Decentralization and Elections in Burkina Faso

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Introduction

Over the past three decades, the decentralization of public services in developing countries has been a major trend, moving government functions from capital cities and major urban centers to rural areas. Providing these services closer to the site of use should allow for efficiency gains in local public goods provision, increasing the information about needs to the provider as well as decreasing transaction costs of accessing them. Furthermore, local governments should be more accountable and responsive to citizen concerns. At the same time, however, there has been a great deal of concern and some evidence that politics at a local scale could be more vulnerable to elite capture (Bardhan and Mookherjee, 2000). Ultimately, these important trade-offs lead to empirical questions that speak to the appropriate level of government service provision. Therefore, it is interesting to look at how local actors' political behavior changes in response to the experimental decentralization of land offices in Burkina Faso.

Local municipal councils were created in Burkina Faso in 2006 to provide services for local populations, although local decision-making was limited. Beginning in 2012, the legal framework governing land rights changed, and land administration, including the documentation and formalization of existing land rights, was moved to local governments. This decentralization made control of local governments more valuable: municipal council members could expect to exert some influence over the documentation process, and in particular, could help resolve conflicts stemming from overlapping rights and perhaps redistribute rights between claimants. This was particularly true in anticipation,

as it was believed control would be very local (in practice, there was substantial oversight during the pilot phase under study). Therefore, politically-engaged residents outside of the traditional political elite who heard about the creation of land offices in their municipality faced incentives to compete for local political office, even in historically uncompetitive elections. These incentives were stronger in areas that could see more change in land rights due to formalization, including pastoralist areas (with more overlapping claims to a given piece of land) and peri-urban areas (where documentation would allow outsiders to fulfill latent demand for land).

In this paper, I answer two main economic questions. First, do local political actors respond to a national-level policy of the decentralization of land offices? To answer this question, I use the experimental pilot phase of the decentralization funded by the Millennium Challenge Corporation, along with municipal election returns. This allows me to measure the causal impact of decentralization on the behavior of political parties and voters. Additional political parties are more likely to field candidates for office in a given municipality in response to the announcement that their municipality will receive a pilot-phase local land office. At the same time, voter turnout drops in these municipalities, an unexpected result for fully-informed voters facing a suddenly more-important local government choice. After implementation of local land offices, both political parties and voters again behave similarly to their counterparts in control municipalities, although voter perceptions of corruption at the local level have decreased. Secondly, how do we understand these observed responses? I also use other data sources, including Afrobarometer survey data, in order to explore the mechanisms at work. They are overall consistent with an understanding of local land offices as a potential source of rents for politically-informed actors. Furthermore, the results are strongest in pastoralist areas of the country, where the process of documenting land rights creates the largest change from the status quo, as traditional pastoral land rights are institutionally further from documented formal rights. This suggests that the process of documenting land rights is itself politically valuable and subject to elite control, going beyond traditional concerns with embezzlement of government resources.

The timing of the experimental pilot phase of decentralization in Burkina Faso has advantageous features for understanding political responses. I combine this experiment with three local-level elections that occurred at baseline, after treatment status was announced but before implementation, and after land offices had been created and had begun documenting rights. I therefore can look at differences in the changes over time between treatment and control localities in order to identify first the anticipation effects of the announcement and secondly the impacts of the implementation

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on political behavior.

Literature Review

This paper speaks to several strands of literature on the political economy of developing countries, bringing together themes of decentralization and political behavior, governance, and traditional institutions.

Essentially, the creation of land offices at the municipal level can be understood as a decentralization of a fundamental function of the state, guaranteeing property rights. There has been substantial interest in the effects of decentralization: voters should have better information about local politicians, so may be better able to hold them to account (Casey, 2015), but more local governments may suffer from capacity constraints that lower the quality of public services provided (Lago-Peñas et al., 2011; Ponce-Rodríguez et al., 2018). The impacts of these capacity constraints, in terms of bureaucratic experience, ability to tax, and bargaining power for resources from the central government, have been shown to be detrimental to nighttime light density in Burkina Faso (Billing, 2019).

Brollo et al. (2013) use data from Brazil to demonstrate another concern with governance in decentralized units: in a regression-discontinuity design, they show that larger transfers to local governments allow incumbents to be more corrupt without being punished, as imperfectly-informed voters still receive sufficient public goods. Furthermore, they show that lower-quality candidates run for office, which further reduces the costs of corruption for incumbents as the outside option for voters is less attractive. Their model distinguishes between expected and actual government transfers, though they are unable to separate these empirically as they only observe realized transfers; my unique setting does allow me to distinguish these two effects temporally.

There has additionally been some attention in political science to the decentralized election, although much of it relies on cross-country regressions which are difficult to interpret, as subnational structures differ widely. Blais et al. (2011), for example, find no effect of decentralization on national turnout when looking across countries, although they do see some within-country evidence of increased turnout in subnational elections as their relative importance increases. Gélineau and Remmer (2005) show that voters have trouble with attribution at the subnational level, often rewarding or punishing candidates according to national administrative performance.

This paper also fits within a sprawling literature in political economics on incentives faced by politicians, and in particular, strands looking at elite capture. Elite capture of political institutions is widely recognized in Burkina Faso: "the brokerage implies that the leader will 'eat' part of the money ... although the 'eating' of money is not condemned unless the leader fails to nourish his clients" (Hagberg, 2004). However, in the context of municipal elections in Burkina Faso, attribution to individual politicians is difficult given the institutional structure, so I use political parties as the unit of analysis. Casey et al. (2019) point out, though, that for individuals there can be returns to candidacy even for those who lose elections, "as candidacy opens up avenues to public sector employment."

Within political science, party entry has also been the subject of theoretical and empirical attention. The basic theoretical framework used by Margit Tavits and others considers the expected benefits of entering political competition (probability of winning and benefits of office) against the costs of entry (Tavits, 2006). My model considers these parameters explicitly, but observed differences between treatment and control municipalities are likely to be driven primarily by the expected benefits of holding office changing in response to new issues (land offices) emerge. Tavits points out that "we would expect that the *benefits of office become more significant in entry calculations if the probability of getting elected is small*" [italics in original] (Tavits, 2006). She further argues that new party entry is more important in emerging democracies such as Burkina Faso (Tavits, 2008). And importantly, "even if a new party poses no threat of replacing an established one, its presence will have an impact on electoral competition" (Tavits, 2006).

This paper also draws upon a well-established qualitative literature that is beginning to receive some acknowledgment among economists and policymakers, exploring the importance of customary institutions in democratic politics. Although traditional leaders are often considered as parallel to the state, drawing upon different sources of legitimacy, Adotey (2019) points out that in West Africa, they are often mutually dependent, and claims to both land and citizenship call on the same patterns of 'custom' and 'history', creating "intersecting tensions over eligibility for land access and political participation" (Berry, 2009). For example, in Burkina Faso, the "administration benefits from the informality of [pastoralists]. As long as they only have informal power, they need the support of the administration in order to be able to manage pastures effectively" (Benjaminsen and Ba, 2009). Although this is particularly true for pastoralists who have historically had less-formalized land rights, as managing herds requires flexible access to multiple locations throughout time rather than

exclusive use rights, the relationship between traditional [agricultural] chieftancies and the state also rests on informality. On the other hand, clientelism operating through traditional institutions pervades the bureaucracy, where "any position in the public services is assessed first and foremost according to the access to privileges that it provides" (De Sardan, 2008). Customary land rights in particular are always being negotiated, with actors claiming "the same resources under different logics" (such as state-sponsored documentation and 'traditional' rights) (Pernes, 2012). As we think about political contestation of land offices, then, it may not be corruption in a strict sense but rather protecting potentially valid claims. Although when considering mechanisms, I will devote some attention to this, in a reduced-form sense we would expect much the same effects from both drivers of contestation.

Quantitatively, Eifert et al. (2013) finds support for these mutually-constitutive formal and informal institutions: using Afrobarometer surveys from across the continent, they show that ethnicity is used as a way to mobilize people in (more competitive) election. They speculate that this could either be elites mobilizing ethnicity to win voters, or voters mobilizing their ethnic identities to claim patronage.

The current paper is in some ways very close to work by Lierl and Holmlund, who have a series of papers experimentally looking at different dimensions of municipality governance in Burkina Faso (Lierl and Holmlund, 2019, 2017; Lierl, 2015). In the first experiment, which occurred prior to the 2016 municipal elections, they used a lab-in-field setting to see if electing leaders could impact embezzlement, even absent incentives for reelection (Lierl and Holmlund, 2017; Lierl, 2015). They argue that this experiment shows that transparency of public funds is a necessary complement to elections for accountability. The second experiment took place in municipalities in which the national ruling party had been incumbent for almost 10 years (Lierl and Holmlund, 2019). Experimenters gave local citizens information on the incumbent's performance on service delivery targets in the treatment condition, as opposed to merely information on what the targets and indicators were. They find that the additional information had no significant effect on turnout nor the likelihood of voting for the incumbent (whether the information revealed was good or bad). Their results are consistent with ambiguity aversion, where voters prefer the candidates or parties they have the most information about, even if that information was bad. Another empirical paper with similar questions in the same setting is that by McMillan et al. (2011), who find that recent increases in rural population density in Burkina Faso (due to exogenous migration) improves local infrastructure,

public services, and the regulation of land rights ("more reliance on the rule of law to adjudicate disputes").

Despite the substantial and active literature across several disciplines that this paper draws upon, it makes several additional contributions. I am able to exploit an experimental context to provide causal evidence on political behavior; in particular, behavioral changes incentivized by decentralization. Furthermore, I am able to explore several mechanisms for the observed behavior, which shed light on how these incentives function in political calculations.

Context

Land Rights in Burkina Faso

Although these reforms can be understood as any other decentralization of public services, they are also a land reform, documenting and formalizing customary land rights. Understanding what customary land rights look like in Burkina Faso, then, can help us contextualize and understand the value of land offices. Qualitative scholars have pointed to the importance of host/stranger relationships in negotiating land rights in much of West Africa: first settlers in a region will have strong rights to use the land, but they are also responsible for allocating it to later migrants whom they 'host' in the region. The rights held by both hosts and long-settled migrants are often framed as ownership, and the extent of these rights is being continually negotiated. Claims to land are often based on continuity of cultivation, with fallow periods (a key investment in soil fertility) opening up competition for that land, particularly with increasing population pressure on the land (Kevane and Gray, 2001; Goldstein and Udry, 2008). Customary authorities are deeply involved in mediating these negotiations, and this role is recognized by the Burkinabe constitution. In recent years, these processes have at times spilled into open conflict, particularly in areas where pastoralists have negotiated access rights to water points or dry season forage near farms. Over time, there has also been a "tendency towards management of land rights by increasingly smaller units, such as households and individuals as opposed to extended families and lineages" (USAID, 2013).

Despite long-standing and locally-recognized customary rights to land, under Thomas Sankara's regime in 1984, an agrarian and land tenure reorganization law denied formal recognition of customary rights, instead vesting all land in the state. This means that landholders did not have any formal legal documentation of their claims, as a rule. This contributed to the finding by Prindex (2019)

that Burkina Faso was the most tenure-insecure country of 33 studied with 60% of tenure-insecure respondents citing government seizures as the source of their insecurity. In conjunction with MCC's Rural Land Governance project, however, the regime of Blaise Compaoré passed two laws pertaining to rural land rights. The first of these, Law 34/2009 "On Rural Land Tenure," was passed in 2009 and recognized customary rights as legitimate, created legal mechanisms for the formalization of these rights, and laid out plans for municipality-level *Services Fonciers Ruraux* (SFRs), which would be supported by MCC in the pilot phase. Then in 2012, Law 34/2012 "On Agrarian and Land Reform in Burkina Faso" described rural land certificates: these *Attestation de Possession Foncière Rurales* (APFRs) would fall between full titles and defined use rights, presenting the opportunity for further formalization into private land rights in the future. Although these documents provide important flexibility in a customary context, there was some ambiguity about how they should treat collective, lineage, customary, or pastoral lands, leading to substantial local variability. Both of these laws have been held up by some as examples for recognizing customary land rights while still allowing for their evolution and change. These reforms also set up Village Land Commissions (CVF) along with Village Development Councils (CVD) and Village Conflict Conciliation Commissions (CCFV), which would be involved in the most local levels of formalizing rights and mediating conflicts, in addition to the municipality-level Rural Land Tenure Services (SFRs) which would hold primary responsibility for documenting rights and would have a permanent staff. There were some difficulties at the start: "The National Municipal Association of Burkina Faso (AMBF) blames the slow implementation of new, decentralized land services on the lack of autonomy allowed to local governments to use funds transferred from the central government as they see fit, and on the reluctance of deconcentrated technical services to support local empowerment (Kaboré et al 2014)" (USAID, 2013). In particular, the final approval of APFRs initially required action by the central ministry responsible for lands, which delayed delivery of documents. Overall, however, the Burkinabe experience has been held up as an example for the continent in documenting customary land rights. MCC is conducting an impact evaluation which looks at the impacts of SFR offices and the documentation of land rights on tenure security and investment in land; I use their randomization of treatment locations but do not have the data to look at these (important, but distinct) outcomes and instead focus on the political incentives created.

Politics in Burkina Faso

The political context in which these reforms took place is important to understand, both to fully comprehend what specifically we are able to observe in the data as well as to reconcile some surprising findings that emerge from the particularities of the context. Burkina Faso was dominated by a single party, and a single president, for decades. Blaise Compaoré and the CDP took power in 1992 in a coup against the previous long-serving president, Thomas Sankara, and proceeded to overturn many of Sankara's leftist and third worldist policies. In 1993, the government passed the first decentralization laws, but it was not until 2004 that authority and finances were transferred to local governments, and many rural 'communes' (municipalities) were created to fill these governance roles. Each municipality would be governed by a council made up of two elected representatives from each village in the municipality, along with a mayor elected by the council. The first municipal elections were held in 2006, in which the CDP won 72% of council seats; allied political parties came in second while the opposition only won a few seats. Participation nationally was around 49%.

In November 2010, Compaoré was easily elected for another term as president. However, by the 2012 joint legislative and municipal elections, what was seen as a viable opposition party (the UPC) had emerged, which was mobilized by concerns that the CDP would amend the constitution to allow Compaoré to be reelected. Turnout was 76% nationally, "attributable to the perception that the newly established UPC would present a credible challenge to the CDP and the ADF-RDA at the polls, whereas a CDP victory was viewed as a certainty in the 2007 pre-election period" (Pryce and Nascimento, 2014). Nevertheless, the CDP won 70 of 127 legislative seats. The opposition's worries turned out to be well-founded, as in October 2014, Compaoré did try to amend the constitution to extend his rule, which prompted a popular uprising. The political upheaval lasted for 18 months, although in November 2014 a transitional government (backed by the military) was installed until elections could be held. The transitional government suspended municipal councils and sent 'special delegations' to fill administrative roles until new elections could be held (Lierl, 2015), although local bureaucracies, including SFRs, remained in place. November 2015 saw presidential and legislative elections, which barred allies of Compaoré from running; turnout was around 60% nationally and former Prime Minister Roch Marc Christian Kaboré was elected president. The transitional period was finally brought to a close with municipal elections in May 2016.

There are a few features of the broader political environment which are also worth noting. First,

in order to contest elections at a municipal level, candidates must belong to a political party, and ballots list parties rather than individual politicians. However, these party affiliations are unstable: a "leader builds up power and popularity through a network of alliances and relationships rather than through a program or an ideology; this is why party affiliation can change overnight" (Hagberg et al., 2018), and party alliances are determined in each locality and may not reflect national alliances between parties. There is a constitutional ban on ethnic affiliations for political parties, although some have noted that at a local level, ethnic divisions or tensions often play a role in understandings of parties.

The municipal councils which are the focus of this analysis are also worth highlighting briefly. Decentralization efforts were ongoing nationally, giving municipal governments at least partial responsibility for primary schools, health centers, water point maintenance, and administrative services such as civil registries (Lierl and Holmlund, 2019). However, this was primarily a deconcentration of functionality, rather than a delegation of decision-making power to local levels; staff and decisions were sent from the central level to merely implement locally. Additionally, the experimental setup of the RLG pilot phase should guarantee that the transfer of these other responsibilities was happening orthogonal to treatment status, and thus not drive the observed results. Ideally, I will be able to control for other kinds of transfers from the central government using budget or other administrative data.

MCC Rural Land Governance Project & Impact Evaluation

Finally, it is important to have a clear sense of the 'treatment' under consideration. The Millennium Challenge Corporation (MCC) signed a 5-year, $480.9 million, compact with the government of Burkina Faso in 2009. One component of this compact was a Rural Land Governance Project, which would proceed in 2 phases and represented a $58 million investment. By improving land tenure security and land management, the project aimed to increase investment in land and rural productivity.

During the first phase of the project (2009-2012), MCC supported the government in drafting two land laws, described above, as well as piloted land offices in 17 municipalities. These 17 locations were chosen as priorities (although the exact criteria are unclear), and are not balanced at baseline when compared with their phase I comparison municipalities nor with the rest of the country. This period also saw national-level legal changes, so the first phase is less useful in causal identification

of impacts.

In mid-2012, however, plans were made for the second pilot phase of the project, when an additional 30 municipalities would be brought in. These locations were chosen in matched pairs, and then one would receive the land office (SFR) during the pilot phase, in order to conduct a rigorous impact evaluation (ongoing, focusing on impacts on tenure security and investment at a micro-level). All documentation I can find implies that the selection of which municipality in a pair was chosen was either random or as good as random, as both were selected using the same criteria including land conflicts or uncertainty. However, I cannot rule out that there was some selection of treatment areas to politically benefit the incumbent party, as studied in Ghana by Briggs (2012). However, unlike in his study, I find no strong baseline differences between treatment and control areas (nor between study areas and those outside the study, for that matter) on political outcomes, which he does. The timing of the announcement of treatment locations, however, almost certainly was politically expedient: prior to 2012's municipal elections. It is difficult to pinpoint the precise public announcement of Phase II treatment locations, but the baseline evaluation report submitted in August 2012 lists them, and they are highlighted on a public map dated November 2012 (figure 2). Therefore, we can consider that in the 2012 elections, local elites in treatment municipalities at least had been made aware that they would in the future receive local land offices, and any responses are due to the anticipation effects of this announcement. I have been unable to locate local news announcements of these coming land offices prior to the 2012 election, however, so it seems unlikely that a majority of voters was fully aware. Therefore, I would interpret responses by political elites (including parties) as stemming from the announcement, but responses by voters (including turnout and vote choices) as being proximally caused by the behavior of political elites.

After the 2012 municipal elections, the second implementation phase of treatment began. In 2013, trainings were held in treatment municipalities, rural land offices (SFRs) were created, and two agents were hired to staff them: a mapmaker (skilled, often recruited from the city) and a communication agent (recruited locally, and generally suggested by the mayor or council members in practice). Several people involved in the process noted that the mapmakers often abandoned what was seen as boring, low-paid work in rural areas, so communication agents were trained to take over mapmaking responsibilities; additionally, although all positions should have been filled competitively, local politicians had significant influence in their selection. Despite plans for SFRs to function as part of the municipality administration, several experts expressed concern in interviews that the external

funding of the pilot phase created a distinction between SFRs and other municipality functionaries, as well as that the ability for SFR offices to collect fees to support themselves (with the potential to keep 'extra' revenue) undermined "the logic of public administration" (personal interviews).

In addition to municipal-level administration, village-level land institutions were recruited in treatment areas during the pilot phase. These village-level institutions began by creating participatory land use maps, which brought the community together to demarcate overlapping rights and claims to land. Then, land rightsholders were able to request documentation of their rights, which would be reviewed by the SFRs before documents were prepared from 2013-2014. During this period, over 60,000 stakeholders were trained on conflict resolution and land management. In MCC's report as they closed out the compact in July 2014, they noted that 47 communal land cover maps had been created (in 17 phase I and 30 phase II municipalities), 78 land administration offices had been established or upgraded, and 47 municipal buildings (holding SFRs) had been created (Millenium Challenge Corporation, 2014). These buildings were purposely located near other administrative offices, to facilitate a 'one-stop shop' for all necessary documentation. Although 13,447 applications for APFRs had been received by mid-2014, only 2,167 had been approved by local governments and only 403 documents had actually been distributed. MCC's own impact evaluation focuses on perceptions of tenure security, the frequency and types of land conflict, an investment decisions. A midline review that focused on Phase I municipalities found a decrease in some perceptions of tenure insecurity (such as concerns over the arrival of newcomers or return of previous villagers, although not on concerns about inheritance disputes or issues between pastoralists and farmers), but no statistically significant change in conflicts or agricultural productivity; this is expected from shorter-term data only a few years after the intervention, as these variables take longer to change (Millenium Challenge Corporation, 2014). MCC was very involved with the process throughout the pilot phase, and have emphasized their safeguards against corruption and elite capture in discussions of the project. Further implementation, particularly the rollout to additional municipalities, was largely put on hold during the political unrest from 2015-2016. Fortuitously for this research, that means that control municipalities have not yet received any treatment by the 2016 municipal elections. A full timeline of MCC's project, as well as major political events, can be seen in figure 1.

7 2012

"Agrarian and Land Reform" passed

10 2009

"Rural Land Tenure" passed

8 2012

Phase II locations announced

9 2009

1 2013

RLG project launches

Offices established

MCC Compact

Documentation

2006 2007 2008 2009 2010 2011 2012 2013 2014 2015 2016 2017

Popular uprising

11 2014

Compaoŕe steps down

11 2010

11 2015

Compaoré relected president

Pres. and legis elections, Kabore elected

4 2006

12 2012

5 2016

Municipal elections

Muni. and legis. elections
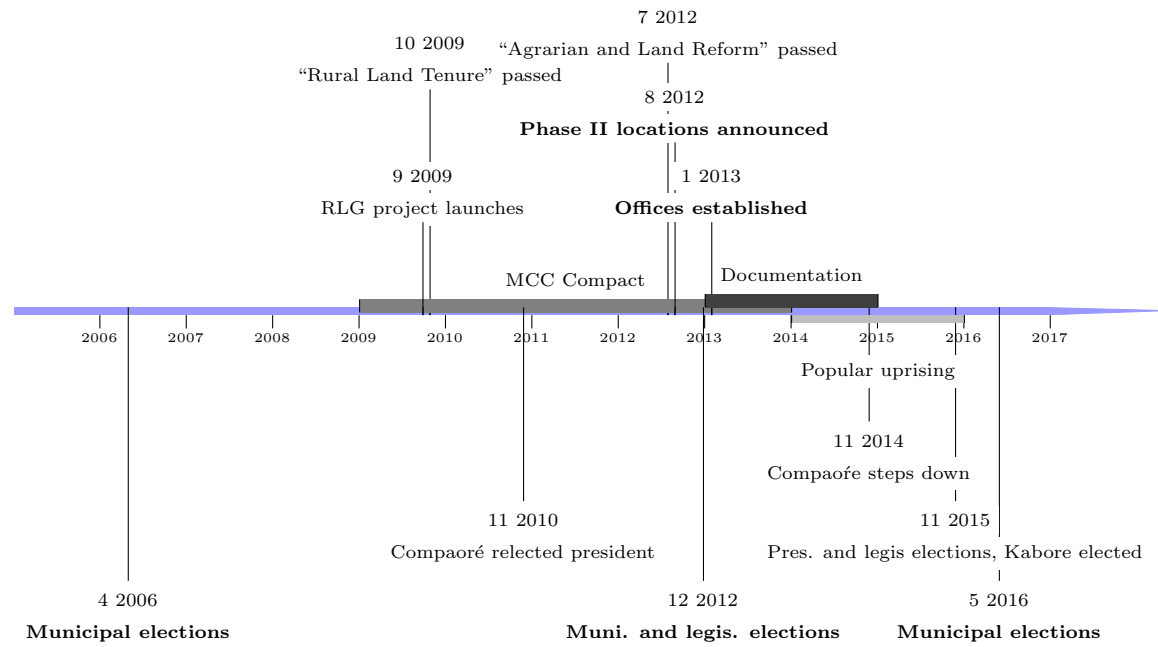
Municipal elections

Figure 1: Politics, Land Rights, and Impact Evaluation Timeline in Burkina Faso. Observed data and treatments in bold.
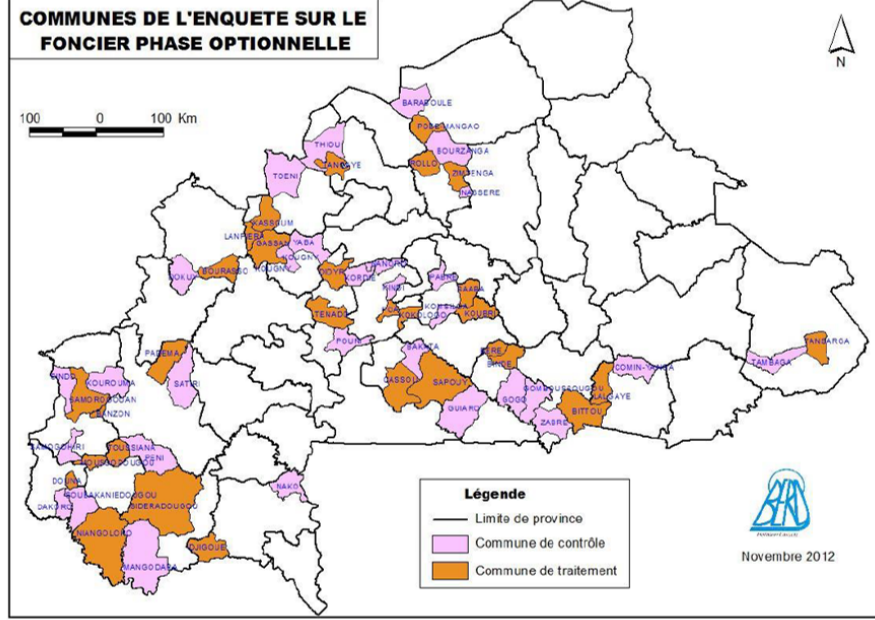
Figure 2: Map of Rural Land Governance Project Impact Evaluation Municipalities

Model

Start from a traditional model of party competition (I draw from ? and Bardhan and Mookherjee (2000), who draw from a ?-style model which is relatively common). However, I add two features to this style of model: first, I allow for party entry rather than assuming 2 parties (modeling party entry with a standard model as in work by Tavits (2006), and secondly, allowing for up to 3 potential parties. There are other minor modifications which I will discuss as they emerge.

Setup

Consider a stylized village which has three potential classes of people: farmers (f), pastoralists (p), and a third group, which I will call Mossi (m), in proportions of the vilage population α_c. These Mossi are generally more politically-connected to the central government, and often act as local power-brokers. Each class can choose to create a political party and contest local elections, although this is costly (with lower costs for Mossi than the other groups, due to their political connections). Parties that win election set policies π, and receive (constant) class-specific rents E_g.

Parties have intrinsic preferences over the interests of the population, weighted by class (that is,

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politicians of class g weight the population's utility according to $\sum_c \alpha_c w_c^g U_c(\pi))$.

Disputes (d) occur between pastoralists and farmers, such as when herds damage crops in a farmer's fields. When a dispute occurs, each party to the dispute pays elected officials to resolve it, with the payment determined by their own class and the elected class. I assume that disputants do not pay their own party if they are in office, but that both farmers and pastoralists pay less to Mossi officeholders (who serve as a neutral third party) than they would to their opponents in the dispute. All members of a given class pay this dispute-resolution fee, but only elected leaders receive this as a rent.

Therefore, a party g will choose to contest an election against party h if:

$$\psi_g \left[E_g + d \sum_c \alpha_c e_{cg} + \sum_c \alpha_c w_c^g U_c(\pi_g) \right] + (1 - \psi_g) \left[\sum_c \alpha_c w_c^g U_c(\pi_h) \right] - C_g \geq \sum_c \alpha_c w_c^g U_c(\pi_h)$$

Which simplifies to:

$$\psi_g \left[E_g + d \sum_c \alpha_c e_{cg} + \sum_c \alpha_c w_c^g (U_c(\pi_g) - U_c(\pi_h)) \right] - C_g \geq 0$$

Where ψ_g is the probability of party g winning the election, as in a standard Tavits-style model of party entry.

This probability of winning is, in turn, an increasing, continuously-differentiable, function of the vote share won by the party. Politically-informed voters base their choice on their expected utilities if governed by each class, plus an additive (normal random) valence towards each class' potential party. That is, each voter of class c has a valence towards party g. I normalize the valence towards a voter's own party as zero, so each valence is the relative (dis)preference for other parties. Therefore, voters of class c vote for party a over party b if $U_c(\pi_a) - de_{ca} + v_{ca} \geq U_c(\pi_b) - de_{cb} + v_{cb}$, where π_g is the policy choice of party g.

Pre-reform solutions

As a benchmark, consider the political equilibria before the announcement of the land rights documentation decentralization. Local governments have relatively little autonomy, so we can ignore the policy choices π_g to focus on the effects of dispute-resolution payments.

Additionally, I assume that the relative costs and benefits of holding office for the Mossi are such that for any non-zero probability of winning, they will always contest when they are present in a village (and therefore we can take their participation as given when solving the extensive-margin decision of other classes).

In villages without both farmers and pastoralists, there are no disputes for local elected officials to resolve. In the absence of policy choices and dispute-resolution payments, non-Mossi parties g contest the election if $\psi_g[E_g] - C_g \geq 0$. ψ_g, the probability of group g winning the election, is determined by voters choosing entirely on their (randomly-distributed) valences towards each party, and the share of voters of each class in the village. The comparative statics are relatively simple: non-Mossi groups are more likely to contest if the constant rents of holding office are higher, if the costs of contesting are lower. Additionally, as the share of non-Mossi in the village increases or their average bias against the Mossi increases, or the average bias of Mossi voters against their opponent weakens, the non-Mossi are more likely to field a party.

If instead we consider a village with farmers and pastoralists but without a Mossi presence, there are likely to be disputes that emerge between the two groups. When these occur, the class that is not in power must make payments to elected officials. Voters consider these payments in their decision, and so are unlikely to vote for a party not of their own class. Therefore, vote shares are in large part determined by the relative populations of the two groups.

Finally, we consider villages with all three classes present. Once again, assume that the Mossi will always contest the election. Because both farmers and pastoralists are present, however, there are disputes that emerge and will be resolved by whichever party is in power. Farmers and pastoralists will pay the elected officials for this dispute-resolution role, according to their respective classes. In particular, farmers would make the following payments to each elected class g: $0 = e_{ff} < e_{fm} < e_{fp}$ and pastoralists $0 = e_{pp} < e_{pm} < e_{pf}$. Intuitively, if your own class is in power, they do not charge you for resolving disputes, but your opponent in the dispute in power would charge more than the Mossi (who can extract lower rents from both parties).

This means that in general, most farmers will prefer to vote for Mossi parties over pastoralist ones, and likewise pastoralists will prefer Mossi parties over farmers. In a range of cases, then, only the Mossi will contest the election, as both farmer and pastoralist parties are happy enough with the lower fees paid to the Mossi for dispute-resolution to not make incurring the costs of contesting worthwhile. This is consistent with the empirical observation of largely uncompetitive municipal

elections in 2006, prior to the reform.

Reform announcement solutions

Once a municipality learns that they will receive SFR offices after the 2012 elections, however, the political calculus changes. Potential political parties anticipate that this reform can function in two ways. First, when implemented, the reform will clarify rights over land, eliminating (in the extreme) the need for local elected authorities to play a role in mediating disputes.Secondly, in the process of demarcating and clarifying land rights, there is some flexibility which local government agents anticipate they can use to reallocate rights between farmer and pastoralist claimants. That is, the elected official anticipates they will have some latitude in appointing bureaucrats to carry out the formalization process who will favor a given group. This is parameterized in the model by $\pi \in [0, 1]$, with $\pi = .5$ indicating the formalization process documents existing rights 'fairly', without any reallocation (and lower values of π reallocating rights towards pastoralists/away from farmers, higher values towards farmers). Therefore $U_f(\pi)$ is increasing in π and $U_p(\pi)$ is decreasing in π.

In the first two village cases, which have the Mossi and either farmers or pastoralists (but not both), there are no disputes to resolve nor inter-class reallocation to occur. Therefore, there will be no change in the equilibrium party entry nor vote outcomes.

When we consider villages with both farmers and pastoralists, but without Mossi, the land reform becomes important. By carrying out the land reform, the potential for disputes over ambiguous land rights will decrease (perhaps to zero), reducing the rents to holding office. However, the distribution of land rights in the reforme between farmers and pastoralists (π) becomes important. Voters will now choose the party that (net of valence) gives them higher utility, which is in most cases the one that favors them in the redistribution of land rights (that is, farmers favor the party with the larger π, and pastoralists the opposite).

Parties, in choosing whether to contest, evaluate the difference in the class-weighted sum of voters' utilities under the two policies (each taking the other party's policy choice as given) against the cost of running. Intuitively, this means that if the difference in policies between the two parties makes one's own class enough worse off, the party will contest the election (and the share of each class in the population will largely decide the election).

Most interestingly, we now turn to villages with all three classes. In addition the the Mossi always contesting, I assume they are forward-looking in their policy choice π_m, anticipating how farmers and

pastoralists will respond in deciding whether to contest the election and on their policy positions. A relatively unrestrictive assumption that aids clarity is to benchmark the Mossi as preferring $\pi = .5$: a totally fair demarcation and formalization of existing rights, without any reallocation.

As a thought experiment, imagine that the pastoralists were unable to form a political party to contest the elections. Then we would only consider the farmers' decision to contest. The Mossi, however, could anticipate this, and by setting $\pi_m > .5$, could make the policy amenable enough to farmers to prevent them from contesting the election. Note, however, that this strategic choice by the Mossi makes π_m less favorable to pastoralists, who would then have a greater incentive to run in opposition. This 'pulls' the optimal π_m back towards a fair distribution. However, in many cases, even with $\pi_m = .5$, farmers and pastoralist parties will still contest the election, to prioritize their own class in the redistribution of land rights (even if they have relatively little chance of winning office). The utility difference for their own class among alternate policies is large enough that even with a small probability of winning, the expected benefits are large enough to justify the costs of running a campaign.

This leads to a fundamental result of the model: that after the announcement of the land rights reform, additional parties, even uncompetitive ones, will contest elections in jurisdictions where the coming reform could entail a redistribution of land rights between natural political classes. Importantly, this model predicts the exact heterogeneity that I find empirically, with additional parties contesting in areas with greater farmer-pastoralist diversity.

Data

In this paper, I use several data sources, matching them at the municipality-level with MCC's pilot-phase treatment status.

CENI Electoral Returns

There have been three municipal elections since decentralization reforms created municipalities as an administrative unit with a democratically-elected council. These occured in 2006, 2012, & 2016. The Commission Electorale Nationale Indépendante (CENI; Independent National Electoral Commission) publicly reports certified results of all elections, including these municipal elections. These electoral returns specify, at the municipality-level, the number of registered voters, the number of

Year	Treatment	Control
2008	5 Municipalities	2 Municipalities
	80 Respondents	40 respondents
2012	11 Municipalities	10 Municipalities
	88 Respondents	80 Respondents
2015	7 Municipalities	4 Municipalities
	96 Respondents	64 Respondents

Table 1: Distribution of Afrobarometer survey respondents in pilot-phase municipalities

votes cast, as well as the performance of each party contesting the election (number of votes and seats won). They do not include the party affiliation of the mayor indirectly elected by the council, nor any information on candidates or winners from party lists.

CENI currently reports online the electoral results from the 2015 presidential election, 2015 legislative elections (reports at the province level), and 2016 municipal elections (reports at the municipality and village levels) (Commission Electorale Nationale Independente du Burkina Faso, 2016). However, the Internet Archive contains municipality-level results for both the 2006 and 2012 municipal elections, which I use along with the 2016 municipal elections for consistency between observations.

Afrobarometer Surveys

In order to examine the attitudes and perceptions of voters which may be driving my results, I also use data from the subnationally geocoded Afrobarometer survey data (Benyishay et al., 2017). The Afrobarometer surveys use nationally representative samples of 1,200 citizens, geocoded to the municipality of residence. There have been three rounds of this survey in Burkina Faso to date, in 2008, 2012, and 2015, which neatly parallels the timing of municipal elections and the MCC intervention. Each wave of this repeated cross-section asks many of the same questions on political attitudes, including beliefs about and preferences for democratic functioning, perceptions of corruption, political identity, and voting intentions.

Despite being representative of the country as a whole, they do not survey citizens in every municipality. This restricts the sample in pilot-phase municipalities considerably, particularly as pilot municipalities were specifically chosen as priority areas in land conflicts (and are thus not necessarily representative of the country as a whole). The distribution of respondents in pilot-phase municipalities in each wave is given in Table 1.

Although survey respondents are reasonably well-distributed between treatment and control municipalities in each round, they are not as well-balanced within experimental pairs. Therefore, in working with this data, although we want to control for unobserved factors that link respondents who live in a region together, if we include experimental-pair fixed effects, we would be identifying effects off of only three pairs which have both treatment and control municipalities within a pair in the same year (and that only in 2012). Therefore, we need to include a higher level of fixed effect, to ensure we are not only capturing noise. My preferred specification uses regional fixed effects (and clusters errors at the region level), which has much better balance across treatment and control municipalities in each round.

Pastoralism

To understand the importance of formalizing and documenting land rights in Burkina Faso, it is crucial to examine the pre-existing land tenure arrangements prevailing in the country. Although there is an enormous diversity of these institutions, one clear dimension to look at is the roughly regional split between pastoralist and agriculturalist areas. Pastoralism is very important in Burkina Faso: the livestock sector makes up more than 25% of total GDP, much of which is under a transhumant system, particularly in the Sahel which is ill-suited for crops (Leonhardt, 2019).

In order to distinguish intensive pastoralist areas, I use FAO livestock systems data to calculate the share of cattle in each of 13 regions under a pastoral (as opposed to agro-pastoral or intensive) system of production (FAO, 2018). I then create an indicator variable which takes a value of one in regions with above-average shares of cattle under pastoral production.

Other data

Finally, I plan to use several other datasources to explore mechanisms and rule out alternative explanations. These include local government budgets, which will allow me to track and control for other decentralization efforts that were ongoing during the period under study. I also am hoping to use local government performance scorecard data, collected as part of the Suivi de la Performance Muncipale (SUPERMUN), which tracks public service delivery and institutional capacity in municipalities nationwide.

Balance at Baseline

Although the experimental setup of the pilot phase should guarantee (in expectation) balance between treatment and control municipalities, it is important to examine outcomes of interest at baseline. We can additionally compare pilot-phase municipalities to the country as a whole, to get a sense of how generalizable the findings may be. We can see from table 2 that on most measures, treatment and control municipalities look statistically similar, as well as similar to areas not in the study. We can see that there are some differences in perceptions of corruption in the 2008 Afrobarometer survey between study areas and other muncipalities, but although this may affect the external validity of our conclusions, there is little room for concern with the experimental, internal validity.

Empirical Strategy

Due to the timing of municipal elections in Burkina Faso, in conjunction with the pilot phase of the Rural Land Governance Compact, I can use an empirical strategy that stems from the intuition of a difference in differences. That is, by comparing changes in treatment municipalities to changes in control municipalities over the same period of time, any differences can be attributed to the randomly-assigned treatment. Any time-invariant municipality-specific differences will be differenced out over the time dimension, and any common shocks to all municipalities will be controlled for.

This empirical method relies on the assumption of parallel trends: in the absence of treatment, treated units would follow the same trend in outcomes as untreated units. Although we cannot directly test this assumption, it seems plausible in a randomized context (where in expectation treatment and control groups are identical). As additional support, we can check whether variables that we would expect not to be influenced by the creation of land offices have parallel trends over the period in question. For example, we can check if the number of council seats available for election, determined by a formula (2 seats per village in the municipality, supplemented proportionally by village population if there are fewer than 10 villages), seems to follow a common trend, as it appears to in figure 3. A variety of other placebo measures are discussed in the appendix, and do not give cause for concern about differential trends in the municipalities under consideration.

Although I observe municipalities voting in 3 elections, all treated units receive 'treatment' at the same time: first, the announcement that land offices will be created in these municipalities,

Variable	(1) Phase 2 treat Mean/SE	(2) Phase 2 control Mean/SE	(3) Not in study Mean/SE	(1)-(2)	T-test Difference (1)-(3)	(2)-(3)
A: Municipality-Level Variables						
Seats Available	44.633 (4.008)	48.103 (5.194)	49.410 (1.992)	-3.470	-4.776	-1.306
Registered Voters	8658.100 (750.101)	8225.655 (706.072)	10950.218 (863.770)	432.445	-2292.118	-2724.563
Voter turnout rate	0.496 (0.019)	0.482 (0.018)	0.503 (0.006)	0.014	-0.007	-0.021
Parties Contesting	4.200 (0.357)	3.586 (0.279)	4.857 (0.318)	0.614	-0.657	-1.271
Effective # Parties (votes)	2.163 (0.108)	2.158 (0.102)	2.407 (0.087)	0.004	-0.244	-0.248
Pastoralist	0.600 (0.091)	0.586 (0.093)	0.425 (0.030)	0.014	0.175*	0.161*
N	30	29	266			
B: Afrobarometer Survey	Mean/CI	Mean/CI	Mean/CI			
All/most corrupt: president	0.17 (0.01 - 0.32)	0.33 (-0.47 - 0.93)	0.21 (0.11 - 0.31)	-0.15**	-0.03	0.06**
All/most corrupt: local gov	0.11 (-0.00 - 0.24)	0.30 (-1.17 - 0.83)	0.24 (0.16 - 0.32)	-0.19*	-0.13**	0.03
All/most corrupt: gov officials	0.14 (-0.01 - 0.37)	0.40 (-1.72 - -1.36)	0.24 (0.17 - 0.32)	-0.26*	-0.11	0.08
Trust somewhat/a lot: local gov	0.63 (0.44 - 0.87)	0.68 (0.18 - 2.36)	0.63 (0.50 - 0.76)	-0.05	0.000	0.03
Leaders should not favor own group	0.40 (0.14 - 0.52)	0.25 (-0.38 - 0.70)	0.35 (0.29 - 0.41)	0.15	0.05	-0.05
Trust CDP	0.57 (0.43 - 0.75)	0.53 (-0.55 - -0.41)	0.51 (0.39 - 0.62)	0.05	0.06	0.01
N	80	40	944			
Clusters	5	3	12			

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. For CENI Data, standard errors are clustered at the experimental-pair level, with all non-experimental municipalities in one cluster. Afrobarometer data includes regional fixed effects and wild cluster bootstrapped confidence intervals clustered at the regional level.

Table 2: Balance at Baseline
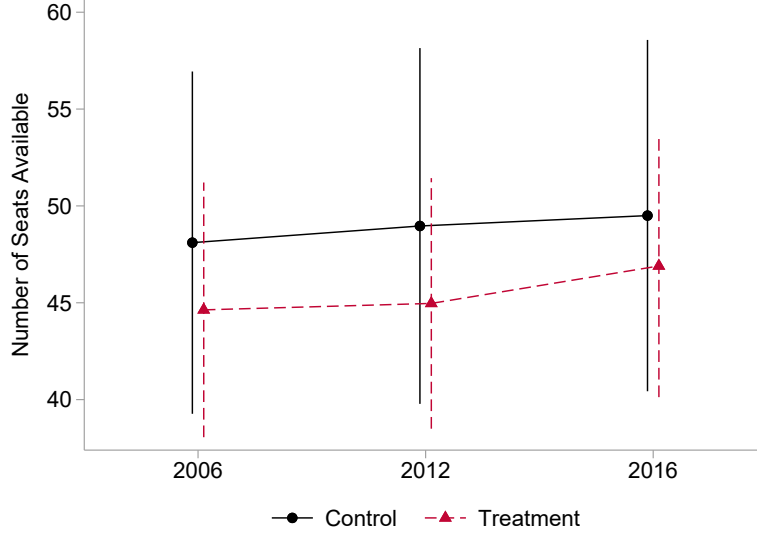
Figure 3: Parallel trends between treatment and control in seats available

immediately before the 2012 election, and then, the actual creation of land offices and associated activities from 2012-2014, prior to the 2016 elections. The main coefficients of interest in regression tables will be on the interaction of a municipality's treatment status with 2012 and/or 2016 year dummies. It is important to note that observations from 2016 keep the 2012 dummy 'turned on', so coefficients should be interpreted additively. This intuitively matches the treatment: the effects seen in 2016 are of the marginal effect of implementation, above and beyond the announcement and anticipation of treatment.

For most outcomes, I report two main specifications. Both restrict the sample to Phase II municipalities (30 treatment + 29 control), clustering standard errors at the municipality-pair level. This level of clustering is shown by de Chaisemartin and Ramirez-Cuellar (2019) to be the appropriate one in matched-pair experimental settings such as this one. In the second specification, I also include pair fixed effects, which control for regional heterogeneity or other pair-specific factors.

For outcomes from the Afrobarometer survey data, I also use an empirical strategy that accounts for the spatially-clustered and unevenly distributed observations between treatment and control municipalities and across survey rounds. My preferred specification includes region (rather than experimental pair) fixed effects, which is the minimal geographic unit that consistently includes both treatment and control municipalities in a given survey round. I also use the Wild Cluster Bootstrap

to estimate p-values (clustering at the region level), following Cameron and Miller (2015) in cases with few clusters, particularly with limited variation in treatment.[1] I separately bootstrapped each coefficient of interest, so the interpretation of results post-treatment is as above: the additional impacts of implementation over and above those of the announcement, rather than their joint significance. Although most outcomes I consider from the Afrobarometer data are binary, I use a linear fixed effects model rather than a binary outcomes model such as a logit, as the logit cannot be Wild Cluster Bootstrapped.[2]

Responses by Political Parties

Turning to the results of my analysis, I first consider responses by politically sophisticated actors who have the potential to control local governments. Although we cannot directly measure the returns of holding office and thereby controlling local land offices, we can make inferences based on their choices that they anticipate these returns will be substantial.

Number of Parties Contesting

The primary observable way that political actors respond to changes in the political environment is deciding to contest the election. Therefore, if we use the number of parties contesting a given municipality's election in the CENI electoral data as the outcome in our difference-in-difference framework, we find the results presented in figure 4 and table 3. Both specifications in the table use only municipalities involved in the second pilot phase of the RLG project, clustering standard errors at the experimental pair level. Column (2) also includes experimental pair fixed effects, to control for unobserved heterogeneity between different regions of the country.

We can see that there was a substantial (and statistically significant in the non-FE specification) increase in the number of parties contesting in 2012 (increase of .66 parties on a base of 3.6), which was a historically competitive election across the country. Nevertheless, there is an even larger, statistically significant, increase in treatment municipalities (an additional .77 parties, p-

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[1] As Cameron and Miller (2015) suggest, the preferred specification reported uses the Webb 6-point distribution rather than the default Rademacher 2-point distribution, as it performs better with 12 or fewer clusters. However, the results are robust to the choice of distribution, as well as to omitting fixed effects, clustering at municipality or province levels (which are less conservative). I also consider survey weighting using Afrobarometer's computed weights; however, these are calculated to achieve national representativeness rather than representativeness of pilot municipalities.

[2] The Wild Cluster Bootstrap requires additively separable errors; even the Score Wild Bootstrap which was developed for nonlinear models may give inconsistent estimates of coefficients (Cameron and Miller, 2015).
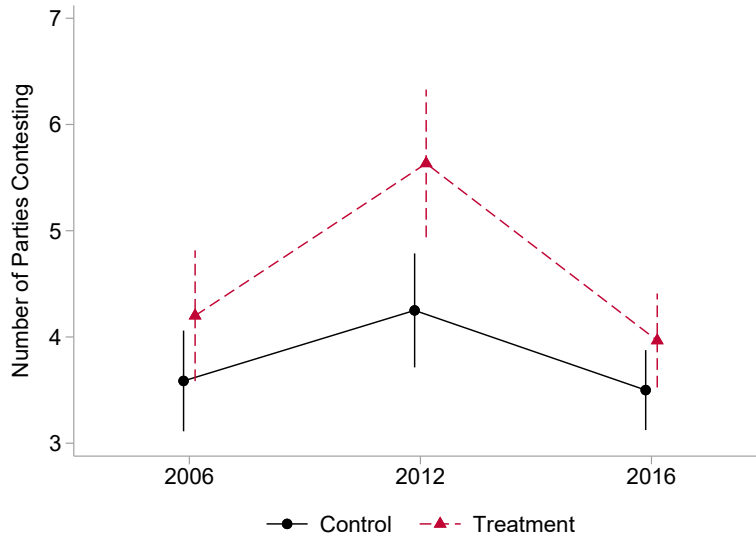
Figure 4: Parties enter when treatment is announced in 2012

value 0.096). This result is consistent with political actors observing the announcement of land office locations, which they anticipate may be subject to local government control. In treatment municipalities, then, the expected benefits of winning local elections (and thus being in charge of the local land office and documentation process) increases, making it worthwhile for more marginal parties to enter the race.

However, by the 2016 elections, the number of parties contesting had fallen everywhere in comparison with 2012, with a greater fall in treatment municipalities to bring their numbers back into line with control areas. This is an important result, as land registration was ongoing in the municipal offices in 2016, so it seems reasonable that holding office would continue to be valuable. However, in interviews, some involved with the MCC project suggested that there was a strong emphasis on making the process fair and not subject to political control or elite capture, including participatory land use mapping exercises. If the land offices were in practice not under local political control, then the benefits of holding office are no higher in treatment municipalities. Others suggested that the main avenue for political control was in appointing the SFR staff, who would not change with political turnover after implementation. We shall further explore the implications of these explanations when looking at voter perceptions.

VARIABLES	(1) Parties Contesting	(2) Parties Contesting	(3) Parties Contesting
Treatment	0.614	0.633*	0.636*
	(0.441)	(0.337)	(0.337)
2012	0.664	0.635*	0.629*
	(0.448)	(0.347)	(0.347)
Treatment*2012	0.770	0.798*	0.805*
	(0.626)	(0.441)	(0.440)
2016	-0.750*	-0.737***	-0.734***
	(0.452)	(0.247)	(0.246)
Treatment*2016	-0.917	-0.930**	-0.933**
	(0.629)	(0.371)	(0.369)
Constant	3.586***	3.586***	3.583***
	(0.314)	(0.283)	(0.219)
Observations	175	175	175
R-squared	0.154		0.281
Pair FE	No	No	Yes
Cluster SE	None	Pair	Pair
Number of comp		29	29

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table 3: Political parties contest municipal elections when treatment is announced

	(1)	(2)	(3)	(4)
	Effective # Parties	Effective # Parties	Parties Winning	Parties with
VARIABLES	(Votes)	(Seats)	No Seats	$\leq 10\%$ Vote Share
Treatment	-0.00222	-0.0631	0.468	0.803**
	(0.106)	(0.0931)	(0.333)	(0.333)
2012	0.282***	0.133	0.248	0.386
	(0.0979)	(0.0892)	(0.316)	(0.351)
Treatment*2012	0.152	-0.0103	0.318	0.680
	(0.117)	(0.0915)	(0.423)	(0.478)
2016	0.00726	0.104	-0.906***	-0.892***
	(0.111)	(0.109)	(0.237)	(0.266)
Treatment*2016	0.124	0.235	-0.727**	-1.208**
	(0.153)	(0.143)	(0.329)	(0.442)
Constant	1.623***	1.408***	1.208***	1.515***
	(0.0645)	(0.0605)	(0.207)	(0.219)
Observations	175	175	175	175
R-squared	0.221	0.169	0.245	0.342
Number of comp	29	29	29	29
Pair FE	Yes	Yes	Yes	Yes
Cluster SE	Pair	Pair	Pair	Pair

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table 4: Electoral competitiveness does not seem to increase in treatment municipalities

Electoral Competitiveness

Although Tavits (2008) shows that the entry of even uncompetitive political parties can shape the political environment, in a system like Burkina Faso's historically dominated by one-party rule, it is interesting to see if the new parties induced to enter by the announcement of land offices do in fact change the electoral competitiveness of elections. That is, do voters face a greater choice of viable parties in treatment municipalities? An interest in this question can also help us isolate the expected probability of winning office, which Tavits (2006) points to as a key determinant of party entry. Across multiple measures of electoral competitiveness suggested by the literature, I find no evidence that treatment municipalities became more competitive politically.

Effective Number of Parties

One primary measure of political competition is the effective number of parties, constructed in a similar manner to measures of market competition such as Herfindahl-Hirschman indices. The

effective number of parties can be computed either using the number of votes or seats won, which have slightly different interpretations. The former measures how competitive parties are in winning voters, while the latter combines this with structural factors that determine how votes are translated into seats.

The classic measure of the effective number of parties was proposed by Laakso & Taagepera in 1979, which is equivalent to an inverse Simpson index of diversity. However, Golosov (2010) proposes an alternative which performs better in highly fragmented or highly concentrated party systems:

$$N = \sum_{i=1}^{n} \frac{p_i}{p_i + p_1^2 - p_i^2}$$

Where n is the number of parties with at least one vote, p_i is a given party's proportion of all votes (seats) won, and p_1 is the largest party's vote (seat) share.

Results for the Golosov effective number of parties are presented in columns (1) (computed using vote shares) and (2) (computed using seat shares) of table 4. Other measures are also presented in table A7, in the appendix. Regardless of the measure used, however, we do not see significant differences between treatment and control municipalities, and the magnitudes are also relatively small. Therefore, although more parties compete in treatment municipalities in 2012, they do not seem to make the elections meaningfully more competitive.

Small Parties

There are other potential ways to look at the expected probability that any potential party entrant wins. One is to see what number of parties fail to win any council seats (possible in multi-seat elections such as these), as shown in column (3) of table 4. Another is to take advantage of a constitutional clause on the funding of political campaigns: all parties must pay a deposit to be included on the ballot, which entitles them to some public campaign funding; if they receive 10% of votes in the election, then they are reimbursed their deposit. Although this deposit is not large for municipal elections, it may be economically substantial in rural areas. Therefore, we can consider the number of parties that fail to reach this 10% threshold as electorally uncompetitive, in column (4).

When we look at either of these measures, although we do not see statistical significance for the positive difference between treatment and control in 2012, we do find a larger decrease in 2016 for
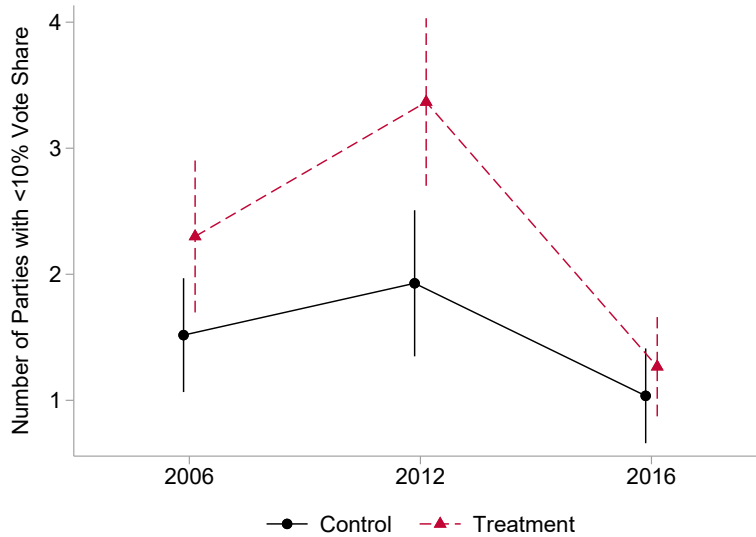
Figure 5: Parties entering in response to treatment in 2012 fail to reach the 10% vote share for reimbursement

treatment municipalities. When we examine these results graphically in figure 5, we see a similar pattern to that for the number of parties: in 2012, treatment municipalities have more parties that fail to reach the 10% vote share threshold. Therefore, although we see more parties competing in elections, it seems clear that they largely are not presenting a serious challenge to the dominant parties.

Party Alliances

Although the results presented so far are consistent with independently-competing political parties attempting to win representation, the potential for party alliances may add another layer of complexity to the calculations made by political actors. Although it is difficult to track municipal-level alliances between parties, which are often informal support arrangements, there is qualitative evidence that smaller parties ally with (usually ruling) larger parties in determining policy platforms and while governing. In Burkina Faso, parties often split prior to elections due to personality differences at multiple levels of the party hierarchy. This is particularly common as most parties do not offer distinct policy positions or agendas, and rarely compete on the basis of platforms.

It would be interesting to explore what the incentives are for political actors who are considering how best to gain access to power and political resources. They could simply join the ruling party

for the election and work within its hierarchy, rather than running against it and the later forming an alliance.

It seems plausible that in elections dominated by one party, the best strategy for a political actor to access patronage positions or political resources such as the land offices is to join the dominant party and work within it. However, in a newly competitive election, running as an independent party allows the agents to hedge their bets: it is unclear who the winning party will be, so joining either too early could leave the agent out of power. But by conducting an independent (even if uncompetitive) campaign, the agent is able to demonstrate their role as a political actor who will need to be included in any eventual settlement. And indeed, the 2012 election in particular was perceived as truly competitive for the first time in Burkina Faso's history, at a national level in legislative elections as well as in municipalities. This could explain why we see such a large increase in the number of parties in 2012, when it would have been difficult to predict who would win in a given municipality. By 2016, however, it may have been clearer to political actors who was going to win in the post-upheaval environment dominated by a new national party. Therefore, the desire to be control the land offices could incentivize joining the new ruling party, rather than running independently. Because we cannot observe individual candidates, though, this is merely speculative at the moment.

Ruling Party & Main Opposition

Given that most of the new parties competing in municipal elections in response to the announcement of treatment status are uncompetitive, it is instructive to look at the behavior of individual parties that we know to be competitive at a national level. The ruling party nationally between 1993 and 2014 was the CDP, which was affiliated with the president Blaise Compaoré and dominated the legislature and local elections during that time. Indeed, it won more than 70% of seats across all municipalities in the 2006 and 2012 elections. Because this party dominated national politics so thoroughly, and so was closely involved in the implementation of the Rural Land Governance project, we can assume that the CDP expected the creation of these land offices to benefit them politically, particularly given the announcement of treatment locations immediately before an election where they faced a viable challenge for the first time. This assumption would be in line with the findings of Briggs (2012), that external aid is politically targeted and ruling party electoral performance improves in areas which receive it.
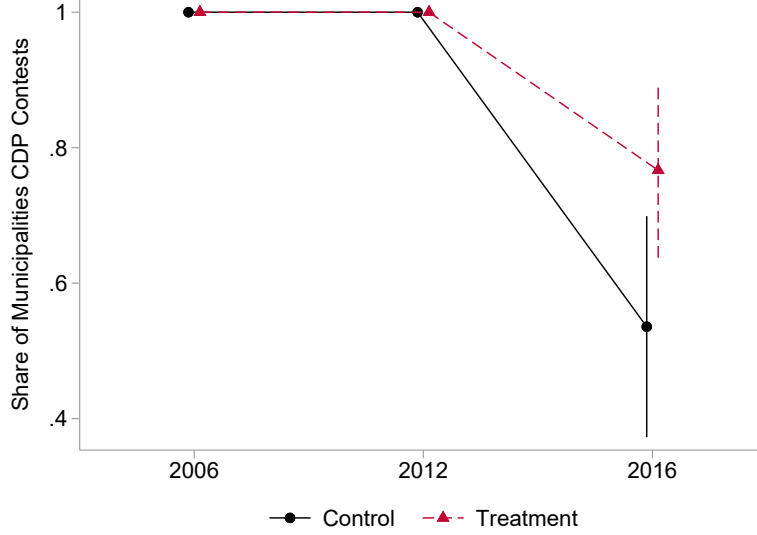
Figure 6: CDP is more likely to contest in treatment municipalities, post-transition

If, then, we look at the CDP's electoral performance, we note some interesting patterns. In 2012, for instance, the CDP's vote share decreases slightly across all municipalities in the study, as expected with a newly viable opposition. However, we see (in table A8) no difference between treatment and control municipalities in this year. We can also consider the extensive margin: does the CDP contest every election? Prior to the popular uprising in 2014, they ran a party list in essentially every municipality in the country. However, in the 2016 elections, they were much less likely to contest nationwide, due to their lost legitimacy. In municipalities where rural land offices had been created, though, this decrease is half the magnitude, as seen in figure 6. This may be because CDP leadership was able to point to the workings of the rural land offices as a positive benefit of CDP tenure, therefore justifying running again by increasing the likelihood of getting elected.

In order to see if voters do indeed reward the CDP for their involvement in these land offices, post-implementation, I need to use a statistical method to account for the extensive margin, determining whether or not the CDP is running. Latent variable or generalized Tobit models are the general class of methods used for these cases of censoring. I intend to use a sample selectivity model that accounts for two separate decisions: first, whether or not the CDP competes, and then its vote share. However, these models require different determinants for the two latent variables representing these

decisions. It may be possible to use incumbency from the 2012 elections as a determinant of vote share but not CDP participation, due to a quirk in the context: during the period of political unrest from 2014-2016, the caretaker government sent 'special delegations' to replace elected municipal councils. Therefore, the normal benefits of incumbency for beginning a political campaign would be much weaker, while voters may remember the performance of previous incumbents and favor them due to uncertainty aversion, as described in Lierl and Holmlund (2019). However, this work is still ongoing.

Opposition parties, on the other hand, are much less stable over time in the Burkinabe context. The main substantial opposition to the CDP between 2012 and 2016 (the main period under consideration) was the UPC. However, we see no statistically significant differences in support for the UPC between treatment and control municipalities in either year. The MPP, which broke away from the CDP over the issue of term limits during the 2014 unrest and later won national elections in 2015 ran candidates in municipal elections in 2016; however, I see no significant differences in their performance between treatment and control municipalities that year.

Voter Responses

We are not only concerned with how informed political actors respond to the announcement and creation of land offices: it is also interesting to see how voters respond. One theory would predict that voters observe that local governments will now be in charge of more services, and thus the import of local government leadership increases. This should increase the incentive to vote, so citizens can participate in choosing those who will be responsible for allocating land rights, and so we would expect to see an increase in voter turnout in response to treatment. However, as we shall see, citizens are not more likely to vote in treatment municipalities; in fact, we see the opposite effect. This may be driven by the mediating effect of political parties' behavior discussed above.

Voter Turnout

Although voters' decisions to vote as well as who to vote for are theoretically interesting, the latter is subject to a complex relationship with the strategic decisions made by political parties to gain support; therefore, I first focus on voter turnout as an outcome. The difference-in-difference specification with treatment and matched control municipalities is presented in columns (1) and (2) of
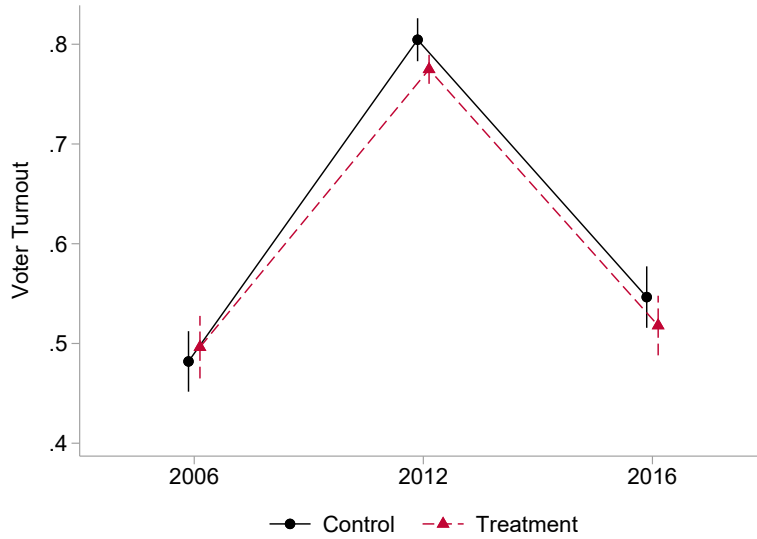
Figure 7: Voter turnout increases less in treatment communes

table 5, pooled and then with pair fixed effects. We can see that although voter turnout increases substantially in 2012, as expected for a competitive election simultaneous with national legislative elections, this increase is statistically significantly smaller in treatment municipalities. The difference is also relatively large in magnitude, representing 5% of the 2012 control municipality turnout rate (a magnitude similar to the increase in turnout due to get out the vote experiments in the US, as reviewed by Green et al. (2013)). Matched pairs are generally within the same province (a second-tier administrative unit within Burkina Faso within regions), so legislative electoral environments should be similar; therefore, it seems reasonable that differences at the municipality level are driving these results. Turnout in 2016 appears to drop again, although equivalently in treatment and control municipalities.

It does, though, seem difficult to understand voters being less eager to vote in municipalities that will be receiving this local public service. Therefore, I would suggest that unsophisticated voters may not be observing the announcement of treatment status, but they are observing its result: an increase in the number of political parties contesting the election, as demonstrated above. Ballots in Burkina Faso only list party names and symbols; in control municipalities in 2012, most voters are faced with either 3, 4, or 5 parties, while in treatment municipalities, the average municipality has 5.6 parties contesting, with as many as 10 parties on the ballot, as seen in figure 8. Therefore, we can

VARIABLES	(1) Voter Turnout	(2) Voter Turnout	(3) Voter Turnout	(4) Voter Registration
Treatment	0.0149		0.0169	524.4
	(0.0231)		(0.0237)	(742.3)
2012	0.323***	0.289***	0.325***	558.2
	(0.0165)	(0.00631)	(0.0162)	(399.7)
Treatment*2012	-0.0441**		-0.0416**	781.1
	(0.0198)		(0.0198)	(704.6)
2016	-0.260***	-0.258***	-0.263***	2,237***
	(0.0152)	(0.00632)	(0.0153)	(337.6)
Treatment*2016	0.00356		0.000675	390.1
	(0.0175)		(0.0163)	(363.8)
Number Parties		-0.00685***	-0.00309	
		(0.000673)	(0.00425)	
Constant	0.482***	0.534***	0.493***	8,183***
	(0.0129)	(0.00548)	(0.0184)	(566.1)
Observations	175	1,089	175	175
R-squared	0.817	0.701	0.818	0.217
Number of comp	29		29	29
Pair FE	Yes	No	Yes	Yes
Cluster SE	Pair	Pair	Pair	Pair

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table 5: Voter turnout decreases with more parties and in response to the announcement of treatment

imagine that the costs of learning about the parties and deciding how to vote may be much higher for citizens in treatment municipalities, leading some to stay home. Indeed, across all municipalities in Burkina Faso, we do see a negative correlation between the number of parties competing and voter turnout (column (3) of table 5 and figure 9). However, this may not be the only factor driving these results, as when I control for the number of political parties in column (4), we still find a significant difference between treatment and control municipalities in 2012.

Voter Registration

There is another potential explanation for the observed decrease in voter turnout in treatment municipalities in 2012: voter registration, the denominator of voter turnout, could have increased. It is unlikely that citizens had time to move to treatment municipalities en masse after the announcement of treatment status before the election, but there could be concerns that these locations were targeted for public services because of recent in-migration, as McMillan et al. (2011) shows happens
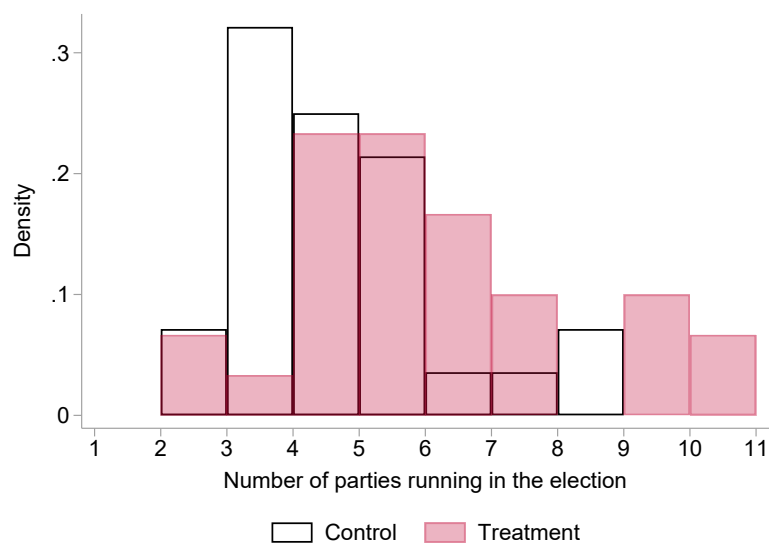
Figure 8: Higher mean and dispersion of parties in treatment areas in 2012
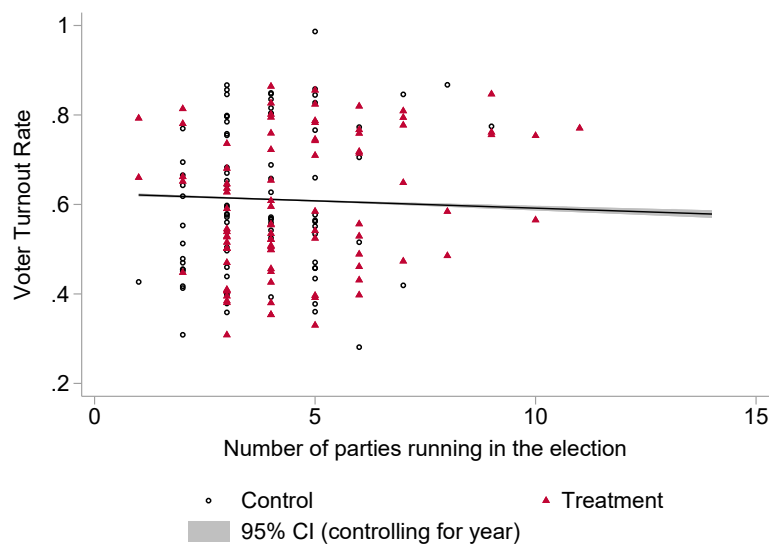
Figure 9: Slight negative relationship between number of parties contesting and voter turnout

in Burkina Faso. However, the experimental setup of the pilot phase, where treatment and control status was assigned randomly within targeted pairs of municipalities, allays this concern. Voter registration could also be related to the increase in the number of parties contesting the election: new parties may have encouraged voter registration in their campaigning, but these new voters failed to show up. The 2012 election did entail a large voter-registration drive, led by NGOs (Pryce and Nascimento, 2014), who may also have targeted treatment municipalities. However, I find no differences between treatment and control municipalities in voter registration, casting doubt on this mechanism.

Incumbency

Another potential explanation for the 2012 drop in turnout in treatment municipalities bears exploring. This election saw the emergence of a valid opposition party for the first time, which excited voters nationwide. However, in treatment areas, the CDP had announced that it was bringing a new resource to treatment areas, which would make the opposition relatively less appealing. If voters respond to this increased value of both the CDP and the opposition by staying home rather than voting for either, we could see lower turnout. We would expect this effect to be stronger in municipalities where the CDP was incumbent for the 2012 elections, as it would be easier for voters to attribute the announcement of treatment to the ruling party nationally and locally. This would indicate a negative coefficient on the interaction of treatment and the CDP being incumbent in columns (2) or (3) of table 6 (as these models exclude 2006, when there is no incumbency history). However, this coefficient is statistically insignificant and close to zero in magnitude, indicating this explanation is not the primary driver of these results.

This model does indicate that after implementation, in 2016, voter turnout is significantly higher in treatment areas where the CDP was incumbent, despite being insignificant overall. These heterogenous effects seem not to be robust to the inclusion of pair fixed effects, but may bear further exploration.

Voter Attitudes and Perceptions

Although election results data do not allow us to closely examine voter responses to the treatment, we can also make use of surveyed voter attitudes and perceptions from the geocoded Afrobarometer survey data. Each survey round is nationally representative, but does not survey respondents in

VARIABLES	(1) Voter Turnout	(2) Voter Turnout	(3) Voter Turnout	(4) Voter Turnout
Treatment	-0.0262**	-0.0363	-0.0280	-0.0282
	(0.0131)	(0.0225)	(0.0240)	(0.0238)
CDP Provincial Support			-0.0381	
			(0.0276)	
Treatment*CDP Provincial Support			0.148***	
			(0.0445)	
Baseline CDP Provincial Support				-0.102***
				(0.0284)
Treatment*Baseline CDP Provincial Support				0.153***
				(0.0447)
CDP Incumbent	-0.0234	-0.0328		
	(0.0174)	(0.0211)		
Treatment*CDP Incumbent	-0.00289	0.00949		
	(0.0226)	(0.0322)		
2012			0.336***	0.326***
			(0.0234)	(0.0213)
Treatment*2012			-0.0244	-0.0163
			(0.0227)	(0.0228)
2012*CDP Provincial Support			-0.0213	
			(0.0345)	
Treatment*2012*CDP Provincial Support			-0.0882**	
			(0.0383)	
2012*Baseline CDP Support				-0.0183
				(0.0315)
Treatment*2012*Baseline CDP Support				-0.0839**
				(0.0398)
2016	-0.234***	-0.246***	-0.293***	-0.266***
	(0.0160)	(0.0163)	(0.0239)	(0.0189)
Treatment*2016	-0.0731*	-0.0572	0.0429	0.00367
	(0.0418)	(0.0436)	(0.0351)	(0.0205)
2016*CDP Provincial Support			0.0745*	
			(0.0374)	
Treatment*2016*CDP Provincial Support			-0.0877*	
			(0.0499)	
2016*CDP Incumbent	-0.0279	-0.0142		
	(0.0254)	(0.0270)		
Treatment*2016*CDP Incumbent	0.0869*	0.0684		
	(0.0475)	(0.0512)		
2016*Baseline CDP Support				0.0280
				(0.0320)
Treatment*2016*Baseline CDP Support				-0.0113
				(0.0352)
Constant	0.824***	0.831***	0.492***	0.510***
	(0.0129)	(0.0146)	(0.0176)	(0.0165)
Observations	116	116	175	175
R-squared		0.874	0.844	0.848
Number of comp	29	29	29	29
Pair FE	No	Yes	Yes	Yes
Cluster SE	Pair	Pair	Pair	Pair

Robust standard errors in parentheses

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

Data source: CENI Electoral Returns

Table 6: Voter Turnout higher in post-intervention municipalities controlled by the CDP

every municipality (and therefore, we do not observe a balanced panel from our study municipalities). However, the timing of these surveys neatly parallels the timeline of the study: one wave was in 2008, prior to the signing of the compact with MCC (and thus should be unaffected by treatment status which was assigned later); the second occurred immediately prior to the 2012 municipal election (so should capture voter's perceptions after treatment status was announced but prior to implementation); and the third wave was in 2015 (after land offices had been created and were functioning). Therefore, we can use a similar difference-in-differences framework to those used above.

First, the Afrobarometer asks many questions about perceptions of corruption in different levels and branches of government. These questions are generally asked in the form "How many government officials [of X group] are corrupt? None of them, Some of them, Most of them, All of them, Don't know." I then recode responses into a binary indicator equal to zero for 'none of them'/'some of them', and equal to one for 'most' or 'all of them'. The primary specifications I report in table 7 use a linear probability model, although I can also use a logit model with experimental pair fixed effects to check robustness. In column (1) of table 7, I look at perceptions of corruption in the office of the president as a placebo check: given that treatment occurred at a local level, we should not expect perceptions of the national government to change substantially. Indeed, we find no significant differences between treatment and control municipalities in any year. In column (2), the question instead asks about corruption among government officials, a category which would include the functionaries working in newly-created land offices. Interestingly, although there is a statistically-significant difference between treatment and control municipalities at baseline (insignificant with the wider standard errors estimated only using 2008 data in table 2), even controlling for this, there appears to be a statistically-significant increase in the perception of corruption among government officials in treatment municipalities in 2012, while the more heavily contested election campaigns are ongoing. We see a similar pattern when looking at perceived corruption among local government (municipality) council members in column (3). In this year, the decrease in perceptions of corruption in treatment municipalities between 2012 and 2015, when implementation was happening, is also significant. This pattern parallels the results for the number of parties contesting, which supports my interpretation of the observed party behavior as rent-seeking. The observed decrease in perceptions of corruption in 2015 was unsurprising to those involved in the MCC project, as they felt that the Rural Land Governance project had paid particular attention to avoiding corruption, including

	(1)	(2)	(3)
	Corruption in President's Office	Corruption in Gov Officials	Corruption in Local Gov
Treatment	-.0698054	-.3111226	-.2588241
	(0.67)	(0.05)	(0.19)
2012	.0110917	-.0632625	-.0878433
	(0.88)	(0.67)	(0.57)
Treatment*2012	.0911531	.2895166	.3102148
	(0.38)	(0.08)	(0.15)
2015	-.0051325	-.0004007	.0708754
	(0.96)	(0.99)	(0.50)
Treatment*2015	-.1661832	-.1386811	-.2395636
	(0.36)	(0.48)	(0.16)
Constant	.3591549	.49182	.3888866
Observations	358	370	388
R^2	0.018	0.039	0.039
Number Clusters	12	12	12
Fixed Effect	Region	Region	Region
Standard Errors	Wild Cluster Bootstrap	Wild Cluster Bootstrap	Wild Cluster Bootstrap
Years Asked	08/12/15	08/12/15	08/12/15

Wild Cluster Bootstrapped p-values in parentheses, cluster at regional level

Data source: Afrobarometer

Table 7: Perceptions of corruption at the local level increase in anticipation of treatment

participatory land use mappings with communities that would prevent elite capture of the land offices. The results in column (3) are perhaps encouraging that concerns about political capture raised by party responses to the announcement of treatment in 2012 can be dealt with effectively.

We can also use the Afrobarometer surveys to test whether pilot land offices were targeted to areas that supported the ruling party, as well as if their presence changed opinions about the CDP or about the need for term limits for the president (the trigger for 18 months of civil unrest in 2014-2015). Table 8 does not support either of these theories, however, as we see no systematic differences between treatment and control municipalities.

Mechanisms: Where is Control of the Land Office more Valuable?

The primary analysis above simply uses the experimental assignment of treatment and control municipalities to look at average treatment effects, which are causally identified. However, it is also interesting and extremely policy-relevant to understand why these effects are happening.

	(1)	(2)
	Support Term Limit	Trust CDP
Treatment	-.1651713	.163052
	(0.47)	(0.37)
2012	-.023049	-.010422
	(0.94)	(0.92)
Treatment*2012	.1908113	-.0635544
	(0.55)	(0.62)
2015	.1613045	
	(0.13)	
Treatment*2015	.034328	
	(0.69)	
Constant	.7313894	.5162566
Observations	421	229
R^2	0.078	0.013
N_g	12	11
Fixed Effect	Region	Region
Standard Errors	Wild Cluster Bootstrap	Wild Cluster Bootstrap

Wild Cluster Bootstrapped p-values in parentheses, cluster at regional level

Question on Trusting CDP only asked in 2008 & 2012

Data source: Afrobarometer

Table 8: No differential support for Compaoré or term limits

In particular, we are interested in what determines the benefits of controlling decentralized land offices. We can explore this by looking at heterogeneity in effects across different dimensions that may impact the benefits, to see if we can find any differences. At times, cutting our already-small sample may lose too much power to allow us to determine if a given mechanism is at play, but I attempt to approach questions from multiple angles to support any findings. In general, I explore heterogeneity in the number of political parties contesting the election, as this result has some of the strongest differences that are least likely to become underpowered.

Looking at mechanisms for my results also entails examining the actual processes of MCC's pilot, which has implications for external validity. Importantly, I find that control municipalities seem to be anticipating receiving land offices during the 2016 elections, which may be why I cannot statistically distinguish political behavior between treatment and control municipalities in that year.

Distance to Urban Areas

Those involved in implementing SFRs in Burkina Faso often highlighted how effective, and politically valuable, the offices were in municipalities near urban areas. Wealthy urbanites desired to purchase
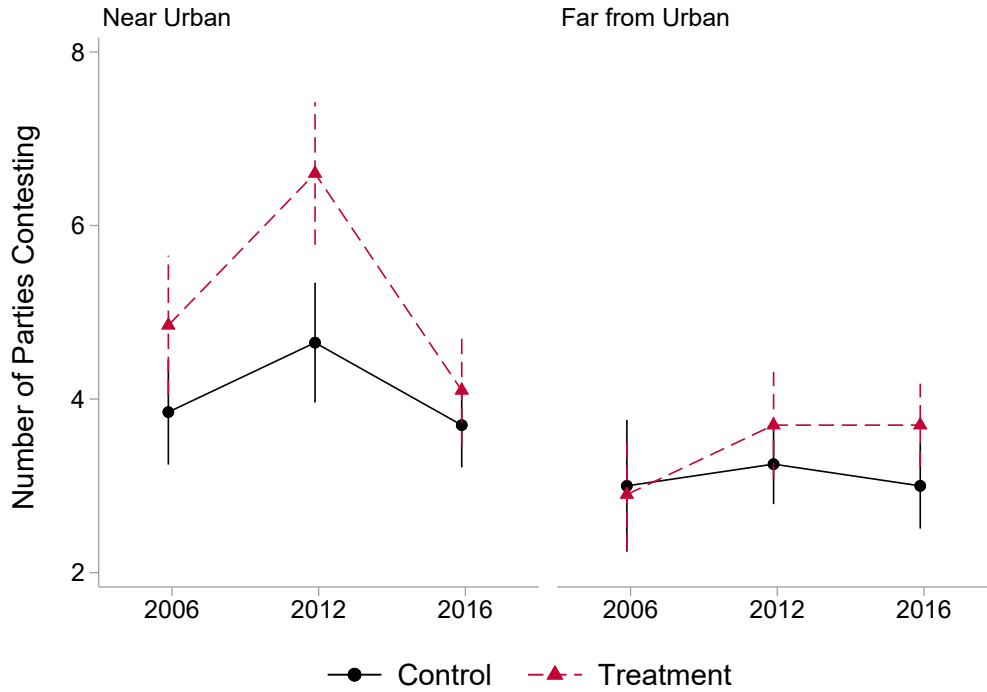
Figure 10: Responses are much stronger in municipalities near urban areas

land near the city, but the informality of traditional land rights made them reluctant to invest without stronger documentation and legal protections. At the same time, the fact that local councils could independently set and keep fees for documentation meant that these municipalities were able to charge high fees, knowing they would be paid by outsiders. These fees feed into the municipal budget, explicitly increasing the benefits of holding office. Therefore, we would expect stronger responses by political actors in municipalities closer to urban areas.

In table 9, I examine this heterogeneity by interacting my treatment effects over time with a dummy for municipalities further than average from urban areas. In columns (2) and (3), we can see that treatment effects are attenuated in treatment municipalities further from urban centers, and in particular, the significant decrease in parties contesting the election in treatment municipalities in 2016 is entirely driven by municipalities near cities. These patterns are easy to see in figure 10, where muncicipalities closer to urban areas (which face intensified incentives around land offices) respond more clearly to treatment.

VARIABLES	(1) Parties Contesting	(2) Parties Contesting	(3) Parties Contesting
Treatment	1**	1.028**	1.008**
	(0.485)	(0.490)	(0.480)
Far from Urban	-0.850	-0.323	0.548
	(0.615)	(0.529)	(0.534)
Treatment*Far from Urban	-1.100	-1.137*	-1.149*
	(0.855)	(0.612)	(0.634)
2012	0.800	0.800*	0.800*
	(0.485)	(0.450)	(0.450)
Treatment*2012	0.950	0.950	0.950
	(0.686)	(0.590)	(0.590)
2012*Far from Urban	-0.550	-0.543	-0.555
	(0.889)	(0.671)	(0.672)
Treatment*2012*Far from Urban	-0.400	-0.407	-0.395
	(1.223)	(0.785)	(0.783)
2016	-0.950*	-0.950***	-0.950***
	(0.485)	(0.311)	(0.311)
Treatment*2016	-1.550**	-1.550***	-1.550***
	(0.686)	(0.465)	(0.465)
2016*Far from Urban	0.700	0.681	0.740
	(0.907)	(0.451)	(0.459)
Treatment*2016*Far from Urban	1.800	1.819***	1.760***
	(1.236)	(0.558)	(0.560)
Constant	3.850***	3.687***	3.414***
	(0.343)	(0.361)	(0.310)
Observations	175	175	175
R-squared	0.330		0.370
Pair FE	No	No	Yes
Cluster SE	None	Pair	Pair
Number of comp		29	29

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table 9: Weaker responses in municipalities far from urban areas

Agriculturalist vs Pastoralist

In thinking about the political value of controlling a local land office in Burkina Faso, we must understand the value of what the land offices are doing in local areas. By documenting existing rights to land, the offices formalize these rights and make them legible to outsiders. This process represents a greater change from the status quo in areas where existing rights are less stable and/or formal. Burkina Faso contains areas that are primarily dominated by agricultural land use systems, where even those without prior documentation can make claims to a particular piece of land they have been farming relatively easily, simply by showing agricultural investments or crops. Although formalizing and documenting these rights may be important for individual landholders, the documentation process likely provides fewer opportunities to renegotiate or expropriate these rights, making control of the process less important. However, there are also substantial areas of the country where pastoralist and transhumant rights are important. Traditionally, these rights are primarily access rights which may be temporally limited (e.g. to grazing on crop residues after harvest) or not exercised every year. For these rights, documentation represents a substantial marginal change from the status quo. Therefore, we would expect to see larger effects in pastoralist-dominated areas, which indeed we do, in figure 11. We can also examine this in a regression framework, interacting a dummy for an above-average share of pastoral livestock in a region with year and treatment dummies. We do find strongly significant effects in pastoral areas, and almost no effect in agriculturalist areas, as seen in columns (2) and (3) of table 10.

It is also worth noting that although pastoralist areas are generally further from urban areas, these two results do stand even when considered together. Although I tend to lose power when splitting the sample 4 ways, a regression that includes interactions with dummies for both pastoralism and distance from urban areas is presented in table A14. The treatment effect in 2016 in pastoralist areas remains statistically significant at the 5% level with the expected sign, and appear to be driving much of the results.

Experimental Setup and Learning

It is also worth exploring whether political actors may be learning from other jurisdictions, given the experimental setup of the decentralization. That is, they may observe another municipality implementing the land office, and its potential political rents, and change their behavior accord-
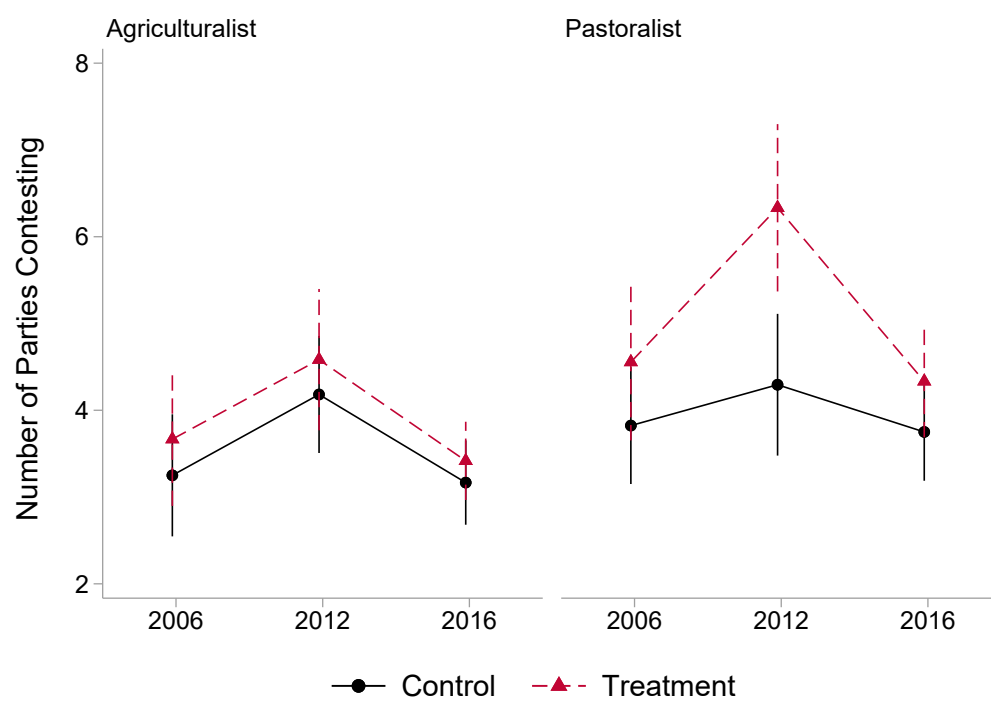
Figure 11: Responses to announcement are much stronger in pastoralist areas

VARIABLES	(1) Parties Contesting	(2) Parties Contesting	(3) Parties Contesting
Treatment	0.417	0.346	0.318
	(0.675)	(0.552)	(0.559)
Pastoralist	0.574	0.514	0.645
	(0.623)	(0.507)	(0.394)
Treatment*Pastoralist	0.315	0.439	0.477
	(0.876)	(0.704)	(0.708)
2012	0.932	0.851*	0.822*
	(0.690)	(0.448)	(0.452)
Treatment*2012	-0.0152	0.0653	0.0947
	(0.965)	(0.490)	(0.492)
2012*Pastoralist	-0.461	-0.381	-0.351
	(0.893)	(0.674)	(0.676)
Treatment*2012*Pastoralist	1.322	1.242	1.212
	(1.248)	(0.802)	(0.807)
2016	-1.015	-0.935***	-0.905***
	(0.690)	(0.314)	(0.312)
Treatment*2016	-0.152	-0.232	-0.261
	(0.965)	(0.276)	(0.277)
2016*Pastoralist	0.471	0.346	0.305
	(0.899)	(0.479)	(0.476)
Treatment*2016*Pastoralist	-1.304	-1.179*	-1.138*
	(1.252)	(0.612)	(0.621)
Constant	3.250***	3.285***	3.221***
	(0.477)	(0.375)	(0.263)
Observations	175	175	175
R-squared	0.221		0.323
Pair FE	No	No	Yes
Cluster SE	None	Pair	Pair
Number of comp		29	29

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table 10: Responses to announcement of treatment status seem to be driven by pastoralist areas

ingly, rather than responding directly to the incentives. Importantly, this type of mechanism could potentially explain the lack of significant difference between treatment and control areas in 2016: if control municipalities anticipate that they will be next to receive a land office, they may be anticipating future treatment and thus behaving similar to treated municipalities, rather than treatment municipalities' responses not persisting.

Learning from Phase I

Recall that there was a first pilot phase of the RLG project, which implemented land offices in 17 chosen priority municipalities. It may be that local actors in other municipalities observed this earlier implementation, which allowed them to (for example) foresee the political benefits of controlling land offices, and therefore decide to run for office. We would expect this learning to be stronger in municipalities near Phase I municipalities, so in column (1) of table 11, we interact the year and treatment dummies with a dummy for municipalities that are in the same province as a Phase I municipality. However, we find no significant differences in responses by political parties, although we begin to lose power.

Later spillovers

More importantly, however, we must consider whether the fact that treatment and control municipalities are statistically indistinguishable in 2016 on most measures is due not to treatment effects dissipating, but rather that control municipalities are beginning to anticipate their own treatment, and thus behaving more like treated areas. One way to test this is to compare control municipalities with those outside of the study over time, to see if their behavior differs. In columns (2) and (3) of table 11, we regress the number of parties contesting a given municipal election on treatment status (phase II treatment, phase II control, or non-study) and year, clustering errors at the province level. In column (3), I also include province-fixed effects, as there are no experimental pairs for municipalities outside of the phase II study. Interestingly, we do see a positive and significant effect for control municipalities in 2016: that is, similar to how treatment municipalities behaved in 2012 when they were anticipating treatment. This is a very important, although not perfectly causal, result worthy of further exploration. Although there is some evidence that MCC did control opportunities for political capture of new land offices effectively, particularly local perceptions of corruption, it may not entirely explain the similarities between treatment and control municipalities in 2016. There may

VARIABLES	(1) Parties Contesting	(2) Parties Contesting	(3) Parties Contesting	(4) Parties Contesting	(5) Parties Contesting
Control		-1.459**	-2.511*	-1.582**	-2.089***
		(0.706)	(1.255)	(0.726)	(0.764)
Treatment	0.914	-0.832	-1.818*	-1.090*	-2.195*
	(0.578)	(0.553)	(1.080)	(0.647)	(1.273)
Phase I Province	-0.704				
	(1.027)				
Treatment*Phase I Prov	-0.377				
	(0.712)				
Office in 2017				-1.491**	-1.955*
				(0.643)	(1.139)
Control*Office in 2017				0.926	-1.353
				(0.717)	(1.999)
2012	0.567	0.711***	0.445	0.719***	0.403
	(0.396)	(0.264)	(0.404)	(0.275)	(0.451)
Control*2012		-0.0555	0.171	-0.0369	0.279
		(0.342)	(0.464)	(0.336)	(0.499)
Treatment*2012	0.683	0.723*	0.988*	0.715*	1.031*
	(0.643)	(0.403)	(0.516)	(0.410)	(0.556)
2012*Phase I Province	0.0997				
	(0.691)				
Treatment*2012*Phase I Prov	0.206				
	(0.919)				
2012*Office in 2017				-0.161	0.156
				(0.459)	(0.429)
Control*2012*Office in 2017				-0.0366	-0.806
				(0.903)	(0.755)
2016	-0.578	-1.267***	-1.276***	-1.323***	-1.335***
	(0.378)	(0.177)	(0.178)	(0.207)	(0.207)
Control*2016		0.515**	0.531**	0.563*	0.549*
		(0.259)	(0.246)	(0.316)	(0.309)
Treatment*2016	-0.505	-0.399	-0.390	-0.344	-0.332
	(0.545)	(0.449)	(0.449)	(0.459)	(0.458)
2016*Phase I Province	-0.289				
	(0.500)				
Treatment*2016*Phase I Prov	-0.684				
	(0.728)				
2016*Office in 2017				0.370	0.382
				(0.276)	(0.275)
Control*2016*Office in 2017				-0.237	0.230
				(0.519)	(0.604)
Constant	3.957***	4.833***	5.265***	5.073***	5.596***
	(0.614)	(0.702)	(0.406)	(0.794)	(0.574)
Observations	175	990	990	990	990
R-squared	0.309		0.079		0.120
Number of comp	29				
FE	Pair	None	Province	None	Province
Cluster SE	Pair	Province	Province	Province	Province
Number of Clusters		45	45	45	45

Robust standard errors in parentheses

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

Data source: CENI Electoral Returns

Table 11: Little learning from Phase I, but control areas may be anticipating treatment in 2016

still be some political competition to be in charge of land offices in treatment areas, while control areas are beginning their own contests in anticipation of future offices.

Conclusion

In this paper, I have documented significant local political responses to the experimental decentralization of land offices to municipal governments in Burkina Faso. When anticipating treatment in a municipality, political parties are more likely to contest local elections, although these new contestants do not increase competitiveness; there is also a decrease in voter turnout in these municipalities. Responses also seem to vary in ways predicted by theory: where the value of documenting land rights is larger, political actors seem to respond more strongly to treatment.

Many of these results seem to diminish in elections after land offices have been created. There is some suggestive evidence that this was due to implementation that was not captured by local political actors: citizen perceptions of corruption at a local level seem to decrease after implementation. This is an encouraging finding, and if true, suggests that MCC should share best practices with others considering decentralization but worried about elite capture. However, there is also some evidence that the observed results are due to control municipalities anticipating their own treatment and responding to try to control their (future) land offices, which points to a more sobering systematic feature of decentralization.

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Variable	(1) Non-experimental Treatment Mean/SE	(2) Experimental[1] Treatment Mean/SE	(3) Never Treated Mean/SE	T-test Difference (1)-(2)	(1)-(3)
Seats Available	54.869 (3.222)	44.633 (4.008)	47.855 (2.217)	10.236	7.014*
Registered Voters	10299.131 (689.234)	8658.100 (750.101)	10976.860 (1018.567)	1641.031	-677.729
Voter turnout rate	0.501 (0.008)	0.496 (0.019)	0.504 (0.007)	0.005	-0.003
Parties Contesting	3.925 (0.213)	4.200 (0.357)	5.109 (0.373)	-0.275	-1.183**
Effective # Parties (votes)	2.129 (0.061)	2.163 (0.108)	2.471 (0.103)	-0.034	-0.342**
Pastoralist	0.449 (0.048)	0.600 (0.091)	0.425 (0.033)	-0.151	0.023
Far from Urban	0.467 (0.048)	0.333 (0.088)	0.380 (0.033)	0.134	0.087
N	107	30	221		

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.
Data source: CENI Electoral Returns
[1]: Experimental refers to only MCC Phase II treatment municipalities.

Table A1: Experimental treatment municipalities seem broadly similar to other municipalities which received land offices, but some differences between non-experimental treatment and never treated.

Additional Balance Tables

Experimental, Non-Experimental, and Never-treated Municipalities

To examine external validity, I present balance tables which compare experimental (phase II) treatment municipalities to municipalities which received treatment under a non-experimental program (either Phase I or non-MCC programs after 2015), as well as to all other municipalities which never received a land office. Table A1 presents balance on variables included in the election returns, and table A2 presesnts balance on variables included in the Afrobarometer surveys.

	Non-experimental Treatment Mean/CI	Experimental[1] Treatment Mean/CI	Never treated Never treated Mean/CI	T-test Difference (1)-(2)	(1)-(3)
All/most corrupt: president	0.17 (0.01 - 0.32)	0.33 (-0.47 - 0.93)	0.21 (0.11 - 0.31)	0.08	-0.01
All/most corrupt: local gov	0.11 (-0.00 - 0.24)	0.30 (-1.17 - 0.83)	0.24 (0.16 - 0.32)	0.15**	-0.02
All/most corrupt: gov officials	0.14 (-0.01 - 0.37)	0.40 (-1.72 - -1.36)	0.24 (0.17 - 0.32)	0.12	-0.05
Trust somewhat/a lot: local gov	0.63 (0.44 - 0.87)	0.68 (0.18 - 2.36)	0.63 (0.50 - 0.76)	0.04	0.01
Leaders should not favor own group	0.40 (0.14 - 0.52)	0.25 (-0.38 - 0.70)	0.35 (0.29 - 0.41)	-0.09	-0.02
Trust CDP	0.57 (0.43 - 0.75)	0.53 (-0.55 - -0.41)	0.51 (0.39 - 0.62)	-0.02	0.04

Notes: The value displayed for t-tests are the differences in the means across the groups. Wild Cluster Bootstrapped standard errors are clustered at the region level. Region fixed effects are included in all estimation regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.
Data source: Afrobarometer survey
[1]: Experimental refers to only MCC Phase II treatment municipalities.

Table A2: Treatment groups are statistically similar on survey measures at baseline.

Placebo Checks

Despite the random assignment of treatment status, it is worth checking that variables that should not be affected by the announcement of and creation of land offices in municipalities do not change differentially between treatment and control municipalities, to lend support to the causal argument. The electoral returns are relatively sparse in this regard: only the number of seats available in a given municipality, which is determined by a formula, is a good placebo. However, table A3 presents the same specification as throughout, with this placebo as the outcome.

In the Afrobarometer data, however, we can look at the provision of other local public goods in the municipality, perceptions of other levels of government and other functionings of government, and opinions about national political issues as placebo checks. Tables A4, A5 and A6 report these checks, respectively, and indeed, we see no treatment effect on any of these outcomes.

VARIABLES	(1) Seats Available	(2) Seats Available	(3) Seats Available
Treatment	-3.470	-3.470	-3.470
	(4.409)	(5.597)	(5.597)
2012	1.825	1.825	1.979
	(4.488)	(1.438)	(1.591)
Treatment*2012	-1.492	-1.492	-1.645
	(6.263)	(1.602)	(1.744)
2016	-0.955	-0.955	-1.194
	(4.535)	(1.503)	(1.642)
Treatment*2016	2.889	2.889*	3.128*
	(6.296)	(1.681)	(1.797)
Constant	48.10***	48.10***	48.02***
	(4.720)	(5.271)	(3.003)
Observations	175	175	175
R-squared			0.017
Number of pairs	29	29	29
Pair FE	No	No	Yes
Cluster SE	None	Pair	Pair

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table A3: Placebo Check: Number of Council Seats Available

	(1) Electricity grid	(2) Piped water	(3) Sewage	(4) School	(5) Police station	(6) Health clinic
Treatment	-.1917588	-.3594043	-.0355205	-.3390796	.5180141	.2306639
	(0.67)	(0.06)	(0.40)	(0.08)	(0.10)	(0.58)
2012	-.1593666	-.1231068	-.0353802	-.0523584	.6888804	.4746795
	(0.68)	(0.73)	(0.41)	(0.55)	(0.08)	(0.35)
Treatment*2012	.3716798	.3631331	.0555865	.3912367	-.5919516	-.3163727
	(0.50)	(0.29)	(0.42)	(0.06)	(0.17)	(0.65)
2015	-.0511418	.7703703	.0251802	.0378365	-.2996438	.0695917
	(0.19)	(0.00)	(0.43)	(0.75)	(0.19)	(0.77)
Treatment*2015	.1321355	.0506836	.0662207	-.1625539	.239895	-.0463234
	(0.62)	(0.86)	(0.47)	(0.60)	(0.27)	(0.95)
Constant	.1741187	.2953581	.018679	1.027353	-.2965989	.2386938
Observations	448	448	448	448	448	448
R^2	0.131	0.763	0.090	0.211	0.206	0.105
Number of regions	12	12	12	12	12	12
Fixed Effect	Region	Region	Region	Region	Region	Region

Wild Cluster Boostrapped p-values in parentheses
Data source: Afrobarometer survey

Table A4: Placebo Check: Public services in Survey Enumeration Areas

	(1)	(2)	(3)	(4)
	Gov handling crime well	Gov handling health well	Gov handling education well	Gov handling water well
Treatment	-.0741821	.1851171	-.0270661	-.0179442
	(0.71)	(0.32)	(0.87)	(0.93)
2012	-.0551552	.2612381	.1489857	-.1478423
	(0.73)	(0.21)	(0.38)	(0.42)
Treatment*2012	.1355472	-.1597682	-.0196819	.0705859
	(0.51)	(0.39)	(0.92)	(0.69)
2015	.0553903	-.301878	-.2471382	.0595489
	(0.67)	(0.01)	(0.01)	(0.55)
Treatment*2015	-.077262	.0366653	-.1032353	-.2610781
	(0.66)	(0.80)	(0.46)	(0.01)
Constant	.5778868	.3678669	.5515825	.3699182
Observations	407	430	429	430
R^2	0.003	0.056	0.070	0.046
Number of regions	12	12	12	12
Fixed Effects	Region	Region	Region	Region

Wild Cluster Boostrapped p-values in parentheses

Data source: Afrobarometer

Table A5: Placebo Checks: Perceptions of how well the government is providing other public goods

	(1)	(2)	(3)
	Opposition parties should cooperate with the government	President should be monitored by NA	Officials often/always go unpunished
Treatment	-.1216905	-.037526	-.1412026
	(0.31)	(0.85)	(0.16)
2012	-.1370136	.1417801	.0055167
	(0.32)	(0.07)	(0.94)
Treatment*2012	.1399332	.080229	.0874819
	(0.41)	(0.60)	(0.39)
2015	.2180273	.0614474	.0764117
	(0.04)	(0.70)	(0.51)
Treatment*2015	-.0476423	-.0146765	.0286156
	(0.77)	(0.92)	(0.85)
Constant	.816052	.6099668	.6981749
Observations	424	424	394
R^2	0.039	0.051	0.026
Number of regions	12	12	12
fe	Region	Region	Region

Wild Cluster Boostrapped p-values in parentheses

Data source: Afrobarometer survey

Table A6: Placebo checks: attitudes about national politics

	(1)	(2)	(3)	(4)
	Effective # Parties	Effective # Parties	Effective # Parties	Effective # Parties
VARIABLES	(Votes)	(Seats)	(Votes)	(Seats)
Treatment	0.0122	-0.110	-0.00222	-0.0631
	(0.145)	(0.117)	(0.106)	(0.0931)
2012	0.457***	0.188*	0.282***	0.133
	(0.130)	(0.104)	(0.0979)	(0.0892)
Treatment*2012	0.251	0.0340	0.152	-0.0103
	(0.182)	(0.117)	(0.117)	(0.0915)
2016	-0.148	0.114	0.00726	0.104
	(0.132)	(0.125)	(0.111)	(0.109)
Treatment*2016	0.0521	0.281*	0.124	0.235
	(0.187)	(0.151)	(0.153)	(0.143)
Constant	2.156***	1.624***	1.623***	1.408***
	(0.0808)	(0.0752)	(0.0645)	(0.0605)
Observations	175	175	175	175
R-squared	0.229	0.204	0.221	0.169
Number of comp	29	29	29	29
Pair FE	Yes	Yes	Yes	Yes
Cluster SE	Pair	Pair	Pair	Pair
Measure	Laasko & Taagepera	Laasko & Taagepera	Golosov	Golosov

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table A7: Two measures of effective numbers of parties

Robustness Checks

Alternate measures of Competitiveness

Although Golosov's method of computing the effective number of parties is preferable when there is a dominant party, the more traditional Laakso & Taagepera formula shows similar results (including the lack of significant differences between treatment and control municipalities in any year).

CDP Performance

Although we saw above that the previously-ruling party, the CDP, was more likely to contest elections in 2016 in those municipalities which had received land offices, despite a large drop in the share of municipalities they contest nationwide, it is interesting to see if there is any difference in voter responses to this party due to treatment. However, a simple regression with the vote share won by the CDP ignores the extensive margin: that is, the outcome is only non-zero in municipalities where

the CDP chose to won. In order to examine the response of the CDP's vote share to treatment, then, we must use a method to account for the extensive margin which determines whether we observe the outcome of interest: that is, whether the CDP contests. A Heckman selection model explicitly models this extensive-margin 'selection' decision, as well as the performance on the outcome of interest. It requires, however, at least one variable to influence the outcome but not participation, so as to separately identify the two equations. I use the CDP's incumbency as this variable: although we would normally anticipate that incumbent parties find it easier to run again, using the levers of state for their own campaign purposes, the 'special delegations' that took over municipal governance in the aftermath of the 2014 unrest mean that the winner of the 2012 elections was not actually in power during the 2016 election campaign. However, Lierl and Holmlund (2017) demonstrate that voters do favor the incumbent party in 2016, as they have more information about their performance. Therefore, it seems reasonable that the vote share received by the CDP will depend to some extent on whether the CDP was incumbent. The results from the Heckman model are shown in columns (3) - (5) of table A8. However, in part because of the small sample size, these models are unstable and in some variations do not converge. The inclusion of the number of parties makes the model stable, and is therefore included.

When we use the Heckman model, we find that there are no significant differences between CDP vote share in treatment and control municipalities, before or after treatment. Therefore, although the party seems to be responding to treatment, it may be that less-informed voters are unable to attribute the land offices to a particular party (consistent with Lierl and Holmlund (2017)'s findings that voters know little about local government performance, even on regularly-used services), or that they do not see the land office as valuable enough to reward politicians for.

Binary Outcomes Model for Afrobarometer Data

The preferred specification for Afrobarometer outcomes presented above uses a Wild Cluster Bootstrap (with a Webb 6-point distribution), with Region fixed effects and clusters at the region level. However, to ensure my results are robust to a variety of modeling choices, I check a variety of alternate specifications. In tables A9 - A13, I show that results are broadly similar across my preferred specification (column (1)), the same bootstrap using survey weights (column (2)), calculating cluster-robust standard errors analytically (columns (3) and (4)). I also have checked robustness to different levels of fixed effects and clustering, as well as to bootstrapping with the Rademacher

VARIABLES	(1) CDP Contest	(2) CDP Vote Share	(3) CDP Vote Share	(4) CDP Contest	(5) /
Treatment	0.00407	0.0226	0.0370	0.0551	
	(0.00394)	(0.0465)	(0.0431)	(0.752)	
2012	0.00488	-0.0892***	-0.0826**	-0.718	
	(0.00505)	(0.0315)	(0.0394)		
Treatment*2012	-0.00488	-0.0210	-0.000751	-0.193	
	(0.00505)	(0.0444)	(0.0488)	(1.170)	
2016	-0.468***	-0.417***	-0.360***	-7.086	
	(0.0973)	(0.0325)	(0.0536)		
Treatment*2016	0.234**	0.0611	0.0282	0.649	
	(0.106)	(0.0441)	(0.0538)		
Number Parties			-0.0243**	0.885***	
			(0.0111)	(0.277)	
CDP Incumbent			0.00963		
			(0.0344)		
athrho					-0.0791
					(0.640)
lnsigma					-2.131***
					(0.0624)
Constant	0.997***	0.609***	0.749***	4.910	
	(0.0229)	(0.0237)	(0.0448)		
Observations	175	175	175	175	175
R-squared	0.354	0.740			
Number of comp	29	29			
Pair FE	Yes	Yes	Yes	Yes	Yes
Cluster SE	Pair	Pair	Pair	Pair	Pair
Model	Linear	Linear	Heckman	Heckman	Heckman

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table A8: CDP Performance on the extensive and intensive margin

2-point distribution (not presented here), with broadly similar results.

In looking at changes in voter perceptions of corruption using the Afrobarometer data above, I used a linear probability model. However, this does not explicitly account for the binary nature of the outcome variables (whether or not most politicians are corrupt), and therefore does not bound projected probabilities to be between zero and one. In fact, if we add the coefficients of these models, the predicted probability that most government officials are corrupt is negative when including all interactions. Therefore, it is worth considering binary outcome models as a robustness check. This effort is complicated by our desire to control for fixed effects (to account for regional heterogeneity in surveyed attitudes and responses). The nationally-representative sample in each wave of the Afrobarometer does not include respondents in both treatment and control municipalities for a given experimental pair very often, so cluster fixed effects will reduce the number of observations used for identification too much. Additionally, most clusters are observed only in one year, so there is no variation in time; in others, there is no variation between treatment and control municipalities. Therefore, I include fixed effects at the region level, which control for unobserved heterogeneity at a slightly larger level so as to include more municipalities. The results from a logit with regional fixed effects and region-clustered analytic standard errors (as the wild or score cluster bootstraps are inconsistent with the logit) are presented in column (5) of tables A9 - A13.

Although we can see minor differences between the various models, the results overall present a consistent pattern: there is a significant decrease in perceptions of corruption among local government councillors in treatment municipalities in 2015. Although we cannot directly compare the magnitudes of coefficients between a linear probability and logit model, the signs and significances are roughly the same.

	(1)	(2)	(3)	(4)	(5)
	Corruption in President's Office	Corruption in President's Office	Corruption in President's Office	Corruption in President's Office	Corruption in President's Offic
Treatment	-.0698054	-.1435174	-.0698054	-.1435174	-.309088
	(0.67)	(0.52)	(0.61)	(0.34)	(0.68)
2012	.0110917	.0362828	.0110917	.0362828	.1445132
	(0.88)	(0.58)	(0.89)	(0.58)	(0.69)
Treatment*2012	.0911531	.1208692	.0911531	.1208692	.3354454
	(0.38)	(0.37)	(0.35)	(0.30)	(0.53)
2015	-.0051325	-.0622281	-.0051325	-.0622281	-.0989417
	(0.96)	(0.62)	(0.96)	(0.54)	(0.85)
Treatment*2015	-.1661832	-.0695833	-.1661832	-.0695833	-.7575623
	(0.36)	(0.67)	(0.29)	(0.64)	(0.36)
Constant	.3591549	.3684992	.3591549	.3684992	
			(0.00)	(0.00)	
Observations	358	358	358	358	358
R^2	0.018	0.110	0.018	0.110	
Number of Regions	12		12		
Fixed Effect	Region	Region	Region	Region	Region
Standard Errors	Wild Bootstrap	Wild Bootstrap	Analytic	Analytic	Analytic
Model	Webb 6-point	Webb 6-point	Linear	Linear	Logit
Weight	Unweighted	Survey weighted	Unweighted	Survey Weighted	Unweighted

Wild Cluster Bootstrapped or Analytic Cluster-Robust p-values in parentheses, cluster at regional level

Data source: Afrobarometer

Table A9: Results robust to a variety of specifications

	(1)	(2)	(3)	(4)	(5)
	Corruption in Gov Officials	Corruption in Gov Officials	Corruption in Gov Officials	Corruption in Gov Officials	Corruption in Gov Officials
Treatment	-.3111226	-.3494559	-.3111226	-.3494559	-1.480553
	(0.05)	(0.05)	(0.01)	(0.00)	(0.01)
2012	-.0632625	-.0579099	-.0632625	-.0579099	-.1887773
	(0.67)	(0.64)	(0.61)	(0.61)	(0.71)
Treatment*2012	.2895166	.3469639	.2895166	.3469639	1.342956
	(0.08)	(0.04)	(0.06)	(0.01)	(0.05)
2015	-.0004007	-.0080935	-.0004007	-.0080935	-.1054177
	(0.99)	(0.90)	(1.00)	(0.90)	(0.77)
Treatment*2015	-.1386811	-.1407866	-.1386811	-.1407866	-.5054067
	(0.48)	(0.45)	(0.43)	(0.44)	(0.53)
Constant	.49182	.4829131	.49182	.4829131	
			(0.00)	(0.00)	
Observations	370	370	370	370	370
R^2	0.039	0.106	0.039	0.106	
Number of Regions	12		12		
Fixed Effect	Region	Region	Region	Region	Region
Standard Errors	Wild Bootstrap	Wild Bootstrap	Analytic	Analytic	Analytic
Model	Webb 6-point	Webb 6-point	Linear	Linear	Logit
Weight	Unweighted	Survey weighted	Unweighted	Survey Weighted	Unweighted

Wild Cluster Bootstrapped or Analytic Cluster-Robust p-values in parentheses, cluster at regional level

Data source: Afrobarometer

Table A10: Results robust to a variety of specifications

	(1)	(2)	(3)	(4)	(5)
	Corruption in Local Gov	Corruption in Local Gov	Corruption in Local Gov	Corruption in Local Gov	Corruption in Local Gov
Treatment	-.2588241	-.300364	-.2588241	-.300364	-1.372686
	(0.19)	(0.10)	(0.06)	(0.02)	(0.05)
2012	-.0878433	-.0632249	-.0878433	-.0632249	-.3338814
	(0.57)	(0.69)	(0.52)	(0.65)	(0.62)
Treatment*2012	.3102148	.3297491	.3102148	.3297491	1.567829
	(0.15)	(0.12)	(0.06)	(0.05)	(0.06)
2015	.0708754	.0706392	.0708754	.0706392	.2485434
	(0.50)	(0.39)	(0.42)	(0.32)	(0.53)
Treatment*2015	-.2395636	-.2164227	-.2395636	-.2164227	-1.055188
	(0.16)	(0.17)	(0.05)	(0.06)	(0.07)
Constant	.3888866	.3778106	.3888866	.3778106	
			(0.00)	(0.00)	
Observations	388	388	388	388	382
R^2	0.039	0.103	0.039	0.103	
Number of Regions	12		12		
Fixed Effect	Region	Region	Region	Region	Region
Standard Errors	Wild Bootstrap	Wild Bootstrap	Analytic	Analytic	Analytic
Model	Webb 6-point	Webb 6-point	Linear	Linear	Logit
Weight	Unweighted	Survey weighted	Unweighted	Survey Weighted	Unweighted

Wild Cluster Bootstrapped or Analytic Cluster-Robust p-values in parentheses, cluster at regional level

Data source: Afrobarometer

Table A11: Similar patterns across specifications

	(1)	(2)	(3)	(4)	(5
	Support term limit	Support term limit	Support term limit	Support term limit	Support t
Treatment	-.1651713	-.1950934	-.1651713	-.1950934	-.83
	(0.47)	(0.40)	(0.27)	(0.25)	(0.
2012	-.023049	-.0325595	-.023049	-.0325595	-.185
	(0.94)	(0.89)	(0.89)	(0.85)	(0.
Treatment*2012	.1908113	.2303963	.1908113	.2303963	.940
	(0.55)	(0.41)	(0.31)	(0.25)	(0.
2015	.1613045	.2036297	.1613045	.2036297	.949
	(0.13)	(0.06)	(0.03)	(0.00)	(0.
Treatment*2015	.034328	-.0178935	.034328	-.0178935	.854
	(0.69)	(0.80)	(0.69)	(0.81)	(0.
Constant	.7313894	.7216691	.7313894	.7216691	
			(0.00)	(0.00)	
Observations	421	421	421	421	4
R^2	0.078	0.105	0.078	0.105	
Number of Regions	12		12		
fe	Region	Region	Region	Region	Reg
ses	Wild Bootstrap	Wild Bootstrap	Analytic	Analytic	Ana
Model	Webb 6-point	Webb 6-point	Linear	Linear	Lo
weight	Unweighted	Survey weighted	Unweighted	Survey Weighted	Unwei

Wild Cluster Bootstrapped or Analytic Cluster-Robust p-values in parentheses, cluster at regional level
Data source: Afrobarometer

Table A12: Similar patterns across specifications

	(1)	(2)	(3)	(4)	(5)
	Trust CDP	Trust CDP	Trust CDP	Trust CDP	Trust CDP
Treatment	.163052	.1736554	.163052	.1736554	.6694361
	(0.37)	(0.44)	(0.21)	(0.20)	(0.20)
2012	-.010422	-.0214322	-.010422	-.0214322	-.0462453
	(0.92)	(0.84)	(0.94)	(0.87)	(0.93)
Treatment*2012	-.0635544	-.0786591	-.0635544	-.0786591	-.2756045
	(0.62)	(0.56)	(0.69)	(0.62)	(0.68)
Constant	.5162566	.5443258	.5162566	.5443258	
			(0.00)	(0.00)	
Observations	229	229	229	229	226
R^2	0.013	0.089	0.013	0.089	
Number of regions	11		11		
Fixed Effect	Region	Region	Region	Region	Region
Standard Errors	Wild Bootstrap	Wild Bootstrap	Analytic	Analytic	Analytic
Model	Webb 6-point	Webb 6-point	Linear	Linear	Logit
Weight	Unweighted	Survey weighted	Unweighted	Survey Weighted	Unweighted

Wild Cluster Bootstrapped or Analytic Cluster-Robust p-values in parentheses, cluster at regional level
Data source: Afrobarometer

Table A13: Results robust to a variety of specifications

VARIABLES	(1) Parties Contesting	(2) Parties Contesting	(3) Parties Contesting
Treatment	0.786	0.747	0.727
	(0.854)	(1.045)	(1.044)
Far from Urban	-0.500	-0.308	0.367
	(0.886)	(0.858)	(1.054)
Treatment*Far from Urban	-0.986	-0.870	-0.936
	(1.262)	(1.117)	(1.154)
Pastoralist	0.500	0.479	1.284
	(0.749)	(0.782)	(1.150)
Treatment*Pastoralist	0.368	0.406	0.407
	(1.038)	(1.185)	(1.177)
Far from Urban*Pastoralist	-0.500	0.229	0.434
	(1.318)	(1.014)	(1.204)
Treatment*Far from Urban*Pastoralist	-0.168	-0.535	-0.649
	(1.788)	(1.305)	(1.355)
2012	1.333	1.333**	1.333*
	(0.886)	(0.665)	(0.665)
Treatment*2012	-0.190	-0.190	-0.190
	(1.207)	(0.757)	(0.757)
2012*Far from Urban	-0.933	-1.007	-1.133
	(1.284)	(0.831)	(0.841)
Treatment*2012*Far from Urban	0.390	0.464	0.590
	(1.806)	(1.002)	(0.990)
2012*Pastoralist	-0.762	-0.762	-0.762
	(1.059)	(0.890)	(0.890)
Treatment*2012*Pastoralist	1.696	1.696	1.696
	(1.468)	(1.091)	(1.091)
2012*Far from Urban*Pastoralist	0.362	0.435	0.562
	(1.885)	(1.439)	(1.455)
Treatment*2012*Far from Urban*Pastoralist	-0.896	-0.970	-1.096
	(2.544)	(1.546)	(1.561)
2016	-1.500*	-1.500***	-1.500***
	(0.886)	(0.341)	(0.341)
Treatment*2016	-0.357	-0.357	-0.357
	(1.207)	(0.498)	(0.498)
2016*Far from Urban	1.100	1.174**	1.300**
	(1.284)	(0.480)	(0.471)
Treatment*2016*Far from Urban	0.557	0.484	0.357
	(1.806)	(0.564)	(0.524)
2016*Pastoralist	0.786	0.786	0.786
	(1.059)	(0.540)	(0.540)
Treatment*2016*Pastoralist	-1.775	-1.775**	-1.775**
	(1.468)	(0.794)	(0.794)
2016*Far from Urban*Pastoralist	-0.386	-0.437	-0.416
	(1.987)	(0.779)	(0.790)
Treatment*2016*Far from Urban*Pastoralist	1.775	1.826*	1.805*
	(2.620)	(0.984)	(0.970)
Observations	175	175	175
R-squared	0.378		0.416
Pair FE	No	No	Yes
Cluster SE	None	Pair	Pair
Number of comp		29	29

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Data source: CENI Electoral Returns

Table A14: Both pastoralism and distance from urban areas remain robust to inclusion of the other