Sunshine or Curse? Foreign Direct Investment, the OECD Anti-Bribery Convention, and Individual Corruption Experiences in Africa

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Abstract

It remains unclear if foreign direct investment (FDI) benefits local citizens in host countries. Combining geo-referenced FDI data and household level surveys, this paper uses spatial-temporal techniques to assess how FDI impacts individual corruption experiences. We investigate if this relationship is conditional on the corruption levels, or engagement with the OECD's anti-bribery convention (ABC), of the FDI's source country. We find evidence that FDI flows reduce individual bribery experiences, but only when existing levels of corruption are high. We find it is FDI from comparatively *more* corrupt, and non-ABC engaging, countries that locates to areas of high corruption. Further, FDI appears to improve both the employment prospects and financial positions of local households. Collectively, we argue that these results suggest that individual empowerment via a *wealth* effect, rather than spillovers from firm *professionalization* or *regulatory pressure* mechanisms, is what stems individual corruption experiences.

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"Publicity is justly commended as a remedy for social and industrial diseases. Sunlight is said to be the best of disinfectants; electric light the most efficient policeman." [2]

United States Supreme Court Justice Louis Brandeis (1914)

Introduction

Corruption has long been identified as one of the key contributors to economic and political underdevelopment (Mauro 1995; Gupta and Abed 2002). Corruption can take many forms, but it is mostly often styled as "grand" or "petty" involving either systemic or individual exchanges, respectively. While the causes of corruption are numerous (Treisman 2007), much scholarship has been dedicated to considering how foreign non-commercial (official development assistance (ODA)) and commercial (foreign direct investment (FDI)) flows relate to corruption (Tavares 2003; Kowk and Tadesse 2006; Okada and Samreth 2012). However, ambiguity remains on how these flows impact individual corruption experiences. Do they act as "sunshine", disinfecting the disease of corruption, or do they simply serve as a "curse", increasing the "honey pot" of resources to be skimmed?

Individual bribery experiences can lower public trust not only in government officials but also in broader society (Rothstein and Eek 2009). This type of corruption can inhibit public service delivery both by increasing "prices" but also by discouraging individuals from seeking out public services in the first place (Kaufmann et al. 2008; Hunt and Laszlo 2012). This can entrench clientelism wherein regime supporters gain preferential access to public goods (Bustikova and Corduneanu-Huci 2017). Similarly, officials may use the rents from petty bribery to directly fund clientelism and/or distort elections (Singer 2009). Economically, individuals who run small or micro-enterprises (or informal businesses) face petty bribes in order to access services (Franciso and Pontara 2008) and this can lead to significantly worse economic performance (Seker and Yang 2014).These broad ranging political and economic effects make it crucial to understand if and how foreign presence facilitates or hinders local individual bribery experiences.

[2] https://www.brandeis.edu/legacyfund/bio.html

This paper takes advantage of a recent sea-change in development studies to advance the literatures above. The increased availability of high-quality, geo-referenced, socio-economic data has facilitated research utilizing spatial identification strategies to examine a wide range of both cross-national and sub-national development outcomes (Dreher and Lohman 2015; Van Weezel 2015; Civelli et al. 2017), including the impact of ODA on local corruption (Brazys et al. 2017, Isaksson and Kotsadam 2018a). Examining the FDI-corruption relationship for individuals at the local level has a major advantage over existing cross-country studies.[3] Refined spatial data allows for a more compelling empirical relationship when examining individual-level outcomes. Aggregated, country-level data makes an implicit assumption that events at great distances may influence survey respondents. However, it seems unlikely that a FDI project hundreds of kilometers distant would have much impact on local individual experiences. Linking survey responses to proximate events creates a far more plausible empirical linkage.

Drawing on the existing literature on overseas flows and local corruption, the paper develops general theoretical expectations for how and why commercial flows might promote or mitigate local, petty, corruption experiences along several dimensions. While we consider the arguments that FDI might create "honey pot" which could either increase corruption via a predation mechanism, or diminish it via a substation or wealth mechanism, we also explore arguments for how FDI might alter the local normative environment via a *professionalization* effect. Finally, we investigate if FDI's impact on local corruption might not be driven by a *regulatory pressure* effect. Specifically, we consider if source country commitments via an international institution, particularly the OECD's Convention on Combating Bribery of Public Officials in International Business Transactions ("anti-bribery convention" (ABC)), influence the local corruption environment.

To test these arguments, the paper utilizes geo-referenced, project-level, FDI data sourced from the Financial Times fDi Intelligence "fDi Markets" database. We combine these data

[3] In considering how FDI may impact local corruption, we utilize the distinction between corruption experiences and corruption perceptions (Olken 2009; Olken and Pande 2011; Donchev and Ujhelyi 2014; Gutmann et al. 2015; Belousova et al. 2016). While the former are always local, the latter may not be. Thus, our primary focus is on corruption experiences. Likewise, we delineate between petty and grand corruption, focusing on the former. While grand corruption, public diversion of funds, may have systematic development consequences, the petty corruption, individual bribes, often falls most heavily on the vulnerable in society (Richmond and Alpin 2013).

with experiences of corruption as reported in geo-referenced Afrobarometer household surveys (Ben Yishay 2017). While other survey data exists, the fact that these Afrobarometer surveys have been geo-referenced in multiple rounds allows us analyze the data using spatial-temporal identification techniques similar to those found in Knutsen et al.'s (2017) study of the impact of mining on local corruption in Africa. Accordingly, our empirical focus in this paper is restricted to Africa. In particular, the Knutsen et al. (2017) approach allows us to distinguish between sites where FDI projects are already being implemented at the time of a survey vis-à-vis those sites where a project has yet to (but will) be implemented. While it is important to note that FDI projects are not randomly assigned, this allows for an empirical approach that mitigates the endogenous selection effects of project sites, in particular that existing corruption levels may influence the likelihood of receiving FDI (Beazer and Blake 2018; Zhu and Shi 2019).

The paper finds that local FDI projects are associated with *decreased* bribery experiences. Delving into the *professionalization* and *regulatory pressure* mechanisms, however, the paper finds that it is FDI from *more* corrupt and ABC *non*-signatory countries that is associated with reduced local corruption, contrary to our expectations. However, this appears to be due to the fact that belonging to the ABC, or being a low-corruption FDI source country, impacts the *siting* of FDI projects, with FDI from these countries *going to* locations which experience comparatively less corruption, and thus less "room for improvement." These results, combined with findings that local FDI appears to lead to increased local employment and better household finances, suggest that it is empowerment via a *wealth* effect that accounts for the observed decreases in corruption experiences

Overseas Flows and Corruption

The impact of overseas flows on host-country corruption is a well-visited topic in the political economy literature. Large literatures exist considering the impact of both aid (Charron 2011; Okada and Samreth 2012; Asongu and Nwachukwu 2016) and FDI (Kwok and Tadesse 2006; Jensen et al. 2010; Claassen et al. 2012; Dang 2013; Bojanic 2013; Pinto and Zhu 2016) on corruption at the country-level.[4] The findings in these literatures are both nuanced

[4] Two notable exceptions to these cross-country studies are Gueorguiev and Malesky (2012) and Malesky et al. (2015) where the former consider firm-level impact on corruption in Vietnam finding no impact of FDI on

and often contradictory, with some evidence suggesting inward flows can increase corruption while others suggest no relationship or even a beneficial relationship wherein they reduce corruption. It is only more recently that scholars have begun to turn their attention to the localized impact of foreign flows on corruption, with Zhu's (2017) study of MNC at the province level in China as an important example. Moreover, most previous studies, again with the exception of Zhu (2017), also primarily focus on *firm*-level corruption measures. Several recent papers have examined the proximate impact of natural resources (Knutsen et al. 2017) and foreign aid (Brazys et al. 2017; Isaksson and Kotsadam 2018a) on individual experiences with local, petty, corruption, but, to our knowledge, none have yet looked at the impact of FDI on individual corruption experiences.

FDI and Local Corruption – Sunshine or Curse?

The conflicting empirical results in the cross-country literature may well stem from the fact that there are competing theoretical mechanisms by which FDI may influence governance (Li et al. 2018).[5] At a minimum, FDI brings new financial resources to an area. This may induce an increase in local corruption experiences via what has been coined a "honey pot", (Soysa 2002) or "rent creation" (Zhu 2017) effect. Following Knutsen et al.'s (2017) logic of the impact of mining rents of corruption, this mechanism assumes that FDI could facilitate local economic growth and, in particular, increase employment and wage opportunities for local citizens, either directly at the FDI firm, or indirectly via local suppliers or retailers that cater to the FDI firm and/or its employees (Coniglio et al. 2015). This influx of resources can create a new pool of resources, or "honey pot", which may induce a *predation* effect where local officials increase their demands for bribes in-line with an increase in citizens' ability to pay, increasing local corruption experiences.

However, there is also an argument that increased employment opportunities and higher wages can empower local citizens. As Fried et al. (2010) suggest, a *wealth* effect brought on by new employment and wages can lead to increased socio-economic status which makes individuals less vulnerable to demands for bribes. Tyburski (2012) makes a similar argument

corruption and the latter conduct a firm-level, list, experiment in Vietnam finding that firm propensity to bribe depends on the openness and expect profitability of the sector they are attempting to enter.

[5] Li et al. (2018) also suggest publication bias and/or measurement issues may account for conflicting findings in the FDI-governance literature.

that increased wealth from remittances can empower individuals' *voice* in order to demand accountability of corrupt officials but also increase their ability to "*exit"* i.e. disengage with state provision of public goods. Peiffer and Rose (2018) echo this latter mechanism, arguing that increased wealth can allow individuals to substitute private for public services, thus reducing contact with the state and potential demands for bribes. In these ways, wealth may *empower* local citizens to resist predatory behavior of the state.

Additionally, the FDI "honey pot" could also induce a *substitution* effect by opening up new sources of rents (legitimate or not) for local officials. Local officials may be able to extract bribes from the firm, and/or secure a portion of rents via permitting or licensing revenues. If these rents are sufficiently large and/or easy, they may increase the relative opportunity cost of seeking numerous, smaller, rents in the form of bribes from local individuals. Likewise, theoretical (Besley and McLaren 1993), experimental (Van Veldhuizen 2013), and empirical (Van Rijckeghem and Weder 2001) evidence all suggests that public officials will *reduce* their demands for bribes when public sector wages increase. To the extent that FDI increases host country tax revenue (Okey 2013; Kimm Gnangnon 2017), these rents can be used to increase public sector salaries reducing the demand for bribes. Thus, officials who can obtain rents elsewhere may reduce petty bribe-seeking activity to minimize the consequences of being caught and held to account in this comparatively low-return activity. On balance, these theoretical arguments and empirical evidence leads us to an expectation that the corruption reducing honey-pot mechanisms are likely to outweigh those that would increase individual bribery experiences.

FDI may also reduce local corruption experiences in a more indirect way by altering local corruption norms (Gutmann et al. 2019; Isaksson 2015). Overseas actors may operate under a normatively different culture, or under domestic law with extra-territorial jurisdiction which influences their overseas behavior with respect to corruption (Kaczmarek and Newman, 2011). The presence of a foreign firm can legitimate, undermine, or alter prevailing local corruption norms which in turn alter the frequency of local corruption experiences, particularly if the investment dominates the local economy. In addition to altering officials' norms, the firm may also alter norms in the local economy through its linkages and interactions with local suppliers, distributors, and subcontractors (Amendolagine et al. 2013). Indeed, Kwok and Tadesse (2006) discuss demonstration and *professionalization* effects wherein business-culture and practices from MNCs spill over into the local business

environment, either from local officials and firms emulating MNC practice or by MNCs professionalizing a local workforce. Their cross-country analysis supports the assertion that FDI can reduce host-country corruption.

We argue that both the *professionalization* effect and the Fried et al. (2010) and Peiffer and Rose (2018) conceptualizations of the *wealth* effect share a common thread in that they decrease the opacity of the local environment. In the absence of an outside firm, local officials may be better able to act with impunity against local actors who might have little course of redress. However, the presence of a professionalized foreign firm may change this situation by bringing external scrutiny to local corruption practices. Svensson (2003) finds that larger, more empowered, firms are asked for fewer bribes. It is little stretch to think that employees (be they local or foreign) of large foreign firms may similarly be empowered and protected by that firm in reporting corruption they experience in either their professional or personal capacities. Likewise, the Fried et al. (2010) and Tyburski (2012) argument of personal empowerment may increase individuals' confidence in resisting and reporting corrupt practices. Even if only a handful of individuals are empowered by the mechanisms above, asymmetrical information on who is empowered means that local officials cannot be sure which individuals are connected to foreign firms and/or otherwise empowered by the wealth effect of the FDI. Thus, our overall expectation is that as the risk of having corrupt practices exposed by the "sunshine" of FDI increases, local experiences with corruption will decrease.

Source Country Heterogeneity: Home practice and the OECD Anti-Bribery Convention

The argument that FDI will decrease local corruption rests, to some extent, on an implicit assumption that FDI brings *anti-corrupt* practices and norms. However, if the FDI firms are themselves corrupt and/or are perceived as engaging in corrupt practices, then the logic may not hold. Accordingly, we develop expectations based on FDI source country heterogeneity. First, the *professionalization* effect suggests firms may bring their source country's corporate culture with them when engaging in business practice abroad (Kwok and Tadesse 2006; Albrecht et al. 2009). Firms that are used to bribery or extra-legal influence at home may be more likely to employ it abroad. Alternatively, firms that come from source countries where such practices are taboo will be unlikely to test the corruption waters overseas. Indeed, Beazer and Blake (2018) find just that, with FDI from home states with low judicial

independence heading to similar host states, and vice-versa. If FDI does *not* empower local individuals then the "sunshine" logic will not hold, and, in fact, the corruption experience may worsen. Accordingly, we expect that FDI that is sourced from countries with a high(low) degree of internal corruption will increase(decrease) experiences with local corruption.

Second, source country heterogeneity may not emanate from source country practices, but instead, a firm's home country institutions. Kwok and Tadesse (2006, p. 769) coined this mechanism as the "*regulatory pressure effect*." Perhaps the earliest and most prominent example of this type of institution is the United States' 1977 Foreign Corrupt Practices Act (FCPA) (Sandholtz and Gray 2003). However, empirical studies of the FCPA generally conclude that its effectiveness has been, at best, limited (Cragg and Woof 2002; Krever 2007). Despite this, the FCPA was largely the impetus for a multilateral effort which was realized via the OECD's Convention on Combating Bribery of Foreign Public Officials in International Business Transactions ("anti-bribery convention"), which was signed in 1997 and entered into force in 1999. This international institution committed its country signatories to adopt and enforce laws which sanction their firms for corrupt overseas activities. As of 2018, the convention's signatories included all OECD states as well as 8 non-OECD members. However, compliance with commitments made via international institutions is no sure thing (Simmons 1998) and, indeed, evidence on compliance with, and effectiveness of, the anti-bribery convention is not definitive. While Cuervo-Cazurra (2008) finds that signatories reduced their investment to corrupt countries and D'Souza (2012) finds that signatories reduce exports to more corrupt countries, we are unaware of any research which suggests that FDI from signatory states reduces corruption in host countries.

While the focus of the anti-bribery convention is on stemming corrupt behavior of *firms*, decreased corruption therein could lead to a change in the normative environment which could again lead to decreased local individual experiences with corruption. Indeed, the "sunlight" argument only works if the FDI firms are bring along good corporate practice which empowers local individuals and thus increases the riskiness of engaging in corruption. Accordingly, we expect that FDI from signatories to the OECD's Anti-Bribery Convention will lead to lower experiences with local corruption.

Spatial identification of FDI's impact on individual bribery experiences

Data

Our data on corruption comes from household-level surveys from Afrobarometer that are geocoded by BenYishay et al. (2017) who use a double-blind methodology to geo-code all Afrobarometer enumeration areas from rounds one to six of the survey.[6] In analyzing corruption experiences we employ two questions where respondents are asked if they, during the past year, have 'had to pay a bribe, give a gift, or do a favor to government officials in order to' a) 'Avoid a problem with the police (like passing a checkpoint or avoiding a fine or arrest)', b) 'Get a document or a permit'. Based on these questions we construct a dummy variable indicating if the respondent has experienced paying either bribe at least once during the past year.

The base for our explanatory variable is FDI data that comes from the Financial Times "fDi Markets" dataset. This data is a compilation of publicly sourced data on both greenfield and expansion of physical FDI project announcements and has been used in several recent studies (Gil-Pareja et al. 2013, Owen 2018, Saltnes et al. 2020). Amongst other fields, the data contain information on project source country, destination country/state/city, investment amount, jobs created and sector. The data is gathered from media sources, industry organizations, investment promotion agencies, market research companies and from the Financial Times' own newswires and sources. Projects are cross-referenced to multiple sources with preference for direct company sources. The data is utilized as primary source data on investment trends by the World Bank, UNCTAD and over 100 national governments.

In this paper, we utilize this data for fifty-six African countries. The data codes 9,684 greenfield or expansion FDI project announcements from 126 source countries from 2003 to 2017 with flows estimated at over $1,026 billion. While these data represent *positive* rather than *net* FDI inflows we still compare them to official statistics as a validation measure. World Bank IBRD-IDA data records $612 billion in *net* FDI inflows from 2003 to 2016, compared to $1,008 billion in *positive* FDI inflows in the fDi Markets data.[7] Given the difference in the indicator, the data are of a similar order of magnitude. While the roughly $400 billion in difference seems

[6] While there is concern of response bias in Afrobarometer surveys, particularly in questions on *perceptions* of corruption (Tannenberg 2017), we are unaware of any work which finds bias in bribery experience questions. That said, our use of country fixed effects should somewhat mitigate any time-invariant country characteristics (like regime type) which might induce this sort of respond bias.
[7] Available at: https://data.worldbank.org/indicator/bx.klt.dinv.cd.wd accessed September 6th, 2018

a large figure for total disinvestment of 13 years, some of the remaining difference may be accounted for the by fact that the amount of capital investment is estimated in the fDi Markets database for over 80% (7,920) of the project records. The estimating algorithm is not available to us, but the figures may indicate an overestimation bias for the capital investment amounts. Over time, the series' annual values have a correlation coefficient of 0.643 with a *p*-value of 0.013. The mean annual difference in the series is $28 billion. That this annual difference is relatively stable, with a standard deviation of $19 billion, is plausibly suggestive that the difference lies in the *net* vs *positive* distinction and a capital investment overestimation. While we think the figures are reasonably valid, but given the discrepancy in the amount we focus primarily on the presence of FDI projects rather than *amounts* of capital investment. However, we do consider FDI amount in a robustness check below.

Similar to the approach in Isaksson and Kotsadam (2018a) which considers the impact of aid on corruption, we spatially match the coordinates of our FDI project sites, placed at the center of the city, to individual responses from repeated cross sectional Afrobarometer data using the coordinates of the Afrobarometer enumeration areas. Doing this allows us to identify clusters located within a given cut-off distance of at least one FDI project site. To ensure sufficient spatial precision, we use the 6,133 FDI project records that specify the destination city. This level of precision is similar to categories used in other work that spatially identifies local corruption effects. Brazys et al. (2017) and Isaksson and Kotsadam (2018a) both use precision categories 1 and 2 from Strandow et al.'s (2011) AidData geo-coding methodology when investigating the impact of aid on local corruption. These categories correspond to an exact location (category 1), or, as 'near', in the 'area of', or 'up to' 25 km away from an exact location (category two). In our estimation sample we have a total of 101,792 respondents from 5 survey waves in 36 African countries over the period 2002-2015. We visualize our data in figures 1a and 1b. In figure 1a, we plot the respondent clusters (in purple), along with the locations of all FDI projects (white squares) where the size of the square indicates the corruption level of the source country, with larger squares indicating more corrupt source countries. Figure 1b drops the respondent sites but adds information on the anti-bribery convention signatory status of the source country (in red). These figures both display the spatial dispersion of our data, but also reveal some clustering of projects both by source-country corruption levels and anti-bribery convention signatory status.

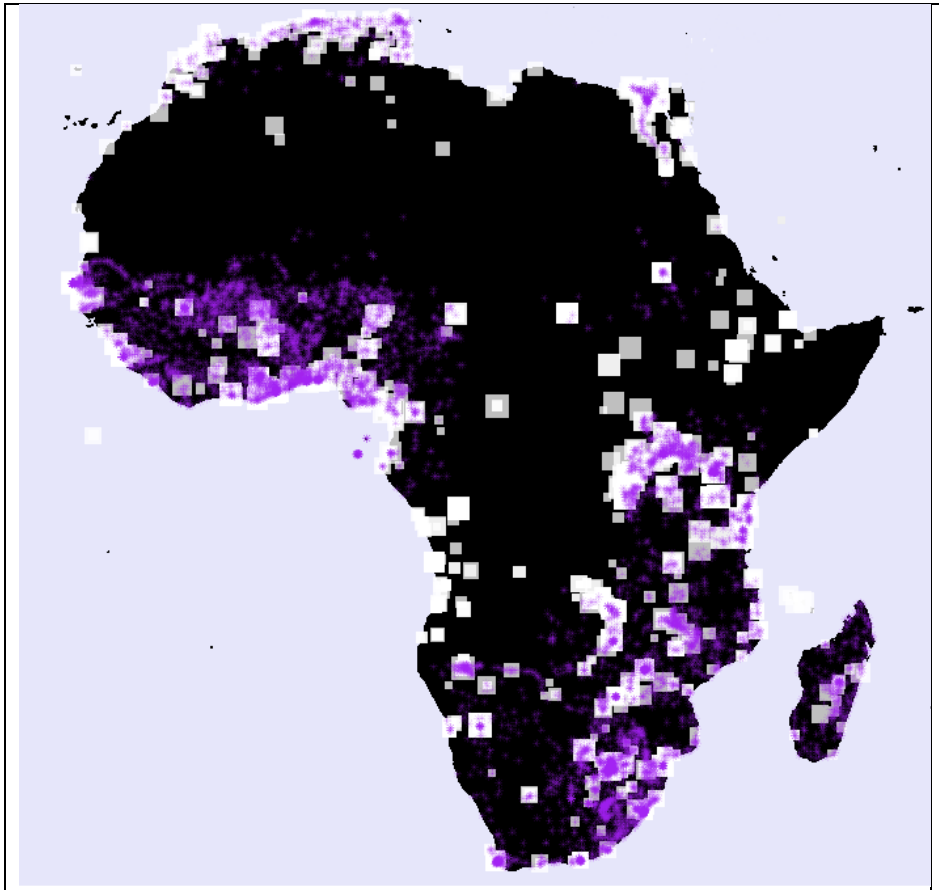

Figure 1a: FDI and Respondent Locations

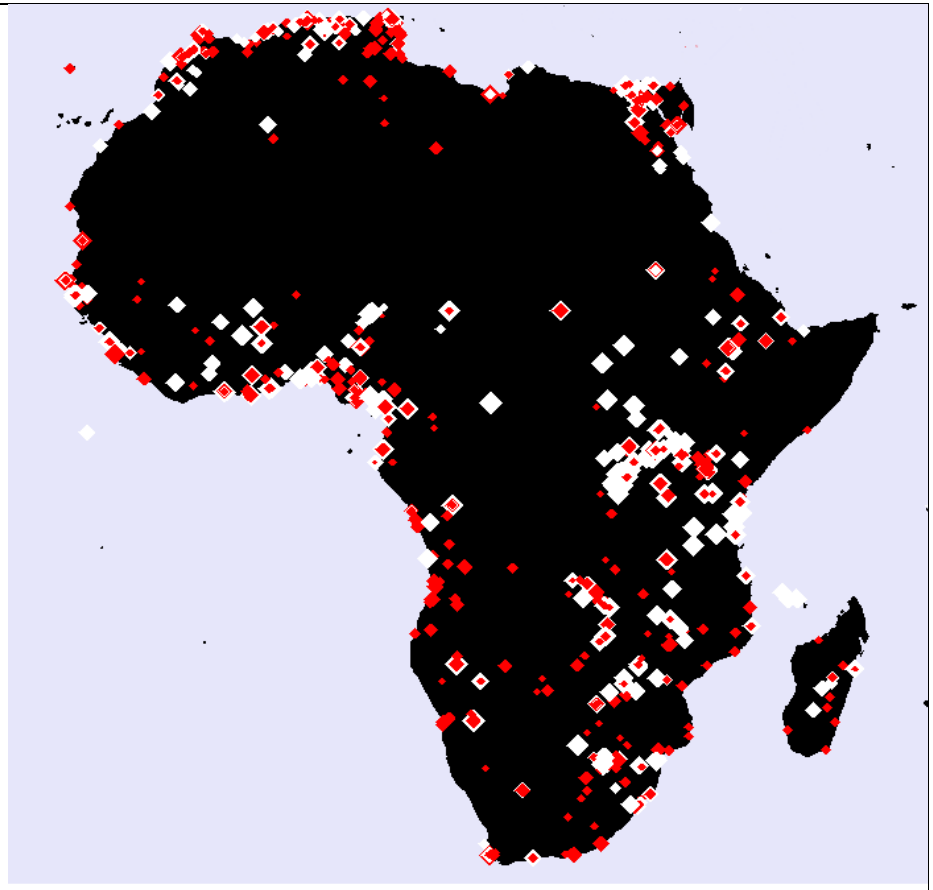

Figure 1b: FDI and Anti-Corruption Convention Signatories

Key. Purple Star=Afrobarometer Site; White Square = FDI Location; White Diamond = non-ABC FDI; Red Diamond = ABC FDI

Estimation strategy

Our spatial-temporal estimation strategy follows that used in Knutsen et al.'s (2017) investigation of mining and corruption and Isaksson and Kotsadam's (2018a) investigation of aid and corruption.[8] Specifically, we differentiate between Afrobarometer clusters where a FDI project is already underway and those areas where a project has yet to be implemented at the time of the survey. The five Afrobarometer survey waves allow us to examine the corruption experiences of African citizens over the recent decade. Although the data is not a full panel of clusters over time, with the estimation strategy we can take advantage of the temporal variation to compare areas with active FDI projects to areas where FDI projects have not yet begun but will. Our identification strategy assumes that corruption is impacted within a cut-off distance and our approach includes three groups of individuals: those 1) within 50 km of at least one active FDI project site, 2) within 50 km of an FDI project site that has not begun but will, and 3) who are not within 50 km of any current or future FDI project site. Our baseline regression is:

$$(1) \quad Y_{ivt} = \beta_1 \cdot active_{it} + \beta_2 \cdot inactive_{it} + \alpha_s + \delta_t + \gamma \cdot \mathbf{X}_{it} + \varepsilon_{ivt}$$

where *Y* is our binary indicator of the briber experience of individual *i* in cluster *v* at year *t*. We regress this outcome on a dummy variable *active* which captures if the individual lives within 50 kilometers of an active project, and a dummy *inactive* for individuals living at sites where an FDI project will occur but has not yet at the time of the survey. Similar to Isaksson and Kotsadam (2018a), we include country fixed effects (α_s) and year fixed effects (δ_t) to account for average corruption levels across countries and time.[9] To account for individual variation in corruption experiences, we include the vector $(\mathbf{X}_i)$ which contains individual-level controls from the Afrobarometer. Again, following Isaksson and Kotsadam (2018a, p. 149), the "baseline set of individual controls are age, age squared, gender, urban/rural residence. To account for correlated errors, the standard errors are clustered at the geographical clusters (i.e., at the enumeration areas which correspond to either a village, a town or a neighborhood)." We estimate the regressions using OLS. This has the advantage of being easy to interpret and it also greatly facilitates interpretations of the difference in difference test and interactions, which

[8] See also Isaksson and Kotsadam (2018b), Isaksson and Kotsadam (2020), Knutsen and Kotsadam (2020) and Kotsadam and Tolonen (2016).

[9] Due to the fact that most Afrobarometer clusters are surveyed in only one year we cannot include district or district-year fixed effects as there would be no variation at the district level in our "active" and "inactive" measures.

are problematic in non-linear models such as logit models (Mood 2010). Nonetheless, we also present results using logit models.

As Isaksson and Kotsadam (2018a, p. 150) note, "interpreting the coefficient on *active* (β_1) in isolation as capturing an effect of projects on local corruption would necessitate that the location of projects is not correlated with pre-existing local corruption levels." As they discuss, this is a very strong assumption as corruption levels (and other factors correlated with corruption, such as population density, economic activity and infrastructure access) may influence FDI project location decisions. Indeed, there exists a substantial literature which finds that corruption impacts local FDI decisions (for examples see Campos et al. 1999, Cole et al. 2009; Godinez and Liu 2015; Beazer and Blake 2018).

As Isaksson and Kotsadam (2018a) elaborate, by including *inactive* we can contrast active project sites to other areas selected as locations for projects, but where the project has not yet begun. Accordingly, we can directly compare these two different types of locations and not only areas close to and far away from project sites using the difference between *active* and *inactive* (i.e. $\beta_1 - \beta_2$). This approach gives us something akin to a difference-in-difference type of measure[10] that accounts for unobservable time-invariant characteristics that may influence both corruption and FDI project siting.

Of note, there are two issues with our FDI data that pose challenges for our identification strategy. First, the data is truncated in 2003. This complicates the identification of "inactive" sites as we do not know the FDI history of a site prior to 2003. Aggregate data suggests that FDI to Africa only accelerated greatly after the year 2000, and it may be that only a small number of sites are thus affected[11]. In any event, we would expect unobserved "active" sites to bias our findings towards a *null* result as unobserved "active" sites should be more similar to our "observed" active sites than true "inactive" sites, thus reducing the magnitude of our difference-in-difference. However, in an attempt to mitigate this issue, we run our models in the robustness checks section below using only observations from 2008 onward. Effectively

[10] Comparing the difference between post-treatment individuals (with an active project within 50 km) and control individuals (with no project – active or inactive – within 50 km) with the difference between pre-treatment individuals (with a yet inactive project within 50 km) and control individuals within the same country and year (due to country and year fixed effects).
[11] See figure A1 in the Appendix.

this “burns in” 5 years of site-history such that we can be certain that “inactive” sites will have had no FDI for at least the preceding 5 years.

Second, the FDI Markets data are project *announcements*. Some project announcements may never physically materialize, some may be delayed, or some may take time to be fully implemented. Again, if anything, this should bias our results towards *null* findings as it would reduce the differentiation between “active” and “inactive” sites. However, we also attempt to mitigate this issue, at least partially, in the robustness checks by using lagged values of the FDI project announcements.

Our spatial approach necessitates an assumption about the impact distance of our FDI projects. Implicitly we assume that if projects affect local bribe experiences, individuals interacting with nearby local authorities are likely to experience the results. However, those that are far away from the FDI project should not be impacted. As discussed in Knutsen et al. (2017) and Isaksson and Kotsadam (2018a), we must decide the appropriate cut-off distance from a project. The decision is an empirical question which makes a trade-off between the size of the treatment group and noise. If we make the distance too small, our sample of “treated” individuals in both the active and inactive categories will be very small. In contrast, if we use too large of a distance, we would end up with many untreated individuals placed in the treatment group, creating attenuation bias. Accordingly, we follow Knutsen et al.’s (2017), Isaksson and Kotsadam (2018a) and Brazys et al. (2017) in using 50 km as the primary cut-off distance. However, we also examine different radii of cut-off distances as robustness checks below.

In the models below, we first evaluate the overall impact of FDI on local corruption experiences. However, we then attempt to shed light on the wealth, professionalization and regulatory pressure mechanisms. As discussed above, we proxy for firm professionalization by looking at FDI source country corruption utilizing Transparency International’s *Corruptions Perception Index* (CPI). We use this information to create an indicator of “high” and “low” corruption (*corr*) countries, differentiating states at the median CPI value of country CPI averages from 2003 to 2017. Likewise, to assess the *regulatory pressure effect* of the OECD’s Anti-Bribery Convention, we create a binary indicator based on signatory (*sig)* status of the

FDI source country.[12] When respondent clusters have more than one active project in the area, we generate the mean value of the CPI and the proportions of anti-bribery convention signatories in the area and compare that mean average to the median values of CPI and signatory status to generate the "high" and "low" indicators. In other words, we consider the average corruption (or WGB signatory status) of an individual's proximate FDI projects when considering if they should be categorized in the "high" or "low" category. In the robustness checks, we approach this issue in an alternate way, by using a count of projects from "high" or "low" corruption or WGB signatory source countries.

Unfortunately, we cannot directly evaluate the *wealth* mechanisms due to mediation bias and due to not having an actual panel dataset. In order for mediation to work we would have to be sure that FDI only affects corruption via wealth. If we would be willing to assume this and if the Afrobarometer would have been a panel data we could have estimated a two-stage model that first considers the impact of active FDI on wealth and the consider the impact of wealth on corruption experience. However, even then, the exclusion restriction in such a situation would be highly questionable. Accordingly, we employ an indirect strategy to try and shed light on the plausibility of this mechanism. Using the "active/inactive" strategy, we consider if active FDI increases respondent employment and wealth, using Afrobarometer questions on employment status and household finances as outcome variables. If the *wealth* mechanism is at play, we would expect to see FDI increasing local employment and improving household finances.

Results

FDI and corruption

The results suggest that FDI decreases local corruption experiences. The main results are presented in Table 1 below, with full results in the online appendix. In column 1 in Table 1 we see the effects of FDI projects from all sources in the local area. The difference-in-difference measure in Column 1 shows that the presence of at least one active FDI project in the vicinity

[12] As many of our respondent sites are proximate to multiple FDI projects, in the models below we average and/or calculate the proportion the source country characteristics for proximate FDI projects that were active as of the time of the survey response.

reduces the probability of a respondent paying a bribe by 1.3 percentage points, a reduction of roughly 7.6% of the sample mean of 17.0. This difference-in-difference is statistically significant at the 5% level. This result is consistent with the cross-country findings of Kwok and Tadesse (2006) who found a relationship between increased FDI and reduced corruption. Of note, the positive and significant coefficient on the *inactive* variable suggests that FDI is more likely to be initially *sited* in more corrupt localities.

We next turn to the examination of the professionalization mechanism, as proxied by source country corruption levels, in columns 2 and 3. When separating source countries by *their* levels of corruption, we see that, contrary to our expectation, FDI from *high*-corruption source countries (column 2), *reduces* local corruption experiences, with the difference-in-difference significant at the 1% level. Like the finding above, FDI from these high-corruption source countries also *goes* to locations that have higher than average corruption. This finding squares with Wright and Zhu (2018) who find that FDI can secure monopoly rents in personalists regimes with high levels of corruption making those locations attractive for firms that are unconstrained in investing there. In contrast, FDI from low source-country corruption countries (column 3) does *not* reduce corruption experiences as the difference-in-difference is statistically insignificant. Moreover, unlike FDI from source countries with higher corruption levels, FDI from states with lower corruption levels, however, does not *locate* to areas higher levels of corruption. Both of these findings support those of Beazer and Blake (2018) who find that FDI is attracted to host states whose institutional quality mirrors their home environment.

We next examine the *regulatory pressure effect* via heterogeneous adoption of the OECD anti-bribery convention. When considering signatory status (columns 4 and 5), the results nearly mirror those of the high/low corruption distinction above. Areas with a high proportion of FDI from signatory states (column 4) see no statistically significant impact of FDI on corruption experience as show by the insignificant difference-in-difference, but also this FDI does not locate to areas with higher levels of corruption. In contrast, areas with a higher proportion of FDI from **non**-signatory states (column 5), *do* experience a statistically significant reduction in bribery experiences. However, again, this FDI also locates to areas with higher than average corruption, as shown by the positive and significant coefficient on *inactive*.

Table 1: Base Results

VARIABLES	(1) All FDI	(2) High Corr FDI	(3) Low Corr FDI	(4) High ABC	(5) Low ABC	(6) DV Without Cash	(7) DV Working
Active	0.010**	0.015***	-0.004	0.004	0.002	-0.025***	0.046***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)
Inactive	0.023***	0.031***	-0.001	0.004	0.022***	0.012*	0.004
	(0.005)	(0.006)	(0.007)	(0.008)	(0.005)	(0.007)	(0.007)
Observations	100,688	100,688	100,688	100,688	100,688	87,383	100,239
R-squared	0.094	0.094	0.093	0.093	0.094	0.223	0.139
Baseline controls	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES
Difference in difference	-0.013	-0.016	-0.003	0.000	-0.020	-0.037	0.042
F test: active-inactive=0	5.134**	4.959**	0.100	0.000	9.906***	20.750***	29.528***
p value	0.023	0.026	0.751	0.986	0.002	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Finally, we turn to the indirect evaluation of the *wealth* mechanism. When setting "Often Without Cash" (column 6) and "Working" (column 7) as the dependent variables, we see highly significant results that a respondent being within proximity of an *active* FDI site both decreases the likelihood of being "without cash" but also increases the likelihood of being employed. The substantive impact is considerably larger for employment, 0.046 or a 12.4% increase in the baseline mean of 0.372 compared to a 0.025 reduction, or a 4.1% decrease in the mean of 0.617 of being without cash. Coupled with the main finding that active FDI reduces corruption experience, this suggests that the *wealth* mechanism of empowerment may be at work.

The results on the impact of source-country corruption and the OECD anti-bribery convention run counter to our expectations – it is areas with a high proportion of FDI projects from high-corruption and ABC **non**-signatories that see broad and marked reductions in local corruption outcomes. However, they make more sense when considered in tandem with the observation that that FDI from source countries with these characteristics also heads to more corrupt places to begin with. To some extent, we take this evidence as both indicative that it is the *wealth* mechanism at play, but that this mechanism mainly plays out via a regression to the mean effect. One would expect the *wealth* mechanism to be invariant to the source of the FDI – employment is employment and wages are wages. In other words, FDI normalizes the corruption environment without prejudice to the source country characteristics – but this only occurs if there is sufficient "room for improvement" based on the existing corruption environment. Empowering local citizens, via wealth, in localities that were formerly more corrupt than normal simply brings them back towards "average" corruption levels.

The lack any impact from FDI from ABC signatories, or low-corruption source countries, suggests there is little evidence that the *regulatory pressure* or *professionalization effects* are influencing the host-country corruption environment. Where these effects may matter, if at all, is in the initial *siting* of FDI projects from signatory countries. That firms whose source countries are party to the anti-bribery convention appear to locate to less corrupt areas (vis-a-vis FDI from non-signatory, or high-corruption, countries) is quite reasonable given that locating to these areas entails a higher risk of running afoul of anti-bribery legislation and thus incurring sanctions associated with that behavior. Indeed, the finding that FDI from these source countries goes to (comparatively) lower corruption locations is again compatible with the country-level findings of Cuervo-Cazurra (2008) and Beazer and Blake (2018).

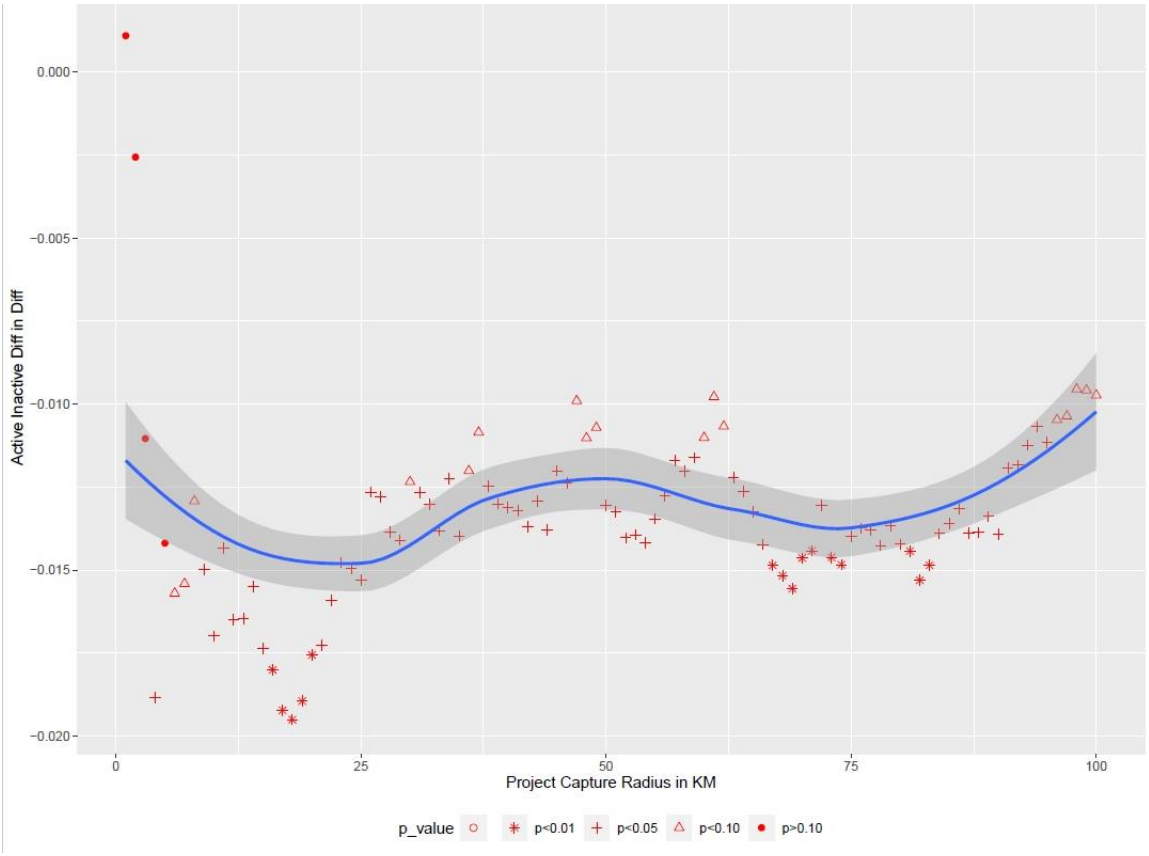

Figure 2: Difference-in-Difference at Different Capture Radii

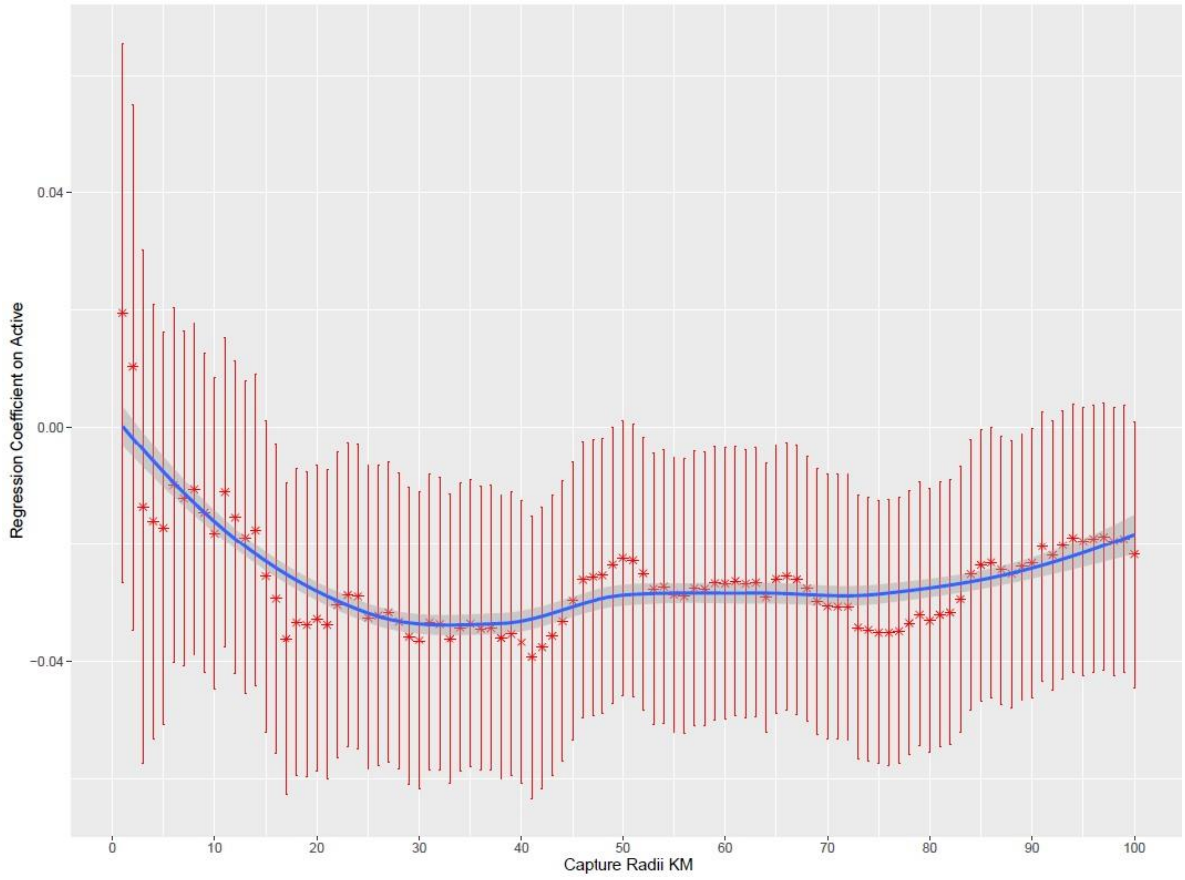

Figure 3: Difference-in-Difference at Different Capture Radii, cluster quasi-panel

Robustness Checks

We run several robustness checks to interrogate our results. First, we investigate if our results are maintained when using a different cutoff point for the treatment distance. We run our baseline model at all capture distances from 1 to 100 km. Figure 2 shows the difference-in-difference between active and inactive for each radii with the p-value indicated by the shape of the indicator. As that figure shows, the difference-in-difference is negative and significant at at least the $p<0.10$ level from a capture distance of 4 km and is significant at at least the $p<0.05$ level for most capture distances. The largest marginal effects are using capture distances between 17 and 19km.

In our next robustness check, we collapse the data to create a panel of Afrobarometer *sites* that were surveyed in multiple rounds, where we average individual responses at the site for a given enumeration round. Assuming that respondent selection is reasonably random (and representative) in any given enumeration area, this gives us a reasonable quasi-panel of data on corruption outcomes. The advantage of this approach is that we are able to employ site-level fixed effects. These fixed effects control for all factors that are stable over time at a particular site, such as long lasting norms about corruption. We again test our main (all FDI) model at all capture radii from 1 to 100km. We include the 50km results in Table A5, but present the coefficient estimate of *active* and the bars representing the 95% confidence interval for all distances in Figure 3. As shown, these results are qualitatively consistent with our main findings. We also take advantage of this collapsed quasi-panel and site-level fixed effects to test the robustness of our results to using the natural-log counts of FDI projects and investment amounts, respectively, as the independent variable. We use both aggregated counts, but also counts identified based on source country corruption or ABC signatory status. These latter approaches allow us to consider the effects of FDI projects from high (or low) source country characteristics controlling for the presence of FDI from the alternative category. Results are again consistent with our main finding and full results are in Table A5.

As noted above, we also run our models using only observations from 2008 onward in order to account for the fact that our FDI data is truncated in 2003, potentially complicating our identification of "inactive" sites. Using only data from 2008 onward ensures that any "inactive" sites had, at a minimum, 5 years of no FDI project. The full results are presented in

Table A1. Qualitatively, the results are maintained for all models except the low ABC signatory countries, where the coefficient "active" projects is now insignificant.

As discussed above, timing may also influence our results as our FDI data is based on *announced* projects. There is almost certainly some delay in those announcements translating into the physical projects and also some measurement error associated with the fact that some FDI project announcements never come to fruition[13]. While there is likely to be variation in the amount of delay from announcement to project, in an attempt to address this issue, we lag our assignment of "active" FDI by one and two years, such that a site is active only if it has been one or two years, respectively since the FDI project announcement. We run our baseline model and our "employment" and "without cash" models using that designation. Full results are presented in Table A2 and are consistent with the main findings above.

Next, we check if our results are robust to employing a non-linear, logit, estimator. These results are displayed in Table A3 and substantively match the results from the linear models above. Finally, we check the robustness of our results to the spatial considerations of our data. While clustering our standard errors should account for any spatial shocks in the error term, there is still the possibility of spatial autocorrelation. If present, given the number of respondents per cluster, this autocorrelation should be far more prevalent within clusters as opposed to between clusters (or countries). To capture the potential for spatial autocorrelation in both, we employed mixed-effects multi-level models with clusters nested within countries (Table A4). The results are substantively similar to those in Table 1.

Illustrative Vignette – Dangote Cement Zambia

While our results point to some causal mechanisms being more likely than others, we note that more in-depth study, such as qualitative fieldwork, is necessary to truly interrogate what mechanisms drive our quantitative findings. That said, we present an illustrative vignette to further support the plausibility of our findings. In order to choose a case for the vignette, we looked for a FDI project that was 1) sufficiently isolated to avoid (too many) confounding projects, 2) sufficiently large such that it plausibly would have an observable effect, 3) near to a sufficient number of Afrobarometer respondents from multiple waves, 4) timed such that

[13] We thank an anonymous reviewer for pointing out this concern.

nearby Afrobarometer waves occurred both before and after the announcement and 5) from a non-ABC source country with relatively high corruption.

One such case that fulfills these criteria is the 2009 Dangote Cement investment outside of Ndola, Zambia. Dangote is a Nigerian conglomerate with a multi-billion USD market capitalization[14]. Nigeria is one of the most corrupt countries in our CPI data, with a mean CPI score in the bottom 5th percentile of our sample, and not an ABC signatory, suggesting the *regulatory pressure* mechanism is not at work. Likewise, Dangote's success has also been attributed to "crony capitalism", suggesting it is not a model for the *professionalization* mechanism (Akinyoade and Uche 2018).

Announced in October 2009, the project included a total investment of roughly $144 million USD to build a new cement plant and a further investment of up to $160 million infrastructure and equipment[15]. The plant and associated works and equipment ultimately cost $400 million and were constructed by 400 workers over the following 5 years, opening in March 2015[16]. The plant is within 50km of 128 Afrobarometer respondents, and 35 km of 97respondents, in 11 cluster areas surveyed in rounds 3 (2005), 4 (2009), 5 (2012), and 6 (2014). 64 of these individuals were surveyed prior to the announcement (rounds 3 and 4[17]), and 64 were interviewed during the construction phase (rounds 5 and 6).

Our data records only one other major project within 10km of the plant, a $325 million investment by Canadian firm First Quantam into their mine at Bwana Mkubwa in 2004 that had opened in 1995. However, this mine closed 2008, briefly restarting in 2010 before closing permanently that year.[18] While a number of other mines exist in the broader area of the Zambian Copperbelt, the Dangote concrete plant is a significant stand-alone investment to the area.

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[14] https://www.reuters.com/companies/DANGCEM.LG accessed 25-03-20

[15] https://allafrica.com/stories/200910161017.html accessed 25-03-2020

[16] http://www.africanreview.com/construction-a-mining/cement/dangote-cement-plant-to-open-in-zambia accessed 25-03-2020, https://www.globalcement.com/news/item/2894-zambian-minister-accuses-dangote-cement-of-bribery, accessed 25-03-2020.

[17] Round 4 was fielded from June 2 to 24th https://afrobarometer.org/data/zambia-round-4-survey-technical-information-2009?page=6 accessed 25-03-20.

[18] https://allafrica.com/stories/201001110091.html accessed 25-03-20, https://www.lusakatimes.com/2010/08/13/bwana-mkubwa-close/ accessed 25-03-20.

Table 2: "Pre" and "Post" Dangote Descriptive Statistics for 128 Surveyed Individuals

	Paid a Bribe	Without Cash	Working
Pre	14/64	53/63	20/64
Post	7/64	32/63	17/64
t-test, p-value	0.096	0.000	0.562

We begin by looking at the descriptive statistics of the 128 surveyed individuals in the "pre" and "post" Dangote Afrobarometer rounds in Table 2 and spatially in Figure 5. Figure 5 shows the location of the Dangote factory as the black square, and the location of each respondent, randomly geo-masked for visibility. The coloring indicates timing of "pre" or "post" status, while the shapes indicate if the individual did or did not pay a bribe (Figure 5.1), was working (Figure 5.2), or was often without cash (Figure 5.3). In Table 2, we see that there is a 50% reduction in the number of individuals paying bribes in the post-Dangote period, a difference significant at the 10% level. However, more interestingly, while we see a nearly 40% fall in the number of people "often without cash", a difference significant at the 0.1% level, we see no noticeable increase in the proportion of people employed[19]. However, the *proportion* of those "with cash" that report a bribe remained roughly the same in both periods. Two of the 8 individuals (25%) "with cash" in the "pre" period paid bribes whereas 5 of the 26 (19%) did so in "post" period.

While only anecdotal, these descriptive statistics may be more suggestive that the drop in bribes comes from a reduction in *demand* or empowerment via *wealth* rather than empowerment via employment status. Lending some support to this notion, Dangote became involved in a high-profile affair when then Zambian Minister for Labour and Social Security Fackson Shamenda accused Dangote of attempting to bribe him[20]. While Dangote denied the allegation and ultimately sued Shamenda for defamation, it opens the possibility that Dangote was providing rents to other local officials as well, who may then have sought fewer petty

[19] What this might suggest is that those interviewed by Afrobarometer aren't necessarily the members of households that are working, but they may be the beneficiaries of others who are working (and/or are able to secure cash through informal activities).

[20] https://www.globalcement.com/news/item/2894-zambian-minister-accuses-dangote-cement-of-bribery accessed 25-03-2020.

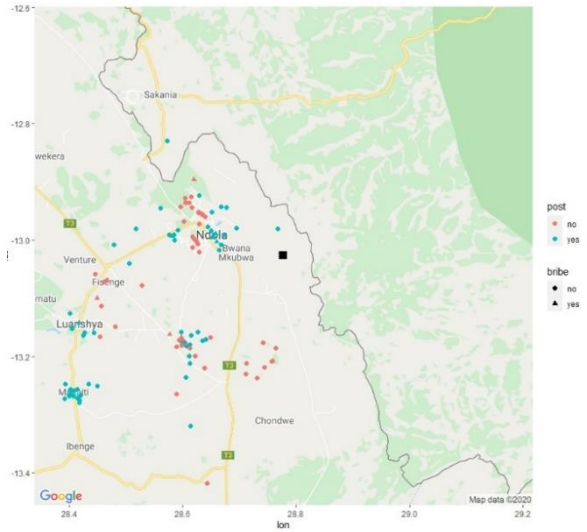

Fig 4.1 Bribe Experience

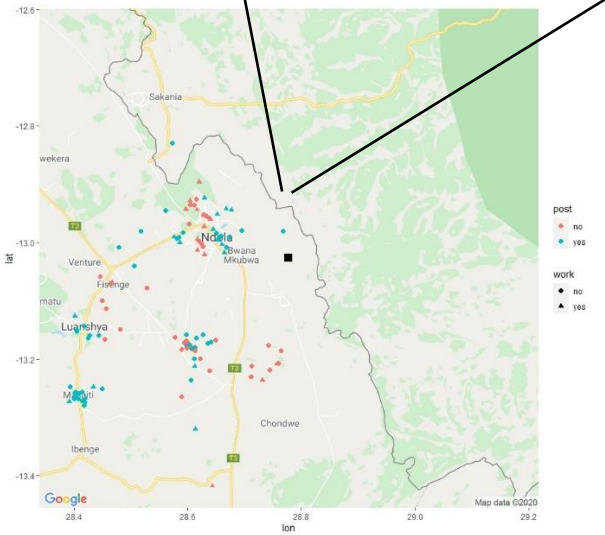

Fig 4.2 Working

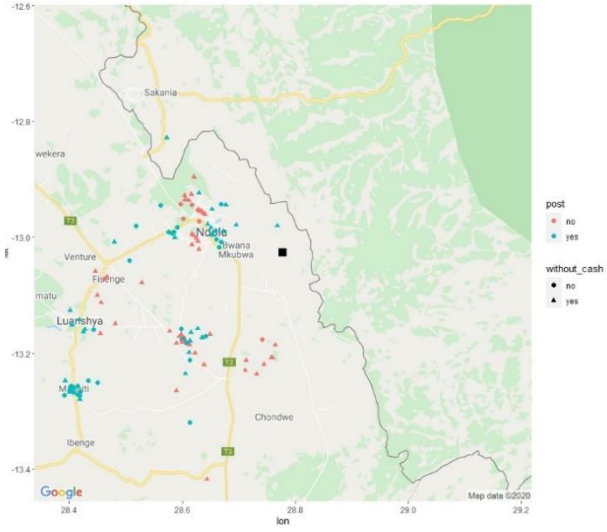

Fig 4.3 Without Cash

Figure 4: Location of Dangote Zambia Cement Factory (black square) and geo-masked Afrobarometer respondent locations and bribe, cash and employment status

bribes from local individuals[21]. We stress that this vignette is not a causal confirmation of our pathways, but rather is an example of a specific investment which shows the plausibility of our mechanisms.

Discussion and Conclusions

The vast bulk of the literature that examines the link between FDI and corruption focuses on the bribery experiences for, or impacts from, *firms*, at the cross-cross country level. We consider a different angle by considering how FDI impacts the bribery experiences of *individuals* at the local level. Our investigation yields two headline findings. First, FDI reduces local corruption experiences, but only when existing levels of corruption are high. This finding is consistent with existing cross-country evidence on FDI and individual corruption perceptions (Kwok and Tadesse, 2006)[22]. Second, sub-national locations with higher levels of existing corruption attract FDI from a more corrupt mix of source countries. This finding replicates the country-level findings of Beazer and Blake (2018). The combination of these findings is suggestive that it is FDI from comparatively more corrupt countries that tends to reduce local corruption. While somewhat counter-intuitive, this result may point to some causal pathways as being more likely than others. First, positive *wealth* effects from FDI are unlikely to depend on source country characteristics. If increased wealth either makes individuals more "difficult" targets for bribery and/or allows individuals to shift consumption from public to private services, then the supply of vulnerable briber payers may decrease, decreasing corruption. Likewise, if officials are able to *substitute* individual bribes for rents directly from firms, and/or states are able to use increased rents from FDI to increase public sector wages, officials may reduce their demands for bribes from individuals.

[21] https://news.yahoo.com/dangote-sues-zambian-minister-over-bribe-claim-111721285.html?guce_referrer=aHR0cHM6Ly93d3cuZ29vZ2xlLmNvbS8&guce_referrer_sig=AQAAACShm8sHi1gSyM1HjFnx9zD7xOk8WvzRNHyQ395WVg3bjWXhcqhN7di5L9iliHIWi8_j48k6Pzbww1rbmD4UaLTs7mZJX3ZmT3T7_cuV1CUSJs_pCMHmRASauYjbOXO3a9Zj-Xtszmz9j-1nRSfrpnNMqfj2T9X2FMYWUEVfbdeV

[22] However, at first glance our results appear to be at odds with Zhu (2017) who looks at a broad range of corruption measure at the province level in China and finds that more MNC activity is associated with more corruption. We propose our findings could be reconciled in several ways. First, while Zhu (2017) includes a broad range of corruption outcomes, we only look at individual bribery experiences. Indeed, in our theoretical discussion we suggest that local officials may *substitute* bribes from firms for bribes from individuals. Thus, one might see an increase in firm bribe experience whilst seeing a simultaneous decrease in individual bribe-paying experience. Second, much of the nuance of our findings appears driven by differences in source-country characteristics but this isn't considered in Zhu (2017). The breakdown of FDI source countries is likely to be sufficiently different for Africa and China. Finally, while Zhu's (2017) analysis is subnational, provinces in China are orders of magnitude larger than out spatial capture areas. It may be, and in fact we argue, that the impact of FDI on individual bribe-paying is likely to be quite localized and the province-level analysis may not have been sufficiently granular to pick up that effect.

However, the fact that FDI from ABS signatory source countries with *lower* corruption has no observable impact on corruption experiences, suggests that the *wealth* effect may only be noticeable when *existing* levels of corruption are comparatively high. This seems plausible as there would need to be some existing *demand* for bribes in order for the *wealth* or *substitution* effect to reduce demand and/or empower individuals to resist demand.

Second, it could be the case that positive *professionalization* effects need not necessarily come from good governance environments. In line with theories of firm hetereogneiety, firms that engage in FDI are likely to be more professionalized than their domestic counterparts, regardless of their home country corruption environment (Greenaway and Kneller, 2007). Thus, multinational firms even from corrupt countries may have good corporate practices which then induce positive demonstration and professionalization spillovers. That said, when restricting our *professionalization* "treatment" to only FDI that comes from comparatively low-corruption source countries we see no reduction in bribery experience. These findings cast doubt on the *professionalization* mechanism. If anything, this mechanism should be *stronger* in cases where FDI comes from low-corruption source-countries and thus the fact that we see either no impact in bribery experiences suggests that professionalization is unlikely to be the main driver of the impact on bribery experiences.

The results also offer insights into the regulatory pressure effects of the OECD's anti-bribery convention. While there is some evidence that FDI from convention signatories locates to less corrupt areas compared to FDI from non-members, there is little evidence that FDI from ABC-signatory states reduces corruption. This suggests that the ABC may be inducing some perverse outcomes. By incentivizing firms to avoid more corrupt areas (for fear of sanction), they may be keeping them from the places where they are needed most, at least in terms of local socio-economic development opportunities.

However, there has also been widespread critique of the implementation and enforcement of the convention. In particular, the non-governmental organization Transparency International has charted implementation by signatory states since 2012 through its series of annual *Exporting Corruption* reports[23]. These reports suggest active enforcement by only a handful of states, while the bulk of signatories have failed, in part or in whole, to meet their commitments

[23] https://www.transparency.org/exporting_corruption accessed 02-04-2020

to the convention. Indeed, it has been shown that the effects of international institutions on state behavior can be largely heterogeneous depending on each state's domestic politics (Botcheva and Martin, 2001). Accordingly, the ABC non-findings above may stem from the fact that the effectiveness of the convention depends on the *enforcement* of its provisions rather than simply signatory status. While there is insufficient data for us to investigate this premise systematically, as more enforcement data becomes available it may be an interesting avenue for future research.

Taken together, these findings highlight the need for nuanced and detailed investigations into subnational political economy phenomena. While our quantitative results are *suggestive* of some mechanisms being more likely than others, we cannot directly test or confirm the precise manner in which FDI influences local bribery experience. While we have provided a small, illustrative, vignette of the Dangote Cement investment in Zambia that suggests the plausibility of our findings, a thorough case-study designs that fully interrogate the FDI-corruption dynamics would be a useful step forward to further uncovering these mechanisms. Foreign flows may often dominate local societies and they can leave substantial impacts on the livelihoods and governance of the areas where they locate. Understanding what these impacts are, and how they might be influenced by national and international institutions, remains an important endeavor for understanding the linkages between development and governance.

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Appendix: Data Sources and Collapsed Summary Statistics (at individual or cluster level) (from 50km Models)

Variable	Source	Max	Min	Mean	Std Dev.	Observations
Bribe	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	1	0	0.171	0.376	100,502
Age	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	130	18	36.89	14.62	100,691
Female	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	1	0	0.501	0.500	101,792
Urban	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	1	0	0.462	0.498	101,792
Working	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	1	0	0.372	0.483	101,330
Often_Without_Cash	BenYishay et al. 2017 http://geo.aiddata.org http://www.afrobarometer.org	1	0	0.617	0.486	88,429
Active (at 50km)	https://www.fdimarkets.com/	1	0	0.370	0.483	101,792
Inactive (at 50km)	https://www.fdimarkets.com/	1	0	0.139	0.346	101,792
Source Country Corruption	https://github.com/datasets/corruption-perceptions-index	8.971	2.239	6.025	1.270	8,777
Anti-Corruption Signatory	https://www.oecd.org/daf/anti-bribery/WGBRatificationStatus.pdf	1	0	0.687	0.279	8,777
Polity IV	https://www.systemicpeace.org/inscrdata.html	10	-10	4.960	4.178	94,067
(ln)GDP	https://databank.worldbank.org/home.aspx	27.27	18.71	23.83	1.543	94,673
GDP_Growth	https://databank.worldbank.org/home.aspx	123.14	-62.1	4.931	3.335	94,681

Table A1: 5-year "Burn In"

VARIABLES	(1) All FDI	(2) High Corr FDI	(3) Low Corr FDI	(4) High ABC	(5) Low ABC	(6) DV Without Cash	(7) DV Working
Active	0.009**	0.013**	-0.003	0.007	0.002	-0.022***	0.044***
	(0.004)	(0.006)	(0.005)	(0.006)	(0.004)	(0.007)	(0.006)
Inactive	0.026***	0.035***	0.003	-0.018	0.033***	0.007	-0.014
	(0.007)	(0.008)	(0.012)	(0.013)	(0.008)	(0.008)	(0.009)
Observations	73,799	73,799	73,799	73,799	73,799	73,138	73,465
R-squared	0.097	0.097	0.096	0.096	0.097	0.243	0.154
Baseline controls	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES
Difference in difference	-0.017**	-0.021**	-0.006	0.025*	-0.031***	-0.028***	0.058***
F test: active-inactive=0	4.977	4.862	0.199	3.358	13.976	9.572	36.431
p value	0.026	0.027	0.656	0.067	0.000	0.002	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Lagged "Active"

VARIABLES	(1) Baseline L1	(2) Baseline L2	(3) DV Without Cash L1	(4) DV Without Cash L2	(5) DV Working L1	(6) DV Working L2
Active	0.011***	0.011***	-0.027***	-0.027***	0.045***	0.050***
	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
Inactive	0.023***	0.022***	0.012*	0.015**	0.003	0.001
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	100,688	100,688	87,383	87,383	100,239	100,239
R-squared	0.094	0.094	0.223	0.223	0.139	0.139
Baseline controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES
Difference in difference	-0.012**	-0.011*	-0.039***	-0.041***	0.043***	0.049***
F test: active-inactive=0	4.019	3.331	22.073	23.014	29.462	35.070
p value	0.045	0.068	0.000	0.000	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3: Logit

VARIABLES	(1) All FDI	(2) High Corr FDI	(3) Low Corr FDI	(4) High ABC	(5) Low ABC	(6) DV Without Cash	(7) DV Working
Active	0.080**	0.113***	-0.029	0.040	0.025	-0.114***	0.222***
	(0.033)	(0.038)	(0.038)	(0.040)	(0.032)	(0.031)	(0.028)
Inactive	0.151***	0.212***	-0.034	0.008	0.145***	0.071*	0.031
	(0.036)	(0.038)	(0.049)	(0.051)	(0.036)	(0.039)	(0.036)
Observations	100,688	100,688	100,688	100,688	100,688	87,383	100,239
Baseline controls	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES
Difference in difference	-0.071*	-0.099**	0.005	0.032	-0.120***	-0.185***	0.191***
Chi2 test: active-inactive=0	3.103	3.975	0.007	0.288	7.174	20.165	25.763
p value	0.078	0.046	0.934	0.591	0.007	0.000	0.000

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4: Multilevel Mixed Effects

VARIABLES	(1) All FDI	(2) High Corr FDI	(3) Low Corr FDI	(4) High ABC	(5) Low ABC	(6) DV Without Cash	(7) DV Working
Active	0.011***	0.003	0.027***	-0.002	0.003	-0.025***	0.045***
	(0.004)	(0.004)	(0.009)	(0.005)	(0.004)	(0.005)	(0.005)
Inactive	0.024***	0.022***	0.005	-0.050***	0.022***	0.001	0.006
	(0.005)	(0.005)	(0.011)	(0.014)	0.005)	(0.007)	(0.006)
Observations	99,975	99,975	99,975	99,975	99.975	86,683	99,531
R-squared							
Number of Countries	35	35	35	35	35	35	35
Number of Clusters	8,729	8,729	8,729	8,729	8,729	8,729	8,729
Baseline controls	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES
Difference in difference	-0.013	-0.019	0.021	0.048	-0.018	-0.027	0.039
χ^2 test: active-inactive=0	7.181***	12.368***	2.637	10.338***	11.04***	14.118***	36.045***
p value	0.007	0.000	0.104	0.001	0.001	0.000	0.000

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A5: Collapse Quasi-Panel, Site Fixed Effects

VARIABLES	(1) Binary DV	(2) Count DV	(3) Count DV	(4) Count DV Corruption	(5) Count DV WGB
Active (binary)	-0.0224*				
	(0.0120)				
ln(ActiveCount)		-0.0113**			
		(0.00484)			
ln(ActiveAmount)			-0.00838**		
			(0.00384)		
ln(ActiveCount high corruption)				-0.0161*	
				(0.00932)	
ln(ActiveCount low corruption)				0.00527	
				(0.0108)	
ln(ActiveCount_WGB_signatory)					-0.00945
					(0.00890)
ln(ActiveCount_WGB_nonsignatory)					-0.00278
					(0.0119)
Constant	0.204***	0.210***	0.209***	0.206***	0.208***
	(0.0605)	(0.0603)	(0.0602)	(0.0604)	(0.0604)
Observations	11,231	11,231	11,231	11,231	11,231
R-squared	0.088	0.089	0.088	0.089	0.089
Number of clusters	8,776	8,776	8,776	8,776	8,776
Baseline controls	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Site FE	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

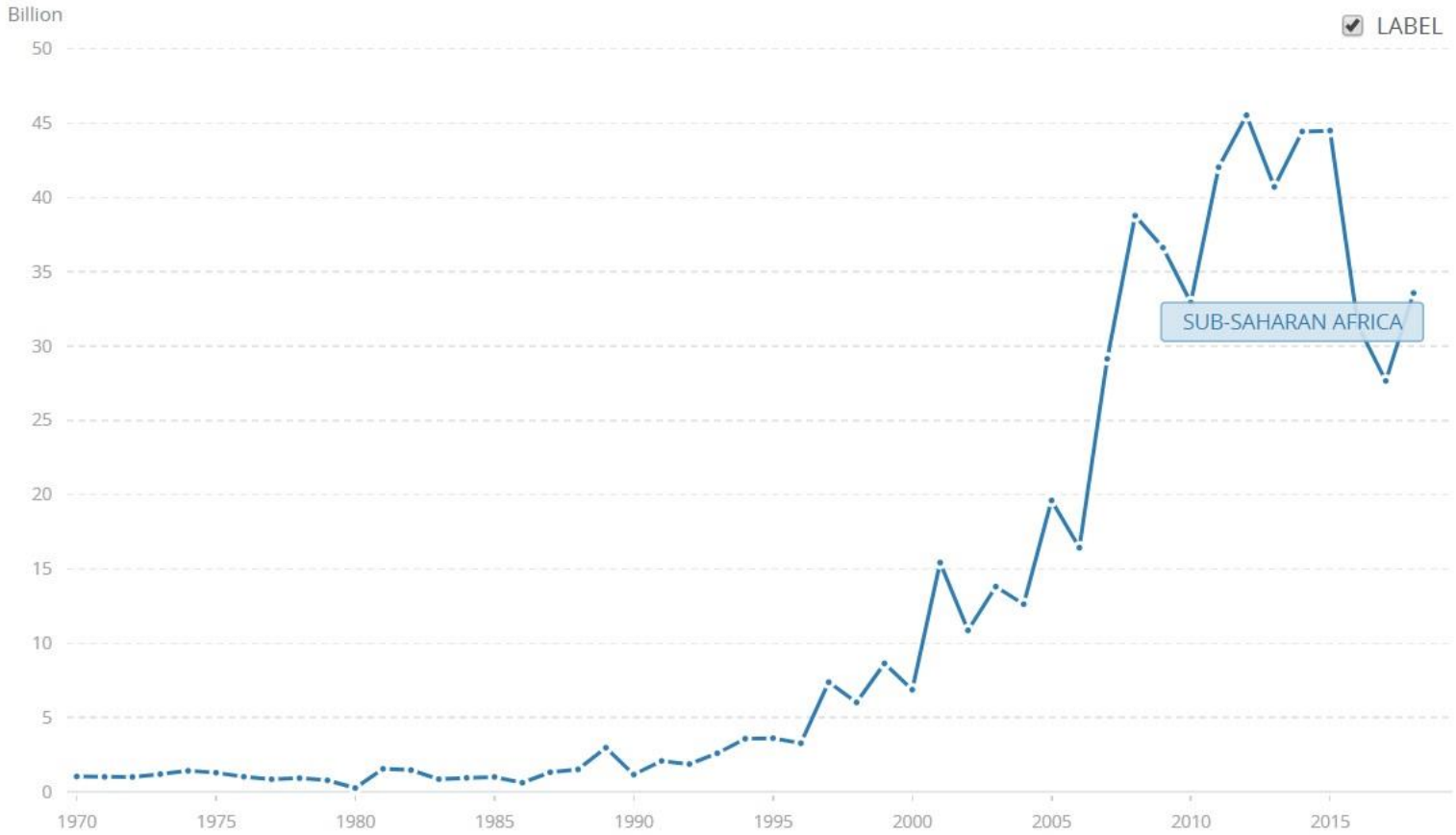
Table A6: Full Model with all Additional Controls

VARIABLES	(1) All FDI	(2) High Corr FDI	(3) Low Corr FDI	(4) High ABC	(5) Low ABC	(6) DV Working	(7) DV Without Cash
Active	0.009	-0.004	0.051***	0.010	-0.008	0.067***	-0.090***
	(0.006)	(0.006)	(0.012)	(0.008)	(0.006)	(0.006)	(0.008)
Inactive	0.052***	0.052***	0.016	0.006	0.048***	0.014	-0.001
	(0.009)	(0.009)	(0.016)	(0.019)	(0.010)	(0.009)	(0.009)
ln_gdp	0.018***	0.019***	0.018***	0.016***	0.019***	0.061***	-0.016***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
gdp_growth	0.006***	0.006***	0.005***	0.006***	0.006***	-0.002**	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
polity2	-0.010***	-0.010***	-0.010***	-0.010***	-0.010***	0.004***	-0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
age	0.002***	0.002***	0.002***	0.002***	0.002***	0.035***	0.007***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
age2	-0.005***	-0.005***	-0.005***	-0.005***	-0.005***	-0.041***	-0.007***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
female	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***	-0.134***	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
urban	0.030***	0.033***	0.033***	0.033***	0.034***	0.043***	-0.112***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.008)
working	0.036***	0.036***	0.036***	0.036***	0.037***		-0.111***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		(0.004)
often_without_cash	0.026***	0.025***	0.027***	0.027***	0.025***	-0.112***	
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Observations	74,936	74,936	74,936	74,936	74,936	74,936	74,936
R-squared	0.049	0.049	0.048	0.048	0.049	0.119	0.134
Year FE	YES	YES	YES	YES	YES	YES	YES
Predominant FDI Dummy	YES	YES	YES	YES	YES	YES	YES
Difference in difference	-0.044***	-0.056***	0.035*	0.004	-0.056***	0.053***	-0.090***
F test: active-inactive=0	20.626	28.317	3.330	0.047	27.606	32.232	73.257
p value	0.000	0.000	0.068	0.829	0.000	0.000	0.000

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Figure A1: Total FDI to sub-Saharan Africa



Foreign direct investment, net inflows (BoP, current US$) - Sub-Saharan Africa

Source: https://data.worldbank.org/indicator/BX.KLT.DINV.CD.WD?locations=ZG Accessed 22-03-20