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Knutsen, C.H., A. Kotsadam, E.H. Olsen, T. Wig, 2017, Mining and Local Corruption in Africa, American Journal of Political Science, Vol 61(2), 320-334, DOI: 10.1111/ajps.12268.

is available at:

https://doi.org/10.1111/ajps.12268

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Working Paper

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Memorandum, Department of Economics, University of Oslo, No. 9/2015

Provided in Cooperation with:
Department of Economics, University of Oslo

MEMORANDUM

No 09/2015

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ISSN: 0809-8786

Department of Economics
University of Oslo

This series is published by the
University of Oslo
Department of Economics

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Mining and local corruption in Africa

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Memo 09/2015-v1

(This version April 2015)

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Abstract

We investigate whether mining affects local-level corruption in Africa. Several cross-country analyses report that natural resource production and wealth have adverse effects on political institutions, for instance by increasing corruption, whereas other country-level studies show no evidence of such "political resource curses". These studies face well-known endogeneity and other methodological issues, and employing alternative designs and micro-level data would allow for drawing stronger inferences. Hence, we connect 90,000 survey respondents in four Afrobarometer survey waves to spatial data on about 500 industrial mines. Using a difference-in-differences strategy, we find evidence that mining increases bribe payments. Mines are initially located in less corrupt areas, but mining areas turn more corrupt after mines open and actively produce. A closer study of South Africa — using even more precise spatial matching of mines and survey respondents — corroborates the continent-wide results. Hence, mineral production is, indeed, a "curse" to local institutions.

Keywords: Resource curse, corruption, minerals, mining
JEL-Codes: Q32, Q33, D73

1 Introduction

Several cross-country studies indicate that dependence on natural resources is related to less democratic regime forms and worse governance institutions and outcomes. (for a recent survey, see Deacon, 2011). In particular, there seems to be a correlation between natural resources and corruption (e.g., Busse and Gröning, 2013). This posited relationship may, for example, stem from natural resource revenues being relatively easy to control and monopolize for political elites (Boix, 2003; Bueno de Mesquita and Smith, 2009), in turn reducing incentives for politicians to provide accountability and transparency. Moreover, high-rent activities such as natural resource production increase the amount of resources available for patronage and unofficial transactions. Hence, one may expect that "resource-abundant countries engender a political state that is factional or predatory and distorts the economy in the pursuit of rents" (Auty, 2001, 839).

Despite these plausible arguments, scholars increasingly question whether the cross-country correlations undergirding the political resource curse thesis reflect *causal effects*. This growing skepticism is related to an increased awareness of the limitations of traditional cross-country designs for drawing inferences, mirroring developments on other, but related, questions, such as how income level affects democracy (Acemoglu et al., 2008). For instance, Haber and Menaldo (2011) argue that the cross-country correlation between natural resource wealth and autocracy largely stems from omitted country-specific characteristics, but also the resource *dependence*–corruption relationship may simply reflect that corrupt countries have poorly performing industrial sectors and few other goods and services to export (see, e.g., Brunnschweiler and Bulte, 2008). Nevertheless, the proposed cures to these ailments, often fixed effects models on country-level panel data, increase the risk of making Type II errors. Exogenous macro-variables measuring natural resources are virtually non-existent, and many of the extant measures employed in panel-regressions are fairly time-invariant within countries, thereby inflating standard errors.

In this paper we provide micro-level evidence — studying the exact localization and opening of industrial mines and how they affect bribe payments and corruption perceptions in their surrounding areas — that mineral extraction affects corruption. More, specifically, we geographically match 90,000 respondents from four Afrobarometer survey waves with spatial data on about 500 mines in 33 different African countries. Our data and design enable us to deal with concerns of country heterogeneity while simultaneously having sufficient variation in our natural resource variable to mitigate the risk of Type II errors. Studying both inactive mines that have yet to open and active ones, we employ a difference-in-differences strategy.

Whereas we often identify a negative relationship between corruption and initial localization of mines, and a positive correlation between corruption and active mines, our core finding is that mining causes corruption to increase. More specifically the opening of mines in African countries — for a wide variety of minerals — is systematically related to increases in local corruption. While not as strong when employing (arguably more problematic) measures of corruption perceptions as when employing measures of bribe payments, this result is quite robust to various specification choices. It is, for instance, retained both in a comprehensive 33 country sample and in a closer study of the country we have the best data from; South Africa.

Although we cannot immediately extrapolate our conclusions to the macro level, these results at least indicate a local political resource curse. While previous studies have documented that mining activities have important consequences for the societies in which they are located (for example by increasing female labor force participation, but also increasing conflict risk), this is the first study of its kind to investigate the local institutional effects of mineral production. Our study thus adds not only to the broader resource curse literature, but also to the literatures on local effects of mining activities and the rapidly growing literature on the determinants of local-level institutions (e.g., Nunn and Wantchekon, 2011; Acemoglu et al., 2014).

In the following, we first review relevant literature (Section 2), before addressing particular challenges to studying the effects of resources on corruption and how our design alleviates many of these problems (Section 3). Thereafter, we present and discuss our data (Section 4), and report and discuss the results for the Africa-wide sample and for South Africa (Section 5).

2 Related literature

2.1 Economic and political resource curses

According to a recent review (Frankel, 2010), the term "Resource Curse" was first coined by Auty (1993). Since then (and even before), numerous studies have addressed the potential economic and political consequences of natural resources production, dependence, wealth, or reserves (see, for instance, Humphreys et al., 2007). Although some studies indicate there may be "resource blessings" (see, e.g., Alexeev and Conrad, 2009; Brunnschweiler, 2008)—at least in the presence of pre-existing strong/inclusive/producer-friendly institutions (e.g., Karl, 1997; Acemoglu et al., 2001; Mehlum et al., 2006b,a; Bhattacharyya and Hodler, 2010)—the debate has largely centered on whether there are resources curses or not.

Regarding the potential negative economic effects of discovering natural resources, the literature has largely focused on GDP growth. Different reasons have been proposed for why natural resources might impede growth (for overviews, see Sachs and Warner, 2001; Frankel, 2010; Torres et al., 2013), including perpetually declining relative prices on natural resources (Prebisch, 1950); large fluctuations in natural resource prices with subsequent negative macroeconomic consequences (Hausmann and Rigobon, 2003); increased incentives for prioritizing rent-seeking activities over productive work (Auty, 2001); lacking incentives for resource-rich governments to provide education opportunities and infrastructure (Isham et al., 2005); *and*, an indirect negative effect through adversely affecting institutional structures and governance. Early empirical studies, relying mainly on cross-country regressions, often found a clear negative association between natural resources and growth (Sachs and Warner, 1995; Leite and Weidmann, 1999), although the effect is stronger when using export-based measures than, for instance, measures of fuel and mineral reserves (e.g., Stijns, 2005). The latter qualification indicates that different endogeneity and selection biases may drive the proposed resource curse relationship (Brunnschweiler and Bulte, 2008).

An equally large literature, spanning both economics and political science, has studied the adverse political and institutional consequences of natural resources (e.g. Ross, 2001; Humphreys et al., 2007; Ross, 2012). Referring to *a* Political Resource Curse involves a—potentially too crude—generalization over the impacts of natural resource activities on various, distinct political and institutional outcomes (see Bulte et al., 2005), such as civil conflict, regime stability, probability of democratization, quality of the bureaucracy and corruption.[1] Notably, there is a large literature on how natural resource wealth may spur authoritarianism. There is evidence that natural resource abundance increases the durability of existing autocratic regimes (e.g., Cuaresma et al., 2011; Andersen and Aslaksen, 2013), and make them less susceptible to democratization (e.g., Boix, 2003; Ross, 2012). Natural resources constitute a source of easily controllable wealth that regime elites may monopolize and subsequently use for co-optation of regime threats or for investing in repressive capacity (e.g., Smith, 2004, 2006; Bueno de Mesquita and Smith, 2009; Cuaresma et al., 2011; Knutsen, 2014). Moreover, Ross (2001) suggests that natural resources enable "rentier states" that

[1] For instance, numerous studies propose that natural resources, notably including oil and diamonds, enhance civil war risk (see, e.g., Collier and Hoeffler, 2001), for example by altering the incentives and the capabilities of rebel groups to contest state authority (for discussions on mechanisms, see Ross, 2004b; Humphreys, 2005). Yet, Ross discusses how mining activities—as opposed to oil production which increases risk of civil war onset—may not affect conflict outbreak, *but* increase the duration of already ongoing conflicts (Ross, 2004a).

escape accountability by providing low taxes, thus relieving pressures for democratization. Despite this, Haber and Menaldo (2011) question the proposed effect of resources on autocracy. Collecting resource data back to 1800, they fail to find any clear time-series evidence of an effect in most countries.[2] In contrast, Aslaksen (2010) does find indications of oil affecting regime type, using GMM models on a cross-country panel data set (for a study using a different identification strategy, but with similar results, see Tsui, 2011). Hence, it remains unclear whether natural resource activities actually cause autocracy.

2.2 Natural resources and corruption

Our empirical analysis tests how mining affects corruption, a phenomenon that likely has wider negative consequences for investment and economic growth (e.g., Mauro, 1995; Mo, 2001). If natural resource dependent economies are conducive to autocratic regimes, they may also be linked to governance outcomes such as control of corruption (and rule of law), provided the latter is contingent on having an open and transparent democratic regime.[3] Nevertheless, natural resources may also directly affect corruption, without the effect being mediated through regime type. First, both within democratic and autocratic regimes natural resource discoveries introduce a "high-rent activity" to the economy, increasing the bribes that involved economic actors can possibly pay while still reaping profits. This, in turn, increases incentives for individual bureaucrats to accept bribes. Furthermore, the economic costs (for example lost tax income) for the government of accepting corruption is lower when the economy is dominated by sectors, such as natural resource extraction, in which capital investments are less price- (and thus bribe-) sensitive (see Leite and Weidmann, 1999). Hence, governments of natural resource-based economies should have less incentives to invest in costly monitoring and control institutions for detecting and punishing corruption. Indeed, leading government actors may themselves benefit from bribes relating to control over natural resource production and exports, as has been the case in numerous African countries (for vivid case stories, see Meredith, 2006).

Vicente (2010) draws on a natural experimental design when investigating the impact of oil discovery announcements on corruption in the small African island country of Sao

[2] Ross and Andersen (2012) question some of the specifications in Haber and Menaldo (2011) and show indications of an effect in more recent decades, after the oil crisis and price shock in 1973, with structural changes to how oil production is governed domestically and internationally.

[3] Yet, the literature on democracy and corruption has failed to identify a clear effect; Rock (2009), for instance, finds that while post-democratization years may actually be associated with increasing corruption, democracy reduces corruption over the medium- to long-term (see also Treisman, 2000).

Tome and Principe. By conducting household surveys, and employing Cape Verde as a control case, Vicente reports evidence that the discovery of oil increases certain types of corruption. Despite this, and despite the theoretical predictions mainly indicating that natural resource extraction increases corruption, the empirical evidence is actually quite mixed. The analyses in Ades and Di Tella (1999), Treisman (2000) and Serra (2006) do not yield clear relationships, particularly when utilizing data from the 1990s (see Busse and Gröning, 2013). In contrast, drawing on cross-country variation from about 70 countries, Leite and Weidmann (1999) find clear associations between fuel production share in GDP and corruption, and particularly between mineral production share in GDP and corruption. Further, Aslaksen (2009) and Busse and Gröning (2013) find indications of an effect when employing panel data models that try to deal explicitly with country-fixed effects and account for the endogeneity of resource-based variables. Nevertheless, the resource–corruption results drawn from cross-section and panel data analysis with country(-year) as the unit vary with the particular specification. For instance, Bhattacharyya and Hodler (2010) find that the relation between resources and corruption turns statistically insignificant once controlling for country-fixed effects. Further, Alexeev and Conrad (2009) find that the estimated impact of oil extraction on corruption depends on whether they correct for income being endogenous to oil production, while they find no effect of mining activities on corruption in any of their specifications.[4]

2.3 Using local-level data to draw inferences

Our paper also contributes to a growing literature on causes and consequences of local institutional variation in Africa. The *modus operandi* of these studies is to use survey-data as proxies for features of local institutions. A seminal contribution is Nunn and Wantchekon (2011) who used geo-referenced data from Wave 3 of the Afrobarometer (see also Nunn, 2010) to study local institutional variation. Nunn and Wantchekon (2011) find robust evidence that areas heavily affected by the slave trade have lower levels of contemporary social trust and institutional quality, results which have also been replicated for Wave 4 of the Afrobarometer survey (Deconinck and Verpoorten, 2013).[5]

[4]The impact of natural resources on corruption may, however, be context dependent. Leite and Weidmann (1999) argue that natural resource discoveries should have a clearer impact on corruption in poor than in rich countries, and this finds support in Busse and Gröning (2013). The impact of natural resources may also depend on the pre-existing institutional framework (e.g., Mehlum et al., 2006b; Bhattacharyya and Hodler, 2010).

[5]Other recent studies have used geo-referenced survey data to investigate various other local-level outcomes. Bellows and Miguel (2006, 2009) assess the institutional effects of civil war violence. Yet other studies

A handful of studies have investigated various local effects of resource extraction. For example, Maystadt et al. (2014) investigate the association between local mining concessions and conflict events in the DRC. Berman et al. (2014) find that mineral production has a sizeable impact on both the probability and intensity of various conflict types in Africa, whereas de la Sierra (2014) finds a positive effect of coltan mining, but no effect of gold, on the emergence of local rebel governance structures in the Democratic Republic of Congo (DRC) — the proposed difference relates to gold being far easier to hide. Dube and Vargas (2013) examine how income shocks affect violence across municipalities in Colombia, and finds that positive oil price shocks increases violence, whereas the same effect is found for *negative* coffee price shocks. Kotsadam and Tolonen (2013, 2014) study how mine openings affect local female labor force participation, finding that mine openings make women more likely to work in the service sector and less likely to work in agriculture. Tolonen (2014) investigates the effects of gold-mining in Africa and finds that mine openings increase women's empowerment. Our study contributes to this burgeoning literature by investigating how local mining activity affects local institutional characteristics in terms of corruption.

3 Endogenous resource extraction

As discussed, the evidence for the existence of economic and political "resource curses" is more mixed than what early, influential cross-country studies (Sachs and Warner, 1995; Ross, 2001) indicated. The empirical results on corruption, more in particular, are also mixed. The results essentially pivot on two issues: country heterogeneity and endogenous natural resource measures. Hence, there still remains uncertainty concerning whether natural resources extraction causes corruption to increase, and, if so, in what contexts this effect operates.

We investigate these questions using a different design and data than recent studies trying to deal with these concerns through country-fixed effects regressions and exogenous variation in natural resources at the national level. These studies necessarily draw on very limited information, since much of the total variation is between-country variation. Truly exogenous country-level measures of natural resources simply do not exist, or they have limited variation

have investigated such effects on social trust (Rohner et al., 2013), and trust in government (Linke, 2013). Different studies have also analyzed local institutional output such as public goods provision (Acemoglu et al., 2014; Glennerster et al., 2013), education (Nunn, 2014), and political participation (Cagé and Rueda, 2014). A distinct line of research has focused on local variation in pre-colonial political structures and their effect on economic development (Michalopoulos and Papaioannou, 2013, 2014; Fenske, 2014).

within countries. Brunnschweiler and Bulte (2008) argue that we should employ "subsoil assets", estimated from World Bank data. Such measures fall short, however, because they are often imputed, extra-/interpolated, or even mere multiples of yearly production. *Even if* we were able to combine within-country variation with truly exogenous measures of natural resources, it is not obvious *when* we should expect the mere presence of these resources to start affecting economic or political outcomes (Deacon, 2011). As a consequence, such analysis could produce "Type II errors", particularly when time series are short (as for most corruption indicators) and the dependent and key independent variables are slow-moving (see Beck and Katz, 2001). Our design and data alleviate this problem, as we draw on rich local-level variation in mining and corruption, which should provide us with far more efficient estimates.

There is another, and more substantive, reason for why many studies using nation-level data may have failed to identify a clear effect of resources on corruption. We noted the difficulties of solving endogeneity issues for this particular question, and there is one critical, and fairly straightforward, source of endogeneity that may downward bias the estimated effect unless successful identification strategies are employed: As highlighted in a voluminous literature, the location and scale of economic activity is strongly affected by the surrounding institutional framework (e.g., North, 1990). Whereas certain institutions reduce production and transaction costs, others increase them. Institutions related to high levels of corruption, in particular, should impose additional costs on firms (see, e.g., Svensson, 2005). At best — when centralized and transparent — bribes work like a tax, and, at worst, pervasive and decentralized corruption generates exceedingly high costs (and uncertainty) (Shleifer and Vishny, 1993). Thus, several empirical studies report that corruption strongly reduces inward foreign direct investments (see, e.g., Blonigen, 2005).

Although the supply curve for natural resources production may be less elastic — and thus less sensitive to expected bribes — than many other economic activities, the level of corruption might make the difference between investing or not investing in some extraction projects. We can only speculate, but it seems unlikely that the expensive tar sand extraction projects in Canada or shale gas extraction in the US would have been initiated in countries with the corruption levels of Afghanistan or DR Congo. Corruption increases costs of doing business *also* in the mining sector, and while several resource economies are corrupt, we expect that the resource revenues of countries such as Afghanistan or DR Congo would increase, *ceteris paribus*, if their corruption levels were to fall. When estimating how mining activities, in turn, affect corruption, we need to account for such "selection effects".[6]

The mixed results in the extant literature, therefore, do not imply there is no strong effect. The more appropriate interpretation is that we, at current, simply do not *know* whether natural resources increase corruption (or lower growth or autocracy, for that matter). We therefore shift focus from problematic macro data to micro data, and employ local variation in mining activities and corruption outcomes. These local data also allow us to trace differences in corruption between areas with *mines that have yet to open activity* and areas with *active mines* following the strategy employed in Kotsadam and Tolonen (2013, 2014). This allows accounting for selection of mining activities into areas with particular characteristics — we expect that firms, everything else equal, want to open mines in less corrupt areas. If the resource curse hypothesis on corruption is true, we should observe significantly higher corruption in areas with active mines than in otherwise similar areas where mines are inactive.

One can, of course, never fully guarantee that these are perfectly comparable: In our sample, inactive mines are usually younger (opened between 2001–2012) than active (opened prior to and during Afrobarometer survey). If companies locate where corruption is low first, active mines would have had lower initial corruption than inactive. Thus, our results — indicating that resources have a clear causal effect on corruption — may actually be downward biased. But, if mines locate where there is scope for bribing officials to speed up construction, our results will be upward biased. Further, corruption in extractive industries has recently received increasing attention, as evidenced by the Extractive Industries Transparency Initiative (EITI) (see, e.g., Corrigan, 2014). Possibly, companies could follow different location strategies today — selecting systematically less corrupt areas — than in the 1980s and 1990s. Comparing active and inactive mines of similar age alleviate such problems associated with endogenous opening, and we show in the Appendix that our main result holds up when restricting the sample only to mines opening within 10 years of the relevant Afrobarometer survey. Perhaps even more important, we also conduct analyses with mine-fixed effects in order to compare corruption levels in the same areas before and after mine opening. We present the data and more specific empirical modeling strategy below.

Our results strongly suggest that opening mines increases local corruption. Hence, one might consider this as evidence pointing towards the more general existence of a political resource curse, at least on corruption. However, we make some cautionary notes on too

[6]There may also be other sources of endogeneity biases. For example, construction permits for mines may be easier to obtain where bribes can speed up the bureaucracy. Alternatively, corruption increases the number of permits required and slows down mine building. Choices of whether or not to give a mining concession may also be affected by corruption.

readily generalizing this to the national level. First, there is no guarantee that the underlying mechanisms are the same locally and nationally, the scale and extent of our natural resource measure differs (opening of single mines versus country-wide resources), and local corruption is not necessarily driven by the same factors as grand-scale national corruption. Second, since we disregard general equilibrium effects, local effects are not automatically transferable to the macroeconomy. An increase in corruption near active mines might, for instance, be correlated with decreases in corruption elsewhere, due to inherently corrupt individuals leaving for the mining communities. Yet, our results, at the very least, document a "local political resource curse", which is significant in itself given the importance of local-level institutions for various outcomes. While we cannot infer this with certainty, however, we are also willing to guess that our results point towards natural resource production inducing corruption also at more aggregated levels.

4 Data and model specification

4.1 Data sources

For measuring our independent variable, we use a novel, longitudinal dataset on large-scale mines, The Raw Materials Database (RMD), from SNL Metals & Mining (2014). This dataset contains information on past and current industrial mines, and future industrial mines with potential for industrial-scale development. The RMD contains data on mines of industrial size and production methods, which often have foreign (mainly Canadian, Australian or UK listed firms in our sample) or government ownership. The dataset excludes small scale mines and informal or illegal mines. The external validity of our results is therefore limited to large-scale industrial mining. Importantly, the data is geocoded with point coordinates and includes yearly information on production levels, enabling us to link them to local survey respondents in the Afrobarometer. We have production levels for the different mines in 1975, and then for each consecutive year from 1984–2013.

Measuring corruption in a valid and reliable manner is difficult (e.g., Svensson, 2005), and — although some are clearly better than others — none of the available measures are without problems. Therefore, we report results drawing on quite different measures of corruption from the Afrobarometer survey. First, the Afrobarometer includes questions on perceptions of local corruption, *and* on whether respondents have actually paid bribes in the last year. Corruption perception measures are, in general, problematic, although perceptions may be a better proxy for actual corruption when pertaining to local-level corruption (e.g., within police and among local councilors) closer to people's everyday lives than to national-level corruption; respondents' replies are more often based on first-hand knowledge. But, even perceptions of local corruption may be subject to bias. Respondents could have priors about the relationship between mining companies and corruption; if some respondents, for some reason, associate mining activities with corruption, they could report perceptions of higher corruption if they simply learn a mine has opened nearby. Thus, we mainly rely on a second set of Afrobarometer items on actually paid bribes as our main measures. The main problem with these measures is quite different; mining-induced corruption activities may affect only certain individuals, such as those working in the mining business or closely related activities. Our moderately sized samples will not always include such individuals, leading to less precise estimates. Further, the mines in our sample are largely foreign- or state-owned, and bribes paid by mine owners to local officials are thus not picked up when surveying the local population.

We employ two Afrobarometer questions on experience with bribes: Respondents are asked if they have paid a bribe for "a document or permit" or "for avoiding problems with the police" and they can answer "Never", "Once or Twice", "A Few Times", "Often", or "No experience with this in past year". We create our two main dependent variables (*bribe to police* and *bribe for permit*) by coding those that have paid a bribe, but not in the last year, *and* those who never paid a bribe as 0. We let the other categories range from 1 (Once or twice) to 3 (Often), but we also test models using dummy variables where the 1–3 categories are collapsed. We complement this with analysis on two perception measures of local corruption. Respondents are asked how many of their local government councilors and how many within the police they think are corrupt, and they can answer "None", "Some of them", "Most of them", "All of them" — in our main models we let these variables range from 0 (None) to 3 (All).[7] As, for instance, Svensson (2005) correctly points out, such corruption measures are, strictly speaking, ordinal. While we employ easy-to-interpret linear models as our baseline, our results are robust to employing ordinal logit models on the above indices or to recoding the indices as dummy variables (see Appendix).

[7]We leave out less theoretically relevant questions about corruption perceptions on national-level groups, such as the president or members of parliament. First, the opening of a single mine (unless very large) should expectedly not affect corruption in the central government by much. Second, it is unclear how people's perceptions of centralized corruption should be attributable to local mineral extraction. It is difficult to interpret any "effect" — given our design which includes country-fixed effects and national corruption must be the same for both sub-samples — as an actual increases in corruption. Doing so would require that respondents in active mining areas are somehow more informed than respondents in inactive mining areas about national corruption.

4.2 Connecting mines with survey respondents, and our samples

The baseline analysis draws on four Afrobarometer waves (Waves 2–5) conducted in 33 countries. We complement this with a case study of South Africa, for which we can validate that we localize individuals very precisely and also have access to "Wave 2.5" Afrobarometer data. We link the mining data to the Afrobarometer data based on spatial proximity. More specifically, we employ point coordinates (GPS) of the surveyed Afrobarometer clusters — a cluster being one or several geographically close villages or a neighborhood in an urban area — to match individuals to mines for which we have accurate GPS coordinates. The location of the mines are mapped out in Figure 1. In total, there are 604 identified industrial mines in Africa, and our 33-country sample allows us to match 496 of them to survey respondents. From a cluster center point we measure the distance to the mines and register if there is at least one mine within 50 kilometers (km). We then construct an indicator variable for whether there is ≥ 1 active mine within this distance (*Active*). If, however, there is no active mine within 50 km we code the individuals as *Inactive* if there is at least one *future mine* that will be opened in the area. All other individuals are coded as living in non-mining areas.

We geolocated respondents based on various geographical information in the Afrobarometer. Wherever possible, we use official enumeration area boundaries to place respondents within their respective enumeration areas (EA). This is the primary Afrobarometer sampling unit and therefore a natural geographical area for analysis. The level of precision then depends on the EA size, which varies between areas of different population densities. Currently, EA boundaries are only available for South Africa and for a number of regions of Sierra Leone.[8] The other observations are either not provided official EA codes, or we were unable to get hold of them even after having contacted different national agencies. Thus, for the other observations we placed each respondent on the *centroid coordinate of reported town, village or neighborhood of residence using Google Maps.* This is a surprisingly effective strategy for precisely locating individuals. Such precision is important for our identification strategy, since we are dealing with relatively short distances.

The algorithm that we used for matching survey respondents to mines was tested using a "natural benchmark" from South Africa: We compared the precision of our Google maps-based coordinates with those in Nunn and Wantchekon (2011) by measuring the distance

[8]We thank Statistics South Africa for access to their GIS data from the 2001 and 2011 censuses. Sierra Leone enumeration areas were retrieved from a public dataset on geocoded waterpoints at `http://www.sl-wash.org`.

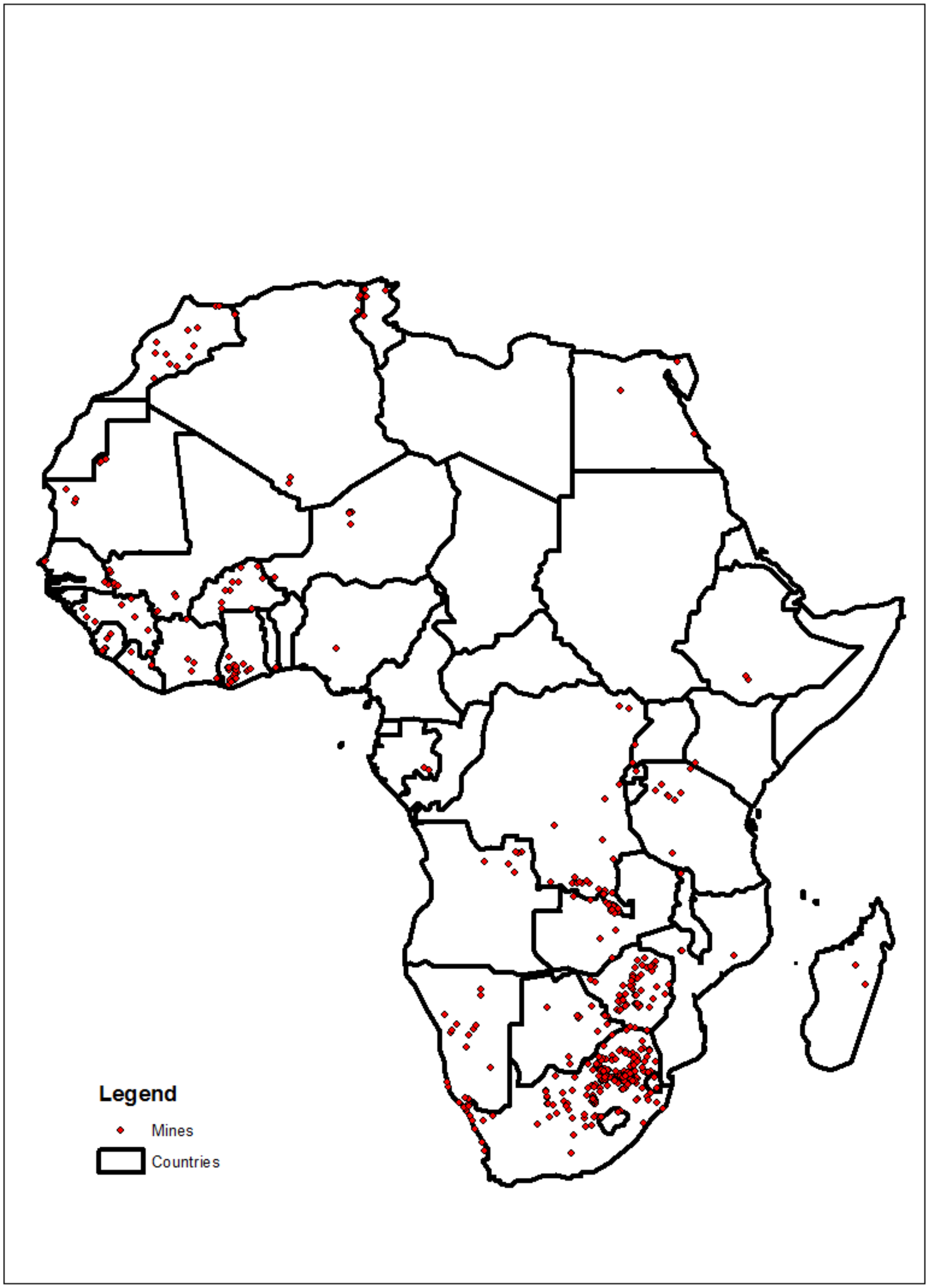

Figure 1: Localization of industrial mines in Africa

between *estimated locations* and *true locations* based on EA information from the 2001 South African census. The average distance from the EA, i.e. the geolocation error, is 124 km for Nunn and Wantchekon (2011) and only 13 km with our Google maps-based coordinates. The 75 percentile distances are 100 km and 9 km, respectively.[9] For Afrobarometer Wave 4, Deconinck and Verpoorten (2013) have more precise coordinates than Nunn and Wantchekon (2011) for Wave 3, but less precise than our geocodes, with 75 percentile distances at 17 km for Deconinck and Verpoorten (2013) and 4.9 km for our coding, respectively.[10] Further testing also showed that the precision for our geocoding is fairly similar for Waves 2 and 5. In total, we are able to locate 97% (11,647 of 11,999) of respondents in South Africa at the EA level over five waves.[11]

Figure 2 shows a map of the the countries included in our sample (see also Appendix Table A.1), and also indicates the more exact survey cluster locations. In order to maximize coverage for the 33 country sample, we draw on existing sources to localize additional individuals — i.e. Nunn and Wantchekon (2011) for Wave 3 and Deconinck and Verpoorten (2013) for Wave 4 — where our localization strategy did not yield results.[12] However, our results are quite similar when only using our mapping strategy, and dropping these additional respondents from the sample.

As noted, we perform a separate study based only on South African data, employing Afrobarometer Waves 2, 2.5, 3, 4 and 5. South Africa is among the world's leading producers of several minerals, including gold and diamonds; a closer study of South Africa is thus interesting in itself. Further, the matching of individuals to mines using geographical coordinates can be done very precisely for South Africa. While only covering one country —

[9]The results are similar when including only enumeration areas smaller than 40 km^2.

[10]Deconinck and Verpoorten (2013) report that they use centroids of districts or regions where towns cannot be geocoded, so the lack of precision is somewhat mechanic, and not necessarily due to geocoding errors.

[11]Observations that we are unable to match at the town/EA level can often be matched to regional districts. However, since these districts can be large, this would increase measurement errors. If the level of precision is random, the effect of including observations with imprecise coordinates will just be a lower average signal-to-noise ratio. This leads to attenuation bias and larger standard errors, working *against* the conclusions in this paper (i.e. strengthening our results). However, the probability of successfully geolocating a town or village depends on whether it lies in a rural or urban area, as Google's information should be better in more densely populated areas. If we include only observations with precise coordinates, this leads to a non-random selection, which is problematic if population density correlates with corruption. This is not a threat to our identification strategy, *per se*, but limits the external validity of our analysis to less rural areas. In our baselines, we thus only include observations located to the EA or town level.

[12]The data from Nunn and Wantchekon (2011) are available at `http://scholar.harvard.edu/nunn/pages/data-0`. We are grateful to Koen Deconinck and Marijke Verpoorten for kindly sharing the geocodes for Wave 4 with us, and to the Afrobarometer for providing access to local-level geographical information.

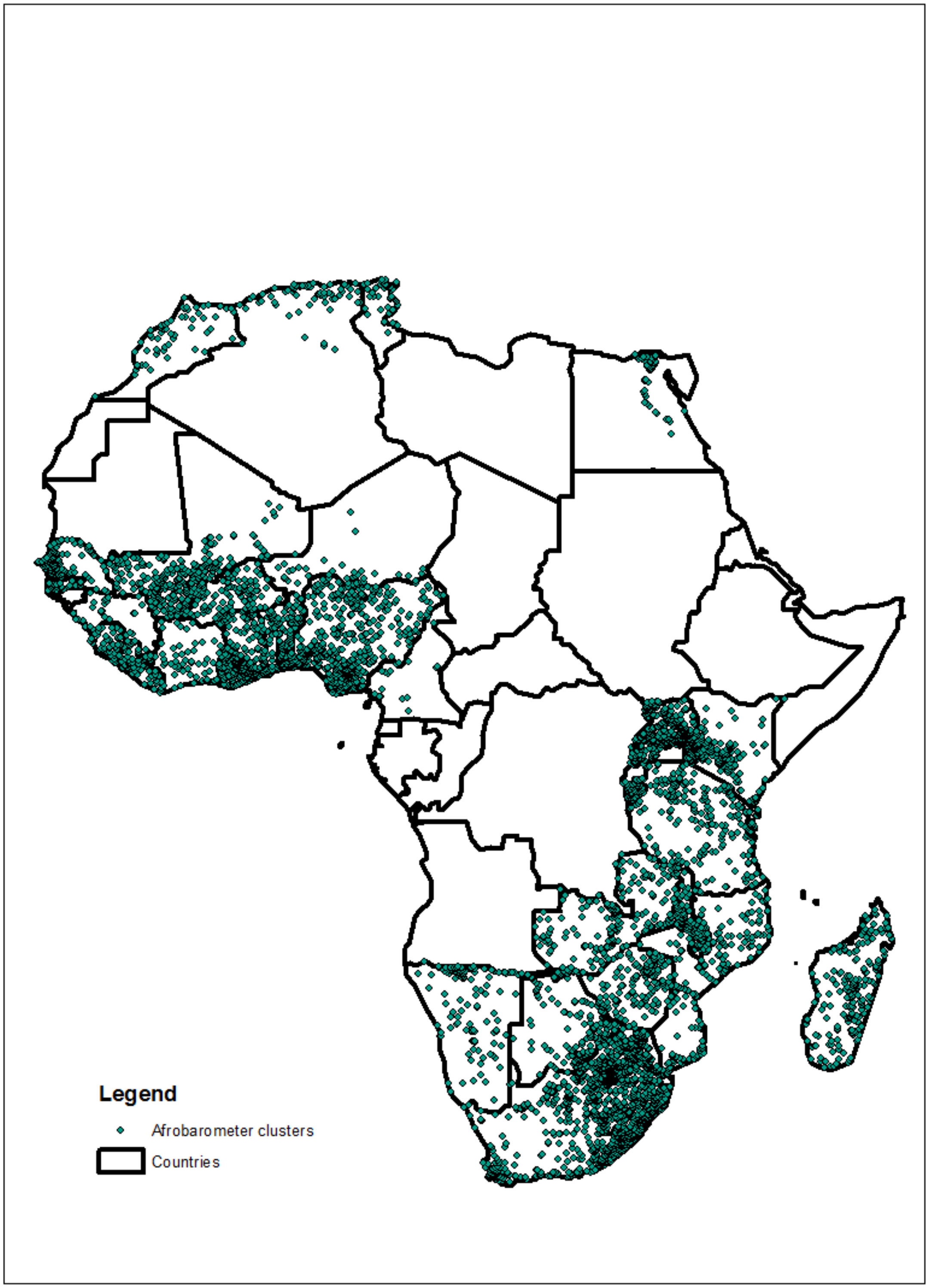

Figure 2: Afrobarometer survey clusters in 33 African countries

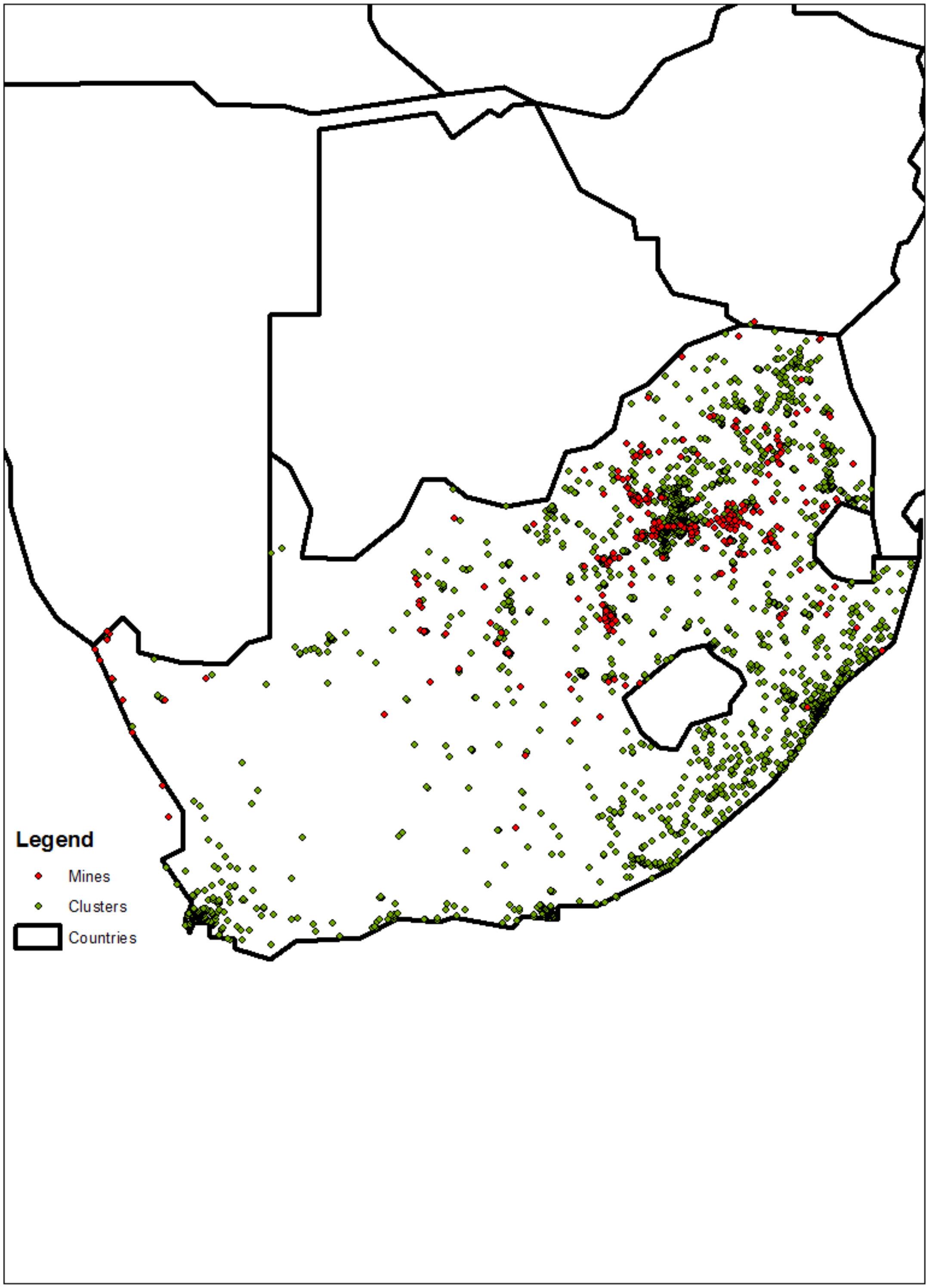

Figure 3: Industrial mines and Afrobarometer survey clusters in South Africa

but a substantial number of survey respondents (more than $\frac{1}{10}$ of the 33 country sample) and of mines (about $\frac{3}{5}$ of the total) — the estimates for South Africa could thus be more precise due to smaller measurement errors stemming from inaccurate matching of mines and survey respondents. Figure 3 provides a more detailed picture of the location of survey clusters and mines for South Africa.

Table 1: Descriptive statistics calculated for the 33-country sample of Model 1, Table 2, and for the South Africa sample of Model 1, Table 3.

	(1)		(2)	
	Total sample		South Africa	
	Mean	SD	Mean	SD
Mining variables				
Kilometers	207.888	(202.537)	85.903	(93.248)
Active 50 km	0.157	(0.363)	0.516	(0.500)
Inactive 50 km	0.014	(0.117)	0.067	(0.251)
Active 25 km	0.080	(0.271)	0.316	(0.465)
Inactive 25 km	0.007	(0.083)	0.026	(0.160)
Dependent variables:	*Paid a bribe last year*			
– to the Police	0.225	(0.657)	0.108	(0.430)
– for a Permit	0.227	(0.624)	0.093	(0.384)
Perception of corruption				
Local councillors	1.306	(0.847)	1.495	(0.853)
Police	1.607	(0.890)	1.469	(0.795)
Control variables				
Urban	0.427	(0.495)	0.659	(0.474)
Age	36.658	(14.622)	38.911	(15.460)
Female	0.498	(0.500)	0.500	(0.500)
Education	3.278	(2.019)	4.150	(1.680)
N	92762		10566	

In the 33 country sample used for Model 1, Table 2 the respondents live, on average, 208 kilometers away from a mine (variable *kilometers*), as shown in Table 1. 15.7% of the 92762 respondents in the Africa-wide sample live within 50 km of at least one active mine. 1.4% live within 50 km of an inactive mine (but no active mines). In total we have 604 mines with information on location and opening year in Africa. In the final sample, we have 496 mines matched to at least one cluster, of which 426 are within 50 kilometers of at least one cluster. For the South African sample (10,566 observations from Model 1, Table 4), respondents, on average, live 86 km from a mine, and 51.6% and 6.7% live within 50 km of active and inactive (but no active) mines, respectively. For South Africa, there are 301 mines matched to one or more clusters, and 277 of these mines are closer than 50 kilometers to at least one cluster.

4.3 Empirical Strategy

Our estimation relies on a spatial-temporal estimation strategy resembling that used by Kotsadam and Tolonen (2013, 2014). As discussed, we test measures of the mine footprint area based on a proximity measure. Assuming that corruption is affected within a cut-off distance, our main identification strategy includes three groups of individuals, namely those (A) within 50 km from at least one active mine, (B) within 50 km from an inactive mine, but not close to any active mines, and (C) more than 50 km from any mine.[13] The baseline regression equation is:

$$Y_{ivt} = \beta_1 \cdot active + \beta_2 \cdot inactive + \alpha_c + g_t + \lambda \cdot \mathbf{X_i} + \varepsilon_{ivt}, \tag{1}$$

where the corruption-measure outcome Y for an individual i, cluster v, and year t is regressed on a dummy *(active)* capturing whether the individual lives within 50 kilometers of an active mine, and a dummy *(inactive)* for living close to a mine that has not started producing at the time of the survey. The regressions also include country- (α_c) and year-fixed (g_t) effects, and a vector (X_i) of individual-level controls (drawing on Afrobarometer data). Our baseline set of controls include living in an urban area, age, age^2, education, and gender, but our results are quite robust to using alternative control sets (see Appendix). Standard errors are clustered at the geographical clusters (EA, town or neighborhood) to account for correlated errors, but our results are retained when, for instance, rather clustering at the closest mine

As the above discussion on endogeneity suggests, interpreting the coefficient for *active* in isolation builds on the premise that the location of mines is not correlated with the institutional characteristics before production starts. This is a very strong assumption because corruption levels — and other factors correlated with corruption, such as population density and accessibility to infrastructure — likely influence mining companies' investment decisions; in particular, mining companies may, everything else equal, be less inclined to invest in highly corrupt areas. Importantly, including *inactive* allows us to compare areas *before* a mine has opened with areas *after* a mine has opened, and not only areas close to and far away from

[13]Mines for which production is known to be zero or production figures are not reported in some intermediate year, but where production is reported to resume thereafter, are coded as active. Areas with previously operating mines where activity is reported to be suspended for longer time periods are excluded from the sample.

mines. For all regressions, we therefore provide test results for the difference between *active* and *inactive* (i.e., for $\beta_1 - \beta_2$). By doing this we get a difference-in-differences measure that controls for unobservable time-invariant characteristics that may influence selection into being a mining area.[14]

The appropriate cut-off distance is an empirical question, and the optimal choice is a trade-off between noise and size of the treatment group. A too small cut-off distance has two problems. First, it would quickly decrease the sample of *inactive* individuals, increasing the probability of a Type II error. Second, any error in geocoding would be inflated relative to the cut-off distance, increasing the probability of defining individuals as treated when they should not be, and vice versa, leading to attenuation bias. A too large cut-off, however, would include too many control individuals into the treatment group, which would pull down the difference between β_1 and β_2. Our choice of a 50 km cut-off is admittedly agnostic, but may, if anything, make our results too conservative due to these potential attenuation biases.

Our first strategy essentially controls for differences between mining and non-mining areas such as initial institutions being different or different geological properties of the areas, which may have shaped institutions prior to the start of mining (Nunn and Puga, 2012). Nonetheless, we also employ mine-fixed effects to get at changes in corruption over time within areas. While this identification strategy is stronger in terms of internal validity, it greatly limits the sample and information we can draw from; it requires observations in different Afrobarometer Waves within the same mining area, both before and after the mine opens. In any case, we also test models including mine-fixed effects and estimate outcomes for individual i, connected to mine m, in year t. Note that, for these specifications, we can directly interpret the coefficient for *active* as the causal effect of opening a mine. We thus estimate the following regression equation:

$$Y_{imt} = \beta_1 \cdot active + \delta_m + \alpha_c + g_t + \lambda \cdot \mathbf{X_i} + \varepsilon_{ivt} \qquad (2)$$

[14]Equation (1) is not a standard diff-in-diff, but can be interpreted as such. We are comparing both post-treatment individuals (active within 50km) and pre-treatment individuals (inactive within 50km) with all other control individuals (outside 50km) within the same country and interview year (due to year and country fixed effects). One single control individual will thus likely be in the comparison group for both pre-treatment and post-treatment individuals, as long as there are both *active* and *inactive* individuals in a given country and interview year. It makes no sense to assign "control" individuals to either the pre-treatment or post-treatment "period", as any such assignment would be arbitrary.

5 Results

5.1 Results from the 33 country sample

We start out by running the baseline regression model on the 33 country sample using four Afrobarometer waves. The results are reported in Table 2. Column 1 shows the results for having paid a bribe to the police last year, and Column 2 for bribes paid to obtain a permit.

The results for *active* indicate that areas where mines are operational are, indeed, systematically associated with higher levels of corruption. *Active* is significant at the 1% level for bribes paid to the police, and at the 5% level for bribes paid for permits. Further, the point estimates are substantially large, particularly for police bribes, with the coefficient in Model 1 (0.024) constituting more than $\frac{1}{10}$ of the sample-wide mean on the police bribe index (0.23). Still, our discussion on endogenous localization of mines suggested that the coefficients for *active* may actually underestimate the effect of mining operations on corruption. Mining companies, everything else equal, likely have incentives to invest in less corrupt areas. This is also borne out in the data; while only the result for police bribes is statistically significant (1%) at conventional levels, the coefficients for *inactive* is negatively signed in both models, indicating that mines are located in areas where corruption is lower.

Most importantly, we corroborate the hypothesis that mining increases corruption. Due to the possibility of endogenous mine placement, we employ the above-discussed difference-in-differences strategy, whereby the effect of a mine opening is calculated as the difference between the coefficients for *active* (treated post-treatment) and *inactive* (treated pre-treatment). This difference-in-differences estimator ($\beta_1 - \beta_2$), and test-results, are presented in the lower rows of Table 2. The difference-in-differences estimators are positively signed, as expected. The result for the police bribe index (Model 1) is statistically significant at 1%, whereas the result for the permit bribe index (Model 2) is weakly significant (at 10%). Further, the point estimates are fairly large, particularly for the police-bribe index; the opening of a mine is predicted to increase the police-bribe index with 0.074, which is about one third of this index' sample mean.

As discussed, the corruption perception measures are more problematic than the items asking about actual bribe payments. Yet, also for the regressions run on perceived local councilor corruption and perceived police corruption, reported in Columns 3 and 4, the coefficients on *active* are positive and significant at 10% and 1%, respectively. The results for *inactive* are mixed. Column 3 reports the expected negative coefficient, indicating that mines are originally located in areas with lower perceptions of local official corruption, whereas

Table 2: Effects of mine openings on corruption in the 33 country sample.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.024***	0.015**	0.026*	0.069***
	(0.0079)	(0.0072)	(0.014)	(0.013)
Inactive 50 km	-0.050***	-0.024	-0.089*	0.063*
	(0.013)	(0.022)	(0.051)	(0.033)
Difference in differences	0.074	0.039	0.115	0.006
F-test: active-inactive=0	28.315	2.950	4.813	0.033
p-value, F-test	0.000	0.086	0.028	0.855
Mean dep. var	0.23	0.23	1.31	1.61
No. of observations	92762	92863	63481	83860
R-squared	0.08	0.06	0.10	0.10

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50 km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

inactive is positive for perceived police corruption. Most importantly, the point estimates for $\beta_1 - \beta_2$ are positive also for the perception measures, although only statistically significant ($p = 0.028$) for perceived corruption among local councilors, and not among police. Thus, while our results are not robust across the different measures, they always point in the same direction; opening a mine seems to increase corruption.

In the Appendix, we report a number of robustness tests. The main result from Table 2 — namely that mining activities increase corruption — is largely robust to different specification choices as the point estimates are fairly stable and achieves statistical significance in several specifications, particularly when employing the corruption measure asking whether respondents have paid bribes to the police during the last year. For instance, the result is retained when rather clustering the standard errors at the level of the closest mine, or when restricting the sample so that we only include active mines that have opened less than 10 years from the timing of the Afrobarometer interview. Moreover, the result holds for different sets of controls. For instance, education may be a bad control, since it is potentially affected by mining activities and thus induces post-treatment bias. Yet, the results are quite similar when we omit education (or any of the other individual level control variables).

The results are weakened when we exchange the 50 km distance from a mine threshold with a lower 25 km threshold for coding *active* and *inactive*. This is, however, not so surprising since the number of treated and control observations fall dramatically. Furthermore, even with our precise geolocation strategy, our validation test on South Africa showed the average location error was 13km, and errors are likely larger for many other countries. Hence, using the narrow 25km threshold could lead to substantial measurement errors for both *active* and *inactive*, inducing attenuation bias. Further, since the outcome variables are ordered choice variables, OLS — assuming equal distances between the outcome categories — may be inappropriate. However, ordered logit regressions yield very similar results as the OLS specifications.[15] The results are also retained if we rather employ dummy variables simply differentiating all individuals that answer the corruption items in the positive versus individuals reporting to not having paid bribes or who do not consider any councilors or policemen to be corrupt.

In Table 3 we present the results from our mine-fixed effects specification. This conservative specification has the advantage of only comparing situations from the same areas before and after the opening of a mine. The disadvantage, however, is that there are only few mines for which we have Afrobarometer observations both before and after a mine opening, making it more likely that we will conduct Type II errors. Nonetheless, the results show that mine openings cause an increase in reported bribe payments to the police. This is a very strong result, given the data limitations, and clearly corroborates the existence of a "local resource curse". However, there is no statistically significant effect for the other three outcome variables.

In sum, our results based on the 33-country sample indicates that mining activities increase corruption, particularly when we use a measure of corruption that taps whether respondents have paid bribes to the police during the last year. However, this result is not completely robust, and is weaker when we employ perception-based measures of local corruption. While our Google-maps based algorithm for locating individuals, and subsequently coding them as living close to either active or yet inactive mines, seems to be fairly accurate, the degree of precision in the location may vary quite a bit across contexts. Hence, our data are likely to contain measurement errors, and more so for some countries than others. As a consequence, our results could be affected by attenuation bias, since, for instance, some individuals living close to active mines are measured as not living in mining areas, despite our best efforts to avoid this. We therefore run the same models on a sample where we know that such errors are minimized.

[15] The difference in means is not straightforward to interpret in the ordered logistic regression, and thus not reported. Qualitatively, however, the coefficients for active and inactive strongly indicate the results are the same.

Table 3: Effects of mine openings on corruption in the 33 country sample. Mine-fixed effects

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.088***	-0.012	0.011	-0.026
	(0.028)	(0.038)	(0.11)	(0.066)
Mean dep. var	0.17	0.18	1.32	1.54
No. of observations	15813	15820	9510	14592
R-squared	0.01	0.01	0.03	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. All regressions control for mine-, country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50 km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5.2 Results from the South African sample, and a brief discussion

For our 33 country sample there is only data for, at most, four Afrobarometer waves, and likely substantial measurement errors stemming from inaccurate location-matching of mines and survey respondents in many countries. For South Africa, we have five waves of Afrobarometer survey results (> 10000 individuals for some models), *and* we know from our above-described validation test that the matching of respondents to mines — using the official Enumeration Areas — is very accurate.

In Table 4 we report estimated effects of mine openings on corruption in South Africa. When considering our favored dependent variables — those measuring reported bribes paid — the results over the South African sample replicates that of the Africa-wide sample. First, also these regressions find that active mining areas are positively correlated with bribes. Second, and more importantly, South Africans systematically report paying more bribes — both to the police and for obtaining permits — once a mine opens within 50km. The difference between *active* and *inactive* is significant at all conventional levels. The point estimates are substantial; opening a mine increases the bribe-item scores with 0.10 (police) and 0.05 (permit). In comparison, the *average scores* on these measures for the South African sample are 0.11 and 0.10, respectively.

As for the 33 country sample, the results are somewhat weaker for the corruption perception measures. While the *active* coefficient is statistically significant for both the perception measures as well, our preferred difference-in-differences estimators have the anticipated signs but is statistically significant at 10% for local councillor corruption perceptions. Neverthe-

Table 4: Effects of mine openings on corruption in South Africa.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.087***	0.054***	0.13***	0.14***
	(0.0097)	(0.0088)	(0.027)	(0.019)
Inactive 50 km	-0.0092	0.00077	-0.024	0.11***
	(0.014)	(0.014)	(0.086)	(0.040)
Difference in differences	0.096	0.053	0.149	0.025
F-test: active-inactive=0	38.378	13.116	3.082	0.429
p-value, F-test	0.000	0.000	0.079	0.512
Mean dep. var	0.11	0.10	1.49	1.47
No. of observations	10566	10574	5818	10020
R-squared	0.02	0.01	0.02	0.03

Notes: Robust standard errors clustered at EA level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer. geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50 km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

less, we ran the same battery of robustness tests as described above for the South African sample, and report tables with results in the Appendix. Also here, the results hold up, for instance, when adjusting the set of control variables or when using different estimation techniques. Although the coefficients are somewhat smaller, the main results also hold up when excluding South Africa from the sample.

Finally, Table 5 presents the results from mine-fixed effects models. Also for the South African sample, we find that the result is robust ($t \approx 4.5$) when bribe payments to the police is the dependent variable. While the estimated effects are in the expected direction also for the other measures, they are insignificant at conventional levels. However, we again note that this is a very conservative estimation strategy, where we can only draw on limited data and where it is difficult to identify an effect even if mining activities were to increase corruption. Bearing this caveat in mind, the South Africa results corroborates those from the 33 country sample. Mining activities seemingly increase corruption at the local level, at least when we employ measures of corruption that are not based on perceptions but rather on the reporting of having actually paid bribes. The finding is particularly clear when we consider bribes paid to the police.

The results presented above indicate that the opening of a mine increases the bribes that people living in the area nearby report to pay for permits, but also to the police. This pattern is identified both in the Africa-wide sample, but also in the sample using (better) data

Table 5: Effects of mine openings on corruption in South Africa. Mine-fixed effects

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.13***	0.023	0.013	0.016
	(0.029)	(0.027)	(0.077)	(0.043)
Mean dep. var	0.14	0.11	1.55	1.52
No. of observations	6160	6167	3286	5919
R-squared	0.01	0.00	0.02	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. All regressions control for mine- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50 km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

from South Africa only. Our measures of corruption, drawing on Afrobarometer surveys of locals, are not likely to pick up large-scale corruption associated with the interaction between mining companies (which are most often foreign- or state-owned) and government officials. Rather, the question on police bribes, in particular, likely measures smaller-scale, "everyday corruption" that locals encounter when engaging with government officials. Hence, our results may be considered surprising; why would industrial mining activities affect bribes that locals pay to the police? While our data does not allow us to drill very deeply into the exact mechanisms underlying this result, plausible suggestions exist:

First, mining activities in a geographic area may boost the local economy, at least in the short term (we discuss the less clear long-term economic effects in the conclusion). Not only does mining, quite naturally, increase mining output, but mining activities could also increase market demand for various types of services by mine workers and others associated with the mining operation. Thus, mine openings may generate ripple effects throughout the local economy, and also put upwards pressures on wages through increased labor demand. Increased income may, in turn, allow local government officials, such as the police, to request more bribes, knowing that locals can now afford to pay them. Second, the upstart of mining activities may simply increase the presence of corrupt officials locally. While the size of police districts vary across African countries, many countries have quite large such districts, and there is almost certainly some leeway for the police to select which areas to operate in more frequently. The opening of a mine, and related increase in local income, could tempt corrupt officials to spend more time in the area nearby the mine, thereby increasing the number of interactions locals have with these officials. Also, corrupt officials would, given the increased local income and opportunities for successfully demanding bribes, have stronger incentives to apply for jobs in police districts where there is an active mine. Hence, there may be a systematic selection of corrupt officials into areas when mining activities start up. In any case, our analysis indicates that mining activities increase local corruption, independent of whether increased local economic activity mainly increases the number of bribes asked for by already present officials, or by generating a flow of corrupt officials into mining areas.

6 Conclusion

There is still no consensus as to whether or not natural resource extraction leads to increased corruption, or constitute a curse to political institutions in general. The literature contains numerous cross-country regression studies, but they face well known endogeneity issues. Recent responses to these issues — such as using panel specifications including country-fixed effects — have problems of their own, notably the lack of variation in the natural resource variables and/or attenuation bias, both increasing chances of making Type II errors. In this paper we have shifted the focus from macro- to microdata, which allow us to come closer to establishing a causal link between mineral resource extraction and corruption. We connected some 600 mines in Africa to more than 90,000 survey respondents in the Afrobarometer. This enabled us to deal with concerns of endogeneity — due, for example, to selection associated with companies' incentives to avoid the most corrupt areas when locating new mines — and control for country-specific effects on corruption, while at the same time preserving variation in natural resources by focusing on the opening of single mines. Using a difference-in-differences strategy, we find that the opening of mines has a positive and statistically significant effect on bribe payments, and to a lesser extent on corruption perceptions.

The paper thus makes two contributions to extant literatures. First, it provides new evidence of the much-debated causal link between natural resources and corruption; our results show that starting up mining activities increases corruption. Second, it adds to the growing literatures on local effects of natural resources as well as on the determinants of local-level institutions. While several important contributions have recently been made to the study of these phenomena, we are the first to study how mining and local-level institutions relate in a systematic manner. Our results clearly suggest mining activities have a deleterious impact on the quality of such local-level institutions, at least when it comes to corruption.

Our design and results open up for different avenues of future research. For example, mining may also affect other institutional and political processes at the local level, and specific arguments on how mining affects local bureaucratic capacity or political participation could be investigated by employing resembling designs and data sources. Furthermore, the long-term effect of mining activities on local-level economic development remains unclear. Indeed, we highlighted how mining activities may increase corruption through boosting production and economic activities in the short-term, thereby increasing opportunities for officials to demand bribes. While the short-term effect of mining on local economic activity may be positive, this needs not be the case in the longer run. The negative long-term development consequences of corruption and poor governance is fairly well documented. Further, previous studies have suggested that the ability to reap the potential economic development benefits from natural resource extraction is highly contingent on having a "high-quality" institutional environment (Mehlum et al., 2006b; Bhattacharyya and Hodler, 2010), and we show that local-level "institutional quality" is hampered by mining activities. Taken together, this indicates that local economies might indirectly suffer from mining activities in the *longer run*, despite the short-term economic boost such activities likely provide. Whether or not there is, on net, a (long-term) local "economic resource curse" is thus an important question for future empirical studies to answer.

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A Online Appendix

This Online Appendix contains robustness tests discussed, but not reported in tables, in the paper. The Appendix is divided into two sections, the first containing the robustness tests for the (total) 33 country sample, and the second the tests for the South Africa sample. In both sections, we report models using alternative strategies for clustering our errors, alternative sets of control variables, alternative "buffer zones" (i.e. how close an individual needs to live to a mine to be matched to it), ordered logit models run on the various corruption indices, models using dichotomized versions of these indices, and when restricting the mines we consider to only include more recent mines.

First, however, we provide a table detailing our sample, by reporting how many individuals are included from the 33 countries under the different Afrobarometer waves.

Table A.1: Numbers of individuals in our sample, broken down by country and Afrobarometer wave

	Afrobarometer wave					
Country	2	2.5	3	4	5	Total
Algeria	0	0	0	0	887	887
Benin	0	0	1,165	1,189	881	3,235
Botswana	1,013	0	1,121	1,156	607	3,897
Burkina Faso	0	0	0	1,081	576	1,657
Burundi	0	0	0	0	728	728
Cameroon	0	0	0	0	235	235
Cape Verde	0	0	708	561	626	1,895
Cote D'Ivoire	0	0	0	0	966	966
Egypt	0	0	0	0	860	860
Ghana	799	0	1,016	1,068	1,053	3,936
Guinea	0	0	0	0	473	473
Kenya	0	0	1,259	1,051	2,006	4,316
Lesotho	1,148	0	1,153	1,112	1,022	4,435
Liberia	0	0	0	1,146	671	1,817
Madagascar	0	0	1,304	1,327	721	3,352
Malawi	1,040	0	1,132	1,136	2,001	5,309
Mali	0	0	1,117	1,216	340	2,673
Mauritius	0	0	0	0	1,057	1,057
Morocco	0	0	0	0	803	803
Mozambique	761	0	1,079	836	1,127	3,803
Namibia	544	0	1,071	1,194	342	3,151
Niger	0	0	0	0	593	593
Nigeria	1,989	0	2,010	2,255	1,030	7,284
Senegal	0	0	968	1,090	938	2,996
Sierra Leone	0	0	0	0	506	506
South Africa	1,931	2,268	2,239	2,202	1,926	10,566
Swaziland	0	0	0	0	207	207
Tanzania	1,109	0	979	1,189	1,100	4,377
Togo	0	0	0	0	252	252
Tunisia	0	0	0	0	465	465
Uganda	1,918	0	2,307	2,324	1,874	8,423
Zambia	1,091	0	1,158	1,145	914	4,308
Zimbabwe	719	0	987	719	875	3,300
Total	14,062	2,268	22,773	24,997	28,662	92,762

A.1 Robustness checks for the 33-country sample

Table A.2: The baseline estimation with standard errors clustered at closest mine.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.024**	0.015	0.026	0.069***
	(0.012)	(0.0098)	(0.021)	(0.016)
Inactive 50 km	-0.050***	-0.024	-0.089	0.063*
	(0.016)	(0.021)	(0.060)	(0.035)
Difference in differences	0.074	0.039	0.115	0.006
F-test: active-inactive=0	23.290	3.485	3.600	0.031
p-value, F-test	0.000	0.063	0.059	0.860
Mean dep. var	0.23	0.23	1.31	1.61
No. of observations	92762	92863	63481	83860
R-squared	0.08	0.06	0.10	0.10

Notes: Robust standard errors clustered at closest mine in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.3: Effects of mine openings on corruption in the 33 country sample. Robustness testing when excluding education as control.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.025***	0.017**	0.028*	0.071***
	(0.0080)	(0.0072)	(0.014)	(0.013)
Inactive 50 km	-0.050***	-0.024	-0.087*	0.064*
	(0.013)	(0.023)	(0.051)	(0.033)
Difference in differences	0.075	0.041	0.115	0.007
F-test: active-inactive=0	29.484	3.223	4.851	0.049
p-value, F-test	0.000	0.073	0.028	0.824
Mean dep. var	0.23	0.23	1.31	1.61
No. of observations	92945	93047	63610	84014
R-squared	0.07	0.06	0.09	0.10

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects, and for urban area, age, age^2 and female. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.4: Effects of mine openings on corruption in the 33 country sample. Robustness testing when excluding education and urban as controls.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.036***	0.024***	0.045***	0.088***
	(0.0079)	(0.0074)	(0.014)	(0.013)
Inactive 50 km	-0.043***	-0.021	-0.080	0.065**
	(0.013)	(0.022)	(0.052)	(0.033)
Difference in differences	0.079	0.045	0.125	0.023
F-test: active-inactive=0	32.311	4.019	5.609	0.479
p-value, F-test	0.000	0.045	0.018	0.489
Mean dep. var	0.22	0.23	1.31	1.61
No. of observations	94755	94876	63610	85621
R-squared	0.07	0.06	0.09	0.09

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects, and for age, age^2 and female. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.5: Effects of mine openings on corruption in the 33 country sample. Robustness testing when excluding all individual level controls.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.038***	0.027***	0.048***	0.092***
	(0.0080)	(0.0075)	(0.015)	(0.013)
Inactive 50 km	-0.038***	-0.016	-0.075	0.069**
	(0.014)	(0.023)	(0.052)	(0.033)
Difference in differences	0.076	0.043	0.123	0.023
F-test: active-inactive=0	27.060	3.406	5.480	0.474
p-value, F-test	0.000	0.065	0.019	0.491
Mean dep. var	0.22	0.23	1.31	1.61
No. of observations	96028	96153	64097	86536
R-squared	0.06	0.05	0.09	0.09

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.6: Effects of mine openings on corruption in the 33 country sample. Robustness testing with 25 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 25 km	0.0027	-0.0020	0.019	0.041***
	(0.0090)	(0.0081)	(0.018)	(0.015)
Inactive 25 km	-0.023	0.013	-0.21**	0.013
	(0.020)	(0.037)	(0.083)	(0.048)
Difference in differences	0.025	-0.015	0.225	0.028
F-test: active-inactive=0	1.426	0.156	7.063	0.333
p-value, F-test	0.232	0.692	0.008	0.564
Mean dep. var	0.23	0.23	1.31	1.61
No. of observations	95028	95138	65260	85922
R-squared	0.08	0.06	0.10	0.10

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 25km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.7: 50 kilometer buffer zones, ordered logit.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
main				
Active 50 km	0.22***	0.15***	0.059*	0.15***
	(0.048)	(0.042)	(0.034)	(0.029)
Inactive 50 km	-0.30**	-0.18	-0.26**	0.14*
	(0.15)	(0.17)	(0.13)	(0.072)
No. of observations	92762	92863	63481	83860
Pseudo R-squared	0.08	0.07	0.04	0.05

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests not presented because they have no straightforward interpretation in an ordered logit regression. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.8: 25 kilometer buffer zones, ordered logit.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
main				
Active 25 km	0.12**	0.076	0.045	0.088***
	(0.056)	(0.050)	(0.043)	(0.031)
Inactive 25 km	-0.12	0.0068	-0.54**	0.052
	(0.15)	(0.20)	(0.23)	(0.11)
No. of observations	95028	95138	65260	85922
R-squared				

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests not presented because they have no straightforward interpretation in an ordered logit regression. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 25km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.9: Effects of mine openings on corruption using dummies. 50 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.017***	0.012***	-0.0094	0.0054
	(0.0040)	(0.0041)	(0.0066)	(0.0041)
Inactive 50 km	-0.023**	-0.017	-0.046*	0.013
	(0.0093)	(0.011)	(0.026)	(0.014)
Difference in differences	0.039	0.029	0.037	-0.008
F-test: active-inactive=0	17.180	6.001	1.987	0.307
p-value, F-test	0.000	0.014	0.159	0.580
Mean dep. var	0.13	0.14	0.85	0.91
No. of observations	92762	92863	63481	83860
R-squared	0.08	0.07	0.09	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. Dependent variable is a dummy taking the value 1 if respondent answers positively on the bribery/corruption question, and 0 otherwise. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A.10: Effects of mine openings on corruption: first mine opens +/− 10 years from interview year. 50 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.023*	0.016	0.020	0.051**
	(0.012)	(0.013)	(0.032)	(0.023)
Inactive 50 km	-0.037***	-0.015	-0.080	0.075**
	(0.013)	(0.022)	(0.052)	(0.034)
Difference in differences	0.059	0.031	0.100	-0.025
F-test: active-inactive=0	11.864	1.324	2.804	0.410
p-value, F-test	0.001	0.250	0.094	0.522
Mean dep. var	0.23	0.23	1.30	1.61
No. of observations	81750	81857	56351	73586
R-squared	0.08	0.07	0.10	0.11

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. Sample restricted to observations where the first active mine within 50 km opened within a range of –10 to 10 years from interview year. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer, as well as round 2.5 for South Africa. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in South Africa is based on census enumeration areas, as are some observations in Sierra Leone. Areas with only suspended mines within 50km are excluded from the sample.

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

A.2 Robustness checks for the South Africa sample

Table B.1: The baseline estimation in South Africa with standard errors clustered at closest mine.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.087***	0.054***	0.13***	0.14***
	(0.014)	(0.013)	(0.038)	(0.024)
Inactive 50 km	-0.0092	0.00077	-0.024	0.11***
	(0.011)	(0.0085)	(0.068)	(0.040)
Difference in differences	0.096	0.053	0.149	0.025
F-test: active-inactive=0	46.901	13.805	5.332	0.587
p-value, F-test	0.000	0.000	0.022	0.444
Mean dep. var	0.11	0.10	1.49	1.47
No. of observations	10566	10574	5818	10020
R-squared	0.02	0.01	0.02	0.03

Notes: Robust standard errors clustered at closest mine in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.2: Effects of mine openings on corruption South Africa. Robustness testing when excluding education as control.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.088***	0.055***	0.12***	0.14***
	(0.0097)	(0.0088)	(0.027)	(0.019)
Inactive 50 km	-0.0094	0.00047	-0.019	0.11***
	(0.014)	(0.014)	(0.086)	(0.040)
Difference in differences	0.097	0.054	0.144	0.026
F-test: active-inactive=0	39.342	13.560	2.870	0.462
p-value, F-test	0.000	0.000	0.090	0.497
Mean dep. var	0.11	0.10	1.49	1.47
No. of observations	10580	10588	5823	10029
R-squared	0.02	0.01	0.02	0.03

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects, and for urban area, age, age^2, and female. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.3: Effects of mine openings on corruption in South Africa. Robustness testing when excluding education and urban as controls.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.084***	0.051***	0.13***	0.15***
	(0.0097)	(0.0090)	(0.027)	(0.019)
Inactive 50 km	-0.020	-0.0088	-0.0075	0.13***
	(0.014)	(0.014)	(0.086)	(0.040)
Difference in differences	0.103	0.060	0.136	0.011
F-test: active-inactive=0	44.955	16.675	2.561	0.080
p-value, F-test	0.000	0.000	0.110	0.777
Mean dep. var	0.11	0.10	1.49	1.47
No. of observations	10580	10588	5823	10029
R-squared	0.02	0.01	0.02	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects, and for age, age^2 and female. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.4: Effects of mine openings on corruption in South Africa. Robustness testing when excluding all individual level controls.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.085***	0.052***	0.13***	0.15***
	(0.0099)	(0.0090)	(0.027)	(0.019)
Inactive 50 km	-0.018	-0.0083	0.0025	0.14***
	(0.014)	(0.014)	(0.086)	(0.040)
Difference in differences	0.104	0.060	0.132	0.013
F-test: active-inactive=0	43.875	17.049	2.387	0.105
p-value, F-test	0.000	0.000	0.123	0.746
Mean dep. var	0.11	0.10	1.49	1.47
No. of observations	10710	10719	5880	10143
R-squared	0.01	0.01	0.02	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.5: Effects of mine openings on corruption in South Africa. Robustness testing with 25 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 25 km	0.063***	0.041***	0.091***	0.062***
	(0.011)	(0.010)	(0.029)	(0.020)
Inactive 25 km	0.022	0.025	0.0057	0.16***
	(0.031)	(0.031)	(0.11)	(0.060)
Difference in differences	0.041	0.016	0.086	-0.098
F-test: active-inactive=0	1.664	0.244	0.568	2.533
p-value, F-test	0.197	0.621	0.451	0.112
Mean dep. var	0.11	0.10	1.51	1.48
No. of observations	10914	10925	6109	10366
R-squared	0.01	0.01	0.02	0.02

Notes: Robust standard errors clustered at EA level in parentheses. "Diff-in-diff" tests are presented in bottom rows. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.6: 50 kilometer buffer zones, ordered logit.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.94***	0.66***	0.29***	0.34***
	(0.100)	(0.096)	(0.060)	(0.046)
Inactive 50 km	-0.098	0.13	-0.075	0.25**
	(0.24)	(0.19)	(0.19)	(0.096)
No. of observations	10566	10574	5818	10020
Pseudo R-squared	0.03	0.02	0.01	0.01

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests not presented because they have no straightforward interpretation in an ordered logit regression. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.7: 25 kilometer buffer zones, ordered logit.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 25 km	0.66***	0.44***	0.21***	0.16***
	(0.088)	(0.091)	(0.062)	(0.048)
Inactive 25 km	-0.031	0.14	0.021	0.41***
	(0.29)	(0.28)	(0.24)	(0.15)
No. of observations	10914	10925	6109	10366
R-squared				

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests not presented because they have no straightforward interpretation in an ordered logit regression. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.8: Effects of mine openings on corruption using dummies. 50 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.060***	0.041***	0.0078	0.011*
	(0.0058)	(0.0057)	(0.0094)	(0.0064)
Inactive 50 km	-0.0044	0.0057	-0.0083	0.022*
	(0.0095)	(0.0100)	(0.032)	(0.012)
Difference in differences	0.064	0.035	0.016	-0.011
F-test: active-inactive=0	39.920	11.257	0.268	0.855
p-value, F-test	0.000	0.001	0.605	0.355
Mean dep. var	0.07	0.07	0.90	0.92
No. of observations	10566	10574	5818	10020
R-squared	0.02	0.01	0.02	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. Dependent variable is a dummy taking the value 1 if respondent answers positively on the bribery/corruption question, and 0 otherwise. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.9: Effects of mine openings on corruption: first mine opens +/− 10 years from interview year. 50 kilometer buffer zones.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.061***	0.040***	0.12**	0.079**
	(0.017)	(0.014)	(0.049)	(0.034)
Inactive 50 km	0.00060	0.012	-0.016	0.097**
	(0.014)	(0.014)	(0.088)	(0.041)
Difference in differences	0.061	0.028	0.137	-0.018
F-test: active-inactive=0	8.686	2.399	2.074	0.137
p-value, F-test	0.003	0.122	0.150	0.712
Mean dep. var	0.07	0.08	1.44	1.41
No. of observations	6187	6189	3276	5755
R-squared	0.01	0.01	0.02	0.02

Notes: Robust standard errors clustered at EA/town level in parentheses. "Diff-in-diff" tests are presented in bottom rows. Sample restricted to observations where the first active mine within 50 km opened within a range of −10 to 10 years from interview year. All regressions control for year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 2.5, 3, 4 and 5 of the Afrobarometer for South Africa. Geocoding in South Africa is based on census enumeration areas. Areas with only suspended mines within 50km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table B.10: Effects of mine openings on corruption in the 32 country sample, excluding South Africa.

	(1) Bribe to Police	(2) Bribe for Permit	(3) Local Corruption	(4) Police Corruption
Active 50 km	0.0022	0.0025	-0.0011	0.042**
	(0.010)	(0.0093)	(0.016)	(0.017)
Inactive 50 km	-0.037*	-0.011	-0.12*	0.066
	(0.022)	(0.044)	(0.063)	(0.055)
Difference in differences	0.039	0.014	0.115	-0.023
F-test: active-inactive=0	2.828	0.092	3.164	0.167
p-value, F-test	0.093	0.761	0.075	0.683
Mean dep. var	0.24	0.25	1.29	1.63
No. of observations	82196	82289	57663	73840
R-squared	0.08	0.06	0.10	0.11

Notes: Robust standard errors clustered at EA/town level in parentheses. All regressions control for country- and year-fixed effects, and for urban area, age, age^2, female and education. The sample includes round 2, 3, 4 and 5 of the Afrobarometer. Geocodes for all rounds are from our own Google-maps matching algorithm. When missing, the data are complemented with geocodes from Nunn and Wantchekon (2011) and Deconinck and Verpoorten (2013) for round 3 and 4, respectively. Geocoding in Sierra Leone is based on census enumeration areas. Areas with only suspended mines within 50 km are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$