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BEWARE OF THE MINE!
THE POLITICAL ECONOMY OF MINES IN NORTHERN MOZAMBIQUE

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Beware of the Mine! The Political Economy of Mines in Northern Mozambique*

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Abstract

We examine the effect of natural resources on the social and political fabric of low-income communities. We combine geospatial data on mining activity with household surveys we conducted in Northern Mozambique. We find that mines decrease the level of trust, especially in neighbors, local and national leaders. In the same direction, households living in mining areas contribute less to public goods. A significant negative effect on participation to local community groups only emerges when using matching methods. On the political side, mineral endowments lead to institutional degradation in the form of lower level of democratic decision-making in the community, lower preference for democratic decisions by the households and increased corruption in the allocation of public funds, which suggest rent-seeking behavior of both the political elite and the population. We also document weak evidence of violence within and around mining areas. These results unveil the presence of both social and political mechanisms behind the natural resource curse and call for carefully monitoring the ongoing expansion of the extractive industries in Africa.

Keywords: Political Economy; Natural Resources; Resource Curse; Mines; Trust; Rent-Seeking; Mozambique; Africa.

JEL Classification Numbers: N57, O13, P16, Q34.

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1 Introduction

Since the dawn of time, the discovery of valuable natural resources, such as oil, gas, or minerals, has attracted the desire and longing of human beings, who see in those assets a fast and easy path to prosperity. However, resource-rich regions have been repeatedly shown to enjoy lower growth rates than resource-poor ones. In particular, in countries with a fragile institutional framework, natural resource abundance turns out to be a curse, rather than a blessing, for their economic and institutional prospects.

In this paper, we contend that the existing literature misses to discern between different mechanisms and channels through which natural resource wealth may be deleterious for underdeveloped economies. We aim to partially fill this gap by exploring how individual behavior relates to resource riches at the microeconomic level. Using observational cross-sectional data allows us to draw stronger inferences in comparison to cross-country variations, which may suffer of omitted variable bias and within-country unobserved heterogeneity.

In our work, we investigate the effect of natural resources, namely mineral assets, on the social and political fabric of low-income communities. In particular, we examine the impact of mines on a wide range of outcomes, such as trust, social capital, democracy in local institutions, demand for political accountability, and rent-seeking behavior.[1]

We use household surveys we conducted in Cabo Delgado, Northern Mozambique. Cabo Delgado is a province with substantial natural resource endowments, which have started to be exploited only recently. Its subsoil hides many valuable assets, which may prove to be crucial (or prejudicial) to its future economic development. According to *Foreign Policy*, the district of Montepuez is thought to hold some 40 percent of the world's known supply of ruby, one of the most precious colored gemstones.[2] Also reserves of gold, graphite and marble are present in the region. This makes of Cabo Delgado an ideal context for investigating the interplay between society, politics and valuable natural assets in economically and institutionally weak

[1] Ross (1999) critically explained the prevalence of economic theories regarding the problems related to natural resources with ≪the failure of political scientists to carefully test their own theories≫ (p. 297). To some extent, our paper aims to address this deficiency.

[2] See two reportages on the discovery of the allegedly largest ruby deposit in the world and the spiral of violence that has arisen, especially against local villagers and illegal miners, in the areas concerned, here `http://foreignpolicy.com/2016/05/03/the-blood-rubies-of-montepuez-mozambique-gemfields-illegal-mining/` and here `https://www.youtube.com/watch?v=-wRNT5dl0pw&t=261s`.

communities. Moreover, we argue that our findings may apply to several African regions, which share with Cabo Delgado similar features in terms of poverty, quality of institutions, and recent discovery of natural resources.

We estimate OLS regressions on several survey outcomes of interest, both from the social and political sphere. In particular, we distinguish the heterogeneous effect in areas with ongoing mineral exploration from the one in sites already subject to extraction activity. As robustness checks, we apply matching methods, we interact the main explanatory variable with migration, in order to control for household selection to mining areas, we vary the geographic unit of observation, and we restrict the sample to bordering locations.[3] We also employ alternative behavioral measures as a way to reduce measurement error bias stemming from self-reported survey data.

We find that mines decrease the level of trust, especially in neighbors, local and national leaders. This confirms the so-called "Pearl Hypothesis" (Kolstad and Wiig, 2012), according to which windfall gains and increased stakes may trigger greed and undermine cooperative behavior and norms.[4] In the same direction, contribution to public goods decreases with mines.[5] A significant negative effect on participation to local community groups only emerges when using matching methods. On the political side, mining areas are associated with institutional degradation in the form of lower level of democratic decision-making in the community and lower preference for democratic decisions by the households, which suggest rent-seeking behavior of both the political elite and the citizenry, as predicted by theoretical models of Robinson et al. (2006) and Tornell and Lane (1999). We also find an increase in corruption in the allocation of public funds in line with empirical evidence by Vicente (2010) and Knutsen et al. (2017). Finally, more violent events are reported within and in the immediate proximity to mining areas: this finding tallies with empirical evidence by Couttenier et al. (2017), who document interpersonal violence as a dimension of the resource curse.

The remainder of the paper is organized as follows. Section 2 provides a brief literature review on the "natural resource curse", with a particular emphasis on studies on mines.

[3] Further sensitivity analysis is shown in the Annex.

[4] Johansson-Stenman et al. (2005) present experimental evidence that increasing stakes significantly decrease money sent in trust games.

[5] A similar finding was also reached by Couttenier and Sangnier (2015), who document that mineral resources foster individualism.

Section 3 describes the data collection and structure. Section 4 presents the estimation strategy, and Section 5 reports the main econometric results and the related sensitivity tests. Section 6 concludes by suggesting some policy implications arising from our findings.

2 Literature Review

Over the last years, economists and development practitioners alike have been more and more interested about natural resources. This concern arose from the first osbervation of a negative correlation between economic growth and the ratio of natural resource exports to GDP, in Sachs and Warner's (1995) seminal cross-country study. Many theoretical hypotheses were raised in order to explain the "natural resource curse", defined by Caselli and Cunningham (2009) as a decrease in income following a resource boom. The initial attempt was purely economic: resource discoveries lead to a boom in the resource sector matched by a decline in the primary and manufacturing sectors through an appreciation of the real exchange rate (i.e., imports become cheaper and make tradable goods less competitive in international markets). Deindustrialization is then likely to have negative externalities on the whole economy, especially in the aftermath of an adverse shock.[6]

Later studies suggested that the key issue is political (Auty, 2001; Mehlum et al., 2006; Cabrales and Hauk, 2011): institutional arrangements are decisive for the existence of a curse as they determine the extent to which politicians and entrepreneurs find attractive to specialize in renting. In particular, scholars have divided between centralized and decentralized theories of rent-seeking. According to the former type of models, political elites react to resource booms by decreasing their propensity to share resources and increasing their investment in patronage in order to hold on to power (Robinson et al., 2006). Moreover, resource revenues allow "rentier governments" to escape accountability and thwart democratization by providing low tax rates (Ross, 2001). On the other hand, resource discoveries make rent-seeking more attractive also for private agents and thus diminish the number of entrepreneurs running a productive business.[7]

[6]The "Dutch Disease" model dates back at least to Corden and Neary (1982). Further contributions were made by Van Wijnbergen (1984), Krugman (1987), Gylfason et al. (1999), Torvik (2001).

[7]See Tornell and Lane (1999) for the so-called "voracity effect"; Baland and Francois (2000) and Torvik

These theoretical mechanisms have been supported by empirical evidence. Leite and Weidmann (1999) study cross-country variations in corruption and find a negative association with natural resource abundance. Drawing on panel data, Bhattacharyya and Hodler (2010) contend that this relationship depends on the quality of democratic institutions. Vicente (2010) exploits oil discovery announcements in São Tomé and Príncipe, as a natural experiment, documenting increases in perceived corruption in a variety of public services and allocations. Monteiro and Ferraz (2010) and Caselli and Michaels (2013) use geographic variation in oil windfalls among Brazilian municipalities and present some evidence of patronage spending, rent sharing and embezzlement by top municipal officials following resource booms.

Other research has investigated the adverse consequences of natural riches on democratic institutions. Ross (2001) finds that oil wealth hinders democracy and nurtures autocratic rule in poor countries. Jensen and Wantchekon (2004) also associate natural resource dependence to authoritarianism, higher levels of government spending and worse governance. Using information on dictators, Crespo Cuaresma et al. (2011) indicate that oil endowment is related with higher duration in power of autocratic leaders, while Andersen and Aslaksen (2013) show that natural resources support political stability and survival of intermediate and autocratic regimes. On the contrary, Haber and Menaldo (2011) do not find any long-run relationship between resource reliance and regime type within countries over time.

Natural riches have also been linked to violence.[8] A growing body of literature, both theoretical and empirical, presents evidence that natural resources raise the risk of conflict by making rebellion financially feasible for the fighting groups, by raising the rent that can be captured through the control of the state, by providing incentives for separatism of ethnic minorities, by hindering the formation of state capacity, and by provoking grievances.[9] More recent studies associate natural resource endowments with surges in violence by focusing on particular countries, e.g., Colombia (Dube and Vargas, 2013) and the Democratic Republic of Congo (Maystadt et al., 2014; Sánchez de la Sierra, 2017).

While most of the economic literature has focused on oil, diamonds, or aggregate measures

(2002) for alternative approaches to rent-seeking.

[8]This strand of research was pioneered by Collier and Hoeffler (2004), who find primary commodities to be the single most powerful predictor of the outbreak of civil wars.

[9]A relevant survey of the literature can be found in Ross (2015).

of natural resources, the recent availability of large datasets on the mining sector has allowed researchers to empirically explore the impact of nonfuel mineral discovery and mines opening on the political and economic fabric of developing countries. Although early evidence by Davis (1995) suggested that the resource curse did not concern mineral-based economies, later studies found less optimistic results. The emergence (and persistence) of the Sicilian Mafia have been attributed to abundance in sulfur, Sicily's most valuable export commodity (Buonanno et al., 2015). Using Afrobarometer surveys, Knutsen et al. (2017) find that extraction activity increases bribe payments and that mining locations turn more corrupt after the opening of industrial mines. Berman et al. (2017) combine information on conflict events with georeferenced data on mining activity for all Africa and find that mines fuel armed fighting at the local level and across territory and time. The latter two studies are the ones that relate more closely to our paper as they use georeferenced local level data in order to explore subnational heterogeneity. Yet, the main singularity of our approach consists in employing a wide range of cross-sectional survey and behavioral measures.

3 Data Description

The data we use in this paper come from household surveys that were conducted by field teams, recruited and trained by the author and colleagues, in Cabo Delgado during the period 5 August to 17 September 2016. The survey was administered to 2070 households, in 207 enumeration areas.[10]

We combine our surveys with geospatial data on mining areas. Namely, we employ the Mozambique Mining Cadastre Portal, which is developed by the Mozambique Ministry of Mineral Resources (MIREM) and Trimble Land Administration, in partnership with the Extractive Industries Transparency Initiative (EITI), «in order to improve transparency and promote investment in the Mozambique mining sector».[11] It provides location and shape of

[10] For information about the main research project behind the data collection, visit http://novafrica.org/research/on-the-mechanics-of-the-natural-resource-curse-information-and-local-elite-behavior-in-mozambique/ or http://www.theigc.org/project/on-the-mechanics-of-the-political-resource-curse-behavioural-measurements-of-information-and-local-elite-behaviour-in-mozambique/.

[11] The database is publicly visible at http://portals.flexicadastre.com/Mozambique/EN/. MIREM owns the intellectual property rights to the data.

every area on which mineral tenure and state mining contracts are enforced, in the whole Mozambican territory. This, in turn, allows us to identify whether a household from our sample lives, or not, in any of these areas and to create a "mining" dummy variable that will serve us as main explanatory variable in the regression analysis.

3.1 Sampling

The data collection process employed a standard two-stage sampling design, which allows us to have a representative sample of households of Cabo Delgado. First, we randomly selected 205 communities, which include rural villages and urban neighborhoods, in Cabo Delgado province.[12] Communities were identified by the corresponding catchment area of polling places, which were registered in both 2009 and 2014 Mozambique general elections.[13] Surveys were randomly submitted to 10 adults in each community: we stratified on households, by choosing one adult per household, and followed a "random walk" sampling procedure. Namely, enumerators departed from the center of the village, which was often the polling location itself (typically, a primary school or meeting room), and headed in 5 different directions.[14] Households were then sought following an interval that was previously determined based on the registered voting population of each enumeration area, in order to guarantee an equal likelihood of visit to all households within the enumeration area, independently from their position in the village. Each selected subject was required to be the household head, to be 18 years or older, and to be available for an interview in the following year.[15] If one of these conditions was not fulfilled, enumerators moved to the next house.[16]

[12]The sample we use in the paper is of 207 enumeration areas because 2 more communities were accidentally surveyed and then substituted with the correct locations. Results are robust to the exclusion of these enumeration areas.

[13]These data were provided by the Technical Secretariat for Election Management (STAE) of the National Electoral Commission (CNE) of Mozambique. The decision of selecting polling places, which existed in both elections, is explained by the fact that they are more likely to exist also in the future elections; this, in turn, makes it more likely that we obtain electoral data from the same communities for further research.

[14]The fieldwork was carried out by 4 teams, each with 5 enumerators and one supervisor (26 enumerators in total). These teams were contemporaneously distributed across the 16 districts of Cabo Delgado, excluding Ibo island. The surveys were submitted in the local dialect (Makua, Makonde, or Mwani) or in Portuguese, and the answers were recorded mainly using tablets. In addition, field operations were assisted and coordinated by the author or by other colleagues mentioned in the acknowledgments.

[15]The latter requirement is explained by the fact that this survey will serve as baseline for a randomized control trial (RCT). Therefore, post-treatment surveys will seek the same respondents as the baseline.

[16]Only 97 invited households did not meet the required conditions.

Moreover, a community survey was administered to a group of people that typically included the political establishment of the village and other available respondents, who contended to know in great detail the history and the characteristics of the community. This survey allows us to detect the presence of a wide range of public infrastructures and services (school, electricity and water supply, sewage, etc.), which we will use as enumeration area controls in the main regressions.

3.2 Survey Design

The survey was tailored to measure aspirations, trust, social capital, democracy in local institutions, demand for political accountability, and rent-seeking behavior. Most of the questions use a subjective scale (e.g., 0 to 3 for trust): interviewers referred to this scale by using precise language qualifiers (e.g., 'Not at all', 'a little', 'yes', 'a lot') but never mentioned the underlying numerical scale. Some questions were asked following a two-stage iterative process in order to elicit the attitudes or preferences of the respondent with more accuracy. First, the enumerator employed the basic scale options 1-2, 3, and 4-5 (e.g., 'disagree' 1-2, 'neither disagree nor agree' 3, 'agree' 4-5); after the interviewee had chosen one of these options, the interviewer, in the first and last cases, asked about the two different alternatives within the class previously identified by the interviewee (respectively, 'strongly disagree' or 'partly disagree', and 'partly agree' or 'strongly agree').

We standardize survey-question measures, by subtracting the mean and dividing by the standard deviation, in order to compute z-scores. Thus, we combine them in summary indices by taking the equally weighted average of the normalized variables, with the sign of each component oriented consistently with the corresponding index label.[17] As suggested by Kling et al. (2007), this aggregation implies significant gains in statistical power to detect effects that go in the same direction within a domain. We prefer this methodology to other techniques for building composite indices, such as principal component analysis, since it allows us to be more transparent about the index construction, namely about the weight given to each survey question.[18] Moreover, the magnitude of the effects on z-score indices

[17] Please refer to *Table A1* in the Annex for the construction of each index that we employ as dependent variable in the next section.

[18] The baseline results shown in Section 5 are robust to using principal component analysis indices. All

are comparable across outcomes, as they are expressed in standard deviation units.

3.3 Behavioral Measures

We also collected behavioral measures of participation, cooperation, and power of the village chief in the communities.[19] In particular, we designed a "contribution game" in all the enumeration areas of our sample. A community meeting was organized in order to decide about the collection of a fund to be used for small-scale community improvements. If the community decided to gather a fund, it would be assigned an extra value equal to half of the amount collected.[20] The field teams took note about the participation of the population to the activity, the preference for contribution by the leader and the citizens, the formation of the decision during the meeting, and the final decision. Particular attention was given to the behavior of the village chief and to the level of discussion during the meeting. As with survey questions, we standardize behavioral measures and create a battery of z-score indices, which are analyzed in the robustness section.[21]

4 Estimation Strategy

In this section, we describe our estimation strategy regarding the impact of mines on a set of social and political attitudes measured through survey and behavioral measures. We begin with the following multiple OLS regression,

$$y_{ild} = \alpha + \beta \times mine_{ild} + \boldsymbol{W}'_d \delta + \boldsymbol{Z}'_{ld} \lambda + \boldsymbol{X}'_{ild} \gamma + \varepsilon_{ild} \tag{1}$$

where y is the outcome of interest (trust, social capital, democracy, corruption, violence), i, l, d are identifiers of individual/household, enumeration area and district, $mine_{ild}$ is a dummy

coefficients have the same direction and significance with the exception of contribution at the intensive margin, which is not statistically significant at the 10% level, because of the large number of missing values in this variable (see *Tables A7-A8* in the Annex).

[19]These data were collected during the treatment of the RCT, mentioned in Footnote 10, between 31 March and 30 April 2017.

[20]The amount collected was kept by a committee of villagers. The field team would go back to the community in order to deliver the bonus. The maximum value of this bonus was set to 2,500 MZN, i.e., 39.05 USD, per village.

[21]The composition of these indices is summarized in *Table A2* in the Annex.

variable equal to one if the household lives in a mining area and zero otherwise, $\boldsymbol{W}'_d$ is a vector of 16 district dummies, $\boldsymbol{Z}'_{ld}$ is a vector of enumeration area characteristics and $\boldsymbol{X}'_{ild}$ of socio-demographic individual characteristics. ε_{ild} is the error term. Our coefficient of interest is β, which represents the effect of mines on the dependent variable. As we are using z-score indices as explained variables, we obtain the average effect for the family of outcomes instead of for each selected outcome.[22] Standard errors are clustered at the enumeration area – in all OLS regressions presented in the remainder of the paper – in order to accommodate correlation within each cluster.

The structure of the geospatial data we employ allows us to differentiate between the effect of mining areas where the extraction is already under way and locations where only the exploration process (prospection or recognition) has started.[23] Thus, we distinguish between two more rigorous specifications of the variable *mine* in order to disclose heterogeneous effects arising from the presence of different mining licenses in the community.

5 Econometric Results

5.1 Descriptive Statistics

Descriptive statistics on a wide variety of characteristics are depicted in *Table A5*. 73.4% of the interviewed population was male, due to the sampling method we used, which required the respondent to be the head of the household. The average age of the recruited respondent was 44.84 years, and the average size of the household was 1.74 adults (besides the head) and 2.92 children. The vast majority of the households were couples – 51.2% married and 33.7% unmarried. The average level of education of the household head was 3.67 years of schooling completed. The sample interviewed mainly belongs to 2 ethnicities, 62.7% Makua and 27% Makonde, and 2 religions, 56.7% Muslim and 37.5% Catholic. Finally, 41.2% of the households were not native of the community but migrated there afterward. This represents

[22] As in Kling and Liebman (2004) and Clingingsmith et al. (2009), we also estimate the average effect for the family of outcomes by using a seemingly unrelated regression system, which accounts for correlation across outcomes. We find very similar results, both in terms of sign and magnitude, and confirm the consistency of our estimates. We describe this method in Section A3.2 and show the related results in *Tables A9-A10* in the Annex.

[23] Summary statistics on mining areas are displayed in *Table A3* in the Annex.

a possible source of endogeneity that we will explore in the sensitivity tests of our baseline specification.

We also gathered information about occupation, property and income of the respondents. 74.9% of the sample are farmers, while other represented occupations are vendors (4.2%), artisans (3.8%), manual workers (3.4%), and public officials (2.9%). Most of the population possesses land, whereas the ownership of assets is very diversified across households and types of item: on average, 55.5% own a cell phone, 49.9% a radio, 48% a bicycle, 14.6% a motorcycle, 13.7% a television, and 6.4% a fridge. The average monthly expenditures of a household are equal to 2,161.78 MZN, i.e., 33.77 USD, while the reported monthly income is 4,862.99 MZN, i.e., 75.96 USD.

5.2 OLS Results

This section reports OLS estimates of the effect that living in a mining area has on a set of social and political survey outcomes. Namely, *Table 1* displays results on trust and social capital, and *Table 2* on democracy, corruption and violence. For each dependent variable, the estimates in the first column are obtained by controlling for 16 district dummies, while in the rest of the columns we control also for characteristics of the enumeration area, which include a wide range of public infrastructures and services (school, electricity and water supply, sewage, etc.), and individual socio-demographic characteristics of the household, such as gender and age of the household head, years of completed schooling, number of adults and children belonging to the household, ethnic and religion dummies, occupation, assets, expenditure, and income.[24]

Results in *Table 1* suggest that mines do not affect the level of generalized trust but decrease our index of particularized trust by 0.08 standard deviations (significant at the 5% level). In particular, living in a mining area is associated with lower trust in neighbors, local leaders and national leaders – see *Table 3* for the decomposition of the effect on trust. Participation to local community groups, such as religious groups (e.g., church or mosque), trade unions or professional associations, local associations (e.g., community solidarity), local

[24]Note that enumeration area controls are presented in *Table A4*, and individual/household controls in *Table A5* in the Annex.

committees (e.g., water management), is not affected by mines at standard significance levels. On the other hand, people living in mining areas contribute less to public goods, in the form of monetary value or hours of work for community improvements. This effect is statistically significant both at the extensive margin (10% level), i.e., whether people contribute or not, and at the intensive margin (5% level), i.e., how much they contribute in total. The magnitude of this effect is, respectively, of 0.09 and 0.15 standard deviations. In particular, the results on contributions at the extensive margin are driven by areas with mining concessions or licenses, and, at the intensive margin, by areas with mineral exploration, prospection or recognition. This divergence means that households living in areas where the extraction process is already ongoing tend to not contribute at all to public goods; on the other hand, in locations still under mineral exploration, a significant negative effect emerges only by looking at the size of the contributions.

This array of findings has a counterpart in the political life of communities. *Table 2* shows that the presence of a mining concession or license in the village leads to lower level of democratic decision-making in the community and reduced preference for democratic decisions by the households (respectively, at the 10% and 1% levels of statistical significance). The magnitude of these effects is larger than the one on social outcomes (respectively, 0.21 and 0.15 standard deviations). However, these results do not hold in areas where the exploration process is still under way, presumably because rents are not existent yet and local institutions may take a longer time to change. The evidence of decentralized rent-seeking behavior is reinforced by the positive coefficient on corruption in the allocation of public funds. Mines increase the bribery level by 0.23 standard deviations (significant at the 5% level). This effect is the largest in magnitude of our analysis, but it should be noted that this outcome variable was measured only on the sample of 504 households that applied to public funds (e.g., the national program known as "7 milhões").

Moreover, households living in a mining area report to have been involved in more violent events. In particular, mining areas increase the probability of physical violence involvement by 2.2 percentage points. This effect is significant at the 5% level – see *Table 4* for the decomposition of the effect on violence.[25] This increase in violence may be due to criminality

[25]Consistently, logit and probit regressions yield marginal effects equal to 2.6 pp. See *Table A11* in the

within the community or to clashes between newcomers and existing residents. We try to disentangle the two hypotheses by interacting *mine* with survey measures of migration in the final part of the paper.

5.3 Robustness

In this subsection, we run a battery of consistency checks, relating to selection bias, definition of mining area and survey-based measurement error. Some of the tables are relegated to the Annex for the sake of brevity.[26]

5.3.1 Propensity Score Matching

We use an alternative control strategy for minimizing the possibility of selection bias and mimic random allocation of mines to households.[27] Instead of employing a linear model of the effect of the covariates, we predict the probability of treatment, i.e., living in a mining area, conditional on a set of observable confounders, $P(X_i) = Pr(mine_i = 1|X_i)$. In particular, we assume that the outcome, y, is independent of treatment exposure (unconfounded), conditional on observed characteristics, X.[28]

$$y(0),\, y(1) \perp (mine|X)$$

We estimate a series of matching regressions with different specifications: namely, we use nearest neighbor matching, radius matching and kernel matching. Despite what is common in applied work, Abadie and Imbens (2008) have shown that bootstrap estimators do not provide reliable standard errors with nearest neighbor and radius matching due to the non-smoothness of the estimators. On the contrary, we follow the method derived by Abadie and Imbens (2011, 2012) in order to estimate correct standard errors of the coefficients. On the

Annex.

[26] All the estimation results not shown in the paper nor in the supplementary annex are available upon request from the author.

[27] We attempt to develop a counterfactual or control group that resembles as much as possible the treatment group in terms of observed characteristics. Given the non-experimental nature of the data, this is the best approach we have to improve on causality.

[28] We match for age, gender, household size, marital status, religion, education, farmer dummy, radio, motorbike, bicycle, and expenditures (in log) as to have a substantial region of common support and satisfy the balancing property, $\hat{P}(X|mine = 0) = \hat{P}(X|mine = 1)$.

other hand, in kernel-based matching methods, the number of matches increases with the sample size: as these methods are asymptotically linear, bootstrap provides valid inference. The average treatment effect estimates are reported in *Table* 5.

Most results are in line with OLS. The effect on particularized trust loses statistical significance, but a negative impact on group participation arises after matching for household characteristics.[29] The effect on contribution to public goods is now significant only at the intensive margin. On the political side, there is robust indication of lower democratic decision-making in the community, lower preference for democratic decisions by the households, and higher level of bribery in mining areas. Finally, the impact on violence is not statistically significant at conventional levels.

5.3.2 Migration

We are concerned that our baseline results are driven by selection. In particular, it is conceivable that households with certain attitudes move to mining areas and thus affect our outcomes of interest. For instance, selfish people may be more likely to move to mining locations, and the increase in violence may be induced by migration of crime-prone individuals from neighboring villages – or even from adjacent countries – in the search of more lucrative occupations than farming. In this case, the interpretation of our estimates would bear different policy implications.

Therefore, we control for migration, and we interact *mine* with a dummy variable equal to one if the household is a migrant household and to zero if its head was born in the community.

$$y_{ild} = \alpha + \beta_1 \times mine_{ild} + \beta_2 \times migrant_{ild} + \beta_3 \times mine_{ild} \cdot migrant_{ild} + \boldsymbol{W}'_d\delta + \boldsymbol{Z}'_{ld}\lambda + \boldsymbol{X}'_{ild}\gamma + \varepsilon_{ild} \quad (2)$$

We also use different specifications of migration (if the subject moved there in the last 10, 20, or 40 years, if he/she moved there in adulthood, the average of the "migration variable" for the village) and consistently find that the interaction term, β_3, is not statistically different

[29] We consider membership in organizations to be a better measurement of social capital and community participation, as it is less likely to suffer of hypothetical or "cheap-talk" biases (Barr et al., 2014). These survey questions, differently from the ones on trust, which concern perceptions by individuals, regard actions from everyday life, and thus should not vary with the interpretation of the question.

from zero.[30] We conclude that the effect of mines on our social and political outcomes does not seem to be explained by self-selection to mining locations. There is no evidence of a "migration mechanism" behind the local resource curse found in our data.

5.3.3 Buffer Analysis

In this subsection, we conduct robustness analysis on the size of our units of observation. If the real mining areas are on average larger than in our definition, we may be underestimating the spatial extent of the impact that mines have on our outcomes of interest. We create buffers of different lengths around the mine location, by enlarging the nodes of mining area polygons, as defined in the geographic database we use in the paper.[31] We find that most of the OLS estimates with the specification in Equation (1) are not robust to buffering, suggesting that the effect may be limited to the mining area and not affect neighboring villages.[32] We interpret these results as confirming that mining areas, as defined in our data, are generally not smaller than the actual ones.

As can be seen in *Table A3*, some of these areas are extremely wide. Namely, areas subject to exploration, prospection or recognition have an average surface of 93.82 km^2. Given this feature of the data, our measures of mines may fail to identify the villages that benefit (or are damaged) the most from resource endowments but provide a general proxy of where mineral resources are located. This may lead to some attenuation bias in our estimates, but it should not affect the sign of the effect.[33] Overall, we believe that our results are conservative estimates of the effect of mineral endowments.

The only effect that seems to consistently diffuse to the surrounding villages is the one on violence (see *Table 6*). This is mainly driven by the higher levels of violence in locations around areas with mining concessions or licenses. These areas are precisely delimited – the average surface is 19.08 km^2 – as the extractive process is already under way. Therefore,

[30] The different definitions of the *migrant* variable used for the sake of this analysis are described in Section A3.4 in the Annex. Estimation results are shown in *Tables A12-A17*.

[31] The buffers are of 1, 2.5, 5, and 10 kilometers.

[32] The same results are found if we use, as regressor of interest, a discrete variable of "mining intensity", i.e., the number of (buffered) mining areas per community.

[33] Note that our database regards not only large-scale mines, operated by multinational companies or by the central government, but also small-scale extraction sites. The fact of having wide mining areas makes it likely that we are also capturing the effect of illegally-operated mines.

these estimates should suffer of minor attenuation bias.

5.3.4 Restricted Sample

Given the results found in the last subsection, we believe that the definition of mining areas we start with is a good proxy of natural resource endowments and we do not need to enlarge its size. In order to minimize the potential bias stemming from confounding factors and to approximate a natural experiment framework, we restrict the sample to households that live in mining areas or at least at 10 km from a mining area, in the spirit of Acemoglu et al. (2012). Then, we re-estimate Equation (1) in the reduced sample.[34] The results, reported in *Table 7*, are consistent with the regressions run on the whole sample.[35] Again, this confirms the robustness of the baseline-model estimates.

5.3.5 Behavioral Measures

The outputs shown in this paper are mainly measured through self-reported information. Thus, we are concerned that our estimates are biased by measurement error. In order to cope with this issue, we analyze behavioral measures that were collected in order to observe unbiased behavior, unlike typical survey measurements. We estimate OLS regressions with robust standard errors.[36] We control for district dummies and enumeration area characteristics.[37] Results are reported in *Table 8*.

We find that villages in mining areas had lower participation to the community meeting, and lower preference for contributions, confirming the results found with survey measures. Moreover, the coefficient on democracy in the meeting decision is negative and statistically significant only for areas with mining concessions or licenses, in line with our baseline results.

[34] With this method, we aim to compare neighboring villages, which share common geographic and socio-economic characteristics but diverge in mining endowments and contracts enforced in the territory.

[35] *Table A18-A19* in the Annex show estimates with smaller and larger samples (i.e., 5- and 20-km bordering areas). Results are consistent with the ones displayed in the paper.

[36] We also estimate these equations allowing for spatial correlation, following the method developed by Conley (1999). The use of spatially HAC standard errors – with different levels of radius – yields very similar results in terms of statistical significance. See *Table A20* in the Annex.

[37] We also control for a "treatment variable" as these measures were collected during the (randomized) intervention related to the research project mentioned in Footnote 10 and thus may already be capturing the effect of the treatment.

6 Concluding Remarks

Economists have been wondering about the sources of the natural resource curse for a long time. In this paper, we address this question by using micro-level data, and we find a remarkable effect of mines on the social and political fabric of communities, which suggests the existence of a “local resource curse”. Given the fact that resource endowments are often spatially concentrated within a country, subnational variation is shown to play a relevant role on household behavior. Future research should not overlook this local dimension when studying the consequences of resource discoveries.

Our analysis yields results in line with both centralized and decentralized theories of rent-seeking. Mines shrink trust and social capital among communities, determine institutional degradation in the form of lower level of democratic decision-making in the community, lower preference for democratic decisions by the households and increased corruption, and trigger violence events within the mining area and in the neighboring locations. In particular, the results on trust and social capital are novel to the literature, to the extent of our knowledge.

We are aware that our approach faces some noteworthy limitations. First, the analysis is merely static as we are using cross-sectional data on households. On the contrary, the dynamic dimension is particularly relevant to natural resources, as it captures the reaction of communities to new discoveries. Moreover, the use of survey measures suffers of shortcomings in terms of internal and external validity.

However, the fact that our study has a microeconomic framework, as it inspects the attitudes and preferences of households by using disaggregated data, makes it less dependent on the specific features of Cabo Delgado. The findings we document are likely to be applicable to several resource-rich regions, which share with Cabo Delgado fragile economic systems and political institutions. We consider our paper particularly relevant in the light of the ongoing expansion of extractive industries in Africa, which draws on vast and still unexploited mineral endowments. We suggest that transparency and regulatory efforts are extremely needed in order to leverage mineral resource wealth and foster African social and economic development.[38] On the other hand, law enforcement by the state is crucial in order to avoid

[38]Future research should focus not only on the role of local institutions on the resource curse but also on the impact of extractive firms’ characteristics and practices. A first attempt in this direction was done

mining-induced violence, which may escalate into local organized crime and political rebellion.

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Appendix

Table 1: OLS regressions on social survey outcomes

Panel A – Trust and social capital

	Generalized trust				Particularized trust				Group participation			
Mining area	-0.017	-0.002			-0.091***	-0.077**			0.032	-0.008		
	(0.032)	(0.034)			(0.035)	(0.038)			(0.029)	(0.032)		
Mining concession or license			0.037				-0.043				-0.048	
			(0.102)				(0.120)				(0.067)	
Mineral exploration, prospection or recognition				-0.008				-0.066*				-0.000
				(0.033)				(0.036)				(0.030)
Observations	2070	1930	1930	1930	2070	1930	1930	1930	2070	1930	1930	1930
R^2 adjusted	0.073	0.107	0.107	0.107	0.071	0.097	0.096	0.097	0.040	0.128	0.128	0.128
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Panel B – Contribution to public goods

	Contribution (yes/no)				Contribution (amount of money or hours)			
Mining area	-0.092*	-0.093*			-0.107*	-0.145**		
	(0.023)	(0.022)			(0.059)	(0.058)		
Mining concession or license			-0.234***				-0.072	
			(0.041)				(0.078)	
Mineral exploration, prospection or recognition				-0.051				-0.131**
				(0.023)				(0.056)
Observations	2070	1930	1930	1930	1186	1136	1136	1136
R^2 adjusted	0.050	0.091	0.091	0.089	0.015	0.052	0.048	0.052
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. All regressions are OLS. Dependent variables are equally weighted indices of z-scores. Controls include enumeration area characteristics, as public infrastructure and services, and individual socio-demographic characteristics, as gender, age, household size, marital status, years of schooling, ethnicity and religion dummies, occupation, assets, expenditure, and income. Standard errors are clustered at the enumeration area (i.e., polling place) in every specification and reported in parentheses.

Table 2: OLS regressions on political survey outcomes

Panel C – Democracy

	Democracy in the community decisions				Preference for democracy by the households			
Mining area	-0.038	-0.020			-0.003	0.010		
	(0.046)	(0.047)			(0.037)	(0.037)		
Mining concession or license			-0.208*				-0.151***	
			(0.110)				(0.055)	
Mineral exploration, prospection or recognition				0.014				0.034
				(0.046)				(0.036)
Observations	2031	1894	1894	1894	2055	1919	1919	1919
R^2 adjusted	0.102	0.099	0.101	0.099	0.140	0.158	0.159	0.158
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Panel D – Corruption and violence

	Corruption on public funds allocation				Violence involvement			
Mining area	0.176**	0.233**			0.045	0.059*		
	(0.085)	(0.092)			(0.033)	(0.035)		
Mining concession or license			-0.149				0.117	
			(0.222)				(0.096)	
Mineral exploration, prospection or recognition				0.250***				0.037
				(0.096)				(0.034)
Observations	504	474	474	474	2070	1930	1930	1930
R^2 adjusted	0.085	0.124	0.114	0.126	0.009	0.017	0.017	0.016
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. All regressions are OLS. Dependent variables are equally weighted indices of z-scores. Controls include enumeration area characteristics, as public infrastructure and services, and individual socio-demographic characteristics, as gender, age, household size, marital status, years of schooling, ethnicity and religion dummies, occupation, assets, expenditure, and income. Corruption was measured only on the sample of 504 households that apply to public funds (e.g., the national program known "as 7 milhões"). Standard errors are clustered at the enumeration area (i.e., polling place) in every specification and reported in parentheses.

Table 3: Decomposition of the effect on trust

	Trust in:							
	Family	Neighbors	Local leaders	Local people	District government	Province government	Mozambicans	National leaders
Mining area	-0.089	-0.119*	-0.117*	-0.064	-0.068	-0.052	0.013	-0.109**
	(0.055)	(0.062)	(0.060)	(0.054)	(0.049)	(0.050)	(0.060)	(0.054)
Observations	1926	1929	1924	1903	1917	1915	1866	1916
R^2 adjusted	0.053	0.067	0.066	0.094	0.085	0.078	0.050	0.065

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. All regressions are OLS. Dependent variables are z-scores of trust in different groups (original scale 0 to 3). We control for 16 district dummies, enumeration area and individual socio-demographic characteristics. Standard errors are clustered at the enumeration area (i.e., polling place) and reported in parentheses.

Table 4: Decomposition of the effect on violence

	Verbal violence	Physical violence	Violence against women	Theft	Property destruction
Mining area	0.014	0.022**	0.007	0.014	0.007
	(0.012)	(0.010)	(0.011)	(0.022)	(0.012)
Observations	1926	1928	1924	1920	1926
R^2 adjusted	0.011	0.013	0.012	0.035	0.026

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. All regressions use a linear probability model. Dependent variables are binary variables for different types of violence. We control for 16 district dummies, enumeration area and individual socio-demographic characteristics. Standard errors are clustered at the enumeration area (i.e., polling place) and reported in parentheses. Results are robust to logit and probit regressions.

Table 5: Matching regressions

Panel A – Social outcomes	Generalized trust	Particularized trust	Group participation	Contribution (yes/no)	Contribution (amount of money or hours)
Nearest neighbor matching	-0.029	0.051	-0.072**	0.006	-0.170***
	(0.035)	(0.040)	(0.029)	(0.045)	(0.042)
Observations	1971	1971	1971	1971	1159
Caliper=0.005	-0.067*	-0.014	-0.060**	-0.047	-0.165***
	(0.036)	(0.041)	(0.029)	(0.045)	(0.044)
Observations	1956	1956	1956	1956	1140
Caliper=0.0005	-0.042	0.066	-0.081***	0.006	-0.149***
	(0.040)	(0.040)	(0.031)	(0.047)	(0.046)
Observations	1645	1645	1645	1645	800
Kernel matching	-0.050	0.036	-0.066**	-0.021	-0.141***
	(0.032)	(0.035)	(0.025)	(0.037)	(0.042)
Observations	1971	1971	1971	1971	1159

Panel B – Political outcomes	Democracy in the community decisions	Preference for democracy by the households	Corruption on public funds allocation	Violence involvement
Nearest neighbor matching	-0.093**	-0.175***	0.360***	0.036
	(0.047)	(0.039)	(0.103)	(0.032)
Observations	1935	1959	484	1971
Caliper=0.005	-0.140***	-0.154***	0.339***	0.014
	(0.049)	(0.042)	(0.111)	(0.031)
Observations	1920	1944	449	1956
Caliper=0.0005	-0.091*	-0.198***	0.245**	0.036
	(0.053)	(0.048)	(0.124)	(0.032)
Observations	1609	1627	188	1645
Kernel matching	-0.102**	-0.167***	0.282***	0.026
	(0.043)	(0.036)	(0.085)	(0.027)
Observations	1935	1959	484	1971

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. Regressions are nearest neighbor matching, radius matching (with replacement) and Epanechnikov kernel matching, where a logistic model is employed to predict each subject's propensity score. Dependent variables are equally weighted indices of z-scores. Coefficients reported are average treatment effects of the "mine" dummy. Standard errors, reported in parentheses, are adjusted following Abadie and Imbens (2011, 2012) for nearest neighbor and radius matching, whereas we use bootstrap for kernel matching (500 replications).

Table 6: Buffer analysis on violence

Buffer length	Mining area				Mining concession or license				Mineral exploration, prospection or recognition			
1 km	0.059*				0.139***				0.018			
	(0.033)				(0.050)				(0.034)			
2.5 km		0.044				0.082**				0.001		
		(0.031)				(0.040)				(0.032)		
5 km			0.047				0.048				0.026	
			(0.031)				(0.038)				(0.033)	
10 km				0.059*				0.057*				0.023
				(0.035)				(0.034)				(0.033)
Observations	1930	1930	1930	1930	1930	1930	1930	1930	1930	1930	1930	1930
R^2 adjusted	0.017	0.016	0.017	0.016	0.009	0.017	0.016	0.016	0.009	0.017	0.017	0.016

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes all households surveyed at baseline. All regressions are OLS. The dependent variable is a equally weighted z-score index of violence involvement, which includes threats or verbal violence, phyisical violence, violence against women, theft, property destruction or vandalism. We control for 16 district dummies, enumeration area and individual socio-demographic characteristics. Standard errors are clustered at the enumeration area (i.e., polling place) in every specification and reported in parentheses. Results are robust to using, as regressor of interest, a discrete variable of "mining intensity", i.e., the number of (buffered) mining areas per community.

Table 7: OLS regressions on restricted sample (10-km bordering areas)

Panel A – Social outcomes

	Generalized trust		Particularized trust		Group participation		Contribution (yes/no)		Contribution (amount of money or hours)	
Mining area	-0.010	-0.006	-0.116***	-0.102***	0.034	-0.002	-0.124**	-0.122**	-0.078	-0.072*
	(0.034)	(0.037)	(0.037)	(0.038)	(0.032)	(0.035)	(0.051)	(0.051)	(0.052)	(0.038)
Observations	1337	1239	1337	1239	1337	1239	1337	1239	771	738
R^2 adjusted	0.087	0.121	0.072	0.107	0.032	0.119	0.043	0.097	0.066	0.077
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Panel B – Political outcomes

	Democracy in the community decisions		Preference for democracy by the households		Corruption on public funds allocation		Violence involvement	
Mining area	-0.047	-0.002	0.011	0.020	0.228**	0.309***	0.041	0.048
	(0.049)	(0.051)	(0.038)	(0.042)	(0.094)	(0.094)	(0.035)	(0.039)
Observations	1316	1220	1326	1230	302	283	1337	1239
R^2 adjusted	0.139	0.127	0.129	0.143	0.064	0.087	0.011	0.022
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: household. Sample includes households surveyed at baseline, which live within or at maximum 10 km from a mining area. All regressions are OLS. Dependent variables are equally weighted indices of z-scores. Controls include enumeration area and individual socio-demographic characteristics. Standard errors are clustered at the enumeration area (i.e., polling place) in every specification and reported in parentheses. Results are robust to using 5 and 20 km as maximum distance from the mining area.

Table 8: OLS regressions on behavioral outcomes

	Participation				Contribution				Democracy in the decision formation			
Mining area	-0.194**	-0.228*			-0.119*	-0.134*			-0.050	-0.030		
	(0.089)	(0.123)			(0.069)	(0.071)			(0.058)	(0.065)		
Mining concession or license			0.014				-0.201				-0.311**	
			(0.188)				(0.261)				(0.153)	
Mineral exploration, prospection or recognition				-0.218*				-0.091				0.026
				(0.115)				(0.073)				(0.068)
Observations	203	203	203	203	203	203	203	203	203	203	203	203
R^2 adjusted	0.067	0.081	0.073	0.081	-0.018	-0.015	-0.024	-0.023	0.039	0.012	0.026	0.012
District dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Note: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: village. Sample includes all villages, which participated to the "contribution game". All regressions are OLS. Dependent variables are equally weighted indices of z-scores, constructed with behavioral measures. Controls include enumeration area characteristics. White-Huber heteroskedasticity robust standard errors in parentheses. Results are robust to allowing for spatial correlation through Conley (1999) standard errors.