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Does Information Break the Political Resource Curse? Experimental Evidence from Mozambique[†]

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Natural resources can have a negative impact on the economy through corruption and civil conflict. This paper tests whether information can counteract this political resource curse. We implement a large-scale field experiment following the dissemination of information about a substantial natural gas discovery in Mozambique. We measure outcomes related to the behavior of citizens and local leaders through georeferenced conflict data, behavioral activities, lab-in-the-field experiments, and surveys. We find that information targeting citizens and their involvement in public deliberations increases local mobilization and decreases violence. By contrast, when information reaches only local leaders, it increases elite capture and rent-seeking. (*JEL* C73, D72, D74, O13, O17, Q33, Q34)

Since Adam Smith's *Wealth of Nations*, economists have been wary of potential problems arising from the exploitation of natural resources. The *resource curse*, a term coined by Auty (1993), is well defined in the literature as a decrease in income following a resource boom (Caselli and Cunningham 2009), and observed empirically as a cross-country negative relationship between per-capita GDP growth and

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exports of natural resources (Sachs and Warner 1999).[1] Initial theories associate this phenomenon with shifts away from manufacturing and toward nontradable goods, i.e., the Dutch Disease (Corden and Neary 1982, Gelb 1988, Auty 1993). In the 1990s, African countries such as Nigeria, Angola, and Sierra Leone, rich in oil and diamonds, became prominent cases for the development of new theories involving corruption (Treisman 2000) and civil conflict (Collier and Hoeffler 2004; Ross 2004). These theories initially associated resource exploitation with movements toward rent-seeking in the economy, at the expense of more productive activities (Tornell and Lane 1999, Baland and Francois 2000, Torvik 2002). Attention then shifted to the relationship between the resource curse and the quality of institutions (Mehlum, Moene, and Torvik 2006), paving the way for theories centered around politicians' misbehavior when resource rents become available, i.e., the political resource curse (Robinson, Torvik, and Verdier 2006). This mechanism is closely connected to rapacity (Dube and Vargas 2013, Berman et al. 2017), which associates competition for centralized resource rents with conflict.

In the face of the political resource curse, evidence about policies that could counteract its negative effects remains scarce. Promoting local institutions that strengthen political accountability could help if the problem is politicians' misbehavior. Indeed, as windfalls reduce the relative importance of tax revenues, the link between government and citizens is weakened (Karl 1997, Ross 2001).[2] A first step to strengthen political accountability is to inform citizens. However, the literature has limited causal evidence linking this solution to politicians' behavior. Banerjee et al. (2018) is a recent exception: providing information about a redistribution program to beneficiaries led to substantially lower resource capture by local officials in Indonesia. In addition, in the context of community campaigning, the provision of information could not only impact accountability, but also prevent conflict by raising citizens' opportunity costs of joining conflict (Becker 1968, Grossman 1991), or by mobilizing communities against violence (Collier and Vicente 2014).

This paper extends the literature by focusing on the reactions of citizens and local politicians to the dissemination of information about a major resource discovery that will materialize as a future resource windfall.[3] We conducted a large-scale randomized field experiment in 206 communities of Northern Mozambique, after a massive discovery of 180 trillion cubic feet of natural gas in the Rovuma basin, Cabo Delgado province (IMF 2016).[4] Labeled the largest worldwide in many years, the

[1] Within-country evidence is more positive. For Peru, Aragón and Rud (2013) find evidence of a positive effect of a large gold mine on real income. For Mozambique, Toews and Vezina (2016) show positive impacts on job creation of resource-induced FDI.

[2] McGuirk (2013) provides evidence consistent with this claim for the African continent, and Paler (2013) for Indonesia. Following a similar design to Paler (2013), de la Cuesta et al. (2017) find no difference in the demand for accountability between priming on taxation or oil revenues for Ghana and Uganda. However, evidence on local accountability is limited as the literature has often focused on national-level leaders and institutions (Bhattacharyya and Hodler 2010; Andersen and Aslaksen 2013).

[3] It has been common in the literature to study the resource curse at the national level and after the exploitation began. However, in countries with high levels of corruption, central government's inefficiencies are often the result of widespread local capture (see, e.g., Reinikka and Svensson 2004). This justifies the importance of local political structures to understand the curse. In addition, standard economic theory suggests that shocks to expectations about the future exploitation translate to current behavioral change. In aggregate terms, Arezki, Ramey, and Sheng (2017) show that news about resource discoveries have effects on savings, investments, and employment.

[4] The new gas field in Mozambique takes the third place worldwide if one considers the ranking of largest gas fields compiled by Sandrea (2006). The Mozambican field would rank behind only the South Pars field in Iran/Qatar and the

discovery has the potential to transform the country into the third-largest exporter of liquefied natural gas (LNG) in the world (World Bank 2014, Frühauf 2014).[5] This provides a unique setting because the future exploitation of natural gas is expected to generate a substantial impact on the Mozambican economy, but also brings high risks of future resource and revenue mismanagement. Mozambique is a low-income country, ranking seventh from the bottom worldwide in terms of GDP per capita (World Bank 2017). Cabo Delgado province is primarily rural, with a total of 1.8 million inhabitants, and ranks lowest in human development among all the provinces of Mozambique (INE 2015, Global Data Lab 2016). At the same time, accountability and the current management of natural resources score weakly in international rankings (Freedom House 2017, NRGI 2017).

Facing limited media independence and penetration, as well as poor knowledge of the discovery in the province, a broad coalition of governmental and nongovernmental organizations sponsored the large-scale information campaign we follow. Its objective was to provide communities with important details about the discovery of natural gas in Cabo Delgado, the expected size of the future windfall, and the rights of local populations to benefit from its exploitation. To relate information with local political accountability, two interventions were implemented. In a first group of randomly-selected communities, only local political leaders received the information module, which in principle did not contribute to increased accountability.[6] Leaders can gain not only from information about the discovery, but also from the fact that the campaign recognizes their centrality in the community. In a second group, the information module was delivered to both local leaders and citizens, targeting communities at large while aiming to provide higher levels of accountability. In a third (control) group, no dissemination efforts were organized.

The design of the experiment and of the measurements was included in a pre-analysis plan registered on the American Economic Association RCT Registry (Armand et al. 2017). The experimental design incorporates a wide range of measurements, including georeferenced data about violent events, structured community activities (SCAs), lab-in-the-field experiments, baseline and endline surveys. Many measurements were compiled specifically to detect behavioral changes among both local leaders and citizens, consistent with previous theoretical work on the political resource curse. Some behavioral measurements were originally developed for this project, namely SCAs measuring favoritism and rent-seeking, and a rent-seeking game. Other behavioral measurements follow previous contributions, as in Casey, Glennerster, and Miguel (2012); Batista and Vicente (2011); and Collier and Vicente (2014).

Urengoy field in Russia, both discovered in the 1960/1970s.

[5] Major investment plans were approved by the Government in 2017 and 2018, and new projects are currently under approval. See, for instance, "Mozambique to Become a Gas Supplier to World," *Financial Times,* June 27, 2018. The epicenter of action is Palma in the north of the province, where a refinery and a port are expected to be built. Off-shore discoveries can have significant effects beyond the area of extraction when windfalls are distributed (see, for instance, Caselli and Michaels 2013). For Mozambique, this is expected to be the case (Melina and Xiong, 2013). For two recent articles about natural gas in Mozambique, see "Is Mozambique the Next Oil and Gas Hub?," *CNN*, May 3, 2017, or "Mozambique to Become a Gas Supplier to World," *Financial Times,* June 27, 2018.

[6] Leaders could opt to pass this information on to their citizens, potentially forming a cost-effective entry point to inform communities.

We find clear positive effects of community-level information dissemination, most notably, on decreasing violence. This effect is concurrent with a sudden rise in violent events attributed to extremist groups recruiting locally in the province of Cabo Delgado, beginning at the time this project completed operations in the field.[7] As compared to the control group, the probability of a violent event occurring decreased by 9 percentage points in communities where the full campaign was implemented. Consistently, we observe positive effects on awareness and knowledge about the natural gas discovery among citizens. In line with a higher opportunity cost of engaging in conflict, citizens also become more optimistic about the future benefits of the discovery. On the other hand, when only leaders receive information, no change occurs in awareness and knowledge among citizens, while leaders become more knowledgeable. In this case, elite capture and rent-seeking activities by leaders and citizens increase. Such adverse effects are not observed when the information campaign targets entire communities. We report instead an increase in citizens' mobilization, trust, voice, and demand for accountability. These are possible mediators for the reported effects on violence.

This paper contributes to the literature on the resource curse in two main ways. First, this study complements the growing empirical work documenting the political roots of the resource curse. The case of oil in Brazil has inspired a number of contributions. Caselli and Michaels (2013) analyze impacts of oil revenues on the structure of municipality-level income, showing evidence consistent with political pressures. Additional revenues increase corruption and result in less educated local politicians (Brollo et al. 2013). They are also favorable to the incumbents in elections (Ferraz and Monteiro 2014). In line with these results for Brazil, Vicente (2010) shows that, following an oil discovery in São Tomé and Príncipe, perceived corruption increases, especially vote-buying. Second, this paper demonstrates that an information campaign targeting communities at large can counteract the political resource curse, while avoiding elite capture and promoting community mobilization.[8] Moreover, it is capable of preventing conflict after a resource discovery, a real possibility in light of the evidence linking commodity price changes with violence (Fearon 2005, Dal Bó and Dal Bó 2011, Dube and Vargas 2013, Bazzi and Blattman 2014, Berman et al. 2017). To our knowledge, this paper is the first to evaluate a specific intervention to prevent conflict in a newly resource-rich setting. It builds on recent developments in understanding the effects of micro-interventions aimed at preventing violence by transforming institutions (Fearon, Humphreys, and Weinstein 2009), influencing opportunity costs (Blattman and Annan 2016, Armand, Atwell, and Gomes 2020), and improving noncognitive skills (Blattman, Jamison, and Sheridan 2017).

[7] Civilians were the main target. See, for instance, "A Bubbling Islamist Insurgency in Mozambique Could Grow Deadlier," *Economist,* August 9, 2018; "Mozambique's Own Version of Boko Haram Is Tightening Its Deadly Grip," *Independent,* June 20, 2018; "Shadowy Insurgents Threaten Mozambique Gas Bonanza," *Financial Times,* June 21, 2018.

[8] Large-scale civic-education campaigns have already proven to be effective in Mozambique in relation to political participation (Aker, Collier, and Vicente 2017). Closely related to the context of natural resources, Cappelen et al. (2018) show how conveying information through videos about a discovery in Tanzania increases citizens' expectations of corruption, while willingness to engage in corrupt behavior is not affected.

I. The Intervention and Its Setting

A wide coalition of international, national, and local institutions sponsored a large-scale information and deliberation campaign about the management of natural resources in the province of Cabo Delgado, focusing on the recent natural gas discoveries. The campaign was conducted at the community level between March and April in 2017.

The information module started by defining natural resources and the related legal rights of the population, including the presentation of various laws related to land, mines, forests, and fishing (see online Appendix Section A for further details). The campaign provided details about the discovery of natural gas in Cabo Delgado, including plans for exploration, and the expected consequences for local communities. Importantly, the module highlighted the expected size of the natural gas windfall, with the likely positive implications for provincial government revenues and job creation. The campaign cited specifically a report by the International Monetary Fund (IMF) mentioning that tax revenues from the project from the startup to 2045 could reach US$ 500 billion, more than 34 times the revenue generated by Mozambique the year before the campaign (IMF 2016). The campaign emphasized the rights of local communities to benefit from the exploitation of the natural gas, as established by Mozambican law and stated by the government in programmatic documents (INP/MIREME 2014). These included the rights to: be informed; have revenues invested locally; be compensated if directly affected; and be given priority in employment in the extractive sector. Note that there was substantial uncertainty about many of the topics covered. Importantly, the government had not yet made any decision on how to channel fiscal revenues to local authorities. Still, the cases of other resource-rich countries facing similar discoveries were part of the information package, which referred to both successes and failures. All sponsoring organizations involved in the project discussed and approved the final content of the information package, in order to guarantee widespread support and maintain neutrality.

The campaign included two major randomized variations at the community level. Treatment 1, labeled as *leader treatment*, had the information module delivered to the corresponding community leaders only. In Mozambique these individuals are the highest-ranked representatives of the government within each community and are well-defined figures. In rural areas these are known as village chiefs (*chefes de aldeia*), and in urban settlements as neighborhood chiefs (*secretários de bairro*). Communities select both types of leaders, whom the state then acknowledges, meaning that the state hierarchy has significant influence over community leader selection. They are paid a wage from the government and their competencies are mainly related to land allocation, enforcement of justice, rural development, and formal ceremonies. In addition, they must be consulted when natural resources are procured in the community, and aid or public programs are to be implemented (Buur and Kyed 2005, Nuvunga 2013).

In treatment 2, labeled as *community treatment*, the information dissemination was targeted not only to local leaders, but also to communities at large. Community meetings and door-to-door contact were implemented for this purpose in each community. Following Humphreys, Masters, and Sandbu (2006), within this treatment,

one-half of the communities were randomly selected and offered a deliberation module in addition to the information module.[9] Communities in the *control group* received neither information nor deliberation modules.

Due to the low level of literacy among study participants, information was mainly delivered verbally. First, trained facilitators provided an explanation of the information content in local languages. This happened individually to local leaders in treatments 1 and 2, and in community meetings for treatment 2. Online Appendix Section A shows the structure of these presentations. Second, treatment 2 included a live community-theater presentation with the intention of communicating the contents of the information package in an informal manner. For treatment 2, verbal presentations and community theaters also served as a mean to mobilize communities by gathering villagers in large numbers. In these communities, in 91 percent of sampled households, at least one member participated in the meetings. Finally, verbal communication was supplemented with the distribution of a pamphlet (online Appendix Figure A1). It was hand delivered to leaders in treatments 1 and 2, and additionally door-to-door to community members in treatment 2.

II. Sampling and Randomization

We selected a sample of 206 communities in the province of Cabo Delgado. These were randomly drawn from the list of all 454 polling locations in the sampling frame, stratified on urban, semi-urban, and rural areas. To randomly allocate polling stations to different interventions, blocks of four communities were built using Mahalanobis-distance while exploiting the richness of baseline information. Within each block, communities were randomly allocated with equal probability to either treatment 1, treatment 2 without the deliberation module, treatment 2 with the deliberation module, or a control group. This procedure resulted in 50, 51, 50, and 55 communities in each group, respectively. Figure 1 illustrates their geographical distribution.[10]

Sampling of citizens was the product of physical random walks during the baseline survey. In each house, heads of households were sampled for survey interviews and behavioral activities. A total of 2,065 heads of household were interviewed at the baseline, targeting 10 per community. Post-treatment attrition was handled through substitutions in the same household, when possible. Attrition is not significantly different across treatment groups (online Appendix Section B).

Online Appendix Tables B2–B3 provide a characterization of the demographic traits of the sample at baseline. Among household representatives, 25 percent are female, the average age is 45 years old, 30 percent have no formal education, and 57 percent are Muslim. Local leaders are almost all men (only 2 percent are female), are older and more educated than the average citizen, and have been in power for

[9] This component started with the formation of small citizen committees of around 10 people. Each group was invited to meet and deliberate on the priorities for local spending in relation to the future natural gas windfall. District administrators, the main political representative above the community but below the provincial level, received the results of the deliberation meetings.

[10] Disparities between groups are due to the efforts to reduce information spillovers across treatments. Rural communities located within 3 km of one another received the same treatment (see online Appendix Section B).

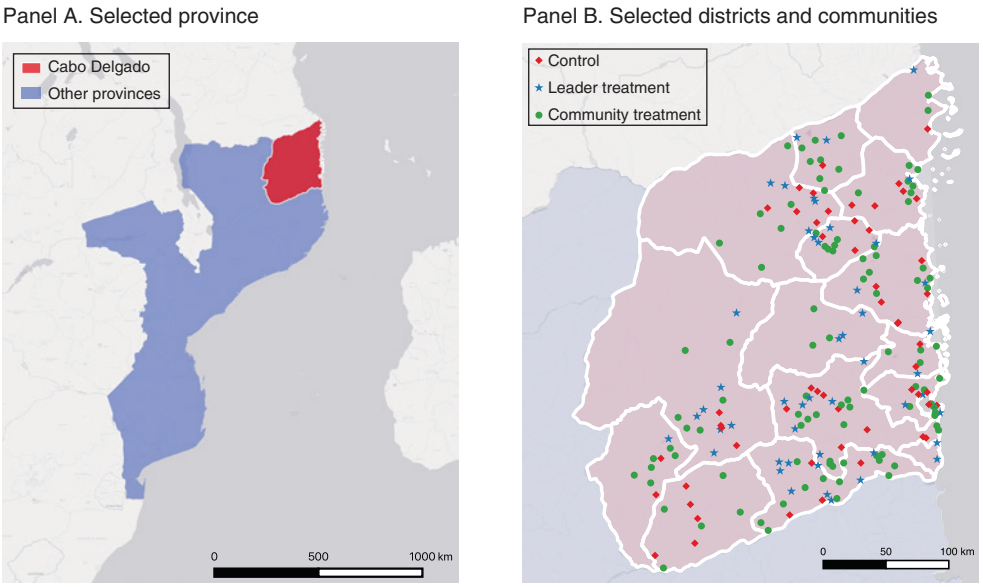

FIGURE 1. SELECTED COMMUNITIES AND ALLOCATION TO TREATMENT GROUPS

Notes: In panel A, Cabo Delgado province is highlighted in red. In panel B, georeferenced coordinates were obtained from tablets' GPS sensors used for interviews. The georeferenced coordinate of each location is determined using the average of all available data points within each location (household interviews, leader interviews, and community interviews). Basemap source: Esri (see online Appendix C for details and attributions).

8.8 years on average. Seven percent of the sample is located in urban areas, and 11 percent in semi-urban areas, which makes the large majority of the sample rural.

III. Measurement and Hypotheses

This paper collates a wide array of measurement instruments. These include administrative data about georeferenced violent events, behavioral data from the holding of SCAs and from lab-in-the-field experiments, and survey-based measurements. Data about violent events are available for the whole period under analysis. Baseline survey data were collected from August to September 2016. Some SCAs were initiated immediately after the treatment activities in March 2017. The endline survey, the completion of SCAs, and the lab-in-the-field experiments were implemented in the period of August to November 2017. Online Appendix C provides full details about the timeline of the activities, all measurements, and data sources.

To analyze the response to the interventions, we examine three sets of outcomes that characterize the behavior of citizens and leaders. The specific variables used in each set are presented in online Appendix Section D. The first set relates to *violence*. This is an important dimension as natural gas is a capital-intensive and easy-to-tax commodity. Its exploitation disproportionately increases state income, which is likely to increase conflict through a rapacity mechanism (Dube and Vargas 2013; Berman et al. 2017). This possibility is given additional plausibility in our context by the increase in violent events attributed to locally recruited extremists, which began in October 2017. Online Appendix Section E provides further details about these events.

The community treatment can reduce violence if, in response to the information and deliberation campaign, citizens feel more included in the process of managing the resources and become more optimistic about their future economic opportunities. This has the potential to increase citizens' opportunity cost of engaging in violence (Becker 1968, Grossman 1991), a prominent mechanism of conflict-prevention (Blattman and Miguel 2010). The community treatment can also increase community mobilization, thereby helping to avert violence (Collier and Vicente 2014). The effect of the leader treatment on violence is more ambiguous. Still, it could make leaders aware of the conflict risk associated with the future resource boom, thus allowing local actions to prevent violence. In fact, expectations about future resources can incentivize local leaders to strengthen control over the territory (Fearon and Laitin 2003, Snyder 2006, Ross 2012). To test these mechanisms we measure local variation in violence through international event-based datasets: the Armed Conflict Location and Event Data Project (ACLED, Raleigh et al. 2010) and the Global Database on Events, Location and Tone (GDELT, Leetaru and Schrodt 2013). Citizens' perceived violence is measured from surveys.

The second set of variables relates to *information and perceived benefits* regarding natural resources. The information campaign can increase awareness and knowledge of the natural gas discovery among individuals targeted by the treatment. This is the most direct potential impact of the interventions as treatment 1 provides information to leaders, and treatment 2 to communities at large. When leaders are the only recipients of the campaign, it is uncertain whether information flows to citizens. Similarly, it is uncertain whether better informed leaders and citizens update their expectations about the future impacts of the natural gas in a positive or a negative way. We study these effects using survey questions related to information and perceptions measured both at baseline and endline, and administered to both local leaders and citizens.

The third set relates to *political outcomes*, which record behaviors related to elite capture, rent-seeking, citizens' mobilization, and demand for accountability. Following Robinson, Torvik, and Verdier (2006), politicians become more focused on increasing their probability of remaining in power when faced with a resource boom or its anticipation (as in our context). This is because low levels of political accountability can enable leaders to gain privately from the exploitation of resources.

If local accountability remains low as in the leader treatment, when faced with information about natural resources local leaders may capture a higher share of present resources as a way to cement power. Capture can also increase if local leaders feel more empowered because they are singled out to receive information. In addition, as a means of influencing the future allocation of political power, leaders can increase their interactions with other political leaders and the intensity of rent-seeking.[11] We study elite capture by local leaders using behavioral measurements, including SCAs on resources intended for community use and on the appointment of a community task force, as well as leader behavior in a trust game. For rent-seeking by local

[11] Some theories of the resource curse emphasize its decentralized nature, anticipating generalized movements toward rent-seeking, see, e.g., Torvik (2002). While measurements used in this paper are able to distinguish decentralized from centralized theories, no generalized opportunities for rent-seeking are yet available as most structural changes to the economy are still to occur. Movements toward rent-seeking are then more likely closer to the political agents, making centralized theories most meaningful in this analysis.

leaders and citizens, we rely on survey questions assessing interaction with leaders, an auction eliciting willingness to engage in rent-seeking, and a novel rent-seeking lab-in-the-field game.

In the community treatment, political accountability is expected to be greater, as information is provided to communities at large, and inclusive deliberation methods are introduced. Robinson, Torvik, and Verdier (2006) predict a different pattern of behavior in this case. Leaders' capture and rent-seeking are not expected to increase as much as in a low-accountability setting. This is because an information campaign targeted at the whole community is expected to increase citizens' mobilization and demand for political accountability. We test this hypothesis by measuring mobilization through survey-based measures, an SCA involving a matching grant activity, including behavior associated with community meetings, and a public goods game. Demand for accountability is measured with survey questions, an SCA involving a postcard activity, and citizen behavior in a trust game.

IV. Results

Online Appendix Tables B2–B3 show mean differences at baseline between the control and treatment groups for a number of characteristics of households, leaders, and communities. Randomization was effective at identifying comparable groups in the experiment. We can therefore estimate treatment effects by restricting the sample to endline observations. For local leader or citizen i living in community j, the outcome variables are defined as Y_{ij}. The following specification is estimated using ordinary least squares (OLS):

$$(1) \qquad Y_{ij} = \alpha + \beta_1 T1_j + \beta_2 T2_j + \gamma \mathbf{Z}_j + \delta \mathbf{X}_{ij} + \epsilon_{ij},$$

where $T1_j$ and $T2_j$ are indicator variables for living in a community in the leader treatment or the community treatment, $\mathbf{Z}_j$ is a set of community control variables, and $\mathbf{X}_{ij}$ is a set of individual characteristics, for either leaders or citizens depending on the outcome at stake.[12] The variable ϵ_{ij} is an individual-specific error term, clustered at the community level to account for correlated errors within the community (when considering outcomes at the level of the citizen). Outcomes defined at the community level are treated analogously to leaders' outcomes. When baseline data

[12] *Community characteristics* include district and stratum indicator variables, an infrastructure index measuring the presence of public infrastructures, presence of natural resources, number of voters, and distance to the city of Palma. The infrastructure index averages 14 indicator variables for the presence of a kindergarten, a primary school, a lower secondary school, a high school, a health center, a facilitator, a water pump, a market, a police station, a religious building, an amusement area, a community room, as well as for the access to electricity and to the sewage system. The presence of natural resources is built by averaging 10 indicator variables for the presence of limestone, marble, sands and rocks, forest resources, ebony and exotic woods, gold, charcoal, graphite, precious and semi-precious stones, mercury, fishing resource, salt, and natural gas. When analyzing leader-level outcomes, district indicators are removed in order to avoid collinearity with stratum indicators. *Citizens' characteristics* include gender and age of the household head, household size, education, religion, and ethnic group indicators, and an indicator for whether the respondent was born in the community. *Leaders' characteristics* include the same variables measured at the leader level.

are available, we implement an ANCOVA specification, by including the baseline value of the dependent variable ($Y_{ij,t-1}$) as a control variable.[13]

Outcomes are selected in line with the pre-analysis plan (Armand et al. 2017), and grouped according to the sets presented in Section III. For all outcome variables studied in the paper (listed in online Appendix Section D), the goal is to test whether treatment 1 had an impact (H_0: $\beta_1 = 0$), treatment 2 had an impact (H_0: $\beta_2 = 0$), and the impact is different across the two treatments (H_0: $\beta_1 - \beta_2 = 0$). Because the set of outcomes is large, we address issues related to multiple inference. We first estimate, in the next section, impacts using equation (1) on indices that aggregate individual outcomes by category within each set of outcomes. This procedure gives the most comprehensive account of the results as it builds on the broadest set of outcome variables we have available. We then present results for individual outcomes while showing statistical significance for the relevant t-tests and for multiple hypothesis testing.

For the latter, *p*-values are adjusted considering step-down multiple testing following the Studentized *k*-StepM method for the two-sided setup (Romano and Wolf 2005, 2016). Further details of the procedure are presented in online Appendix Section D.2. For each group of outcome variables, the test is repeated separately for two sets of hypotheses. The first test considers each treatment effect and the difference across treatment effects separately. For instance, to test that treatment 1 had an impact for a given set of outcomes Y^k with $k = 1, \ldots, K$, a joint test that H_0: $\beta_1^1 = 0$, $\beta_1^1 = 0, \ldots, \beta_1^K = 0$ is performed. This set is indicated as the *row-level* one since hypotheses group coefficients in tables' rows. The second test follows a more conservative strategy and checks significance of both treatment effects and their difference across all outcomes presented in the table. This set is indicated as the *table-level* one. In the tables, *p*-values adjusted for multiple hypothesis testing are presented in squared brackets.

A. *Aggregated Outcomes*

In this section we aggregate outcome variables using indices of *z*-scores for each category of interest (Kling, Liebman, and Katz 2007). Following this procedure, individual outcomes are first normalized in standard deviations from the control group, and then averaged within each set. Indices and their specific components are described in online Appendix Tables D1–D4. Treatment impacts for aggregated outcomes are estimated using equation (1) and summarized in Figure 2.[14]

The community treatment significantly decreases the probability of observing a violent event in proximity to the community, by 0.17 standardized units. This effect is corroborated by a reduction in perceived violence among citizens. We do

[13] Autocorrelations are low for most (subjective) survey outcomes. The ANCOVA specification therefore maximizes statistical power (McKenzie 2012). Results are robust to the inclusion or the exclusion of $Y_{ij,t-1}$. In addition, online Appendix F.8 shows robustness to the selection of control variables or *p*-hacking (Simmons, Nelson, and Simonsohn 2011) using the Post-Double Selection LASSO procedure (Tibshirani 1996; Belloni, Chernozhukov, and Hansen 2014a, b). Difference-in-differences regressions are also estimated, with similar results: they are available upon request.

[14] The same procedure is followed to analyze the effect of adding a deliberation module to the information module in the community treatment. No clear differences are observed on our outcomes of interest (online Appendix Section F.1).

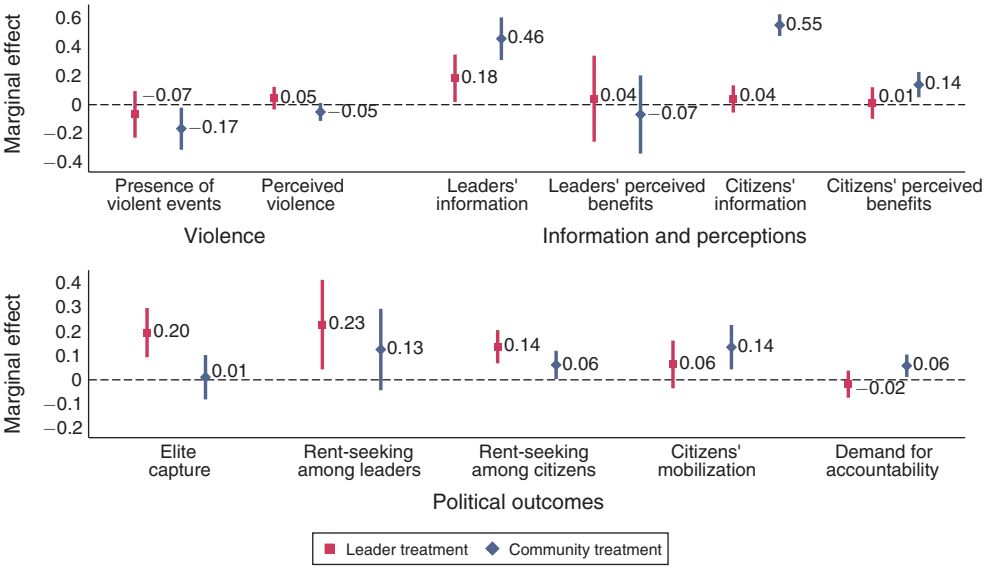

FIGURE 2. RESULTS BY AGGREGATION OF OUTCOMES

Notes: Estimates based on OLS regressions (equation (1)). The full set of point estimates and standard errors are presented in online Appendix Table D11. Confidence intervals are built using statistical significance at the 10 percent level, and standard errors clustered at the community level when employing citizen-level outcomes. The specifications include community and household-level controls (for citizen-level outcomes) or community-level controls (for leader- and community-level outcomes). The full list of controls is presented in Section IV. Outcomes are grouped in indices that are built using the Kling, Liebman, and Katz (2007) procedure. Outcomes are first normalized in standardized units to study mean effect sizes of the indices relative to the standard deviation of the control group and then averaged within each category. Indices and their specific components are described in online Appendix D.1.

not encounter significant effects of the leader treatment on these outcomes. As only the community treatment increased citizens' perceived benefits from the natural gas discovery, this pattern of results suggests that information dissemination targeted at communities diminished individuals' willingness to participate in violent activities by increasing the opportunity costs of fighting.

Turning to information, we show that the leader treatment was effective in raising knowledge about the natural gas discovery among the leaders. Their knowledge increases by 0.18 standardized units. We do not observe a rise in citizen's knowledge when the information is made available to the leader only. Turning to the community treatment, we find large impacts on both leaders and citizens. The effect on leaders (0.46 standardized units) is significantly greater than the one of treatment 1. The effect on citizens stands out with a magnitude of 0.55 standardized units.

Relating to political outcomes, treatment 1 significantly increases elite capture by 0.20 standardized units. This confirms that in settings in which leaders are privately informed about a resource boom and the levels of political accountability are low, the quality of local governance deteriorates. Moreover, not only this is accompanied by an increase in rent-seeking among leaders, but also citizens respond in such a manner. We do not find effects of the community treatment on elite capture or rent-seeking among leaders.

The effects of the community treatment on decreasing violence are possibly mediated by citizens' mobilization and demand for accountability. The community-level

information dissemination significantly increased citizen mobilization by 0.14 standardized units, and demand for accountability by 0.06 standardized units. Community mobilization and the demand for accountability are unaffected by treatment 1.

B. *Individual Outcomes*

The aggregate outcomes provide us with the main structure of results in our paper. This section presents detailed results on central outcomes of interest we selected out of the full set of outcome variables we have available. While standard errors and the corresponding significance levels refer to individual estimates, *p*-values adjusted for multiple hypothesis testing (reported in brackets) take into account the full set of variables reported in online Appendix Tables D1–D4. Results for the complete set of outcome variables are discussed in online Appendix Section D.2. We begin the analysis by focusing on outcomes related to violence. We then look at outcomes depicting information and perceived benefits. Subsequently, we report on political outcomes.

Violence.—Starting in October 2017, Cabo Delgado experienced a rise in violence attributed to extremist groups seeking to gain inroads in the province, recruiting locally, and targeting mainly civilians (Habibe, Forquilha, and Pereira 2019). Panel A of Figure 3 shows how violence evolved over time in the control communities, highlighting the sharp increase observed in the post-intervention period.

Table 1 presents the effects of the interventions on outcomes related to violence. Columns 1–3 show results employing georeferenced violent events from alternative data sources. Column 1 uses the ACLED database (Raleigh et al. 2010); column 2 uses the GDELT database (Leetaru and Schrodt 2013); and column 3 considers both sources (ACLED plus GDELT). Each dependent variable is an indicator variable, taking value 1 if a violent event was recorded within 5 km of the community.[15] The probability that a location in the control group witnessed at least one violent event ranges from 6 percent for ACLED to 9 percent for GDELT (13 percent for ACLED plus GDELT).

In the post-intervention period, we observe significant negative effects for the community treatment when considering all three measures of violence. The magnitude ranges from 5 to 9 percentage points, statistically significant at the 1 or 5 percent levels. The effect for ACLED plus GDELT passes multiple hypothesis testing at all levels. The differences between treatment effects are not statistically significant, even though the *p*-value for GDELT is below 0.15. Panel B of Figure 3 shows differences between the control group and each treatment group for ACLED plus GDELT, estimated separately for pre- and post-intervention periods. The timing of the effect is clearly specific to the post-intervention period. Before the completion of the information campaign we do not observe any statistically significant difference between the control group and the treatment groups. In the post-intervention period treatment 2 decreases not only the probability of observing a violent event, but also

[15] Online Appendix Section E.1 describes in detail the procedure followed to assign an event to a community. Results are robust to varying the distance from the community used to assign events.

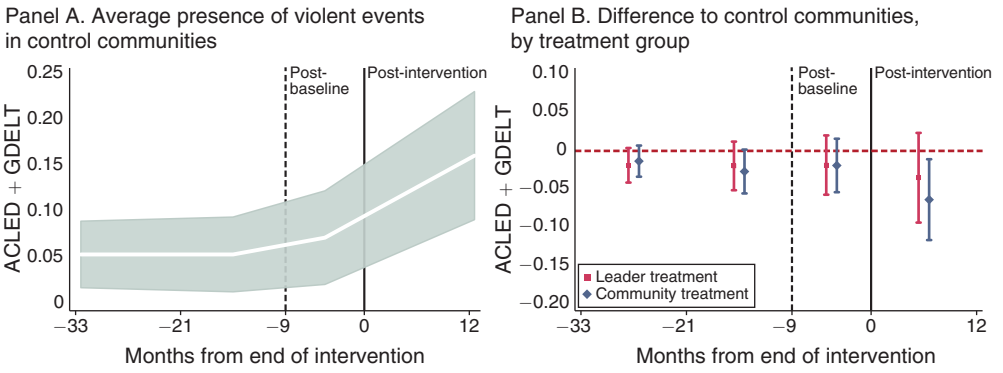

FIGURE 3. TREATMENT EFFECTS BY TIMING OF VIOLENT EVENTS

Notes: Panel A presents the average presence of violent events in the control group, by time from the end of the intervention. The shaded area represents the confidence interval for the mean at the 10 percent level. The presence of violent events is measured by ACLED + GDELT, an indicator variable equal to 1 if an event was recorded in ACLED (attacks against civilians) or GDELT (conventional and non-conventional violence) and occurred in the post-intervention period in proximity to the community, and zero otherwise. Additional details about the variable are presented in online Appendix D.1. For the same variable, panel B presents differences between treatment groups and the control group estimated using OLS regressions (equation (1)) separately for each period (the lower and upper bound of each period are indicated in the horizontal axis). Specifications include community and leader-level controls. The full list of controls is presented in Section IV. Confidence intervals are built using statistical significance at the 10 percent level. Time 0 corresponds to the end of the information campaign (end of April 2017). The *post-intervention* period is the first year following the end of the information campaign (May 2017–April 2018). The *post-baseline* period corresponds to the period between the beginning of the baseline data collection and the end of the information campaign (August 2016–April 2017).

the number of fatalities (online Appendix E.3). These results are not driven by displacement of violence to the control group (online Appendix E.4).

Columns 4 and 5 focus instead on perceived violence by citizens. Column 4 presents an indicator equal to 1 if the citizen believes violence is justified to defend a cause. In column 5 a binary outcome variable is defined as 1 if the respondent reports witnessing and being involved in the three months prior to the endline survey in any type of violence, including physical, against women, verbal, theft, and property destruction. In the control group 32 percent of respondents justify violence, and 19 percent were involved in violence. While interventions do not affect sympathy toward violence, the results based on violent events are supported by self-reported involvement in violence. The community treatment decreases involvement by 5 percentage points, statistically significant at the 5 percent level.

Information and Perceptions.—The most direct dimension targeted by the interventions is information about the discovery of natural gas in Cabo Delgado among local leaders and citizens. At baseline, awareness of the discovery is low among citizens, while a majority of leaders are aware. Nevertheless, even among individuals aware of the discovery, the level of knowledge about the details is very limited (online Appendix Section F.2).

Table 2 presents estimates of the treatment impacts on information and perceptions about the discovery. Panel A refers to leaders, while panel B refers to citizens. Column 1 focuses on a binary indicator of awareness about the natural gas discovery. Column 2 is dedicated to the respondent's knowledge about the natural gas discovery.

Table 1—Violence

	Presence of violent events			Perceived violence	
	ACLED (1)	GDELT (2)	ACLED + GDELT (3)	Sympathy for violence (4)	Involved in violence (5)
(T1) Leader treatment	−0.025	−0.017	−0.047	−0.002	−0.012
	(0.031)	(0.028)	(0.035)	(0.035)	(0.026)
	[0.61–0.61]	[0.61–0.61]	[0.31-0.40]	[0.95–0.95]	[0.87–0.87]
(T2) Community treatment	−0.057	−0.054	−0.085	−0.038	−0.052
	(0.028)	(0.026)	(0.032)	(0.031)	(0.021)
	[0.08–0.16]	[0.08–0.16]	[0.03–0.05]	[0.23–0.51]	[0.04–0.10]
Observations	206	206	206	1,522	1,827
R^2	0.275	0.733	0.656	0.043	0.060
Mean (control group)	0.055	0.091	0.127	0.323	0.187
T1 = T2 (p-value)	0.245	0.145	0.223	0.174	0.087
T1 = T2 (adjusted p-value, row-level)	0.226	0.200	0.226	0.188	0.188
T1 = T2 (adjusted p-value, table-level)	0.458	0.376	0.458	0.478	0.350
Lagged dependent variable	Yes	Yes	Yes	Yes	Yes

Notes: Estimates based on OLS regressions. All regressions present estimates using equation (1), including the lagged value of the dependent variable. Standard errors are reported in parentheses. In columns 4 and 5 standard errors are clustered at the community level. p-values adjusted for multiple hypothesis testing are presented in brackets (see Section IV for details). The first p-value corresponds to jointly testing coefficients grouped by rows (row-level), the second p-value corresponds to jointly testing that T1, T2, and T1 − T2 are different from zero (table-level). Testing is performed separately for columns 1 through 3 and columns 4 and 5. Dependent variables by column: (1) *ACLED*: indicator variable equal to 1 if an event was recorded in ACLED (attacks against civilians) and occurred in the post-intervention period in proximity to the community, and 0 otherwise; (2) *GDELT*: indicator variable equal to 1 if an event was recorded in GDELT (conventional and non-conventional violence) and occurred in the post-intervention period in proximity to the community, and 0 otherwise; (3) *ACLED + GDELT*: indicator variable equal to 1 if an event was recorded in ACLED (attacks against civilians) or GDELT (conventional and non-conventional violence) and occurred in the post-intervention period in proximity to the community, and 0 otherwise; (4) *Sympathy for violence*: indicator variable equal to 1 if the respondent believes violence is justified to defend a cause, and 0 otherwise; (5) *Involved in violence*: indicator variable equal to 1 if the respondent reports having witnessed and being involved in any type of violence (physical, against women, verbal, theft, and property destruction) in the 3 months prior to the interview, and 0 otherwise. Additional details about the dependent variables are presented in online Appendix D.1. Specifications in columns 1 through 3 include community and leader-level controls. Specifications in columns 4 and 5 include community and household-level controls. The full list of controls is presented in Section IV.

Specifically, we employ an index averaging 15 indicator variables related to knowledge about the location of the discovery, whether exploration has started, whether the government is receiving revenues, when extraction is expected to start, and which firms are involved. The index is equal to 1 if the respondent has full knowledge of these elements, and 0 if the respondent reports all answers wrongly or has never heard about the discovery. Online Appendix Section F.2 provides the details about the index and results per component. Columns 3 and 4 restrict attention to respondents who are aware of the natural gas discovery. These columns display the estimated treatment effects on perceived benefits from the natural gas discovery for the community or the household of the respondent (respectively). These are indicator variables equal to 1 if the respondent agrees or fully agrees that the discovery of natural gas will bring benefits for his community or his family, and 0 otherwise.

Among local leaders, awareness increased by 4 and 5 percentage points in both treatment groups. Knowledge about the discovery also increased significantly in both treatment groups (4–6 percentage points). The information campaign was

Table 2—Information and Perceptions about the Natural Gas Discovery

			Perceived benefit to the …	
	Awareness (1)	Knowledge (2)	community (3)	household (4)
Panel A. Leaders				
(T1) Leader treatment	0.043	0.038	0.016	0.014
	(0.019)	(0.018)	(0.065)	(0.079)
	[0.10–0.17]	[0.10–0.18]	[0.94–0.99]	[0.94–0.99]
(T2) Community treatment	0.052	0.056	−0.008	−0.042
	(0.018)	(0.016)	(0.059)	(0.072)
	[0.02–0.04]	[0.01–0.01]	[0.88–0.99]	[0.73–0.98]
Observations	203	203	204	204
R^2	0.146	0.273	0.154	0.125
Mean (control group)	0.964	0.627	0.868	0.830
T1 = T2 (p-value)	0.648	0.255	0.671	0.430
T1 = T2 (adjusted p-value, row-level)	0.781	0.515	0.781	0.669
T1 = T2 (adjusted p-value, table-level)	0.981	0.776	0.981	0.925
Lagged dependent variable	Yes	Yes	No	No
Panel B. Citizens				
(T1) Leader treatment	−0.003	−0.001	−0.009	0.015
	(0.033)	(0.020)	(0.031)	(0.031)
	[0.99–0.99]	[0.99–0.99]	[0.97–0.97]	[0.96–0.96]
(T2) Community treatment	0.251	0.169	0.044	0.071
	(0.023)	(0.015)	(0.023)	(0.026)
	[0.00–0.00]	[0.00–0.00]	[0.08–0.25]	[0.02–0.07]
Observations	1,886	1,886	1,592	1,573
R^2	0.272	0.396	0.135	0.114
Mean (control group)	0.671	0.449	0.779	0.692
T1 = T2 (p-value)	0.000	0.000	0.046	0.050
T1 = T2 (adjusted p-value, row-level)	0.001	0.001	0.098	0.098
T1 = T2 (adjusted p-value, table-level)	0.001	0.001	0.252	0.252
Lagged dependent variable	Yes	Yes	No	No

Notes: Estimates based on OLS regressions. Columns 1 and 2 present estimates using equation (1), including the lagged value of the dependent variable. Columns 3 and 4 present estimates using equation (1). Standard errors are reported in parentheses. In panel B standard errors are clustered at the community level. p-values adjusted for multiple hypothesis testing are presented in brackets and take into account the larger set of variables reported in online Appendix Table D2 (see Section IV for details of the procedure and online Appendix Tables D5–D6 for the results for the full set of outcome variable). The first p-value corresponds to jointly testing coefficients grouped by rows (row-level), the second p-value corresponds to jointly testing that T1, T2, and T1 − T2 are different from zero (table-level). Panel A refers to outcomes related to local leaders, while panel B refers to outcomes related to citizens. Dependent variables by column: (1) *Awareness*: indicator variable equal to 1 if the respondent heard about the natural gas discovery, and 0 otherwise; (2) *Knowledge*: constructed index that averages 15 indicator variables related to knowledge about the location of the discovery, whether exploration has started, whether the government is receiving revenues, when extraction is expected to start, and which firms are involved (online Appendix F.2 provides additional information about the construction of the index); (3) *Perceived benefit to the community*: indicator variable equal to 1 if the respondent agrees or fully agrees that the community will benefit from natural gas, and 0 otherwise; (4) *Perceived benefit to the household*: indicator variable equal to 1 if the respondent agrees or fully agrees that his/her household will benefit from natural gas, and 0 otherwise. Additional details about the dependent variables are presented in online Appendix D.1. In columns 3 and 4, the sample is restricted to respondents aware of the natural gas discovery. Specifications in Panel A include community- and leader-level controls. Specifications in panel B include community- and household-level controls. The full list of controls is presented in Section IV.

indeed effective among leaders, especially given the already high level of awareness among the local elite. When communities at large are targeted, we also observe an increase in the salience of the natural gas discovery for leaders (online Appendix Section D.2.2). While awareness and knowledge increased, no significant effect is observed on leaders' perceived benefits from the natural gas discovery.

Turning to citizens, the intervention generated a large increase in awareness of 25 percentage points when information was distributed to citizens. The community treatment increased not only citizens' awareness, but also their depth of knowledge: the knowledge index increased by 17 percentage points. No effects are observed when the information is distributed only to the leader instead, suggesting that leaders did not introduce any clear within-community effort to disseminate information. Differently from leaders, citizens become optimistic regarding the future benefits to their community and their household, but only in treatment 2. All significant coefficients or tests of differences between coefficients are strong enough to pass multiple hypothesis testing. The exception is the coefficient of treatment 2 for the perceived benefit to the community (significant only at the row level).[16]

Political Outcomes.—Results relating to political outcomes are reported in Table 3. We begin by focusing on the effect of the interventions on elite capture by local leaders. Column 1 is dedicated to an SCA that examined whether leaders appropriated funds that had been set aside to cover food items for the community members during their meetings. This is the funds-for-meetings SCA: see online Appendix C.2.3 for details. We employ as dependent variable the share of the full funds not spent in the meetings (i.e., the share appropriated). Substantial appropriation by leaders arises in this setting: in the control group, 47 percent appropriated funds, with an average share appropriated of 23 percent. We find that the leader treatment significantly increased appropriation, while no effect is seen for the community treatment. The difference between these treatments is statistically different, passing multiple hypothesis testing as well. The point estimate for treatment 1 is large in absolute value (14 percentage points), and significant at the 1 percent level. It passes multiple hypothesis testing at the row level.

We now devote attention to an outcome variable related to the SCA in which a task force was appointed by the leader (see online Appendix Section C.2.1). With this activity, we sought to measure propensity for favoritism or nepotism by leaders choosing individuals for specific tasks. In this case, the leader was asked to select five individuals to take a Raven's test (Raven 1936), a nonverbal test used in measuring abstract reasoning and regarded as a means of estimating intelligence, especially in settings of low literacy. Leaders were truthfully told that if all five individuals got at least one-half of the questions correct, they would earn a monetary prize for their community. Leaders were also instructed that the selected individuals would receive a smaller show-up bonus. Measurement thus centered on the test performance of the selected individuals. We also asked the sample of survey respondents in the community to conduct the same Raven's test. Column 2 in Table 3 reports impact estimates on the leader's preference for mid-performers in this activity. This is defined as an indicator variable constructed for the middle quintiles (second–fourth) in the distribution of the difference between the average score in the task force and the average score among representative citizens surveyed in the community. On average,

[16] The design of the experiment imposed a minimal distance between communities in different comparison groups in order to avoid information spillovers. Information diffusion beyond that minimal distance cannot be completely excluded. However, we do not find evidence of information spillover effects (online Appendix F.3).

TABLE 3—POLITICAL OUTCOMES

	Elite capture		Rent-seeking			Citizens' mobilization		Demand for accountability
	Appropriation	Preference for mid-performers	Interaction between leaders	Citizen-chiefs interaction	Share bid for meeting	Community meetings attendance	Matching grants contribution	Voice
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(T1) Leader treatment	0.144	0.193	0.162	0.092	0.027	0.004	0.152	0.025
	(0.053)	(0.097)	(0.053)	(0.035)	(0.013)	(0.022)	(0.191)	(0.053)
	[0.06–0.14]	[0.24–0.56]	[0.01–0.02]	[0.06–0.12]	[0.13–0.33]	[0.97–0.99]	[0.89–0.98]	[0.76–0.99]
(T2) Community treatment	0.005	0.122	0.114	0.022	0.004	0.039	0.478	0.123
	(0.048)	(0.087)	(0.048)	(0.029)	(0.011)	(0.016)	(0.180)	(0.044)
	[0.99–1.00]	[0.70–0.91]	[0.05–0.10]	[0.83–0.95]	[0.90–0.98]	[0.09–0.26]	[0.07–0.19]	[0.06–0.15]
Observations	205	206	203	1,890	1,922	1,803	1,510	1,718
R^2	0.235	0.145	0.212	0.101	0.022	0.086	0.065	0.068
Mean (control group)	0.227	0.491	0.818	0.531	0.498	0.892	0.892	2.463
T1 = T2 (p-value)	0.004	0.422	0.311	0.022	0.021	0.076	0.070	0.035
T1 = T2 (adjusted p-value, row-level)	0.036	0.928	0.620	0.085	0.085	0.365	0.365	0.175
T1 = T2 (adjusted p-value, table-level)	0.095	0.997	0.829	0.240	0.236	0.640	0.634	0.482
Lagged dependent variable	No	No	Yes	Yes	No	Yes	No	Yes

Notes: Estimates based on OLS regressions. Columns 1, 2, 5, and 7 present estimates using equation (1). Columns 3, 4, 6, and 8 present estimates using equation (1), including the lagged value of the dependent variable. Standard errors are reported in parentheses. In columns 4–8 standard errors are clustered at the community level. p-values adjusted for multiple hypothesis testing are presented in brackets and take into account the larger set of variables reported in online Appendix Tables D3–D4 (see Section IV for details of the procedure and online Appendix Tables D7–D10 for the results for the full set of outcome variable). The first p-value corresponds to jointly testing coefficients grouped by rows (row-level), the second p-value corresponds to jointly testing that T1, T2, and T1 − T2 are different from zero (table-level). Dependent variables by column: (1) *Appropriation*: share difference between available funds and expenses in the funds for meeting SCA (online Appendix C.2.3); (2) *Preference for mid-performers*: indicator variable equal to 1 if the community is in the second, third, or fourth quintiles of the sample distribution of the difference between the average Raven's score of individuals chosen by leader in the taskforce SCA (online Appendix C.2.1), and of representative individuals selected for the survey in the same community; (3) *Interaction between leaders*: indicator variable equal to 1 if the leader reports having talked to or called another political leader (chiefs in other communities, political representatives at the municipal, district, and provincial levels, as well as local party representatives) in the 6 months prior to the interview, and 0 otherwise; (4) *Citizen-chiefs interaction*: indicator variable equal to 1 if the respondent reports having talked to or called chiefs (formal community leader and their closest collaborators) in the 6 months prior to the interview, and 0 otherwise; (5) *Share bid for meeting*: share of total bids allocated by the citizen in the auctions SCA (online Appendix C.2.2) to attend the meeting with the district administrator; (6) *Community meetings attendance*: indicator variable equal to 1 if the respondent attended at least one community meeting in the 12 months prior to the interview, and 0 otherwise; (7) *Matching grants contribution*: amount (reported in logarithms) contributed by the respondent in the matching grants SCA (online Appendix C.2.4); (8) *Voice*: average level of (self-reported) voice with provincial and national authorities (1 = no voice/4 = full voice). Additional details about the dependent variables are presented in online Appendix D.1. Specifications in columns 1–3 include community- and leader-level controls. Specifications in columns 4–8 include community- and household-level controls. The full list of controls is presented in Section IV.

individuals in the household survey got 5 out of 10 correct answers, while those chosen by the leader performed more poorly on average, scoring 3.7. Treatment 1 increases the probability of selecting mid-level performers, by 19 percentage points. This effect does not pass multiple hypothesis testing.

Turning to rent-seeking, we begin with survey-based outcomes concerning interaction between leaders, and between citizens and leaders. This information was built by asking leaders and citizens to list community leaders, members of the district or provincial government, religious leaders, and other influential people whom they could personally contact if they wished, and their interaction with them in the six months prior to the interview. Using names and roles in the community,

unique individuals within and across communities are identified, building a network between citizens and local leaders (see online Appendix Section F.4 for further details). The focus is on *chiefs* (i.e., formal community leaders and close collaborators) and on *other political leaders* (i.e., chiefs in other communities, political representatives at the municipal, district, and provincial levels, as well as local party representatives).

Column 3 depicts interaction between leaders by determining whether local leaders talked to or called other political leaders in their network in the six months prior to the interview. Column 4 determines instead a similar measure for interactions between citizens and chiefs. Both treatments lead to clear increases in the interaction between leaders. Magnitudes are 16 percentage points for treatment 1 and 11 percentage points for treatment 2, statistically significant at the 1 and 5 percent levels, respectively. The effect of the leader treatment is also fully robust to multiple hypothesis testing. For leaders, other impacts on rent-seeking beyond the community and on their response to citizens' rent-seeking are limited (online Appendix Section D.2.3). For citizens, the leader treatment has a clear positive effect on the probability of interaction with chiefs in their own communities. The magnitude of the effect is 9 percentage points, statistically significant at the 1 percent level. This effect is statistically different from that of treatment 2. However, these findings are not fully robust to multiple hypothesis testing. Note that increased interaction with chiefs does not translate into better-informed citizens. The effect of treatment 1 is specific to interaction with the local leader and its closest collaborators, as interventions have no effect on citizens' interaction with other political leaders (online Appendix Section D.2.3).

We now address citizens' rent-seeking outside the community. This is measured using two auctions for activities illustrating the dichotomy between rent-seeking and entrepreneurship (see online Appendix Section C.2.2). The first activity is a meeting, inclusive of lunch and transportation costs, with the district administrator, i.e., the main politician at the district level. This activity provides an environment conducive to rent-seeking activities, and was available to both local leaders and community members. The second activity was instead related to entrepreneurship and provided a productive alternative to the rent-seeking activity. Each player received an endowment and was asked to bid for each activity in an incentive-compatible way. Column 5 presents impact estimates for the share of total bids allocated by the citizens to meeting the administrator. For treatment 1 we find a 3-percentage-point increase in this share, statistically significant at the 5 percent level, and statistically different from the effect of the community treatment. None of these differences are robust to multiple hypothesis testing.[17]

Columns 6–8 focus on outcomes related to citizens' mobilization and the demand for accountability. Column 6 presents estimates of treatment effects on citizens' attendance of community meetings. This is an indicator variable equal to 1 if the citizen reported having participated in at least one community meeting in the last

[17] The effects of treatment 1 on rent-seeking among citizens are more evident in communities with low mobilization capacity at baseline (online Appendix Section F.5). Heterogeneous effects are also explored in other dimensions identified in the pre-analysis plan, such as age, distance to Palma (the town where most of the gas extraction-related activities are taking place), and knowledge of local leaders. Citizens living closer to Palma are also more responsive to treatment 1 in terms of rent-seeking.

12 months. Attendance is generally very high, with 89 percent of control respondents having attended at least one meeting in the last year. Treatment 2 induces a significant increase in meetings participation: a 4 percentage point effect, statistically significant at the 5 percent level. This effect survives multiple hypothesis testing at the row level. The null that both treatment effects are equal is rejected, which is not robust to allowing for multiple hypotheses.

We now turn to the measurement of community mobilization to contribute for local public good provision. We employ for this purpose a matching grants SCA (online Appendix Section C.2.4). Similar to Casey et al. (2012), communities had the opportunity to raise funds toward a community objective. Funds were matched at a rate of 50 percent until a fixed maximum amount. Twenty-two percent of survey respondents reported having contributed, with an average contribution of 30 Meticais (US$0.5 as of mid-2017). Column 7 shows impacts on the individual contribution in the matching activity (reported in logarithms). This variable is self-reported and checked with the administrative information in the community logbooks used for the activity. The community treatment increases contributions by 48 percent, which is statistically significant at the 1 percent level, and robust to multiple hypothesis testing at the row level. The effect of treatment 2 is also statistically different from the effect of treatment 1, although this is not confirmed when testing for multiple hypotheses. Treatment 2 not only increases the intensive margin of contributing, but it also significantly increases the awareness of the activity among citizens (online Appendix Section D.2.4).

The final outcome of interest, in column 8, is survey-based and devoted to the level of voice that citizens have with provincial and national leaders. The scale ranges from 1 (no voice) to 4 (full voice). The community treatment increases voice by 5 percent of the subjective scale. The effect is significant at the 5 percent level and robust to multiple hypothesis testing at the row level. The difference between treatment 1 and treatment 2 is also significant, although it does not pass multiple hypothesis testing. Similar findings are also observed in other measures of demand for political accountability and on trust toward leaders (online Appendix Section D.2.4). Greater demand for political accountability could translate into higher turnover of community leaders. Since there are no formal elections or set mandate for these leaders in Mozambique, leader turnover is low (the average number of years in power in the sample is 8.8). No effects on turnover of leaders between baseline and endline are found (online Appendix Section F.6).

V. Concluding Remarks

The political resource curse has captured the attention of academics and policymakers alike and remains highly relevant for many low and middle-income countries with significant resource endowments. The main idea is that the resource curse originates on competition for centralized rents and related misgovernance by politicians. Related literature has often been purely theoretical, lacking strong causality claims, and/or silent about specific policy solutions. This paper experimentally tests the impact of an information campaign on countering the political resource curse. The context is the recent discovery of natural gas in Northern Mozambique, a low-income country with relatively weak institutions and a record of conflict.

When targeting communities at large, the campaign we follow led to a decrease in violence, namely as reported through news events in administrative datasets. In face of the known association of the resource curse with localized conflict in resource-producing areas, this is a very important finding for policymakers. It represents solid evidence that informing communities and encouraging inclusive participation in decision-making is an effective conflict-prevention tool. Given the detailed measurements we have in this experiment, we are able to establish that community campaigning was effective in raising information held by citizens about natural resources. In the process, citizens became more optimistic about the future, suggesting higher opportunity costs of engaging in conflict. In parallel we observe positive impacts on citizen mobilization and the demand for political accountability. These may have increased local collective action to protect against violence. When information is given to local leaders only, a likely default possibility in the longer run, we do not find this pattern of results. On the contrary, the observed increase in elite capture and rent-seeking is consistent with the political resource curse.

A final note is due about the implications for policy. In the context of the political resource curse, good policies are often limited to the adoption of general standards. Two examples are the Extractive Industries Transparency Initiative and the Natural Resource Charter. The adoption of these standards depends to a great extent on the goodwill of governments. Corrupt governments with access to resource revenues have fewer incentives to take these standards seriously. In this paper we show that a bottom-up intervention widely supported by civil society can be effective at countering the curse. Even self-interested governments are potential supporters, as improving community awareness, mobilization, and trust in institutions can be the pathway to avoiding conflict.

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