Welfare Impacts of Chinese Development Finance in Cambodia

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Abstract

China's development finance program is at odd with the characteristics of traditional aid. The country's lending practices, focus sectors and implementation methods have fueled heated debates on its effectiveness as a donor. Over the last two decades, the detrimental vision of China's aid has gained most traction. I confront this claim with an empirical analysis of the effect of Chinese aid projects on households'welfare in Cambodia. To do so, I use geo-referenced project data combined with demographic information from Demographic Health Surveys. Relying on an innovative Difference-in-difference method that takes heterogeneity of treatment-timing into account, I compute the average treatment effects for two welfare outcomes: education and nutrition. I further investigate potential indirect channels of improvement in welfare: economic well-being and time to travel. Finally, I investigate treatment dynamics by computing treatment effects over different time-periods since treatment. I find that education is positively impacted by the presence of Chinese aid projects, while there is not significant effect for nutrition. Improvement in education is likely to be driven by my two indirect channels, which are both significantly impacted by treatment. Moreover, the dynamic analysis reveal that only groups of households treated for more than 10 years are significantly impacted by treatment. For these groups, the average treatment effects is significantly larger in magnitude than the aggregated treatment effect computed before.

1 Introduction

1.1 General Introduction

According to Hun Sen, who has been the strong man in Cambodia for the last 23 years,"China has a way of doing thing. They talk less but do more. Before anyone knows it, we get bridges, roads, etc. They are all without conditions" (Prime Minister Cabinet Office, 2009). Since the emergence of China as an international donor, scholars have similarly pointed out the differences between Chinese "aid"[1] and western, or traditional, aid. In fact, China is probably the largest emerging donor in the world and owns more official credit than the World Bank or the IMF (Horn et al., 2021).

Chinese development finance emerged more than 60 years ago (Strange et al. 2013; Kobayashi, 2008). These financial flows were initially used to support social leaders but became later oriented toward political goals, such as the recognition of Beijing instead of Taipei. It is after the end of the Cold War, and with the support of its exponential growth, that China started to use external assistance at a larger scale (Kobayashi, 2008; Renard, 2018). This new role has been particularly visible in Africa, under the "strategic partnership" inaugurated at the Forum on China-Africa Cooperation (FOCAC) in 2000. China's strategy was further made public to international observers in 2011 through the release of a "White Paper on China's Foreign Aid" (Strange et al. 2013; PRC, 2011).

Chinese aid significantly differs from western aid (Chin & Gallagher, 2019). Regarding its lending practices, China tends to focus on the "hardware" of development, including notably transportation and energy infrastructure (Dreher et al., 2021; Chin & Gallagher, 2019). Traditional donors on the other side, favor smaller micro-level interventions in education, health or environment, though larger amounts are made available for governance-type reforms (World Bank, 2017). Lending is done through substantial lines of credit and loans, provided by multiple Chinese actors [2] and multinational development banks (Chin & Gallagher, 2019). Resulting from the diversity of actors involved, development finance is provided under many forms: concessional and non-concessional, aid and commercial lending (Chin & Gallagher, 2019). On the opposite, development finance from traditional donor is largely provided under the form of aid and concessional loans from national or multinational development banks. Finally, development finance from China is not tied to clear policy conditionality, contrary to western donors, even though Gallagher and Irwin (2015) have shown that sometimes, conditions on purchases and procurement for projects are added.

[1] In this study, I will use the terms Chinese development finance and Chinese aid interchangeably to represent China's overseas development finance flows coming from public actors. All financial flows considered in this analysis are aligned with the OECD's definitions of Official Development Assistance (ODA) and Other Official Finance (OOF), but they do not include official investment. ODA flows have a grant component of at least 25 percent and aims at improving development and welfare of recipient countries, while OOF are financial flows that do not meet either one of these two criterion. More details on these distinctions will be given in section 2. Literature review.

[2] Chinese development actors represent a complex apparatus. It includes the State Council, the Ministry of Commerce (MOFCOM), China's EXIM bank and the China Development Bank, the Ministry of Finance (MOF) and the Ministry of Foreign Affairs (MOFA). MOFCOM, MOF and MOFA are the most important actors (Strange et al., 2013; Huang, 2011). There also instances where development funds are channeled through private commercial banks.

These differences have fueled two competing visions of Chinese as a global donor. The first one points out that China is complementary to traditional aid, as it focuses on different sectors and engages with countries that do not necessarily work with traditional donors. Furthermore, China possesses tremendous resources, unmatched by traditional donors, which can generate the "big push" that poor countries need to get out of poverty (Rosenstein-Rodan, 1943, 1961). The opposite vision of a "dangerous" China, has expanded over the years and is reinforced by the opacity and secrecy surrounding the country's development flows[3].

China has been blamed for providing "rogue aid", or aid without conditionality to serve its own interests; on the contrary, traditional donors supposedly provide targeted aid, better aligned with recipients' needs (Dreher et al., 2015; Naím, 2007). Selfish allocation motives could indeed hinder the effectiveness of Chinese aid in recipient countries. Among other popular criticisms, Chinese aid has been tied with natural resources exploitation and "rogue states" support due to its non-interference policy (Naím, 2009, Strange et al., 2013). China has also been accused of funding "white elephants" projects through large non-sustainable loans, in order to later exchange debt against geopolitical and economic concessions (Chellaney, 2019). Moreover, China faces regular accusations of environmental and labor laws violation in recipient countries. Critics make similar arguments concerning financial flows channeled through the Belt and Road Initiative (BRI)[4]. However, many studies support the argument that the supposed altruistic allocation of aid from western donors is actually entrenched in political and strategic interests (Kuziemko & Werker, 2006; Vreeland & Dreher, 2014; Öhler & Nunnenkamp, 2014). Furthermore, studies investigating the supposedly positive effects of traditional aid have found mixed evidence. Thus, it remains to be seen whether the characteristics of Chinese development finance at the national and sub-national level truly impede its projects from fuelling any major positive outcomes on the ground.

Empirical analysis on the effectiveness of aid and Chinese aid are numerous but studies on its impact on welfare indicators have been largely overlooked (Kotsadam et al. 2018). Yet, the importance of education and nutrition for a person's life has been well documented (Cruzatti et al., 2020; World Bank, 2018). While China is known for its focus on infrastructure and energy, looking at figures in number of projects rather than financial commitment reveal a slightly different picture. Projects in the social sector like education, health and governance are a lot smaller than infrastructure and energy projects in financial size, but in fact represent a larger number of overall projects (Dreher et al., 2021)[5]. China is notably well-known for being very present in the health and education sectors in Sub-Saharan Africa (Strange et al., 2017; King, 2010; Bräutigam, 2009; Morgan & Zheng, 2019).

[3]The complex apparatus of Chinese development institutions contributes to the opacity of its presence overseas. Contrary to countries such as France or Australia, China does not have a unique Development institution.

[4]Indeed, while BRI projects involve larger loans and an increase in portfolios, the sectoral or geographical composition of China's development finance has not been modified (Malik et al., 2021).

[5]This computation relies on data collected by the authors that encompasses "Chinese project aid, food aid, medical staff and total aid money to developing countries" from 1956 to 2006 (Dreher et al., 2015).

1.2 Problematic and Change Theory

China's lending practices, its focus sectors and implementation methods significantly differ from those of traditional donors. These differences have been used to paint a detrimental picture of Chinese development finance effectiveness in recipient countries. Most of these accusations however, rely on very little empirical evidence (Strange et al. 2013, Wang & Zadek, 2016; Cooper, 2019)

In this study, I investigate the local effects of Chinese development finance in Cambodia empirically. I argue that Chinese development finance could have positive effects on welfare outcomes in Cambodia, through a combination of channels. Chinese projects could impact development indicators directly with public investments in the social sector, such as education and health. Projects in these sectors would likely increase the number of hospitals and schools as well as improve the training for medical personal and teachers, which would both positively impact education and health indicators. Moreover, Chinese projects in the economic sector (infrastructure, communication, energy...) are also likely to impact development, though in an indirect way. Better infrastructure can make social infrastructure more accessible and generate a significant increase in economic well-being due to economic spillovers (agglomeration effects) as well as cheaper and better access to energy sources (Cruzatti et al., 2020; King, 2010). Hence, better economic welfare can also support and reinforce improvements in development outcomes.

I chose to pick an Asian country, Cambodia, as my case-study. While most studies have focused on Chinese development flows to Africa (Dreher et al., 2018, 2021; Guillon & Mathonnat, 2019; Guillon & Mathonnat, 2020; Martorano et al., 2020), the rise of China as a donor is also of great importance for its own region. This can be explained by China's global strategy and neighborhood policy combined with BRI routes and geo-strategic issues in the region (Oh, 2020). Supporting the important role of Asia in China's overseas lending strategy, figures in financial commitments (US Dollars) - rather than numbers of projects - show that Southeast Asia is ahead of Africa in terms of projects which financial value exceeds 1 b US Dollars ("mega-projects"). Moreover, only 6 of the 25 largest Chinese projects are located in Africa (Dreher et al., 2021). Cambodia in particular, is the country with the highest number of Chinese projects; it is also a very poor and autocratic country, which is the perfect case-study to test whether previously identified critics have any traction in this context.

1.3 Contribution to the Literature

In this study, I investigate the effects of aid at the sub-national level, thereby contributing to the literature on local aid effectiveness as follows. First, I rely on geo-referenced project-data and combine them with household surveys for Cambodia to investigate outcomes at the micro-level (Kotsadam & Tolonen, 2016; Isaksson & Kotsadam, 2018; Martorano et al. 2020). I link project information from the Aiddata 2.0, a dataset that was very recently issued in September 2021 and thus used in few studies so far. Second, I take an Asian country as my case-study. To my knowledge, this has not been done before, except in cross-country and qualitative analysis (Dadabaev, 2018; Reilly, 2012). Even though the availability of DHS data was a prominent criteria for selecting Cambodia among other Asian

countries, Cambodia is a large aid recipient that has received a diverse set of projects over a long-time period. Third, I focus on welfare variables rarely investigated in the literature of aid and Chinese aid (Kotsadam et al. 2018; Cruzatti et al., 2020, Martorano et al. 2020). Through this analysis, I provide evidence of positive outcomes of Chinese aid on education as well as on my two indirect channels: economic well-being and travel time. I also provide evidence of differential effects over different periods since treatment. Indeed, changes in welfare do not appear overnight: to better understand the pattern of Chinese aid on welfare outcomes, it is important to investigate its dynamic effects. Fourth, I use an innovative quasi-experimental Difference-in-difference method (Callaway & Sant'Anna, 2021) that accounts for heterogeneity of treatment-timing and effect across groups of households. This model allows me to reduce the endogeneity resulting from a probable selection bias, by using an estimator that relies on both inverse probability weighting and outcome regression for inference (Callaway & Sant'Anna, 2021). This is an important change from the empirical literature on Chinese development finance, which often relies on an instrumental variable [6] or natural experiments.

I will start by presenting the literature on which this study built upon. I will then introduce the characteristics of China's presence as a donor in Cambodia and introduce the different sources of data used to construct the final database. Then, I will present my empirical strategy and the results obtained for each model. Finally, I will conclude with a brief discussion.

2 Literature Review

2.1 Assessing Aid Effectiveness

The birth of development aid (ODA) - as it is currently defined by the DAC - is the result of a historical trajectory that began at the end of the Second World War and the implementation of the Marshall Plan. The term "Overseas Development Aid" was defined in 1972 (Scott, 2015). Nowadays, ODA must meet two criterion : being administrated to promote the "economic development and welfare of developing countries as its main objective" and being concessional in character *i.e.* meeting the threshold for grant component as defined by the OECD. The effectiveness of development aid (ODA) has since fueled many debates and is polarized around two views. The positive one argues that development aid can end poverty through appropriate targeting (Sachs, 2005). The opposite view highlights the tremendous amount of money spent on aid and the very poor results obtained, criticizing aid as inefficient and even harmful (Easterly, 2006; Moyo, 2014; Alesina & Dollar, 2000). Whether international aid carries the potential for positive impact is still widely debated (Galiani et al., 2016; Doucouliagos, 2019) to the point of being called the most controversial debate in development economics (Qian, 2015).

[6] The instrument, defined by Dreher et al. (2019) is inspired by Nunn and Qian (2014). It was initially used to measure the impact of Chinese projects on regional development. The instrument for Chinese aid projects relies on an exogenous supply variable interacted with a local probability of receiving aid. The exogenous supply considered is steel, because amount of aid tend to be higher when there has been an excess supply of steel one year prior. Regarding the probability of receiving aid, it uses the share of years within the sample in which a sub-national entity received aid from China.

This debate is rendered even more difficult when it comes to non-traditional donors such as China. First of all, China does not disclose its official aid figures in accordance with international standards. This explains that Wang & Zadek (2016) reveal a large predominance of anecdotal evidences in studies that investigate Chinese aid. Second, China is not part of the OECD Development Assistance Committee and its development flows do not meet the OECD-DAC categorizations[7] (Strange et al. 2013). Comparisons between development financial flows from China and DAC members' flows is thus doubly challenging (Strange et al. 2013). Bräutigam (2009, 2010, 2011a, 2011b) demonstrated this issue in her investigations of the many forms of Chinese development finance in Africa. Indeed, many studies and media articles tend to mix concessional and non-concessional sources of funding from China, tangling flows with different objectives, sources of financing and grand components.

Faced with these two major challenges, research conducted by the Aiddata research lab from the University of William & Mary gave birth to a series of databases on Chinese Development Finance. Based on a systematic, transparent and replicable methodology, the Tracking Under-reported Financial Flows (TUFF) methodology was first introduced in April 2013 (Strange et al., 2013; Custer et al., 2021). It was then improved over the years (Dreher et al., 2019; Bluhm et al. (2020), Dreher et al. (2021), Custer et al., 2021). Furthermore, the TUFF methodology developed a helpful categorization of Chinese official finance along OECD-DAC definitions, that better supports comparisons between donors. Chinese official development finance is thus categorized into "ODA-like" flows, "OOF-like" flows and "vague official finance". This last category encompass flows that could be either ODA or OOF but for which there is insufficient information (Strange et al., 2013). Another major advance is the introduction of geo-referenced Chinese projects data that enabled more studies to conduct analysis at the sub-national level (Martorano et al., 2020).

2.2 Effectiveness and Allocation of Chinese Aid

The drivers behind Chinese aid allocation have drawn considerable interest from scholars. Specific characteristics of Chinese aid have raised suspicions of selfish allocation drivers, that would reduce its potential for development. Empirical studies however, do not unconditionally support the conclusion that China's aid allocation is more self-interested than aid from traditional donors (Hendrix & Noland 2014, chapter 5; Bader, 2015a; Dreher & Fuchs, 2015). Dreher & Fuchs (2015) use data on Chinese aid flows to highlight the importance of foreign policy interests in Chinese aid allocation, similar to traditional donors. They found

[7]The three OECD-DAC categories are Official Development Assistance (ODA), Other Official Flows (OOF) and Private flows. ODA is defined as "[g]rants or loans to [developing] countries and territories ... and to multilateral agencies which are: (a) undertaken by the official sector; (b) with promotion of economic development and welfare as the main objective; (c) at concessional financial terms (if a loan, having a grant element of at least 25 per cent). In addition to financial flows, technical co-operation is included in aid". OOF includes "[t]ransactions by the official sector with [developing] countries ... which do not meet the conditions for eligibility as Official Development Assistance, either because they are not primarily aimed at development, or because they have a grant element of less than 25 per cent". Finally private flows "consist of flows at market terms financed out of private sector resources (i.e. changes in holdings of private long-term assets held by residents of the reporting country) and private grants (i.e. grants by non- governmental organizations and other private bodies, net of subsidies received from the official sector)". (OECD DAC glossary)

no conclusive evidence that Chinese aid is driven at the national level by natural resources endowment in Africa; Cheung et al. (2011) broadened the data sample to include Foreign Direct Investment and this time, found a positive relationship between Chinese Outward Direct Investments and natural resources endowments.

These conflicting results show how important it is to disaggregate the analysis between types of flows and targeted sectors. Dreher et al. (2018) study the relationships between the different forms of Chinese aid allocated to Africa from 2000 to 2012 with political and economic outcomes. Similar to previous analysis, their findings indicate that concessional ODA flows are related to foreign policy interests but does not favor natural resources endowments nor corrupted regimes. On the other hand, less concessional OOF flows are rather driven by more commercial purposes, such as bilateral trade relations and natural resources endowments. Such a dichotomy is important according to Dreher et al. (2018), who argue that the criticisms surrounding Chinese aid is the result of an absence of distinction between development flows with different goals. Studies on the determinants of Chinese aid also looked at differences in allocation between sectors. Guillon and Mathonnat (2020) investigate allocation by sector in Africa and found that projects in the social infrastructures and services sector align with economic needs as well as foreign policy alignment, while projects in the economic sector however, appear more aligned with natural resources endowments.

The potential link between Chinese aid and corrupted regimes is investigated in a few studies that more or less support the view of Chinese aid as "Rogue aid". Broich (2017) focuses on the allocation of Chinese development finance (without distinction between ODA and OOF) to African autocratic regimes between 2000 and 2011, finding no evidence that authoritarian countries receive more Chinese development finance. Similarly, Guillon and Mathonnat (2020) showed that governance quality (autocracy, corruption) is not a factor for allocation of social infrastructure and social services; however, it is an important factor for the economic infrastructure and production sectors. At the subnational level, Dreher et al. (2019) look at the critics on an allocation of aid allocated primarily to corrupt leaders. This time, they find a positive correlation between African countries' leaders birth region and allocation of Chinese development aid. They also find that Chinese aid is not allocated to the poorest regions, similar to traditional aid (Briggs, 2017). Supported by past research, the authors claim that this concentrated allocation of aid is unlikely to be optimal at the national level in terms of development outcomes (Cohen, 1995; Wright, 2010; Dionne, 2017). This is also supported by Guillon and Mathonnat (2019) who found that Chinese health projects in Africa are allocated following the economic needs of recipient countries but not their needs in health.

Regarding Chinese aid effectiveness, the literature largely focus on its economic impacts (Martorano et al., 2020). Dreher et al. (2019) find positive effects on growth at the subnational level, using an instrumental variable that relies on time variation in China 's production of steel - a material used as a primary input for aid projects - and variation in the geographical probability to receive aid. Dreher et al. (2021) also find positive impacts on economic growth for recipients, two years after projects'commitments. Focusing on transport infrastructure projects, a well-known focus of China, Bluhm et al. (2018) find that Chinese development projects in transport infrastructure reduce special concentration within but not between regions. The effect of transport infrastructure financed by China was explored in other studies, notably in those focusing on the Belt and Road Initiative (BRI) (Baniya,

Rocha & Ruta., 2020; Villafuerte et al., 2016; Zhai, 2018).

Another important strand of the literature on Chinese aid effectiveness focuses on governance outcomes in recipient countries. One prominent criticism displays the view that China's presence lead to increased corruption at the local level. Isaksson and Kotsadam (2018) investigated such claims, relying on the assumption that Chinese disregard for corruption surrounding local projects can lead to an increase in local corruption in Africa. Using precise geographical projects locations to define treatment, they found positive empirical results. Regarding conflict outcomes, Ghering et al. (2018) found that, on average, Chinese aid tend to lower conflict in Africa. According to the authors, these results appear to be driven by projects in two sectors, the transportation and financial sectors.

The effects of traditional aid and Chinese aid on recipients welfare however, have been largely overlooked by the literature (Kotsadam et al., 2018). At the country level, conclusions on the effects of traditional aid on health outcomes are mixed (Chauvet et al., 2009; Nunnenkamp & Öhler, 2011). Sub-national studies focusing on infant mortality in Nigeria (Kotsadam et al., 2018) and the Ivory Coast (Wayoro & Ndikumana, 2019), both find positive results. In terms of education, traditional aid has been shown to support enrolments of basic education but progresses are still to be made on educational quality (Riddell & Niño-Zarazúa, 2016).

Research investigating Chinese aid on welfare outcomes is equally sparse. Grepin et al. (2014) was the first, to my knowledge, to investigate Chinese presence as a health donor. Leveraging new databases on Chinese aid with geographically desegregated data has allowed some authors to enter this gap. Cruzatti et al. (2020) compare mortality rates at the national and sub-national level for a large database spreading across continents. They found contradicting evidence: Chinese development aid both increase infant mortality at sub-national scale and decrease mortality at the country-level. This would mean that Chinese projects do not deliver positive outcomes where they would be supposed to, close to projects locations. According to the authors, these results can be attributed to the "fungibility hypothesis": the presence of a Chinese health project will incentivize the government to investigate its resources in another location. In their study, it is the difference in the focus of Chinese health projects and governments health projects that explains the negative impact on infant mortality at the local level. Regarding education outcomes, Yang & Ma (2015) find that China contributes well to international aid in education. This has been well documented in Africa (King, 2010; King, 2014; Bräutigam, 2009). At the global level, Reilly (2015) shows that Chinese aid in education focus on targeted activities: "higher education, vocational training, Chinese language instruction (...) and school construction" with a recent focus being put on vocational training and Chinese-language instructions. I have not found any empirical analysis on the effectiveness of these programs except for the study probably most aligned with my analysis by Martorano et al. (2020). They use geo-referenced data from Aiddata to investigate the micro-level development impact of China in Africa. They focus on three social development indicators in education and health and found that Chinese development projects increase education and reduce malnutrition. However, they rely on data aggregated at the country-year level, leading to information loss on projects specific locations, and do not take heterogeneity of treatment-timing into account.

3 Chinese Presence as a donor in Cambodia

3.1 Cambodia Profile

Cambodia is a country located in South-East Asia. With a surface area of 181,035 squared-km, the country counted 16 718 971 inhabitants at the last census in 2020. Although its economic growth has been increasing by 7.7% per year on average between 1998 and 2019, allowing the country to access lower middle income status in 2015 (World Bank, 2020)[8], its record in terms of human development remains very poor. Its human development index ranks the country 144th in the world (UNDP, 2020), despite an important evolution from 0.42 in 2000 to 0.59 currently.

This sharp fragility in human development indicators is part of a complex history. In 1863, the country became part of the French protectorate and was later incorporated into French Indochina. The country gained its independence in 1953 but remained marked with political instability. The Khmer rouge regime - a rural communist guerilla movement - brutally took power in 1975 and was responsible for the killing of at least 1.5 million people by 1979. The country started to heal from this tragedy under the People's Republic of Kombuchea (PRK) (1979-1989), a Vietnam-backed regime, and finally regained its political autonomy in the 1990s.

These hardships left important marks on the country and severely constricted its development. Hun Sen, prime minister under the PRK government, used the post-Khmer period to construct an authoritarian and corrupted regime that persist nowadays (Peou, 2019). Under its leadership, Cambodia's economic activities have remained highly centered around agriculture. The country is still at a relatively early stage of urbanization, with only 22% of the population living in urban areas, significantly less than its Southeast Asian neighbors (ADB, 2019). Most striking, the population remains poor and alarmingly unequal despite significant economic growth (UNDP, 2010).

However, trends in welfare outcomes have progressed over the last two decades. According to the World Bank (2022), "the maternal mortality ratio per 100,000 live births has decreased from 351 in 2005 to 160 in 2017". Similarly, "infant mortality decreased from 78 per 1,000 live births in 2000 to 22.8 per 1,000 in 2014". Further progress need to be made in different directions, notably to help bridge its large infrastructure gap and support large-scale education programs, in order to be able to provide essential public services such as health and education to the Cambodian population (UNDP, 2010; World Bank, 2022). International donors like China can play a big part in overcoming these challenges.

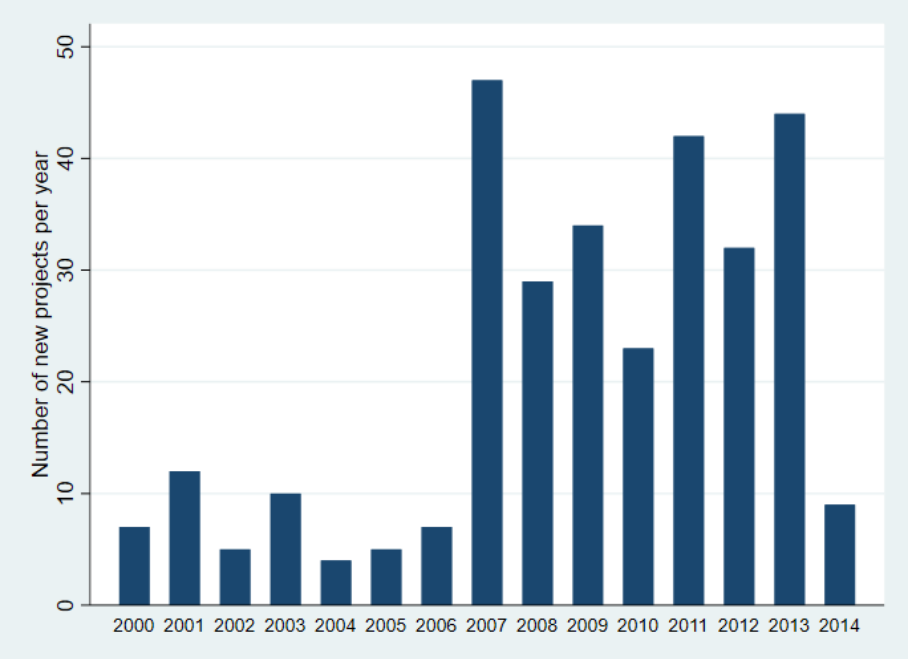
3.2 Chinese Investment in Cambodia

China's presence as a foreign actor in Cambodia has drastically increased over the last two decades and the country is currently Cambodia's biggest investor (Heng, 2012; Dunst, 2020). Cambodia is also the first country in terms of total number of Chinese development projects allocated from 2000 to 2014 (Dreher et al., 2021). This partially reflects the vision of China as the mean for Cambodia's development, supported publicly by Hun Sen.

[8]The World Bank defines the lower middle-income economies as countries for which GNI per capita is between $1,036 and $4,045.

Contemporary relationships between the two countries can be officially traced back to July 1958, rekindling a relationship going back to the 13th Century. Supporting King Sihanouk in the 1960s, China then supported the Khmer rouge regime of Pol Pot. While the extent of China's contribution to the Khmer rouge regime is unclear, Chinese officials confessed their support in the form of "food, hood and scythes" (Dunst, 2020). The support to the regime would actually have been as high as 90% of the Khmer's rouge entire foreign aid (Mertha, 2014). The scale of the sino-Cambodian relationships took a turn after 1997 (Chen, 2018): when a military conflict between the two major political sides in Cambodia broke out, traditional donors suspended their aid flows. In opposite, China extended its full financial support to Hun Sen's political party. Since then, their financial relationship has not stopped growing, experiencing a noticeable peak in 2007 at the time of China's "boom" in development finance flows (Chen, 2018). Figure 1 illustrates well this trend.

Figure 1: Evolution of Chinese presence in Cambodia: number of funded-projects per year. Source: Author's elaboration



According to Heng (2012), motives for this reconciliation can be traced along four lines. The main one is political: Cambodia is the strongest ally of China in Southeast Asia (Kah, 2019). It supports the "One China" policy since 1997 and cooperates on issues of political resistance such as Uyghur deportation. Cambodia's seat at the ASEAN also helps China to shape regional security, notably regarding the South China Sea dispute. Second, Cambodia is a strategic geographic location. The port of Sihanouk province ensure China's access to a route for its exports and energy supply. The infrastructure is part of the "string of pearl" strategy, where China's goal is to establish a series of "pearls" throughout the region, often by gaining access to harbors or airports. Third, there are important economic benefits to their relationship: in return for its assistance, Cambodia has granted China privileged access to Chinese public and private investments. Moreover, Cambodia possesses important

natural resources in crude oil, gas as well as mineral resources (EIC, 2008). China has been granted concessions to invest in these strategic sectors. Economically, China partly relies on Cambodia to outsource its agriculture: from 2000 to 2010, China was the second most important investor in agriculture in Cambodia (CDC 2010). Finally, China has aimed to expand its cultural influence and values over the country.

Thus, Chinese involvement in Cambodia is multifold. It is first a major foreign investor with an average of 640 million US Dollar per year from 1994 to 2005, and 5.3 billion US Dollar from 2006 to 2010 (CDC, 2012). China is also an important trading partner: among all ASEAN countries, it is China's bilateral trade with Cambodia that experienced the highest growth during the 2000-2010 period (Heng, 2012). Finally, China is a major bilateral donor. It became Cambodia 's biggest donor in 2009 with a commitment of 257 million USD (Palit & Palit 2011). Cambodia is thus largely ahead of its ASEAN neighboors in terms of number of projects funded since 2000 (see table 2). Most funding is concessional and pin-pointed for large infrastructure projects (Heng, 2012). The participation of Cambodia to the BRI since 2016 likely reinforced this trend. The level of Cambodia's debt to China is however unknown.

Table 1: Evolution of Chinese presence in ASEAN countries: number of funded-projects from 2000 to 2014. Source: Author's elaboration

ASEAN Countries	2000	2014
Brunei	0	16
Cambodia	7	303
Indonesia	0	88
Laos	0	115
Malaysia	0	12
Myanmar	3	67
Philippines	1	41
Singapore	1	10
Thailand	0	23
Vietnam	4	75

Usual criticisms of Chinese aid could have a lot of traction in the Cambodian context. The “no-strings-attached” policy gives almost total discretionary power to the Cambodian regime. The overall lack of transparency combined with an authoritarian and corrupted regime might reinforce bad governance, local corruption and poor quality of human rights (Isaksson & Kotsadam, 2018). Michaelowa and Weber (2007) further demonstrate that high level of development aid, as it is the case for Cambodia, can generate negative spillovers, such as political capture and deterioration of governance. Degraded governance is a known deterrent of school enrolment and Chinese aid would thus have diminishing returns, notably on primary and secondary education. This could be a important detrimental channel, in a country such as Cambodia, known for its high level of corruption. Moreover, the large focus of China in infrastructure and economic projects questions the ability of Chinese projects to

positively affect welfare outcomes. Indeed, changes in outcomes such as health and education rely on both improved infrastructure and economic well-being but also on behavior changes, with progresses to be made in professor training, nutrition classes or vaccination campaigns (Sanghvi et al., 2017). Thus, it remains to be seen whether positive welfare effects found in other studies also apply in the Cambodian context (Martorano et al. 2020, Kotsadam et al. 2018; Wayoro & Ndikumana 2019; Cruzatti et al. 2020).

4 Data

This study relies on two types of datasets :

- AidData Chinese Global Development Finance dataset, version 2.0
- Demographic and Health Surveys (DHS)

4.1 Aiddata 2.0

The Aiddata Chinese Global Development Finance dataset is the largest source of information on modern Chinese development finance. It stems from extensions and revisions of the TUFF methodology over the years, a methodology initiated as a mean to track underreported Chinese development projects around the world. The latest version of this dataset, version 2.0, was released in September 2021[9]. The entire database contains information on 13,427 Chinese development projects in 165 countries, for a cumulated amount of 843 billion US Dollar. The period spans from 2000 to 2017.

The sample used in this analysis contains information on Chinese projects in Cambodia, covering 13 sectors. Projects are sometimes separated across locations: in this analysis, I use all project-locations in the database as separate projects (Martorano et al., 2020). The database contains information on projects' geographical locations, sectors (using the OECD Creditor Report System (CSR) purpose codes), type of flows (ODA, OOF or Vague), status of completion, total financial amounts, whether a project is co-financed and start/end dates.

I proceed to some verification on the data. First, I only keep projects that are entirely financed by China by excluding projects co-financed with traditional donors. Second, projects whose geographic coordinates are absent are dropped from the database; since I define the treatment group based on geographic distance, projects locations are key to the identification strategy. However, there is still 15% of the projects that have imprecise geographic coordinates. I observe the level of granularity of the geographic data obtained, which remains within a precision code of up to 4 - similar to the first order administrative division (province, state or governorate). This is the preferred threshold applied in previous studies (Dreher & Fuchs, 2015; Briggs, 2018). Third, I check whether projects information are trustworthy - some projects starting dates are "uncertain" and the starting dates reported corresponds to the first mention of them in the media. As such projects only represent 18% of the overall projects and excluding them could bias my estimates, I choose to keep them.

The final database contains 202 projects (all project-location pairs) as depicted in Figure 1. Education and health projects represent 5,45% of the total, but social projects in general -

[9]The first version of the dataset was issued in 2017 as the Chinese Official Finance Dataset.

combining education and health with other social-oriented sectors such as "social infrastructure and services"[10] - account for 15% of all projects. Among all sectors, the most important is "Transport and storage" with 51% of all projects, followed by "Communications" at 11% (see Figure 2). This is consistent with previous research about Chinese aid allocation that puts large infrastructure as the most financed sector (Guillon & Mathonnat, 2020; Chin & Gallagher, 2019). As discussed previously, the large majority of projects is categorized as ODA-like, which represent 72% of the sample, with other official flows (OOF) representing 27% of all projects. Since ODA-like flows account for projects with a development intent, compared to OOF which reflect commercial interest, one could expect that the majority of Chinese aid projects are indeed designed to further Cambodia's development.

My treatment variable is a dummy that indicates the geographical presence of a project but does not take the intensity of financial commitment into account. Indeed, financial figures have not been officially confirmed by China and are subject to potentially important variation. Furthermore, financial commitments are only available for the whole project but not at the project site level. To avoid too many approximations, I solely rely on the number of projects to define treatment.

[10]Social projects are projects in the following sectors "Education", "Health", "Water supply and sanitation", "Government and Civil Society" and "Other Social Infrastructures and Services".

Figure 2: Mapping of Chinese Development Projects by Sector. Source: Author's elaboration on Aiddata

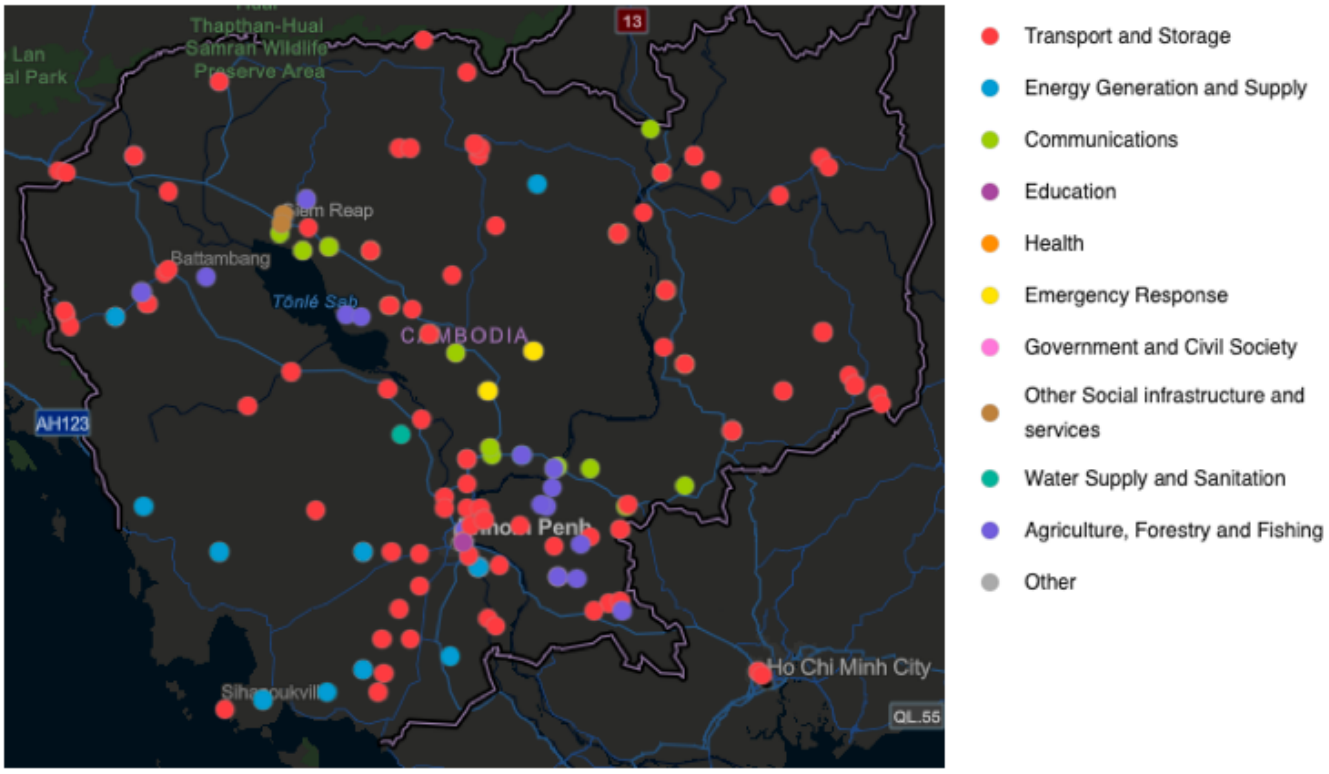
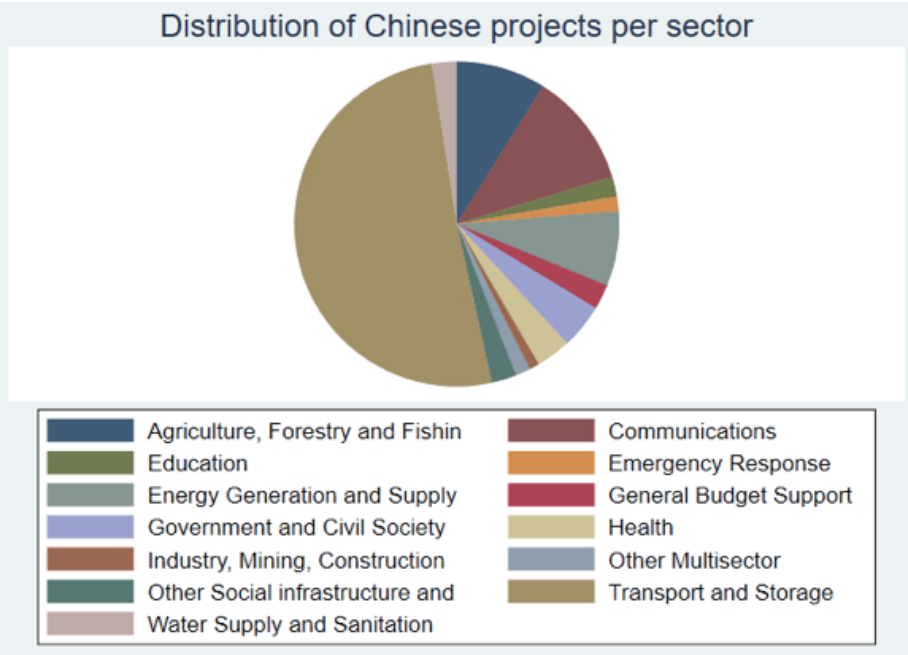


Figure 3: Distribution of Chinese Development projects by sector. Source: Author's elaboration.



Using the Aiddata database for Chinese development finance however presents limitation. The first one is also the most obvious: the data stems from different sources that have not been officially confirmed by the Chinese government (Strange et al., 2013). It is unlikely that the dataset represents Chinese development finance overseas in a completely exhaustive manner. It also poses a serious threat of selection bias: development finance from China might not be allocated randomly at the national and sub-national levels. Only the second threat to my estimation can be tackled through an appropriate identification strategy which will be explained in section 4. I notably rely on inverse probability weighting (IPW) that allows me to reweight the household sample according to probability of treatment.

4.2 DHS

DHS surveys are part of a large-scale survey program that relies on standardized questionnaires. Repeated cross-sectional data for a country can be obtained by combining different survey waves. For this analysis I combine 4 DHS survey waves: 2000, 2005, 2010 and 2014. In 2000, China was still receiving development assistance (Page & Pande, 2018) and there was very little Chinese presence overseas. As a result, the large majority of households in 2000 is not treated by a project and can be used as control for computations. However, 4% of the 2000 sample is treated; these observations will be considered as "always-treated" units and excluded from any computation. All post-2000 survey waves are used to compute an aggregate average treatment effect and dynamic average treatment effects. The final database across 4 time-periods compiles information for 57 589 households. Descriptive statistics for this final database can be found in table 1.

In order to assess the impact of Chinese development finance on household welfare in Cambodia, I focus on two dimensions:

- *Education*: I take the average years of education per household as my first key dependent variable. It is a common measure of a population's education level as well as its "stock of given capital" at a point in time (Roser, Ortiz-Ospina, 2016). It is also a measure commonly used in the literature (Martorano et al., 2020).

- *Health*: I use average anemia levels for women 15-49 as my second key variable, which measures hemoglobin levels for women in the same household. Anemia causes tiredness, weakness and lower tolerance to diseases. It is a particularly important indicator for women in age of pregnancy, as it can lead to premature delivery and low birth weight (Kariyeva et al., 2002). More generally, individuals with anemia have increased morbidity and mortality risks (Sharmanov, 1998). This variable considers 3 levels of anemia: mild, moderate and severe anemia[11].

Additionally, I use two additional variables to check for additional effects resulting from Chinese aid projects that would impact households' development:

[11]The index is higher for women with less anemia. The value 4 is given for women with no anemia, 3 for mild, 2 for moderate and 1 for severe.

- *Index of material living conditions*[12]: this index will be used as a proxy for households economic welfare. Indeed, increases in education and health might be driven indirectly by relevant spillovers of Chinese projects, such as increased local economic opportunities and agglomeration effects. The index of material living conditions aims to capture the increased economic welfare from Chinese projects that could in turn impact development indicators. This proxy is nonetheless imperfect because it reflects consumption of non-necessary goods and would only increase if Chinese projects translate into long-term increased economic well-being.

- *Time travel to water*: the time taken by households to reach drinkable water is an important development indicator. Having access to drinkable water is indeed key for health, since contaminated water propagates diseases and put households at health risks that could easily be avoided. It will thus be used as an additional dependent variable, that could be impacted by Chinese projects in "water supply & sanitation" as well as by transportation projects, which would reduce overall time needed to travel. Hence, it can also also give some idea about overall time to travel in a given location.

One limit with household data from DHS is that it is cross-sectional: I cannot measure the evolutions overtime of education or health variables for the same households. Nonetheless in section 4, I use models that are robust to repeated cross-section.

4.3 Geo-referenced database

I link household data (DHS) to project data (Aiddata) using geo-referenced information. Both households and projects come with GPS coordinates, that can be used to match information. Similar to recent literature using geo-referenced data to identify the effect of aid, I consider my treated households to be those in a 25km radius of at least one completed or ongoing Chinese project (Kotsadam & Tolonen, 2016; Isaksson & Kotsadam, 2018; Kotsadam et al. 2018; Martorano et al. 2020).

With the database obtained, I use my first survey year (2000) to compare the characteristics of households in an area that will be treated in the future with households in areas that will not be treated in the future. By doing so, I try to observe whether there is indeed a selection bias: I suspect that treated households will already be better off than households not treated - if this is the case, the average effect estimated without controlling for this effect will have a positive bias and the coefficient will be overestimated. I do not have panel data, so I have to postulate that different households in the same survey areas will have similar characteristics over the three following survey waves. The results of my t-test can be found in the Annexe (Figure 9). The results show that indeed, households in treatment area are on average more educated, richer and take less time to reach a drinkable water source.

[12]The index of material living conditions is built using DHS data. It aggregates household information on the type of water access, electricity, toilets and number of rooms per person in a household. This index is on a continuous scale and is higher for well-off households.

Table 2: Descriptive statistics

	Count	Mean	Sd	Min	Max
Dependent variables					
Average Years of Education	57589	2.42	2.52	0.00	59.00
Anemia Level	23610	3.43	0.63	1.00	4.00
Material Conditions of Living (index)	57589	3.41	1.39	2.00	8.00
Time to Water	31567	8.00	12.10	0.00	513.00
Households characteristics					
Household size	57589	4.90	2.01	1.00	24.00
Age of Household Members	57589	27.89	12.02	7.25	92.00
Age of Household Head	57589	45.86	14.00	10.00	98.00
Urban Households	57589	0.15	0.36	0.00	1.00
Female-Headed Household	57589	0.26	0.44	0.00	1.00
Dependency Ratio	57589	0.86	0.76	0.00	8.00
Treatment					
Treatment (2000)	12158	0.04	0.19	0	1
Treatment (2005)	14136	0.21	0.40	0	1
Treatment (2010)	15563	0.60	0.49	0	1
Treatment (2014)	15732	0.82	0.38	0	1

5 Empirical Strategy and Results

Difference-in-differences (Did) is a very popular quasi-experimental method, used to estimate causal impact. The standard version involves two groups over two periods. It further assumes that treatment effects are homogeneous and applied at the same time for all treated units. However in this analysis, households are treated at different periods in time - depending on projects starting dates. This is a typical setup in econometrics analysis, where for example a law is rolled-out at different point in time across different regions of a country. Thus, rather than standard Did, my first step is to rely on the most used approach to estimate the effect of treatment in such settings (de Chaisemartin & d'Haultfœuille, 2020; Goodman-Bacon, 2021): a two-way fixed effects linear regression (TWFE), using OLS and a combination of time fixed effects and group fixed effect at the province level.

However, recent methodological papers have shown that such models have drawbacks (Callaway & Sant'Anna, 2021; e Chaisemartin & d'Haultfœuille, 2020; Goodman-Bacon, 2021). Such papers explore new Did methods with multiple treatment timing. Thus, my second step is to use the method introduced by Callaway and Sant'Anna (2021), specifically designed for setups with multiple treatment-timing. The model allows me to attempt to control for selection bias by relying on both an IPW and outcome regression for inference. Using the same model, I proceed with additional specifications and compute dynamic treatment effects.

5.1 Two way fixed effects regression (TWFE) with multiple time periods

5.1.1 Empirical Strategy

I attempt to estimate the effect of a binary treatment - with treatment defined as being in a 25km radius of a Chinese development project - and exploit variation in treatment-timing across groups of units through a TWFE regression. The goal of this model is to replicate the Did model when treatment timing is heterogeneous: the average treatment effect (ATT) is identified by adding fixed effects for groups of individuals, time-fixed effects and controls (Rios-Avila et al., 2021). Using repeated cross-section data, I consider a setting where there are T total time-periods t where t= 1, ..., T.

$$Y_{it} = \beta D_{it} + Z_{it} + \theta_t * \eta_x + \varepsilon_{it} \tag{1}$$

β is the coefficient of treatment, in an OLS regression where Y_{it} is the outcome of interest of the household i at time t. Z_{it} is a vector of characteristics at the household level including: average age in a household, age of the head of a household, number of members in a household, whether a household is lead by a female, whether a household lives in a urban setting and the dependency ratio. The interaction of time-fixed effect θ_t and province fixed effects η_x allows to control for time trends in provinces and unobserved provinces-specific characteristics that could be correlated with my dependent and independent variables. Finally, ε_{it} is a time-varying unobservable error term.

For computation, I use clustered standard errors for the interaction of province and survey year. Indeed, I expect errors to be correlated among people from the same province and over time, as they will be subjected to the same unobservable factors (such as a change of policies or other common chock). Indeed, the Cambodian administration is highly centralized and provinces are the main administrative units in charge of implementing sub-national measures agreed on at the central level (OECD, 2016).

5.1.2 Results

The results of my OLS estimation are reported in table 2. First, the OLS coefficient in column (1) indicates a positive and significant effect of Chinese development projects on average education levels. There is an increase of little more than a month in the average years of education for households nearby Chinese development projects. The size of the effect is quite small compared to other studies that find an average half a year increase in education levels (Martorano et al. 2020). Second, I find no significant coefficient for anemia levels in treatment areas. This is not surprising: anemia is a nutrition indicator that typically takes a long time to evolve. As Martorano et al (2020) point out, nutrition outcomes rely on a change of behavior as well as increased nutritional intake. Thus, to obtain a positive and significant coefficient, China would need to implemented projects designed to fight the cause of malnutrition - working at the same time on behaviors and nutritional intake (Sanghvi et al., 2017). From the data at hand regarding the number and scale of health projects in Cambodia, it does not seem to be the case. Additionally, the model is quite badly adjusted to explain the data on anemia (r-squared of 0.05) compared to other dependent variables: it

Table 3: Regression results

VARIABLES	(1) Average years of education	(2) Anemia level	(3) Index of material living conditions	(4) Average time to water source
Treatment	0.09***	-0.01	0.05	-0.24
	(0.03)	(0.02)	(0.06)	(0.56)
Constant	-0.50***	3.26***	3.36***	11.25***
	(0.15)	(0.04)	(0.13)	(1.26)
Observations	31,921	14,439	31,921	28,775
R-squared	0.35	0.05	0.49	0.14

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note : All regressions include controls province fixed effects, year fixed effects and province-year fixed effects. Standard errors are clustered both at the province and year level.

could be sensible to use a health variable that is better explained by my model, as anemia might be better explained by variables not added here such as women's health and birth history.

Surprisingly, I do not find a significant effect of Chinese development finance on households material conditions of living, the proxy for economic well-being. Hence, higher level in education would not drawn by higher economic welfare. This result could be explained by the results of Michaelowa and Weber (2007), who explain that with high level of development aid there can be negative spillovers, such as political capture and deterioration of governance. The presence of a corrupt government in Cambodia supports this idea. Moreover, China has been accused of favoring Chinese labor rather than local labor, which could reduce economic spillovers in the construction phase (Wegenast et al., 2017; Cooper, 2019). Finally, the coefficient for time to water is not significant but is nonetheless in the right direction.

However, I have reasons to believe that this model does not truly capture the causal parameters of interest. One legitimate source of worry is selection bias. If Chinese development project target areas in provinces where there is already a significant difference in baseline characteristics before treatment, then coefficients reflect both treatment effects and initial differences in characteristics between treated and control units. Furthermore, methodological papers have recently highlighted shortcomings regarding the interpretation of TWFE coefficients as ATT (Goodman-Bacon, 2021; Borusyak & Jaravel, 2017; de Chaisemartin & D'Haultfœuille, 2020). Goodman-Bacon (2021) developed the issues related to the TWFE applied to Did estimators, known as the Bacon decomposition. He demonstrated that the two-way fixed effects Did estimator can be decomposed in a weighted average of all 2x2 DD combination that can be constructed with the data:

1. newly treated units relative to never treated units

2. newly treated units relative to no-yet treated units

3. newly treated units with already-treated units

Because OLS is "variational hungry" (Rios-Avila et al., 2021), units whose treatment status does not vary over time in a sub-period are used as the control group for units whose treatment status does vary over time (Goodman-Bacon, 2021; Callaway & Sant'Anna, 2021). The last combination (3) is thus a wrong combination, introducing treatment effect dynamics in the estimation(Jakiela, 2019)[13]. The two other combinations are valid. For example in this analysis, households treated in 2005 would be used as control for the group of units treated in 2010, since treatment status of my 2005 units will not change for the 2005-2010 sub-period, based on the staggered adoption assumption. For a visual representation of this phenomenon, a figure from Goodman-Bacon (2021) has been added to the Appendix (see Figure 8).

The second main issue highlighted by the Bacon decomposition is that Did estimators will only give unbiased estimates when treatment effects are homogeneous. If this is not the case, OLS will over-weight units with more variance to recover a most precise estimate. A reweighting procedure will be necessary to obtain the unbiased aggregate average treatment effect on the treated.

Hence, for the TWFE model to appropriately model the ATT, treatment timing needs to be homogeneous (a "one time shock on the outcome") and the effect to be similar across units. In this analysis treatment occurs at different time - depending on when a project starts - and its effects are likely to be heterogeneous - due to different project types. Thus, I expect treatment to have heterogeneous effects on groups of individuals and over time, leading to biased estimates with the TWFE model. As a result, I follow Goodman-Bacon's (2021) recommendation to use alternative approaches.

5.2 Did with multiple groups and time periods

5.2.1 Empirical Strategy

In order to produce robust Beta coefficients in the case of treatment effect heterogeneity between units and over time, several authors developed alternatives to standard Did and TWFE models (Abadie, 2005; Heckman et al., 1997; Imbens & Wooldrige, 2009; Sant'Anna & Zhao, 2020). In this analysis, I use the semi-parametric model developed by Callaway and Sant'Anna (2021)[14]. Their model efficiently estimates the ATT in Did designs with multiple time periods, variation in treatment timing with staggered adoption, and when the parallel assumption only holds with covariates. Inference is done using a doubly robust approach

[13]Jakiela (2019) illustrates this issue with the example of a free education reform. Her hypothetical example consists of two groups of countries, one group that promulgated free primary education in 2000 (the "early" countries") and the other one that did the same thing but in 2005 (the "late" countries"). Using a Did comparison and data for primary school completion rates every year from 1990 to 2010 she obtains a combination of newly treated units with "not-yet-treated" units (the "late" adopters) for the first time period (1990 to 2004). But for the 2005 to 2010 sub-period, units that have already adopted the reform will be used as control – because their treatment status does not change over the period 2000-2010.

[14]Their model has the advantage to work well with repeated cross-section data.

that relies on both inverse probability weighting (IPW) and ordinary least squares (OLS) (Sant'Anna & Zhao, 2020).

This model has three main advantages. First, by reweighting for the distribution of covariates between treatment and control groups it allows me to adjust for selection bias. (Horvitz & Thompson, 1952; Wooldridge, 2007; Martorano et al., 2020). Because covariates can be added to control for changing characteristics of groups treated at different times, conditional parallel trends can be specific across groups (Callaway & Sant'Anna, 2021). Second, it only takes valid combinations of households into account and does not overweight units with more variance: it is thus robust to treatment heterogeneity between units and over time. Third, by taking better account of differences in treatment-timing, the model can be used to compute average treatment effect according to different time-periods since treatment (event-study).

Identification strategy for ATT (g, t), or ATT group-time

An estimation of the average treatment effect that applies to setups with multiple groups and time periods can be done by identifying the ATT "in period t for the group of units first treated in period g"[15](Callaway & Sant'Anna, 2021), denoted by:

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_g = 1] \tag{2}$$

where G equals 1 if a unit i is first treated at time g and 0 otherwise.

The identification strategy for group-time ATT relies on a set basic of assumption regarding the treatment process, notably the irreversibility of treatment (i) – which is also called staggered treatment adoption in the literature - and random sampling (ii)[16]. Additional assumptions need to be made to estimate the group-time ATT:

iii. Limited Treatment Anticipation : households are not aware in advance that they will eventually be treated and/or individuals do not have the right to "choose" whether they will be treated or not. Here, we take $\delta = 0$ as is the norm in the literature.

iv. Conditional Parallel trends on the "never-treated" units.

v. Conditional parallel trends on the "not-yet-treated".

vi. Overlap: no household is treated in the first time-period t=1 and at each time t there is a fraction of household that is treated. Thus, "always treated" units are dropped[17].

The assumption (iv) is a generalization of the standard parallel trends assumption [18] with multiple groups and multiple time-periods (Sant'Anna & Zhao (2020)). Parallel trends

[15]The notation g can also be referred to as "treatment starting-time" for groups or cohorts

[16]The random sampling assumption (ii) implies that panel or cross-section data is available, and that for each year, each unit i is taken randomly from a larger population of interest.

[17]There is no pre-treatment value $Y_t(0)$ for this group in the data

[18]Standard Did relies on the parallel trend assumption, whether conditional on covariates or unconditional. If this assumption holds, the average treatment effect on the treated subpopulation (ATT) can be estimated.

are conditional on pre-treatment covariates, which is useful in this analysis, as households treated by projects across a certain time-range might have different characteristics than households treated by other projects at another time g. This is problematic if these different groups g have covariate-specific trends which when ignored, lead to bias (Heckman et al., 1997) [19]. The choice of pre-treatment covariates is thus important. If outcome variables (in the absence of treatment) depend on such characteristics like household member's average age or whether the household live in a rural or urban environment, it will be important to control for these characteristics in order to make parallel trends more credible (Callaway & Sant'Anna, 2022). However, post-treatment covariates must not be incorporated as they would be affected by the treatment and create bias (Wooldridge, 2005) - controlling for the education level of the head of household is for example excluded in this setting.

The assumption (iv) and (v) differ on the comparison group used. Assumption (iv) relies on the "never-treated" units as comparison groups, conditional on covariates, while the (v) includes "not-yet-treated" units. The authors encourages to favor assumption (iv) when there is a sizable group of "never-treated" units and they are similar enough to the "not-yet treated" units. In this analysis, I suspect a selection bias because it is unlikely that Chinese "aid" is allocated randomly. Despite the presence of covariates, as the authors highlight, it is possible that my "never-treated" units might behave differently from the "not-yet treated" units. As a result, I choose to rely on parallel trends assumption (v), favoring "not-yet-treated" units as controls. Using standard Did methods, Kotsadam et al. (2018) follow the same intuition: they compare individuals in locations where Chinese projects are being implemented at the time of the survey, with individuals in locations where there will be a Chinese project in the future. This way, one can reduce selection bias by assuming that the reasons for implementing a project at a given location at present time will be the same for implementing a project later.

This broad set of assumptions allows to derive the group-time ATT for this analysis, using "not-yet-treated" as the comparison group:

$$ATT^{NYT}_{Cond}(g,t) = E[Y_t(g) - Y_t(0)|X, G_g = 1], \text{for } t \geq g \tag{3}$$

$$ATT^{NYT}_{Cond}(g,t) = E[Y_t - Y_{g-1}|X, G_g = 1] - E[Y_t - Y_{g-1}|X, D_t = 0, G_g = 0], \text{for } t \geq g \tag{4}$$

Where D_t represents staggered adoption: it equals 1 if unit i is treated in period t and remains equal to 1 for t=1, 2...

Because I do not have survey data[20] for every year a household is treated (for example I only have survey year 2005 even though some households are also treated in 2002), I aggregate groups of households treated at different times into each of my survey data - denoted as g.

[19] Heckman et al. (1997) uses the evaluation of a job training program to motivate conditional parallel trends. The distribution of observed characteristics such as employment history or age are often different between people who apply to the program and those who don't. If the employment status and other labor market outcomes (in the absence of treatment) depends on these characteristics, the presence of covariates raise the probability of parallel trends to hold.

[20] For each cohort in the groups g variable, there should be a period in time t otherwise the model will not produce an estimate.

As a result, households treated after the first wave of 2000 (my pre-treatment wave) and between 2001 and 2005 are considered to be treated in 2005. The same is done for households treated between 2006 and 2010, considered to be treated in 2010, and households treated between 2011 and 2014 which are considered treated in 2014.

Further based on the aforementioned assumptions, the authors demonstrate that separately estimated ATT(g,t) can be aggregated to form a single causal ATT. It is estimated with a weighted average of all ATT(g,t), using weights proportional to group size. This approach better takes treatment heterogeneity into account as well as time-periods since treatment, compared to other models (Callaway & Sant'Anna, 2021)

Inference

Based on previous assumptions, there are different manners to recover the ATT (g, t) and aggregate ATT in order to conduct an inferential statistical analysis. In this analysis, I use the doubly robust estimator developed by Sant'Anna and Zhao (2020). It is an improved doubly robust approach that combines both the outcome regression (OR) by Heckman et al.(1997, 1998) and the inverse probability weighting (IPW) approach of Abadie (2005). As a result, it is more robust against misspecifications (Callaway & Sant'Anna, 2021; Sant'Anna & Zhao, 2020).

I focus on the IPW approach to define the factors used in this model. IPW is shown to be particularly robust compared to other matching methods (Busso et al., 2014; Martorano et al., 2020). The idea is to use a logistic approach to correctly model the probability – or propensity - to be in the treatment group, given characteristics (i.e. propensity score). Weights are then calculated as the inverse of the propensity score and applied to create pseudo-populations quasi-identical to treatment groups. To correctly model the probability to be in a treatment group, one must identify the drivers of Chinese project allocation in Cambodia at the sub-national level. I build on the literature to identify such drivers:

- In a study on Chinese development flows in Africa, Dreher et al. (2019) identify a systematic tendency of Chinese aid to be allocated to the birth region of political leaders[21]. The current prime Minister of Cambodia Hun Sen has been in position for the last 23 years and was born in Peam Kaoh Sna in the province of Kampong Cham (Mehta & Mehta, 2013). This province is marked with a dummy variable to add the "political leader effect" to the list of factors used to compute the propensity score.

- Chin & Gallagher (2019) demonstrate that Chinese development finance allocation can be described as a "coordinated credit space model". Projects are financed by China through different structures (combining state banks with commercial ones), but in a coordinated fashion that supports the "big push" approach of Rosenstein-Rodan (1943, 1961). The consequences of this strategy, aimed at promoting synergies between sectors and overcoming coordination failures, are two-folds:

[21] In the same study, Dreher et al. (2019) identify a less robust effect of ethnicity of birth leader. However in Cambodia, the Khmers are largely dominant and constitute 90% of the Cambodian population. Controlling for the political leaders'ethnicity does not make sense in this context of low ethnic diversity.

- Chinese-financed projects might tend to be geographically concentrated, whether with other Chinese projects or western donor projects. To take this into account, I control for the financial amount of development projects financed by the World Bank in each Cambodian provinces. The data is retrieved from Aiddata and takes the period 1994 to 2004 into account. I thus control for treatment intensity in terms of financial volumes but not in terms of number of projects [22]
- Chinese development projects tend to locate close to infrastructures or zones perceived as conducive for development. Transportation infrastructure in particular, is considered as the backbone of economic growth. It has become a key focus of China, especially under the Belt and Road Initiative (Bluhm et al., 2020). Cambodia, from 2000 to 2014, did not have a strong transportation system and was mostly relying on a few roads and railroads for passengers and freight (ADB, 2011). The railroad network in particular, has been in place since the French Indochina empire, and connects all major transportation infrastructures [23]. Hence, to control for the likely allocation of Chinese projects near these major transportation corridors, I construct a variable for all provinces touched by the railroad network. Doing so has two additional advantages. First, the railway network reaches the maritime shores of Cambodia, capturing the tendency of Chinese projects to be allocated near coasts (Dreher et al., 2020). This is particularly visible in Cambodia (Pedroletti, 2022). Second, it accounts for another driver found by Dreher et al (2019), which states that Chinese projects tend to be allocated in the wealthier parts of countries. Since the presence of transport infrastructure tend to increase economic growth (through channels such as reduced trade time and trade costs, increased market access, higher resilience in crisis), provinces which benefit from the railway are likely to be better-off than those which do not. Third, the railroad network is geographically close to the road network in at least half of the country. Accounting for closeness to railroad also takes some account of closeness to the road.
- Martorano et al (2020) use a standard Did method with a similar IPW approach to identify welfare impacts of Chinese projects in Africa. In their study, they identify a list of demographic characteristics used to model the probability of receiving Chinese development finance in Africa. I use a selection of the characteristics used in this study: household size, average age in a household, average age of head of household, as well as the average dependency ratio.

I use the following covariates to compute the aggregate ATT for all appropriate ATT(g,t).

Before proceeding with the model, I assess the credibility of assumption (v), and test

[22] Treatment intensity in financial commitment (size of the project) is not the same as treatment intensity in terms of volume (number of projects) (Dreher et al., 2019; Martorano et al., 2020). However, I do not have access to the data on the number of projects financed by the World Bank and rely on treatment intensity to account for a higher probability of Chinese development projects to be located in provinces where there is also an important financial presence of other donors.

[23] Major transportation infrastructure include the three international airports of Phnom Penh, Siem Reap, Sihanoukville; inlands ports in Pnomh Penh and on the coast in Sihanoukville

indirectly[24] whether the conditional parallel assumptions hold by looking at changes in my outcomes variables (health, education, wealth and travel time) before treatment. It is similar to checking whether all pre-treatment estimates are equal to 0 (Marcus & Sant'Anna, (2021); Callaway & Sant'Anna (2021); de Chaisemartin & D'Haultfœuille, 2020). If the conditional parallel trend assumption hold, the pre-treatment effects can be expected to be 0. Results of these Wald-type tests can be found in table 3. With the p-values obtained, I cannot reject the null hypothesis of a joint pre-treatment effect of 0 for any of my outcome variables, providing indirect support to parallel trend assumption (5) (Marcus & Sant'Anna, 2020).

Table 4: Pre-treatment test results

VARIABLES	(1) Average years of education	(2) Anemia level	(3) Index of material living conditions	(4) Time to water source
Chi2	3.78	2.15	3.75	3.45
P-value	0.28	0.54	0.28	0.32

Note: Pre-trend Test, H0 is all pre-treatment trends are equal to 0. These tests include all covariates mentioned previously.

5.2.2 Results

The results of my Did estimation are reported in table 4. Due to issues with my common support, I have to relax the number of covariates in my IPW model and drop the control for the financial intensity of World Bank projects.

I obtain a significant and positive coefficient at the 10% significance level for the average treatment effect on average years of education. This is in line with previous effects found using the OLS model. The magnitude of the coefficient does remain small, with a small average increase of two months of education for treated households. Regarding health outcome, the coefficient remains insignificant for anemia, aligned with the expectations developed in section 5.1.2.

Results for my indirect channels reveal significant results, this time better aligned with the postulated theory of change. The proxy for household's material living conditions is significant at the 5% level and the coefficient is significantly larger than previously estimated: being treated with a Chinese project improve material conditions of living by 0.37 points on average[25]. Improvements in living conditions typically do not happen very fast, since it requires poor households to save money over a period of time. Poor household do not often have this luxury, as their income tend to be entirely spent for immediate consumption and needs (food, water, or healthcare for a sick member of the family) (Duflo & Banerjee, 2011). An improvement in this indicator might thus indicate a long-term economic gain from treatment. Additionally, the coefficient for time to access drinking water is negative

[24]This approach is qualified as a placebo-type test by Marcus and Sant'Anna (2021).

[25]Over a scale that takes continuous values from 2 to 8.

Table 5: Did with multiple groups and treatment-timing results

VARIABLES	(1) Average years of education	(2) Anemia level	(3) Index of material living conditions	(4) Time to water source
Treatment	0.15*	0.01	0.37**	-4.5***
	(0.78)	(0.03)	(0.16)	(1.34)
Observations	53,501	21,971	53,501	29,301

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note : in the model, individual and time-period fixed effects are already incorporated, as a result I do not add time-varying province fixed effects. Standard errors are clustered both at the province and year level.

and significant at the 1% significant level. It would mean that being treated by at least one Chinese projects leads to an average four minutes reduction in the time taken to reach a water source. This could indicate an overall reduction in travel time, either because more infrastructure have been constructed nearby due to agglomeration effects or because there are more transportation infrastructures.

Several explanations can be advanced for these results. I remain cautious in my interpretation since I do not possess detailed information on health and education projects activities and delivery modes. Regarding the education outcome, it has been shown that only a very low share of total projects are dedicated to education projects. Thus, the improvement in education level is more likely to stem from relevant economic spillovers. This hypothesis is supported by the significant and sizeable coefficient for material living conditions. It is likely that the construction phase of sizeable economic projects led to increased economic activities in the area combined with longer term agglomeration effects for industries. This would affect households economic welfare in different ways (income, increased proximity to workplace...). The importance of economic sector projects in the results obtained is also supported by the proxy for time to travel. Given that half of Chinese projects are dedicated to the transportation sector, this is not a surprising result.

While these results seem overall reasonable in light of the theory, the size of the coefficients call for additional scrutiny. Because welfare variables typically take longer time to emerge, I expect average effects to differ for groups of households treated for longer time-periods. The literature on the subject does argue that when the effects of aid are small, the aggregate estimate might not reflect its real effect (Briggs, 2017; Dreher & Lohmann, 2015). Thus, aggregated ATTs might hide a dynamic relationships between treatment and outcome variable, related to differential time-periods since exposure.

5.3 Did with multiple groups and time period : dynamic effects and event studies

I compute the average effect of treatment for different time-periods since treatment, using the model from Callaway & Sant'Anna (2021). The results are reported in table 5 and figures 3, 4, 5 and 6 - with the x-axis representing different time-periods since the first exposure to treatment. Periods before treatment allow to control for pre-treatment trends based on group-time average treatment effects (Callaway & Sant'Anna, 2021)[26].

As expected, coefficients for education, economic well-being and time travel all turn significant for groups treated for longer time-periods, *i.e.* more than 10 years. For these groups, the size of the effect is larger than the aggregate ATT(g,t) for all my outcome variables in table 4. For groups of households treated for more than 10 years, the average effect of treatment on education is now an additional half a year of education. This is aligned with results found by Martorano et al. (2020). Even though the pre-trend 10 years before treatment is significant at the 10% level of significance, it is not the case anymore 5 years before treatment. Moreover, the test for joint pre-treatment effect of 0 in table 3 reinforce the conditional parallel trends assumption. Groups of households treated for more than 10 years also benefit from an average point increase in the material living conditions index, a significantly larger magnitude than the average effect across all periods of exposure. This would suggest that it is not the construction phase that generated higher economic welfare through punctual job opportunities, but rather the exploitation phase and the subsequent agglomeration effect. Finally, time to water after more than 10 years after exposure to treatment is reduced by 17 minutes on average. Similar to previous estimates, the coefficients for effects on anemia remains insignificant, even after more than 10 years of treatment.

Coefficients for 10 and less years after exposure remain insignificant. This displays a slow but existing change in pattern that was not revealed in the aggregated estimate: there appears to be a dynamic effect of Chinese aid on welfare outcomes and wealth, that only becomes significant and large in magnitude after a prolonged period since exposure. This pattern is similar to traditional aid, with studies showing similar results on health and education outcomes at the sub-national level (Kotsadam et al., 2018; De & Becker, 2015; Marty et al., 2017).

[26]The identification in event studies differ from previous ATT(g,t) identification. Periods after treatment are measured as $E(Y|t)$ - $E(Y|g_{-1})$ with g_{-1} the last period before first treatment for group g. Periods before treatment are measured as $E(Y|t)$ - $E(Y|t_{-1})$

Table 6: Event-study results

VARIABLES	(1) Average years of education	(2) Anemia level	(3) Index of material living conditions	(4) Time to water source
10 years before treatment	0.21*	-0.11	0.38	-5.30
	(0.12)	(0.08)	(0.24)	(4.72)
5 years before treatment	-0.6	-0.01	-0.12	-0.18
	(0.13)	(0.05)	(0.14)	(1.16)
0 to 5 years after treatment	0.6	-0.00	0.15	-1.85
	(0.09)	(0.03)	(0.18)	(1.35)
5 to 10 years after treatment	0.10	0.02	0.29	-1.75
	(0.08)	(0.05)	(0.18)	(1.18)
10 + years after treatment	0.55***	0.06	1.32**	-17.23***
	(0.16)	(0.10)	(0.42)	(3.31)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Standard errors are clustered both at the province and year level.

Figure 4: Event-study: Education

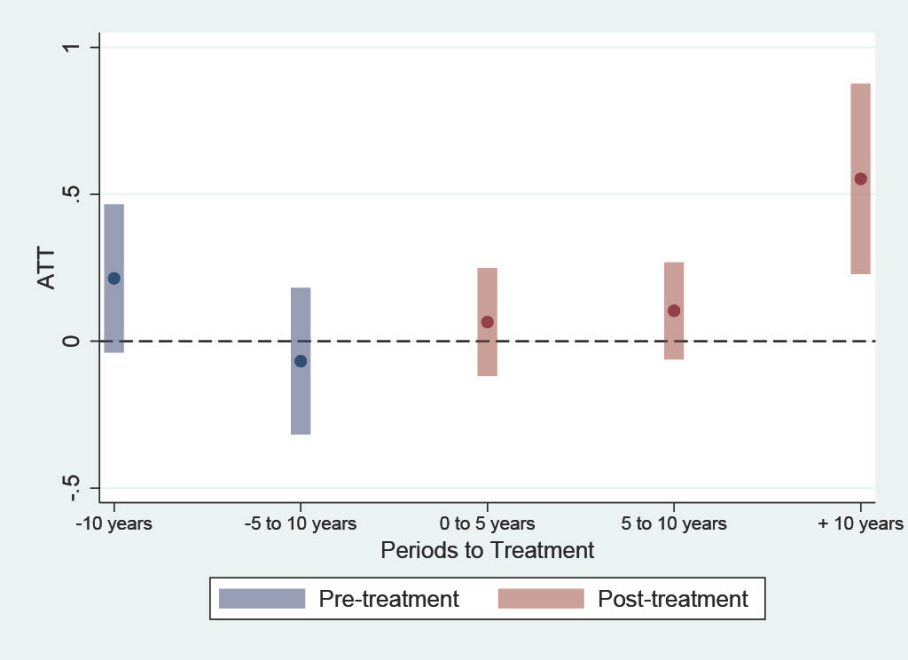


Figure 5: Event-study: Anemia

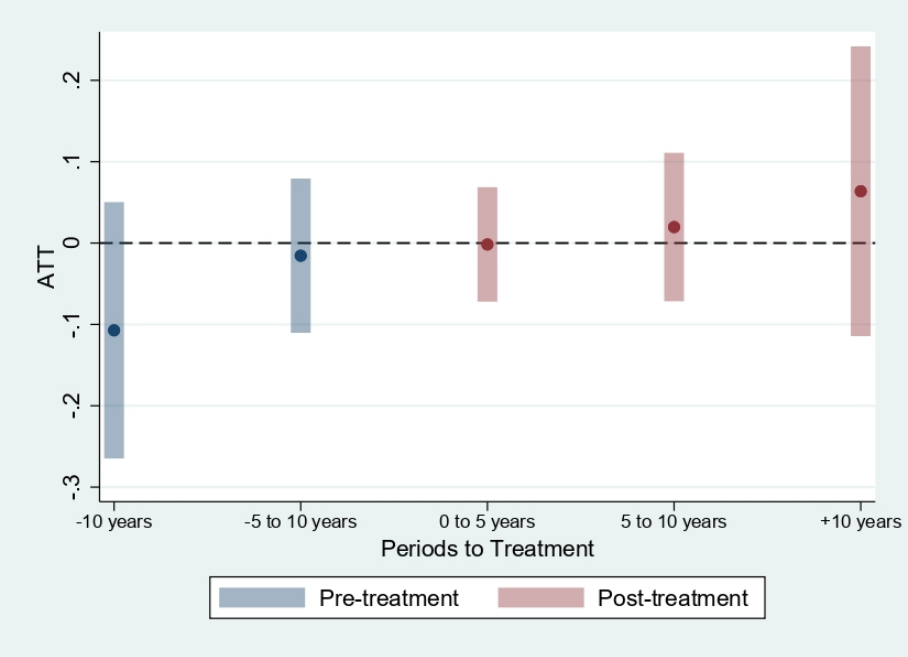


Figure 6: Event-study: Wealth

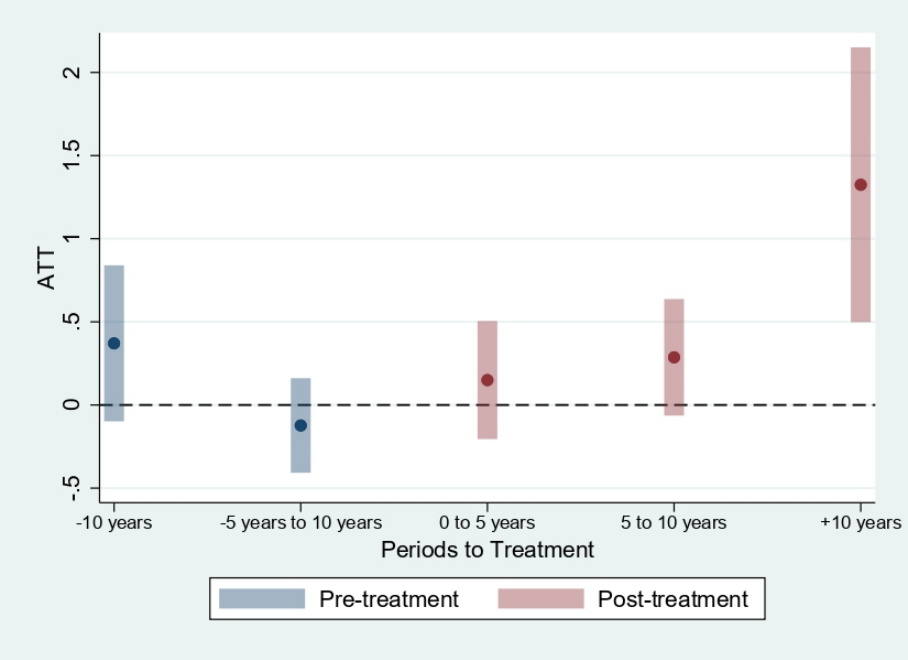
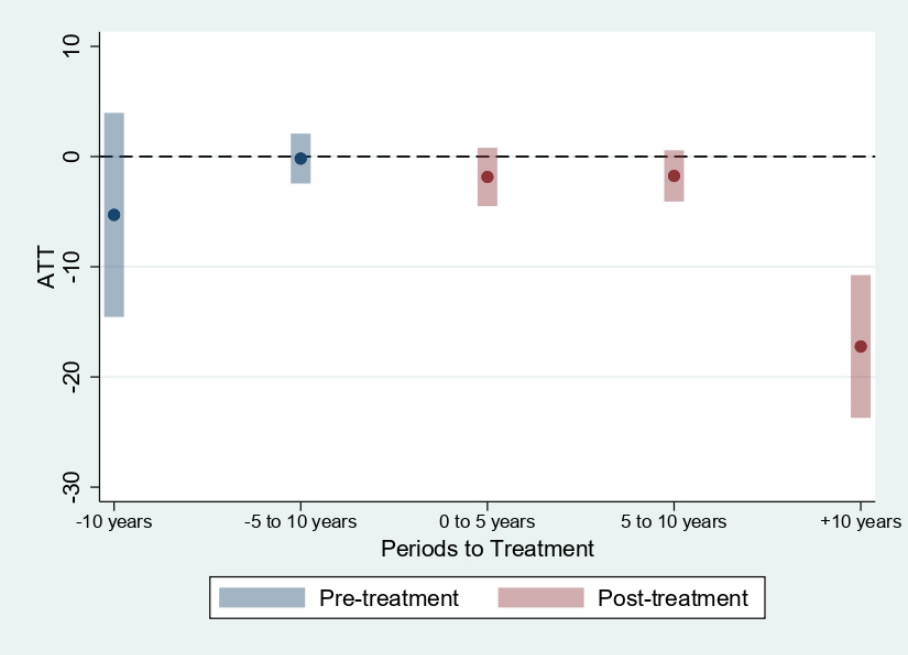


Figure 7: Event-study: Travel time



6 Conclusion

The scale of China's development program has expanded a lot since 2000, notably driven by large-scale initiatives such as the BRI. However, the effects of China's aid disbursements in recipient countries are still not well understood, mostly due to the absence of fully exhaustive and reliable data. This has led to a body of qualitative study blaming China for providing "rogue aid" with a blatant disregard for international safeguards (Wang & Zadek, 2016; Strange et al., 2013). However, the development of the Aiddata methodology, used to collect information on Chinese aid projects, and the subsequent release of Aiddata's Global Development Finance datasets have led to an increase in empirical analysis that put this vision into perspective.

While the focus of the literature was initially put on the various allocation drivers of Chinese development finance (Broich, 2017; Dreher et al. 2018; Dreher et al. 2015; Guillon & Mathonnat, 2020) and on its effectiveness on economic outcomes (Dreher et al., 2019; Dreher et al., 2021; Bluhm et al., 2018), crucial indicators of welfare have remained almost absent of the literature. This has recently started to evolve with a series of papers investigating governance (Isaksson & Kotsadam, 2018; Gehring et al., 2018) and welfare outcomes (Cruzatti et al. 2020; Martorano et al. 2020). Yet, these analysis almost exclusively rely on samples constituted of African countries, except in large cross-countries analysis (Cruzatti et al. 2020). This probably reflects the tremendous development needs of the continent. It is also the result of the concentration in number of Chinese aid projects (Oh, 2019; Dreher et al., 2021). However, figures in terms of project financial size rather than number of projects reveal that Southeast Asia is home to an important share of Chinese projects (Dreher et al., 2021). Indeed, China's global strategy, geo-strategic issues in the region as well as BRI routes explain well the importance of forming strong relationships with neighbor countries.

This study provides local evidence of Chinese development finance's effectiveness on two welfare outcomes: education and nutrition. It contributes to the literature by using georeferenced project data to investigate local welfare outcomes in an Asian country, Cambodia. It further builds on innovative methodological papers that developed difference-in-difference models to account for heterogeneity of treatment effects and timing. My results show that Chinese aid has a positive impact on education in the Cambodian context. I also find significant results for increased economic welfare and reduced travel time in treated areas, two channels likely to support positive outcomes in welfare. Moreover, I show that only households that were exposed to at least one project more than 10 years ago obtain significant results for education, economic well-being and travel distance. These results are not surprising given the low share of education and health projects in Cambodia. The vast majority of Chinese development finance is directed toward large infrastructure projects, which are known to improve economic well-being through various channels and reduce travel time. Thus, improvements in education seem to be supported by improvements in economic wellbeing rather than through specific projects in the education sector - however I remain cautious in my interpretation because I do not possess detailed information on project activities. The next step would be to compute a sectorial analysis by type of projects, to pin-point the specific effects of social projects on welfare outcomes compared to economic projects. Regarding treatment timing, investments in education and health are known to take a long time to materialize: that a period of more than 10 years after a project is necessary to find

significant results is thus aligned with other results found in the literature (Martorano et al. 2020).

The results of this study suffer from limitations that have already been stated. First, studies on international aid carry a risk of endogeneity and omitted variable bias, resulting from its non-random allocation. Even though I used efficient approaches to tackle this issue, one can never be certain that the conditional parallel trends truly hold for the periods under scrutiny. Second, data from Aiddata rely on unofficial and incomplete sources. On this question, Horn et al. (2019) believe that around half of Chinese development flows to developing countries remain unaccounted for. Hence, the conclusions of this study might not hold in reality, even though I mitigated this issue by only taking the presence of a project as treatment and not its financial value. It is clear that further analysis on Chinese development flows would largely benefit from disclosed information from China and/or additional information on activities entailed by projects. Finally, there a lot more welfare variables that could have been investigated in this study, such as consumption pattern, child mortality or subjective well-being. Similarly, future studies should further investigate countries other than African countries, whether Asian or Latin American, in order to understand whether results found in the literature for African countries also hold in other geographical locations.

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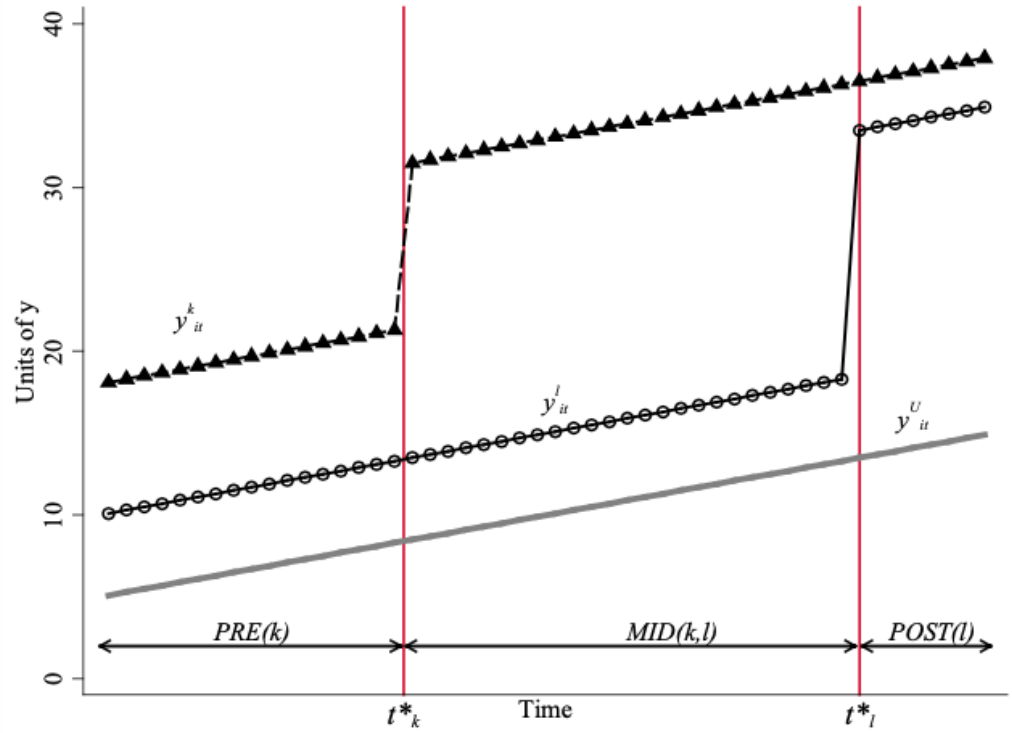
8 Appendix

Figure 8: Visual Representation of Difference-in-Differences Two-Way Fixed Effect Model With Variation in Treatment Timing (3 Groups). Source: Goodman-Bacon, (2021)



Notes: The figure plots outcomes in three groups: a control group, U, which is never treated; an early treatment group, E, which receives a binary treatment at $t_k^* = \frac{34}{100}T$; and a late treatment group, ℓ, which receives the binary treatment at $t_\ell^* = \frac{85}{100}T$. The x-axis notes the three sub-periods: the pre-period for group k, $[1, t_k^* - 1]$, denoted by $PRE(k)$; the middle period when group k is treated and group ℓ is not, $[t_k^*, t_\ell^* - 1]$, denoted by $MID(k, \ell)$; and the post-period for group ℓ, $[t_\ell^*, T]$, denoted by $POST(\ell)$. I set the treatment effect to 10 in group k and 15 in group ℓ.

Figure 9: Results of a Student's T-test: differences in characteristics for households living in treated areas and household living in non-treated areas, using baseline data (survey year 2000)

Ttest

	(1) Average Years of Education	(2) Anemia Level	(3) Material Conditions of Living index	(4) Time to Water Source	(5) Household size	(6) Age of Household Members	(7) Age of Household Head	(8) Urban Households	(9) Female-Headed Household	(10) Dependency Ratio
Household in a treated are	0.53***	0.03	0.39***	-1.62***	-0.06	0.86**	0.97**	0.16***	0.04***	-0.07***
Constant	1.66***	3.22***	4.05***	12.91***	5.38***	24.66***	43.84***	0.01	0.22***	1.07***
Observations	12158	2685	12158	8322	12158	12158	12158	12158	12158	12158
R^2	0.007	0.000	0.011	0.003	0.000	0.001	0.001	0.029	0.001	0.001