be seen in Table 1 of this paper. The "not-yet-treated" parallel trends assumption can be stated as follows:

$$E[Y_t(0) - Y_{t-1}(0) \mid G = g] = E[Y_t(0) - Y_{t-1}(0) \mid D_t = 0, G \neq g]$$

The "not-yet-treated" assumption postulates that for each time period *t*, one can use units not-yet-treated as a comparison group to those that have been treated. In the above definition, *g* represents which treatment group a unit belongs to. For example, let's say a set of countries are treated in 2017. Then they belong to the group "first treated in 2017." The "control group" against which these countries are compared can include both other countries in the data that are never treated but also other countries that have yet to be treated. Those that have already been treated are excluded completely from the comparison. If parallel trends hold before the year 2017 for those that are first treated in 2017 and the comparison "control" group, it would be possible to identify a causal effect of the BRI for those treated in 2017. Using the "not-yet-treated" parallel trends assumption leads to a new definition of the ATT (Callaway & Sant'Anna, 2021):

[7] https://bcallaway11.github.io/did/

$$ATT(g,t) = E[Y_t - Y_{g-1} | G = g] - E[Y_t - Y_{g-1} | D_t = 0, G \neq g]$$

In this case, the treatment effect is the difference between two averages. The first average is the average difference between the outcome in period *t* and the outcome just before treatment year *g* for those treated in year *g*. We subtract from this the average difference between the outcome in period *t* and the outcome in the period just before year *g* for those who have not yet been treated ($D_t = 0$) and those who are not treated in year *g*. For each year *t* and group *g*, one could calculate the observed ATT(g,t), or the treatment effect.

The treatment effect is explicitly calculated using the doubly robust (DR) difference in difference method proposed by Sant'Anna and Zhao (2020). Two other ways that the treatment effect in a DiD model can be calculated are an Outcome Regression (OR) method or Inverse Probability Weighting (IPW) method. What Sant'Anna and Zhao (2020) propose is that instead of choosing between only OR or IPW, one can combine these estimating methods to create a single DR DiD estimand. The full mathematical description and proof of how this should be calculated are described in Sant'Anna and Zhao (2020). In simple terms, the estimand defined in this paper can identify an ATT even if either IPW or OR models are misspecified, which results in a more robust outcome overall.

To understand if this treatment effect is statistically significant, one can construct a 95% confidence interval for each ATT(g,t) using standard errors calculated using the multiplier bootstrap method. This is all possible in the statistical computing package created by Callaway and Sant'Anna (2021). Using this package, I was able to apply the alternative specification set forth by Callaway and Sant'Anna (2021).

Figure 4 presents the disaggregated ATT(g,t) results for each period and group with the 95% confidence band included. I also average the ATT estimates into average treatment effects at different lengths of exposure to the treatment to examine the average dynamic treatment effect in an event study plot. In the event study plot, ATT pre-treatment estimates also remain close to 0 (Figure 5). In both Figure 4 and Figure 5, the pre-treatment ATT estimates are close to 0 or at least include 0 in its 95% percent confidence interval, implying the parallel trends assumption does hold for this model.

Figure 4: ATT(g,t) Results with 95% Confidence Interval Band

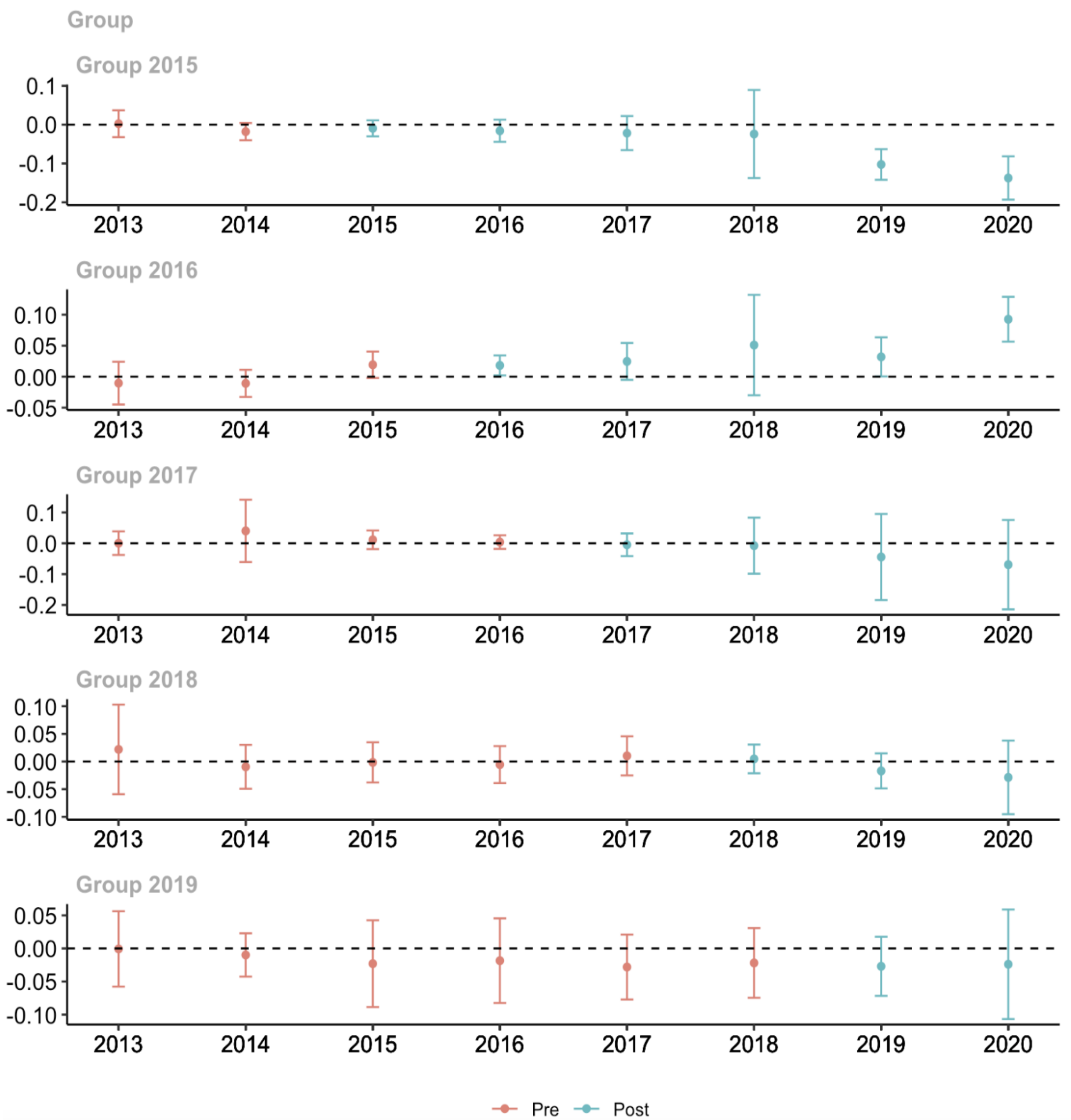
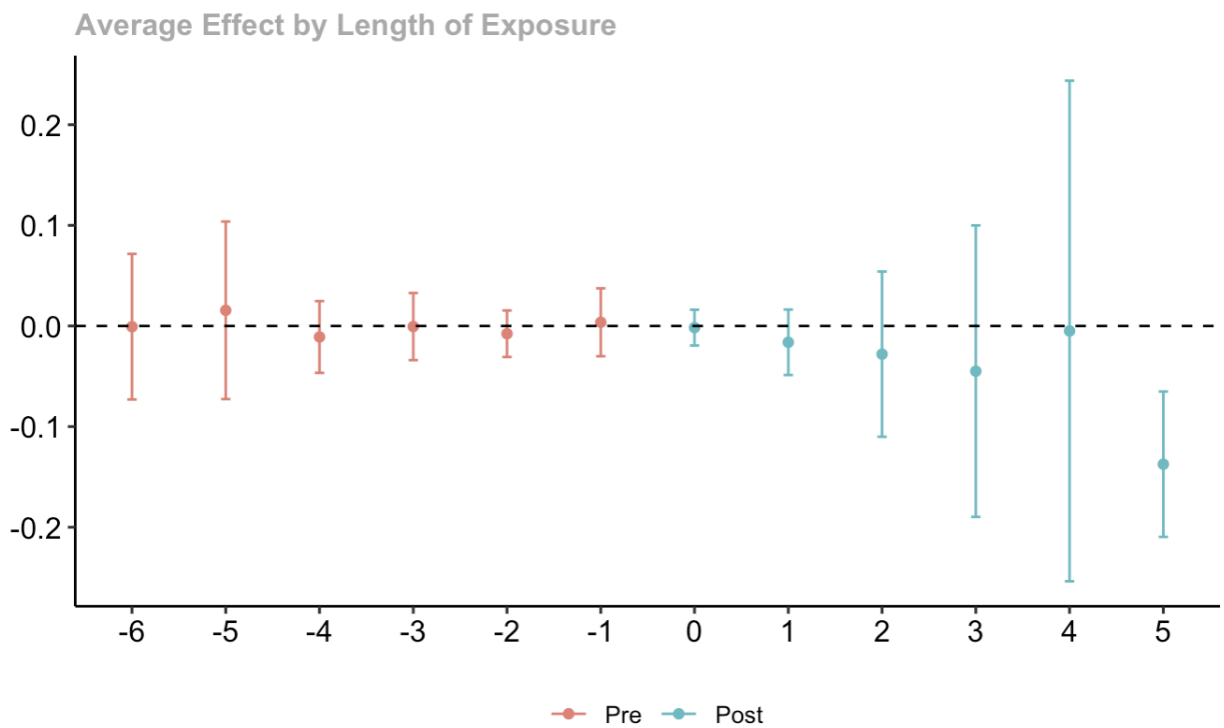


Figure 5: Average ATT(g,t) by Length of Exposure with 95% Confidence Interval Band



The ATT results seen in Figure 4 seem to mostly support my findings from the fixed effects models, but with some more nuance. For almost all of the treatment groups, the 95% confidence intervals for each ATT estimate include 0 for all years, even post-treatment years. This means that the 95% confidence intervals of the average difference in outcome between the treatment and control groups include "zero difference" as a possibility for most years. A full table of these results can be found in the Appendix Table 3A. Based on these results, it is possible that the BRI simply has little to no effect on the log PPP GDPpc of African countries.

However, there are some exceptions in the ATT data that point to a slightly more complex story. When looking at the ATT results for countries treated in 2015 and 2016, the treatment effect becomes significant around 3 to 4 years after the treatment. For those treated in 2015, we can see the treatment effect is negative starting in 2019 and 2020. For those treated in 2016, we can see the treatment effect is positive starting in 2020.

While the effects differ, they do point to the interesting possibility of a lagged effect of the BRI, which is supported by Figure 5. The event study visualization shows that the average effect of the treatment might only be significant 5 years post-treatment. Thus, in another 5 years, it might be possible to more aptly compare the countries treated after 2016, and then conclude upon a more consistent measure of the average effect of the BRI in Africa.

Overall, the results of the ATT analysis suggest that the findings of the fixed effects models were likely subject to the limitation I discussed above. Exercising caution about the causal effect of the BRI was appropriate in this case. Cross-referencing my findings in both the fixed effects model and ATT calculation leads me to conclude that as of now, the BRI has had a limited effect, if at all, on economic growth. However, the results of the ATT analysis also show that it is not unprecedented that the effect of the BRI is lagged. Thus, this question should be revisited when more time has passed, and the benefits of the BRI may have come into effect. In particular, it would be interesting to conduct an event study analysis of the BRI's impacts in the future. In a few more years, the possible effect of the BRI should be observable in more countries and it might be more feasible to get a better picture of the dynamic effects of the treatment.

VIII. Conclusion

A. Overview of Findings

In this paper I conducted an analysis of the economic growth effects of the Belt and Road Initiative (BRI) in African countries. I used a difference-in-differences approach to estimate the treatment effect of the BRI on the purchasing power parity GDP per capita (PPP GDPpc) in constant 2017 international dollars of 53 African countries. With panel data from the World Bank, I conducted my main difference-in-differences analysis using a series of fixed effect regressions to control for static country and year differences. The data I used spanned from 2012 to 2020, and I relied on Chinese news and government sources to determine the treatment year of countries in the study.

Based on the existing literature related to this topic, I initially hypothesized the BRI would have a very strong and positive effect on the growth of PPP GDPpc for African countries. However, my initial results proved contrary to my hypothesis. In a two-way fixed effects regression model, the effect of the treatment proved to be insignificant at the 5 percent level. The addition of a region-year interaction fixed effect was intended to mitigate the possible omitted variable bias in the two-way fixed effects model. The inclusion of region-year fixed effects did result in a negative treatment effect that was significant at the 10 percent level but not at the 5 percent level. Using an event study regression model, I also tested for parallel pre-treatment trends. The results of this analysis showed only moderately parallel pre-treatment trends, which lent limited support to a causal interpretation of my results.

Even if I attempted to draw a weak causal claim about the treatment effect I observed, the negative nature of this effect would still contradict my hypothesis. It is not completely implausible that the treatment effect is negative. If the true nature of an MOU is misinterpreted, the effect may be negative. However, considering the estimated treatment effect was only significant at the 10 percent level and pre-treatment trends were only moderately parallel, I find the results in the fixed effects models are not strongly indicative enough of any causal effect.

After running my fixed effects models, I also conducted a robustness check using a method for estimating treatment effects proposed by Callaway and Sant'Anna (2021). The method in Callaway and Sant'Anna (2021) recommends calculating a group-time average treatment effect on the treated (ATT) in situations with multiple treatment periods and heterogeneous treatment effects. Results from this robustness check mostly supported what I concluded using fixed effects regressions. For the most part, the effect of the BRI on PPP GDPpc is relatively insignificant across the board. The only exception is that the ATT results point to the possibility of a lagged effect of the BRI. This became clear when averaging ATT estimates into average treatment effects at different lengths of exposure to the treatment in order to observe dynamic treatment effects. It seems possible that the BRI might have a lagged effect. Regardless of the ATT results, consideration of the fact that infrastructure takes time to build means a lagged effect of the BRI is very reasonable. Very few countries were actually observed 5 years beyond the treatment, so it is likely best to wait and conduct the analysis at a later year to get a better picture of the dynamic effects of the treatment.

B. Implications and Future Research

The first major implication of my finding is that it undermines some of the expectations in existing literature about the positive effects of the BRI. Based on the results of both the fixed-effects and ATT analysis, the treatment effect of the BRI in Africa is small if not non-existent within the time frame of my analysis. However, consideration of the time it takes to complete infrastructure projects means it might be that the BRI simply has a lagged effect that was not observed in the time frame I studied. Completing a new railroad or maritime port may take many years, meaning that the effect of the BRI may not even be noticeable until many years have passed. Thus, future research that includes more post-treatment years could prove insightful into the possible dynamic effects of the BRI. Further research on this topic might verify the little or weak effect of the BRI on African countries, or it might strengthen the case for a negative treatment effect.

Still, the results I have observed in my analysis demonstrate that the effect of the BRI is not so straightforward. Signing an MOU is definitely not a guaranteed way of improving the economic growth of participating countries, at least in the short term. In fact, it may even be possible that signing an MOU leads to negative growth effects if the results in Tables 3 and 4 model (4) continue to hold in future research. This implies that caution should be taken when promoting the benefits of the BRI as a method of improving economic growth. This is not to say that the future may not show signs of the positive benefits of the BRI in African countries. Rather, it is too presumptuous to hold strong expectations of the positive effects of the BRI in Africa given what I have observed. More caution should be taken.

Overall, in terms of future research, I suggest continuing research on this topic once more years have passed, especially in order to explore the dynamic effects of the BRI. It may also be interesting to conduct analysis with other methods of alleviating possible omitted variable bias such as an instrumental variable regression or regression with other covariates. Even if the effects of the BRI on economic growth are ultimately not significant in Africa, there may also be non-economic effects of the BRI in Africa that are still of interest to political leaders, such as soft power effects. Thus, there are definitely more questions that can be answered in regards to the BRI in future research, and I encourage more exploration of this topic.

References

Callaway, B., & Sant'Anna, P. H. C. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. https://doi.org/10.1016/j.jeconom.2020.12.001

de Chaisemartin, C., & D'Haultfœuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, *110*(9), 2964–2996. https://doi.org/10.1257/aer.20181169

de Soyres, F., Mulabdic, A., & Ruta, M. (2020). Common Transport Infrastructure: A Quantitative model and estimates from the belt and road initiative. *Journal of Development Economics*, *143*, 102415. https://doi.org/10.1016/j.jdeveco.2019.102415

Dumor, K., Li, Y., Ampaw, E. M., Essel, C. H., Essel, E. O., & Mutiiria, O. M. (2021). Situating Africa in the exports patterns of China's belt and road initiative: A network analysis. *African Development Review*, *33*(2), 343–356. https://doi.org/10.1111/1467-8268.12540

Githaiga, N. M., Burimaso, A., Wang, B., & Ahmed, S. M. (2019). The belt and road initiative: Opportunities and risks for Africa's connectivity. *China Quarterly of International Strategic Studies*, *05*(01), 117–141. https://doi.org/10.1142/s2377740019500064

Herrero, A. G., & Xu, J. (2017). China's belt and road initiative: Can europe expect trade gains? *China & World Economy*, *25*(6), 84–99. https://doi.org/10.1111/cwe.12222

Kalu, K., Farrell, C., & Lin, X. (2020). China's belt and road initiative: Implications for intra-regional trade in Africa. *Journal of Public Affairs*, *22*(1). https://doi.org/10.1002/pa.2347

Lall, S. V., & Lebrand, M. (2020). Who wins, who loses? understanding the spatially differentiated effects of the belt and road initiative. *Journal of Development Economics*, *146*, 102496. https://doi.org/10.1016/j.jdeveco.2020.102496

Luo, Z., Wan, G., Wang, C., & Zhang, X. (2021). The distributive impacts of the belt and road initiative. *Journal of Economic Surveys*, *36*(3), 586–604. https://doi.org/10.1111/joes.12436

Ma, S. (2022). Growth effects of economic integration: New evidence from the belt and road initiative. *Economic Analysis and Policy*, *73*, 753–767. https://doi.org/10.1016/j.eap.2022.01.004

Marcus, M., & Sant'Anna, P. H. (2021). The role of parallel trends in event study settings: An a application to environmental economics. *Journal of the Association of Environmental and Resource Economists*, *8*(2), 235–275. https://doi.org/10.1086/711509

McBride, J., Berman, N., & Chatzky, A. (2023, February 2). *China's Massive Belt and road initiative*. Council on Foreign Relations. Retrieved April 23, 2023, from https://www.cfr.org/backgrounder/chinas-massive-belt-and-road-initiative#chapter-title-0-4

Nolan, J., & Leutert, W. (2022, March 9). *Signing up or standing aside: Disaggregating participation in China's belt and road initiative*. Brookings. Retrieved April 23, 2023, from https://www.brookings.edu/articles/signing-up-or-standing-aside-disaggregating-participation-in-chinas-belt-and-road-initiative/

Sant'Anna, P. H. C., & Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, *219*(1), 101–122. https://doi.org/10.1016/j.jeconom.2020.06.003

Sow, M. (2022, March 9). *Figures of the week: Chinese investment in Africa*. Brookings.

Retrieved April 23, 2023, from https://www.brookings.edu/blog/africa-in-focus/2018/09/06/figures-of-the-week-chinese-investment-in-africa/

Wen, X., Ma, H.-L., Choi, T.-M., & Sheu, J.-B. (2019). Impacts of the belt and road initiative on the China-Europe Trading Route Selections. *Transportation Research Part E: Logistics and Transportation Review*, *122*, 581–604. https://doi.org/10.1016/j.tre.2019.01.006

Yang, G., Huang, X., Huang, J., & Chen, H. (2020). Assessment of the effects of infrastructure investment under the belt and road initiative. *China Economic Review*, *60*, 101418. https://doi.org/10.1016/j.chieco.2020.101418

Yu, L., Zhao, D., Niu, H., & Lu, F. (2020). Does the belt and road initiative expand China's export potential to countries along the belt and road? *China Economic Review*, *60*, 101419. https://doi.org/10.1016/j.chieco.2020.101419

Appendix:

Table 1A: List of Countries and Year of Treatment

Country	Year Joined BRI	Country	Year Joined BRI
Algeria	2018	Madagascar	2017
Angola	2018	Malawi	2022
Benin	2019	Mali	2019
Botswana	2021	Mauritania	2018
Burkina Faso	2021	Mauritius	NA
Burundi	2018	Morocco	2017
CAR	2021	Mozambique	2018
Cameroon	2018	Namibia	2018
Cape Verde	2018	Niger	2019
Chad	2018	Nigeria	2018
Comoros	2019	Republic of Congo	2018
Cote d'Ivoire	2018	Rwanda	2018
DRC	2021	Sao Tome & Principe	2021
Djibouti	2018	Senegal	2018
Egypt	2016	Seychelles	2018
Equatorial Guinea	2019	Sierra Leone	2018
Eswatini	NA	Somalia	2018
Ethiopia	2018	South Africa	2015
Gabon	2018	South Sudan	2018
Gambia	2018	Sudan	2017
Ghana	2018	Tanzania	2018
Guinea	2018	Togo	2018
Guinea Bissau	2021	Tunisia	2018
Kenya	2017	Uganda	2018
Lesotho	2019	Zambia	2018
Liberia	2019	Zimbabwe	2018
Libya	2018		

Note: NA means the country has never been treated as of writing. Also note Eritrea has been excluded due to missing data.

Table 2A: Results of Event Study Regression for Two-Way Fixed Effects

	Dependent variable:
	lngdppc_ppp
pre7	0.116**
	(0.046)
pre6	0.061*
	(0.031)
pre5	0.041
	(0.028)
pre4	0.021
	(0.026)
pre3	0.016
	(0.024)
pre2	0.005
	(0.022)
pre0	−0.004
	(0.022)
post1	−0.004
	(0.025)
post2	−0.002
	(0.029)
post3	−0.021
	(0.044)
post4	0.020
	(0.069)
post5	−0.086
	(0.095)
Observations	466
R^2	0.022
Adjusted R^2	−0.154
F Statistic	0.746 (df = 12; 394)
Note:	*p<0.1; **p<0.05; ***p<0.01

Table 3A: Summary of ATT(g,t) Results

Group	Time	ATT(g,t)	Standard Error	[95% Simulatenous	Confidence Band]
2015	2013	0.0023	0.0142	-0.0334	0.0381
2015	2014	-0.0179	0.0089	-0.0404	0.0045
2015	2015	-0.0095	0.0081	-0.0299	0.0108
2015	2016	-0.0157	0.0116	-0.0448	0.0133
2015	2017	-0.0217	0.0174	-0.0654	0.022
2015	2018	-0.0241	0.0451	-0.1371	0.089
2015	2019	-0.1026	0.0150	-0.1402	-0.0649 *
2015	2020	-0.1374	0.0212	-0.1905	-0.0843 *
2016	2013	-0.0104	0.0141	-0.0457	0.0249
2016	2014	-0.0108	0.0090	-0.0333	0.0117
2016	2015	0.0192	0.0081	-0.0012	0.0396
2016	2016	0.0182	0.0062	0.0026	0.0338 *
2016	2017	0.0246	0.0126	-0.0069	0.0562
2016	2018	0.0510	0.0331	-0.0320	0.1341
2016	2019	0.0319	0.0127	0.0001	0.0638 *
2016	2020	0.0926	0.0138	0.0579	0.1274 *
2017	2013	0.0003	0.0161	-0.0401	0.0408
2017	2014	0.0402	0.0404	-0.0612	0.1416
2017	2015	0.0113	0.0127	-0.0205	0.0431
2017	2016	0.0039	0.0086	-0.0178	0.0256
2017	2017	-0.0048	0.0139	-0.0397	0.03
2017	2018	-0.0077	0.0357	-0.0972	0.0818
2017	2019	-0.0445	0.0566	-0.1864	0.0974
2017	2020	-0.0693	0.0559	-0.2096	0.071
2018	2013	0.0219	0.0301	-0.0536	0.0974
2018	2014	-0.0095	0.0158	-0.0493	0.0302
2018	2015	-0.0016	0.0147	-0.0386	0.0354
2018	2016	-0.0055	0.0122	-0.0360	0.0249
2018	2017	0.0102	0.0140	-0.0248	0.0453
2018	2018	0.0047	0.0104	-0.0214	0.0309
2018	2019	-0.0170	0.0116	-0.0462	0.0122
2018	2020	-0.0287	0.0277	-0.0981	0.0408
2019	2013	-0.0007	0.0219	-0.0556	0.0541
2019	2014	-0.0098	0.0122	-0.0403	0.0207
2019	2015	-0.0230	0.0246	-0.0846	0.0387
2019	2016	-0.0185	0.0253	-0.0819	0.0449
2019	2017	-0.0281	0.0192	-0.0764	0.0202
2019	2018	-0.0219	0.0198	-0.0715	0.0278
2019	2019	-0.0270	0.0177	-0.0714	0.0173
2019	2020	-0.0239	0.0317	-0.1034	0.0556

*Note: Significance code * means confidence band does not cover 0*