

## CSAE Working Paper WPS/2018-11

# Cutting the Head off the Snake: An Empirical Investigation of Hierarchical Corruption in Burkina Faso

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### Abstract

*This paper exploits a natural experiment to assess whether arresting the head of Burkina Faso's Customs Ministry for corruption charges had any trickle-down effect on the petty corruption behaviour of lower-ranked customs officials. I measure petty corruption using a unique micro-dataset collated by the USAID West Africa Trade Hub, which records over 257,000 bribes paid by truck-drivers across six West African countries from 2006 to 2012. Using a difference-in-difference methodology, I find that the arrest significantly reduced the bribe-taking behaviour of Burkinabé customs officials relative to non-customs officials in Burkina Faso and to customs officials in three neighbouring countries. These results are robust to a variety of robustness tests and alternative explanations. Overall, my findings support the select use of leadership decapitation as an anti-corruption tool, though the dynamics of the results post-arrest point to the need for further research before its long-term effectiveness can be more confidently asserted.*

**Key words:** corruption, hierarchies, leadership decapitation

**Acknowledgements:** I am greatly indebted to both Pramila Krishnan and Doug Gollin for their helpful supervision of this work. I am also very grateful to the Borderless Alliance for providing me with access to their dataset, as well as to Toni Oki for informing me of its existence.

*Dans ce pays-ci, il est bon de tuer de temps en temps un amiral pour encourager les autres*

Voltaire (1759), *Candide*

## I. Introduction

On the evening of the 31<sup>st</sup> December 2011, as the rest of Burkina Faso was preparing for the New Year, the gendarmerie silently gathered outside the house of Ousmane Guiro, the longstanding Director of Burkina Faso's Customs Ministry. Earlier in the day, the armed police had raided the house of his nephew, Sana Wahabo, and found two suitcases containing nearly US\$4million in cash that were immediately traceable back to Guiro. Upon learning of the gendarmerie's discovery, Guiro quickly gave himself up and was arrested that same evening. The next day Ouagadougou was alight with rumours – something big had happened to the Director of Customs the night before, but what? To quell reports of his suicide, the President of Burkina Faso Blaise Compaoré and the Head of the Gendarmerie Hubert Yaméogo appeared on a national news programme on the 2<sup>nd</sup> January to outline Guiro's crimes and to deliver an official decree relieving him of his duties.

This paper analyses whether this event had any 'trickle down' effect on the petty corruption behaviour of lower ranked customs officials. I measure petty corruption using a unique micro-dataset that records 50,816 bribes paid by truck drivers to various officials in Burkina Faso over 7,804 journeys between 2006 and 2012. To isolate the causal effect of interest, I employ a difference-in-difference identification strategy, exploiting the fact that three different types of officials solicit bribes in Burkina Faso.

Overall, I find that the arrest of Ousmane Guiro *decreased* the value of bribes taken by customs officials at each individual stop by 28% and *increased* the number of trucks they let pass without soliciting a bribe by 10%. This result is robust to alternative counterfactuals, a test of parallel trends and to different specifications of the post-event window. My findings provide initial optimism for the use of leadership decapitation as an effective anti-corruption tool, though the fact that the results become less pronounced through time points to the need for further research before its long-term effectiveness can be more confidently asserted.

This paper proceeds as follows: Section II reviews related literature; Section III describes the event in more detail; Section IV describes the data used; Section V outlines a theoretical model; Section VI discusses my empirical methodology and baseline results; Section VII presents a series of robustness checks; Section VIII tests a pair of alternative explanations and Section IX concludes.

## **II. Literature Review**

The theoretical study of corruption in hierarchies has an established history in development economics. Seminal papers include Basu *et al.* (1992), who criticise Becker and Stigler's (1974) workhorse corruption model for failing to take into account the possibility that bribes can move up the chain of officials. In their framework, hierarchical corruption is best conceptualised as a dynamic bargaining game, whereby indicted officials can bribe their superiors to avoid being punished. Bac (1996) and Bag (1997) follow up this paper by studying how effective organisation design can mitigate corruption, arguing that a decentralised 'steep' hierarchy is superior to a centralised 'flat' hierarchy, provided the monitoring technology exhibits constant or decreasing returns to scale. Kofman and Lawarree (1992) and Mishra (2001) pursue the related question of how vertical hierarchies compare to horizontal structures (where several supervisors monitor the agent and compete for the reward or bribe income), arguing that the latter tends to induce less corruption but is not necessarily welfare enhancing.

Despite these theoretical advances, the empirical study of hierarchical corruption remains largely non-existent. One reason for this is that changes to bureaucratic structures typically come about organically – rather than via random junctures – which makes natural experiments difficult to identify. A more obvious reason is that corruption is an inherently difficult dependent variable to measure in the first-place (Olken, 2012). In analysing how a 'shock' to the top of a hierarchy permeates down to the bribe-taking behaviour of lower-ranked officials, this paper represents, to the best of my knowledge, the first attempt to empirically assess the relationship between hierarchies and corruption. In doing so, I hope to inspire a greater empirical impetus to this topic as well as to resolve some of the theoretical ambiguities manifest in the literature.

This study also ties in to four other papers that have used this dataset to study roadside corruption in West Africa. Oki (2016) makes the most general use of the dataset, aggregating data across all six countries to assess how road traffic affects the intensity of bribe-taking. Cooper (2015) also aggregates across countries to explore the interaction between petty bribery and election cycles, finding that officials extort more in the build up to an election, but only when the election is competitive. Foltz and Bromley (2010) use an incomplete version of this dataset to find evidence of price discrimination behaviour amongst officials, corroborating the findings Olken and Barron (2009) glean from their similar dataset in Indonesia.

My paper bears most similarity to a study by Foltz and Opoku-Agyemang (2015), who find that that the doubling of police salaries in Ghana *increased* the bribe-taking behaviour of Ghanaian policeman, in contrast to the predictions of mainstream economic theory and to results gleaned from laboratory experiments (Armantier and Body, 2011; Van Veldhuizen, 2013). Their findings caught popular interest at the time, being reported by MSN News and The Economist (The Economist, 2016), though were generally used to support a pessimistic narrative that corruption was an unfortunate but intractable feature of developing countries. In showing that a different type of intervention *can* lower corrupt behaviour, I hope to undermine this narrative somewhat and argue that there is definite scope for anti-corruption policy in the future.

Finally, my paper also contributes to the broader question of whether leadership decapitation can effectively destabilise an undesirable status quo. The strategy of targeting leaders has been used extensively in other contexts: for instance, it has characterised the USA's approach to both the war on terror and the war on drugs, through their pursuit of Osama Bin Laden and Pablo Escobar respectively. The effectiveness of this strategy in these contexts has been studied extensively by other papers: Rigterink (2018) looks at how targeting leaders with drone strikes affects the internal organisation of terrorist groups; Phillips (2015) assesses how leadership removal affects violence between Mexico's drug cartels; and Johnston (2012) looks at whether decapitation can be effective in combatting counterinsurgency campaigns.

### III. The Arrest of Ousmane Guiro

Ousmane Guiro was appointed as Director of Customs in early 2006. Rumours of Guiro's corruption had circulated ever since an accusation of fraudulent tax exemption was levied against him in 2008, a case that was subsequently dropped in opaque circumstances. This controversy was exacerbated by the fact that under his auspices, the Customs Ministry had become the 'most corrupt department in Burkina Faso' every year since 2007 according to surveys conducted by REN-LAC, an independent anti-corruption network (The Economist Intelligence Unit, 2012). Thus, when Guiro was arrested on New Year's Eve in 2011, the news sent shockwaves around Burkina Faso, dominating the headlines for the first few days of 2012 and even receiving coverage on BBC News (BBC News, 2012).<sup>1</sup> Not only had Guiro's Customs Ministry become the grotesque emblem of the wide-scale corruption pervading the country, but, as Jeune Afrique acknowledged at the time, "it was the first instance of such a high personality in Burkinabé politics to be arrested for corruption" (Jeune Afrique, 2012).

While it is important to emphasise the significance of Guiro's arrest, it is equally important to emphasise that this does not *in itself* produce a credible natural experiment. Crucially, for my diff-in-diff strategy to be plausible, the event must have had a degree of unpredictability, in the sense that the agents studied could not have adapted their behaviour in advance, as well as being somewhat isolated, in the sense that the event should not have occurred contemporaneously to other 'shocks' that may have affected Burkinabé officials in some way. Fortunately, the wider circumstances surrounding Guiro's arrest lend themselves to both these requirements.

Firstly, although Guiro had aroused suspicion for quite some time, his actual arrest appears to have come as a total surprise to everyone involved. According to Le Faso, the largest online newspaper in Burkina Faso, the chain of events leading to Guiro's arrest began in early December 2011, when the gendarmerie became suspicious of the extravagant behaviour of several young people living in Sector 18 of Ouagadougou (Le Faso, 2012). Not knowing who they were, the gendarmerie arranged a raid of several

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<sup>1</sup> The arrest even features on BBC's timeline of major events in Burkina Faso (BBC News, 2018), which is incidentally how I became aware of it.

houses to see if they could find evidence of anything which could justify their profligate lifestyle. On the 31<sup>st</sup> December 2011, a small group of gendarmerie raided Sana Wahabo's house, a member of this group, where they found two suitcases containing vast sums of cash. Wahabo initially claimed the money was his; however, after bundles of notes were found with Ousmane Guiro's name on it, Wahabo confessed that he was in fact Guiro's nephew, and that the money belonged to his uncle. This precipitated the raid of Guiro's house in the evening which culminated in Guiro's confession and subsequent arrest – all within 12 hours of when the initial raid took place. The fast-moving and ultimately serendipitous nature of these events show that Guiro was essentially caught 'red handed' – not as a result of a well-publicised and long-running investigation. Not only this, but, as The Economist notes, Guiro was even decorated as an "Officer of the National Order" on December 11<sup>th</sup> (a few weeks before his arrest), one of the country's highest awards (The Economist Intelligence Unit, 2012). Taken together, the weight of qualitative evidence would suggest that Guiro's arrest could *not* have been predicted in advance – even by his own customs officials – supporting the idea that the event constituted a sudden 'treatment' to the agents studied.

Secondly, there is compelling evidence that the arrest was an isolated occurrence and not symptomatic of a wider crackdown on corruption at the time. For example, Freedom House note the Guiro arrest as the only notable incident in their treatment of anti-corruption policy in Burkina Faso 2011-2012 (Freedom House, 2013). Burkina Faso's Ease of Doing Business Rank also remained virtually unchanged during this period: moving from 151<sup>st</sup> to 150<sup>th</sup> (The World Bank, 2012). This is encouraging for my identification, for if Guiro's arrest was simply one of several anti-corruption measures brought about at the same time it would be difficult to attribute any subsequent changes in corruption to the arrest itself.

To provide a complete contextualisation of the event, Guiro was replaced at the head of the Ministry by Kuibila Jean Sylvester, a long-serving civil servant who had similarly risen through the ranks in the Customs Ministry. There is no qualitative evidence of Sylvester being involved in any corruption scandals (either in his 3 years as Director or in his 20 years in the Ministry prior) and when he stepped down in 2015 he did so voluntarily and has retained an active role in the Customs Ministry ever since (Sidwaya, 2015).

## IV. Data

I assess the petty corruption behaviour of low-ranked officials using a unique micro-dataset collated by the USAID-West Africa Trade Hub project, which analyses the bribe payments paid by truck drivers on their cross-country voyages of West Africa. In the dataset, each checkpoint stop for a driver represents an individual data point, which produces 257,100 observations across six West African countries across six years (2006-2012). The study ends in September 2012, which offers 9 months of post-arrest data to analyse. Each line in the dataset observes which driver paid the bribe, where and when the bribe took place, how much was paid (sometimes 0), which type of official solicited the bribe as well as a range of truck and driver characteristics (e.g. nationality of the truck and driver).

The data was collected as follows: local enumerators would meet with truck drivers in depots at the start of their journey and ask drivers if they would be willing to contribute to a research project on corruption by filling out a survey for each time they were stopped at a checkpoint on their route. These surveys were then collected by another team of enumerators wherever the driver finished his journey. Surveys were only distributed to drivers travelling across a whole trade route and with papers and cargo in order. Drivers were not compensated but willingly participated in the study in the hope that the information they conveyed would be relayed on to authorities.

### **Is the data credible?**

As the surveys were only distributed with drivers with correct papers/cargo, the data represents a lower bound on bribe values paid, since trucks without correct papers are guilty of an infraction, and hence likely to pay higher bribes. An obvious concern with the data is that the bribes may be misreported, particularly as drivers have an incentive to exaggerate the severity of bribery in the hope that it would precipitate a greater crackdown on corruption. However, since I focus on relative (rather than absolute) levels of bribery, this concern is less relevant to my analysis, under the fairly unrestrictive assumption that reporting bias was unaffected by Guiro's arrest. Another concern relates to the drivers unwillingness to disclose their bribe-paying behaviour, since paying

bribes is technically illegal. However, since bribery is so commonplace in Burkina Faso, it is not a taboo topic of conversation and hence unlikely to prompt misreporting. Oki (2016) and Foltz and Opoku-Agyemang (2015) both make similar arguments.

Summary statistics of the data are outlined below:

**Table 1: Descriptive Statistics**

Variable	<i>Pre-Arrest</i>			<i>Post-Arrest</i>		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
<b>Bribe value (CFA)</b>	<b>41,038</b>	<b>1,429</b>	<b>1,431</b>	<b>9,778</b>	<b>1,362</b>	<b>1,315</b>
- Customs	26,084	1,161	1,542	6,696	959	1,091
- Gendarmerie	6,348	2,058	1,013	1,493	2,270	1,244
- Police	8,606	1,780	1,084	1,589	2,211	1,416
<b>No Bribe dummy</b>	<b>41,038</b>	<b>0.122</b>	<b>0.327</b>	<b>9,778</b>	<b>0.199</b>	<b>0.399</b>
- Customs	26,084	0.166	0.372	6,696	0.266	0.442
- Gendarmerie	6,348	0.042	0.200	1,493	0.073	0.260
- Police	8,606	0.045	0.208	1,589	0.033	0.179
<b>Checkpoints per trip</b>		<b>6.5</b>			<b>6.6</b>	
- Customs		4.2			4.5	
- Gendarmerie		1.0			1.0	
- Police		1.3			1.1	

There are several things of note: first, from the outset, it is worth pointing out that customs officials were significantly more likely to let trucks go without soliciting a bribe after the arrest (27%) than before (17%). While these are descriptive results and should not be interpreted causally, this nevertheless serves as an interesting precursor to the more careful empirics conducted in Section VI. Second, it is also worth noting that the average bribe extorted by customs officials is significantly less than the police or gendarmerie – both before and after Guiro’s arrest – in contrast to the results of the REN-LAC surveys, which consistently rank the customs agency as the most corrupt ministry in Burkina Faso. This could be explained if the agency’s corruption manifested itself more in other activities, or if people’s perceptions of corruption do not correlate perfectly with reality, as Olken (2009) finds in Indonesia. Third, neither the average number of checkpoints per trip nor the composition of officials encountered changes

substantially before and after the arrest – a fact used to corroborate my discussion of causal mechanisms in a later section. Finally, the scales involved show that roadside bribery in Burkina Faso is a non-negligible problem: the average bribe solicited at each stop is 1,416 CFA (US\$2.78)<sup>2</sup> and each driver is stopped on average 6.5 times, meaning the average driver would pay 9,204 CFA (US\$18.07) in bribes to pass through Burkina Faso. This is a non-trivial amount, especially when you consider that 43% of the Burkinabé population live on less than \$1.90 per day (The World Bank, 2018).

To provide a further illustration of the experimental setting, a map showing the major highway routes and checkpoints in Burkina Faso is outlined below (see Figure 1).

**Figure 1: Map of Major Trucking Highways in Burkina Faso**



*Source: Author created. Blue dots represent checkpoints in major cities (A) and at borders (B). White dots represent less frequented checkpoints. Geocoded on Google Maps.*

<sup>2</sup> All \$ conversions are done using the prevailing exchange rates at the end of the data collection period.

## V. Theoretical Model

Before a theoretical framework is sketched out, it is helpful to envisage the circumstances by which bribes are solicited. When approaching a checkpoint, truck drivers are typically flagged down by an official (with their arm if during the day, or with a torch if at night). The official will usually ask the driver for his license, cargo and registration papers. If the driver does not hand over a bribe in advance, the official can refuse to return the drivers documents until a bribe is received. If the driver still refuses, the official can threaten to not let the driver pass, confiscate his cargo or even resort to physical violence.<sup>3</sup>

It is also helpful to consider the bureaucratic environment within which customs officials operate. Crudely, the organisation structure of a civil service in a developing country is hierarchical, in which low-ranked bureaucrats are accountable to their ministerial head who is in turn accountable to the executive government. Abstracting from any ‘middlemen’, this creates a simple accountability framework comprised of three players: *G* (the government); *B* (the ministerial boss) and *C* (the customs official).



Prima facie, I speculate three possible effects Guiro’s arrest may have had: it may have changed the ‘corruption-type’ of player *B*; it may have generated a ‘signal’ pertaining to *G*’s willingness to monitor *C* directly; or it may have generated general uncertainty, the type that is present in more general instances of leadership transition. In Section VIII I run a series of experiments to test between these alternative explanations. For the purpose of this section, however, I focus on the first of these effects: how having a less-corrupt supervisor affects the decision-making of lower-ranked officials.

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<sup>3</sup> Foltz and Bromley (2010) note an incident in Kita, Mali, where a policeman shot at a transport van that had refused to stop at a checkpoint, killing one of the apprentices hanging off the back of the van

In focusing on the relationship between  $B$  and  $C$ , one assumption which immediately becomes important is what we assume happens to the bribes after  $C$  solicits them: do they remain in  $C$ 's pocket or are they passed up the chain to  $B$ ? This question cannot be answered by the dataset and is likely to vary substantially by context, depending on how 'institutionalised' or 'opportunistic' petty corruption is (Baaz and Olsson, 2011; Wade, 1988). Nonetheless, which assumption we choose is vitally important, since it can drive divergent and potentially ambiguous results.

### **Case I: Bribes are kept by the officials**

To see this, assume initially that a low-ranked official keeps the entirety of bribes he<sup>4</sup> solicits. Assume also that the official earns a wage  $w$ , and chooses how much effort  $e$  to expend extracting bribes  $b$  in order to maximise expected utility  $U$ .

The official is caught for engaging in illegal corruption behaviour with probability  $p(e, \pi(v))$ . The probability he is caught depends positively on how hard he tries to solicit bribes:  $e$  as well as on how carefully he is monitored by his supervisor  $B$ :  $\pi(v)$ . This, in turn, depends on how corrupt  $B$  is ( $v$ ).

If not caught, the official  $C$  receives a bribe  $b(e)$  which supplements his wage  $w$ . The bribe value is increasing in effort. If caught, he is sent to  $B$  who must decide on his punishment.  $B$  either fires  $C$ , in which case  $C$  receives outside option  $w_0$  ( $w_0 < w$ ), or allows him to return to work 'scot-free', in which case he continues to receive a wage of  $w$ .<sup>5</sup> The ministerial head fires the official with probability  $q(v)$ .

**Assumption 1:**  $\pi'(v) < 0$  *Corrupt ministerial heads are less likely to monitor lower-down officials.*

**Assumption 2:**  $q'(v) < 0$  *Corrupt ministerial heads are less likely to punish indicted officials.*

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<sup>4</sup> The masculine subject is used throughout, since both drivers and officials are almost exclusively male.

<sup>5</sup> For simplicity, I assume that punishment takes the form of a binary outcome. This model could plausibly be extended to account for a situation where the boss can pick from a spectrum of punishments (ranging from soft to severe), though this layer of complexity changes nothing of the intuition that follows.

The logic behind these assumptions is as follows: first, corrupt ministerial bosses are less likely to acquiesce to norms pertaining to the unacceptability of corruption (Harris, 2018), and hence be less motivated to exert personal effort in stamping it out. Second, corrupt bosses may adopt a strategy of wilful oversight in the hope that this invokes a reciprocal response, adopting a policy which tacitly signals: “I won’t monitor you if you don’t monitor me”. Third, establishing proper monitoring channels is expensive, and a corrupt ministerial head may be reluctant to unnecessarily spend the Ministry’s resources if this diminishes the amount he is able to ‘skim off’ for his own private uses. Finally, lower down officials may be more likely to ‘have dirt’ on corrupt bosses (if we acknowledge the possibility for upstream monitoring) which could facilitate a tit-for-tat like strategy, whereby indicted officials threaten to ‘punish’ their superiors by revealing this information should they be punished first. Faced with these incentives, corrupt bosses are more likely to ‘collude’ with lower-ranked officials by turning a blind eye to their various misdemeanours.

Regardless of being caught or punished, officials incur a cost  $c(e)$  of exerting each unit of effort when soliciting bribes. The cost can be conceptualised as the time commitment it takes to solicit bribes, or more broadly as the psychological shame the official bears knowing he is engaging in an illegal activity.

Assuming risk-neutrality, the official chooses  $e$  to maximise:

$$(1) U = [1 - p(e, \pi(v))][w + b(e)] + p(e, \pi(v))[(1 - q(v))w + q(v)w_0] - c(e)$$

Subject to  $b, e, v \geq 0$ . When  $U < w$ , no bribe is solicited. Otherwise, assuming  $U$  is concave in effort ( $U_e'' < 0$ ), optimal  $e^*$  is derived from the first order condition:

$$(2) U_e'(e, v) = [1 - p(e, \pi(v))]b'(e) - p_e'(e, \pi(v))[w + b(e) - [(1 - q(v))w + q(v)w_0]] - c'(e) = 0$$

Rearranging:

$$(3) [1 - p(e, \pi(v))]b'(e) = p_e'(e, \pi(v))[w + b(e) - [(1 - q(v))w + q(v)w_0]] + c'(e)$$

Or, intuitively, effort is chosen to equalise expected marginal benefit with expected marginal cost.

To observe how  $v$  effects bribe-taking, I implicitly differentiate (2) with respect to  $v$  to give:

$$(4) U''_{ev}(e, v) = -\pi'(v) \left[ p'_\pi(e, \pi(v))b'(e) \right. \\ \left. + p''_{e\pi}(e, \pi(v)) \left[ w + b(e) - [(1 - q(v))w + q(v)w_0] \right] \right] \\ + q'(v)[p'_e(e, \pi(v))](w_0 - w)$$

Where:

$$-\pi'(v) \left[ p'_\pi(e, \pi(v))b'(e) + p''_{e\pi}(e, \pi(v)) \left[ w + b(e) - [(1 - q(v))w + q(v)w_0] \right] \right] \\ > 0$$

Due to changes in monitoring, and:

$$q'(v)[p'_e(e, \pi(v))](w_0 - w) > 0$$

Due to changes in the probability of being punished. Adding these terms together makes the total differential positive, implying that, when officials keep their bribes, optimal effort and bribe values unambiguously *decrease* when the corrupt ministerial head is replaced by a less corrupt one.

### **Case II: Bribes are passed up the hierarchy**

However, things become more complicated when we incorporate the possibility for bribes to move up the chain of officials. As per Basu *et al.* (1992), we could imagine this could take the form of a bargaining game, where an indicted official is able to bribe  $B$  in order to keep his job. To keep things simple, though, it is easier to imagine that  $B$  and  $C$  are engaged in a sharecropping-like agreement, in which the official  $C$  agrees to pass on a fixed proportion ( $\tau$ ) of the bribes they solicit to  $B$ . How much  $B$  demands is, in turn, determined by how corrupt he is ( $v$ ).

**Assumption 3:**  $\tau'(v) > 0$  *Corrupt ministerial heads demand a greater proportion of bribes.*

With this additional twist, the objective function facing  $C$  becomes:

$$(5) U = [1 - p(e, \pi(v))][w + (1 - \tau(v))b(e)] + p(e, \pi(v))[[1 - q(v)]w + q(v)w_0] - c(e)$$

The first-order condition is now:

$$(6) U'_e(e, v) = [1 - p(e, \pi(v))](1 - \tau(v))b'(e) - p'_e(e, \pi(v)) [w + (1 - \tau(v))b(e) - [(1 - q(v))w + q(v)w_0]] - c'(e) = 0$$

And the second-order condition becomes:

$$(7) U''_{ev}(e, v) = -\pi'(v) \left[ p'_\pi(e, \pi(v))(1 - \tau(v))b'(e) + p''_{e\pi}(e, \pi(v)) [w + (1 - \tau(v))b(e) - [(1 - q(v))w + q(v)w_0]] \right] + q'(v)[p'_e(e, \pi(v))](w_0 - w) + \tau'(v)[p'_e(e, \pi(v))b(e) - [1 - p(e, \pi(v))]b'(e)]$$

Unlike before, the total sign of this differential is now uncertain, illustrating the idea that once bribes can be passed onwards, the effect of removing a corrupt ministerial head becomes theoretically ambiguous. On the one hand, the fact that officials are now more likely to be monitored and punished prompts a reduction in their bribe-effort (monitoring channel); on the other hand, the fact that their bribes are now effectively 'taxed' less prompts an increase in their bribe-effort (tax channel). Which effect dominates depends crucially on the internal dynamics of bribe-taking, as well as how sensitive both the tax rate and monitoring probability are to  $B$ 's own personal engagement in corruption. This theoretical ambiguity cautions us against being overly-complacent about the effectiveness of leadership decapitation, and makes the need for hierarchical corruption to be empirically investigated all the more compelling.

## VI. Identification Strategy and Baseline Results

My baseline identification strategy relies on a diff-in-diff methodology, comparing the ‘treatment’ group of Burkinabé customs officials to the ‘control’ group of Burkinabé gendarmerie and policeman. Let  $Y_{ijt}$  be the value of bribe (in CFA) extorted by an official on truck  $i$  at checkpoint  $j$  at time  $t$ . My basic equation of interest becomes:

$$(8) Y_{ijt} = \alpha + \beta_1 Customs_{ijt} + \beta_2 PostArrest_t + \beta_3 Customs_{ijt} * PostArrest_t + \gamma X_i + T + \delta_j + \varepsilon_{ijt}$$

where  $\beta_3$  is the diff-in-diff parameter of interest;  $T$  is a vector of year-month dummies;  $\delta_j$  are checkpoint fixed-effects; and  $X_i$  is a vector of truck and driver characteristic dummies, such as whether the driver is a foreigner, whether the truck is a ‘newer’ tanker truck,<sup>6</sup> or whether the truck is heading from the port.<sup>7</sup> By including checkpoint fixed-effects, I focus on *within* checkpoint variation, controlling for the fact that some checkpoints are inherently more lucrative than others.<sup>8</sup> This would bias my coefficients if the profile of checkpoints visited changed before and after Guiro’s arrest.<sup>9</sup> The inclusion of year-month dummies further limits my variation to within month. While confounding time trends at the aggregate level are captured in the post-arrest dummy, this estimate masks significant seasonality in bribe-taking behaviour which is useful to control for (Oki, 2016; Cooper, 2015). I also include a Ramadan dummy in vector  $T$ , since the psychological costs of engaging in corruption may be higher when the importance of moral behaviour is greater emphasised.<sup>10</sup>

Other papers using this dataset express the dependent variable in logarithms to moderate the influence of outliers. I choose not to do this, because outliers are rare in Burkina Faso and because analysing the raw numbers lends itself to a more natural interpretation. In any case, my results are robust to both the exclusion of outliers and a

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<sup>6</sup>Tankers are generally newer and hence less likely to have identifiable infractions.

<sup>7</sup>Trucks returning from the port are more likely to be empty and hence less likely to violate weight restrictions, which could attract higher bribes.

<sup>8</sup>As Oki (2016) finds, checkpoints located in medium-traffic areas and those near country borders are able to extort the most in bribes.

<sup>9</sup>This is not implausible: since many checkpoints are unofficial, two trips taken 6 months apart may encounter entirely different sets of checkpoints.

<sup>10</sup>Burkina Faso is a predominantly Muslim country (World Atlas, 2017).

logarithmic specification. I cluster standard errors at the truck level because it is likely to be the greatest source of heteroskedasticity due to unobservable characteristics. Again, my results are robust to an alternative clustering at the checkpoint level.

### Baseline Results

**Table 2: Baseline Bribe Value Regressions**

	<i>Preferred Model</i>		
	(1)	(2)	(3)
	Bribes	Bribes	Bribes
Customs	-283.1 <sup>***</sup> (28.23)	-308.5 <sup>***</sup> (27.08)	-236.4 <sup>***</sup> (27.27)
Post Arrest	137.1 (96.47)	140.9 <sup>***</sup> (32.81)	
<b>Customs*Post Arrest</b>	<b>-394.0<sup>***</sup></b> (38.78)	<b>-385.5<sup>***</sup></b> (38.37)	<b>-282.4<sup>***</sup></b> (39.55)
Constant	989.2 <sup>***</sup> (81.39)	1219.1 <sup>***</sup> (34.20)	
Truck Characteristics	Yes	Yes	No
Checkpoint Fixed Effects	Yes	Yes	Yes
Year-Month Fixed Effects	Yes	No	No
Driver Fixed Effects	No	No	Yes
R-squared	0.20	0.18	0.29
Observations	52,065	52,065	50,782

*Notes:* Dependent variable is bribe value in CFA. Standard errors clustered at the truck-level in parentheses. Truck characteristics include a tanker or container dummy, a dummy for whether the truck is heading from the port, a foreign truck dummy and a foreign driver dummy, the latter also interacted with both a foreign truck and AFCON dummy.

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

Column 1 outlines the baseline results. The interaction of customs and arrest dummy variables produces a significant 394 CFA decrease in bribes at each stop, suggesting that Guiro's arrest substantially *reduced* the bribe-taking behaviour of low-ranked customs officials. To put this result into context, 394 CFA represents a ~28% reduction in magnitude compared to the average bribe-value solicited throughout the whole sample period (1,416 CFA).

As a preliminary robustness check, I re-estimate my baseline equation with different variations of fixed effects. A potential concern in my baseline equation is that by including year-month dummies, I take out too much of the temporal variation in the data. To see how much difference this makes, I re-run equation (8) but omit time fixed-effects and present the results in column 2. As the table shows, this omission does not produce large changes to my point estimates.

In contrast to Oki (2016) and Foltz and Opoku-Agyemang (2015), I omit driver fixed effects ( $\mu_i$ ) from my baseline regression. These authors argue that driver fixed-effects control for any unobserved changes in truck 'quality' before and after the event, as well as the potentially confounding effect of truck characteristics unobservable in the data but observable to an official, such as a broken headlight. However, for this to present an endogeneity concern, we would have to believe the unconvincing idea that these are somehow related to the *type* of official that drivers encounter, and that this relationship changed after Guiro's arrest. For this reason, I choose to omit driver fixed effects in my preferred model, though run a regression with driver fixed effects for full disclosure. As column 3 shows, the diff-in-diff parameter remains reassuringly negative and significant, though it is worth noting that the absolute value of the point estimate decreases.

Though the descriptive statistics displayed in Table 1 indicate that customs officials were more likely to let trucks pass without soliciting a bribe after the arrest, I now test this more formally by estimating (8) as a linear probability and probit model, in which the dependent variable becomes a dummy variable which takes the value of 1 if a truck is stopped but does not have to pay a bribe. This proves useful in testing whether my negative results are driven by changes in the *extensive* margin (letting more trucks go), or by changes in the *intensive* margin (extorting a lower amount from the same proportion of trucks).

**Table 3: Probability of Paying No Bribe**

	(1) Linear Probability	(2) Linear Probability	(3) Probit
Customs	0.133*** (0.00469)	0.188*** (0.00543)	0.815*** (0.0294)
Post Arrest	0.193*** (0.0315)	0.0784*** (0.0295)	4.044*** (0.171)
<b>Customs*Post Arrest</b>	<b>0.0957***</b> (0.0114)	<b>0.109***</b> (0.0107)	<b>0.270***</b> (0.0588)
Truck Characteristics	Yes	Yes	Yes
Checkpoint Fixed Effects	No	Yes	No <sup>11</sup>
Year-Month Fixed Effects	Yes	Yes	Yes
Observations	52,065	52,065	52,065
R-squared	0.10	0.22	
Log Likelihood			-17624

Notes: Dependent variable = 1 if no bribe was paid. Standard errors clustered at the truck level in parentheses

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

As Table 3 shows, the crucial interaction term is both positive and highly significant under each specification, which suggests that my results are in large part driven by customs officials letting more trucks pass after Guiro was arrested.

These results provide initial optimism for the view that Guiro's arrest *reduced* the corruption behaviour of lower-ranked customs officials. If my theoretical model is correct, this implies that, in this context, the improved monitoring associated with a 'cleaner' supervisor was more important than the reduced 'tax' rate. The results in Table 3 are largely supportive of this conjecture: the fact that customs officials were significantly more likely to let trucks go after Guiro was arrested seems indicative of a belief that they were more likely to get caught.<sup>12</sup> This conjecture is also supported by several indications that roadside corruption in Burkina Faso is largely decentralised and hence not amenable to institutionalised 'taxation'. First, in conducting ethnographic

<sup>11</sup> I choose not to include checkpoint fixed effects because of the incidental parameter problem.

<sup>12</sup> This would have been harder to defend had the reduction in bribery been driven solely by changes in the *intensive* margin, as the probability of getting caught presumably does not vary wildly by the bribe amount.

fieldwork with drivers and officials in West Africa, Cooper (2015) finds no convincing evidence of bribes being passed upwards in an orchestrated manner. Second, as Table 1 shows, the fact that the average number of customs checkpoints did not change substantially after Guiro's arrest (going from 4.2 to 4.5) suggests a decentralised organisation structure comprised of individual agents making their own decisions.<sup>13</sup> Taking both the baseline results and these insights into account, it appears that leadership decapitation *can* be an effective strategy to tackle petty corruption – but only when the bribe-taking environment is sufficiently decentralised.

## VII. Robustness Checks

In this section, I supplement my qualitative description of Guiro's arrest with more formal testing of whether the arrest lends itself to a credible natural experiment. Crucially, for my baseline diff-in-diff strategy to be plausible, two key conditions must be met: first, the treatment and control groups had to have been following common trends before the event took place and second, there must be a clear treated/untreated dichotomy, in the sense that the event should not have also 'treated' the control group in some way (known as the stable-unit-treatment-value-assumption, SUTVA).

### Testing for Parallel Trends

To test for the presence of parallel trends, I generate 3 'placebo' arrest dates occurring 27, 18 and 9 months before the actual arrest took place (there are 9 months of post-arrest data). I run three separate regressions, interacting each placebo arrest with the customs dummy and then ending the sample nine months after the placebo arrest in each case. All fixed effects and controls are the same as in the baseline diff-in-diff specification (8). If my identification strategy is plausible, the coefficients on these interaction terms should also be insignificant: if not, it suggests that my significant baseline result may instead be an artefact of random trends that persist in the data.

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<sup>13</sup> If bribery was being orchestrated from the top, we might expect the average number of checkpoints per trip to respond to changes in leadership.

**Table 4: Parallel Trends Test**

	27 months	18 months	9 months
<b>Customs*Post Arrest</b>	-78.51 (51.47)	<b>-65.99*</b> (34.89)	49.48 (41.72)
Observations	22,949	33,335	41,038
R-squared	0.14	0.15	0.16

Notes: Dependent variable is bribe value in CFA; full specification in Equation (8). Robust standard errors clustered at the truck level in parentheses

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

Table 4 outlines the results. While the estimates for the diff-in-diff coefficient in my baseline regression is significant at the 1% (and even 0.1%) level, the diff-in-diff coefficients are insignificant for the 9 and 27 month placebo events and only mildly significant for the 18 month placebo. While falling short of perfect support for parallel trends, the fact that the 9 month placebo is insignificant suggests that any shock which may have occurred 18 months before to prompt a divergence in trends would have dissipated by the time the actual arrest took place.

### **Testing the SUTVA**

The SUTVA is best scrutinised by revisiting the baseline results in Table 2. In the table, the post-arrest dummy captures the different behaviour of non-customs officials after the arrest. In my preferred model, the post-arrest dummy is statistically insignificant, though we should be cautious of the large standard error which masks a relatively large point estimate. When we take out time fixed-effects (column 2), the standard error diminishes and the point estimate becomes strongly statistically significant.

This result casts doubt on the validity of the SUTVA assumption and should prompt us to consider whether non-customs officials could have also been ‘treated’ by Guiro’s arrest in some way. This certainly seems plausible: non-customs officials may have interpreted the arrest as a harbinger of greater scrutiny being placed on the customs agency by the central government. Since the time and resources required to monitor a department are scarce, more scrutiny on the customs agency may mean less scrutiny on their own department, incentivising them to take greater risks with their own bribe-taking behaviour. Alternatively, after the arrest, non-customs officials may have correctly anticipated that customs officials would be less likely to solicit bribes in the

future, leading to less ‘competition’ over the total bribes a single driver would be willing to pay and allowing them to capture more than they were able to before.<sup>14</sup>

Overall, these considerations jeopardise the SUTVA assumption my baseline identification relies upon. For this reason, I now turn to alternative counterfactuals, expanding the dataset to include customs officials in neighbouring countries who are assumed to have been less affected by Guiro’s arrest.

### **Testing Against an Alternative Counterfactual**

Being landlocked, Burkina Faso relies heavily on its highway links with neighbouring countries to access international markets. My unrestricted dataset has data on four countries that border Burkina Faso: Côte d'Ivoire, Ghana, Mali and Togo. In theory, customs officials in any of these four countries could serve as an alternative counterfactual. I choose to focus on Togo for three reasons: first, customs officials in Togo were not treated by any easily identifiable contemporaneous country-wide shocks around the event window (e.g. elections, coups or recessions). Second, bribes in Togo and Burkina Faso are paid in a common currency: the West African franc (CFA). By contrast, bribes in Ghana are paid in Ghanaian cedis. Third, data on Togo begins in 2006 which creates a long ‘burn in’ period before the event; in contrast to Côte d'Ivoire, where data collection only began in late 2010. Though I draw particular attention to Togo, I present regression results from each country to provide full disclosure.

I conduct four separate diff-in-diff regressions in which customs officials in Togo, Ghana, Côte d'Ivoire and Mali take the place of non-customs officials in Burkina Faso as the relevant counterfactual. I thus estimate a diff-in-diff regression of the following form:

$$(10) Y_{ijt} = \alpha + \beta_1 Burkina_{ijt} + \beta_2 PostArrest_t + \beta_3 Burkina_{ijt} * PostArrest_t + \gamma X_i + T + \delta_j + \varepsilon_{ijt}$$

Again, all fixed effects and controls are the same as in the baseline specification (8).

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<sup>14</sup> This explanation is more in keeping with Shleifer and Vishny’s (1993) ‘market structure’ framework.

**Table 5: Diff-in-Diff Regression against Alternative Counterfactuals**

	<i>Preferred Model</i>			
	(1) Togo	(2) Ghana	(3) Côte d'Ivoire	(4) Mali
Burkina	342.0*** (34.69)	386.2** (156.8)	8.190 (111.7)	641.7*** (126.8)
Post Arrest	79.50 (62.78)	-1070.9*** (129.4)	190.2 (122.9)	-383.2*** (89.98)
<b>Burkina*Post Arrest</b>	<b>-129.4**</b> (27.72)	<b>-54.87**</b> (25.87)	<b>-252.3**</b> (101.4)	204.1*** (57.09)
Year & Month Fixed Effects	Yes	Yes	Yes	Yes
Checkpoint Fixed Effects	Yes	Yes	Yes	Yes
Truck Characteristics	Yes	Yes	Yes	Yes
R-squared	0.19	0.33	0.25	0.34
Observations	49,426	45,868	35,771	47,584

Notes: Dependent variable is bribe value in CFA. Standard errors clustered at the truck-level in parentheses

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

Table 5 outlines the results. Encouragingly, the diff-in-diff parameter from the Togo regression remains negative and strongly significant, supporting the result that Guiro's arrest did substantially reduce the bribe-taking behaviour of Burkinabé customs officials. However, the absolute value of my point estimate changes substantially – falling from 394 CFA to 129 CFA – suggesting that the magnitude of the previous results was in part attributable to the offsetting behaviour of non-customs officials.

These results are largely corroborated by the Ghana and Côte d'Ivoire regressions though not from the Malian one. The positive coefficient on the diff-in-diff interaction term suggests that customs officials in Burkina Faso *increased* their bribe-taking relative to Malian customs officials after the arrest. This anomalous result is best rationalised by speculating that Mali experienced a contemporaneous shock around the time of Guiro's arrest. A candidate for this shock is not hard to identify – Mali experienced a full-scale coup in March 2012 (just two months after the arrest) which, as Oki (2016) shows, had a substantial impact on bribe-taking behaviour.

The Malian result points to a general shortcoming of conducting diff-in-diffs with counter-factuals across borders – namely that we rely on the fairly heroic assumption that the two countries would have followed similar trajectories in the absence of any treatment. For this reason, this regression represents a robustness check rather than a baseline specification. In theory, the problem of different country-wide shocks can be ameliorated by re-expanding the dataset to include all officials and then estimating a triple-difference regression of the form:

$$(11) Y_{ijt} = \alpha + \beta_1 Burkina_{ijt} + \beta_2 Customs_{ijt} + \beta_3 PostArrest_t + \beta_4 Burkina_{ijt} \\ * Customs_{ijt} + \beta_5 Burkina_{ijt} * PostArrest_t + \beta_6 Customs_{ijt} \\ * PostArrest_t + \beta_7 Burkina_{ijt} * Customs_{ijt} * PostArrest_t + \gamma X_i + T \\ + \delta_j + \varepsilon_{ijt}$$

Where  $\beta_5$  absorbs the effect of contemporaneous country-wide shocks. When I run this regression, the triple-diff parameter  $\beta_7$  becomes negative and statistically significant for Mali, as well as for each other country. I omit including these results due to space constraints and because they are difficult to interpret. Nevertheless, the fact that the triple-diff parameter remains negative and significant for all four regressions provides strong corroborating evidence that Guiro's arrest *did* reduce petty corruption behaviour in the Customs Ministry, lending credibility to my baseline results.

## VIII. Testing for Alternative Explanations

Thus far, I have accounted for these results by speculating that Guiro's arrest led to a change in the 'corruption-type' of Player *B*, which changed the behaviour of lower-ranked officials via the different monitoring and disciplinary procedures this brought about. However, as noted at the beginning of Section V, this is not the only way these results can be rationalised: they can also be explained by an 'uncertainty' and a 'signalling'-type argument. It is important to test these alternative explanations since, if they are deemed plausible, this would undermine the causal story captured by my theoretical model and perhaps my endorsement for leadership decapitation more generally.

## **Alternative Explanation 1: The Uncertainty Explanation**

One alternative explanation would be to suggest that these results are due to the fact that petty bribery *always* decreases whenever ministerial leadership changes hands. Since this is what happens by default, we cannot assign anything meaningful to Guiro's personal corruption charges, or to the effectiveness of leadership decapitation as an anti-corruption strategy. If correct, this alternative explanation has profoundly different policy implications: rather than targeting leaders we believe to be corrupt *ex ante*, we should simply keep all leadership post extremely transient, regardless of whether the leader is corrupt or not. This could be achieved by the imposition of shortened ministerial term limits, for instance.

To test the plausibility of this explanation, I examine bribe-taking behaviour in aftermath of two *voluntary* leadership transitions that occurred in the police department during my sample period. On 23<sup>rd</sup> May 2008, Rasmane Wangraoua replaced Thomas Dakoure as Head of the Burkinabé police force; on 13<sup>th</sup> May 2011, Paul Sondo replaced Rasmane Wangraoua. Intuitively, if bribe-taking similarly falls in the police department following these unremarkable leadership changes, this would imply that my results are driven less by an explicit change in the type of Player *B*, and more by some broader uncertainty that leadership change generates.

I estimate the bribe-taking behaviour of policeman relative to non-policeman in Burkina Faso using a familiar diff-in-diff regression:

$$(12) Y_{ijt} = \alpha + \beta_1 Police_{ijt} + \beta_2 PostTurnover_t + \beta_3 Police_{ijt} * PostTurnover_t + \gamma X_i + T + \delta_j + \varepsilon_{ijt}$$

All fixed effects and controls are the same as in the baseline regression (8). In both regressions, I restrict the post-event window to 9 months in order to best replicate the baseline experimental setting.

**Table 6: Bribe Value Regression in Other Instances of Leadership Turnover**

	(1) 1st Turnover (May 2008)	(2) 2nd Turnover (May 2011)
Police	-1.911 (61.58)	-22.50 (19.97)
Post Turnover	58.92 (177.8)	109.0 (67.43)
<b>Police*Post Turnover</b>	<b>211.8<sup>***</sup></b> (61.93)	<b>172.1<sup>***</sup></b> (33.05)
Year & Month Fixed Effects	Yes	Yes
Checkpoint Fixed Effects	Yes	Yes
Truck Characteristics	Yes	Yes
R-squared	0.19	0.19
Observations	10,384	45,391

Notes: Dependent variable is bribe value in CFA. Standard errors clustered at the truck-level in parentheses

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

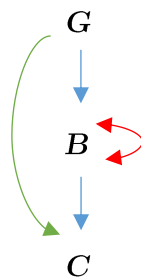
Table 6 outlines the results. In both instances of leadership turnover, bribery significantly *increased* amongst policemen in the 9 month window relative to non-policemen, in stark contrast to the results observed after Guiro's arrest. This is perhaps not all that surprising: in normal circumstances, we may expect bribery to increase when a new leader takes power, as lower down officials exploit the fact that it may take the new leader a while to set up effective monitoring channels and disciplinary procedures. These results totally undermine any claims that the results are attributable to generic leader transition and imply that, if anything, my baseline post estimates *underestimate* the impact of Guiro's arrest, since they mask the tendency towards higher bribe-taking that is present in the more general case of leadership turnover.

### **Alternative Explanation 2: The Signalling Explanation**

A second alternative explanation acknowledges that there was something specific to Guiro's arrest that drives these results, but speculates an alternative causal channel. Rather than being driven by the mechanical act of replacing the leader, this explanation suggests the results were driven by the fact that the arrest generated a 'signal' that the government was to get more involved in cleaning up the customs ministry, by directly

monitoring the lower-ranked officials themselves. Although this did not actually happen, if the officials at the time nonetheless *interpreted* the arrest as a statement of intent, this would have altered bribe-taking via their changed *beliefs* about their probability of getting caught.

Conceptually, this ‘signalling’-type explanation focuses on the importance of the green arrow, while the more ‘mechanical’ explanation imbedded in my theoretical framework focuses on the importance of the red arrow.



To test these two competing explanations, an ideal experiment would be to examine a similar event in which we knew a leader was corrupt *ex ante*, but in which he stepped down from power voluntarily. This would enable us to isolate the ‘mechanical’ effect of replacing a corrupt leader with a less corrupt one, abstracting from any ‘signal’ that Guiro’s arrest may have generated. In the absence of this, we might instead ask what patterns we would expect to see in the data if just one of these mechanisms was at play. Crucial to the signalling story is that the arrest was in reality a ‘false’ signal – as argued in Section III, the arrest did not precipitate wider government involvement in the Customs Ministry or in fighting corruption more generally. Presumably, as the months went by after the arrest, customs officials would have realised this and adjusted their behaviour accordingly. Thus, if only the signalling channel was at play, we should observe a sharp reduction in bribery in the *immediate* aftermath followed by a quick return to normality thereafter. Alternatively, if it was only the mechanical channel at play, an effect may not show immediately, since it would presumably take a new leader some time to set up effective monitoring procedures and establish a reputation as a harsh ‘punisher’ of corrupt behaviour.

To test these competing explanations, I exploit the temporal patterns of bribe-taking after the arrest, re-running the baseline regression (8) four times, restricting the post-event window to 1 month, 2-3 months, 3-6 months and 6-9 months respectively.

**Table 7: Baseline Bribe Value Regression with Restricted Event Windows**

	(1)	(2)	(3)	(4)
	1 Month	2-3 Months	3-6 Months	6-9 Months
<b>DD Coefficient</b>	-18.47 (80.55)	<b>-693.8***</b> (81.44)	<b>-512.6***</b> (44.97)	<b>-298.1***</b> (72.14)
Observations	42,631	43,381	43,746	44,172
R-squared	0.18	0.19	0.19	0.19

Notes: Dependent variable is bribe value in CFA. Full specification in Equation 8.

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

Table 7 outlines the results. The insignificant coefficient on the 1 month post-event window shows that customs officials did not adjust their behaviour in the immediate aftermath of the arrest, a result fundamentally incongruous with the signalling-type explanation outlined above. However, once a result is picked up, the fact that the results remain strongly significant even as we shift the post-event window further into the future is consistent with a mechanical-type explanation driven by structural changes in the ministry itself. This, coupled with the fact that there is no qualitative evidence of Sylvester being corrupt,<sup>15</sup> strongly suggests that it was the change in *type* of Player *B* that generated my results.

With this being said, it is worth noting that we do observe some ‘fizzling out’ in the sense that the point estimates get closer to 0 as the post-event window is extended. To account for this, we may speculate that it takes officials 2-3 months to learn the ‘corruption-type’ of their new boss, and that the effect of a new leader is most pronounced once this learning has taken place. The results also suggest that the mechanical act of leadership change may lose effectiveness over time, as agents may learn ways to exploit weaknesses in the new monitoring technology or ways they can escape punishment. Fundamentally, this final result heralds the need for greater

<sup>15</sup> This point is tempered by the acknowledgement that absence of evidence  $\neq$  evidence of absence – Sylvester may simply not have been caught! Nonetheless, the fact that there is no explicit evidence of him being corrupt is encouraging for my causal story.

research before leadership decapitation can be confidently endorsed as an effective *long-term* strategy in fighting corruption, a point that is discussed further in Section IX.

## IX. Conclusion

To conclude, this paper has analysed whether arresting the head of a Ministry on corruption charges has any impact on the petty corruption behaviour of lower-ranked officials. Overall, I find that Guiro's arrest reduced the magnitude of bribes taken by customs officials by 394 CFA relative to non-customs officials, a result that is robust to a test of parallel trends and several alternative counter-factuals. I argue that these results are most likely driven by the fact that Guiro's deposition led to a change in the 'type' of leader at the top of the ministry, rather than any general uncertainty or signal the arrest may have generated. This squares with my theoretical framework, which incorporates a novel sharecropping-like dynamic to illustrate the curiously ambiguous effect a 'cleaner' leader can have on petty corruption.

These results lend themselves to three main policy implications. First, the fact that Guiro's removal significantly reduced bribe-taking in the Customs Ministry suggests that leadership decapitation *can* be an effective tool in the policy-makers arsenal. However, as argued throughout, this result is highly context-dependent: leadership decapitation may have only worked in my experiment because of the highly opportunistic nature of roadside corruption, which lessens the tendency for bribes to pass up through the chain. In environments where bribe-taking is more institutionalised, removing a corrupt leader may have the paradoxical effect of *increasing* lower-ranked corruption, via the fact that lower-ranked officials are now taxed less by their superiors. This insight cautions against the blind use of leadership decapitation without appreciation for the wider circumstances, and suggests that policymakers should expend effort in understanding the internal norms and dynamics governing a particular organisation before intervening.

Second, the fact that my results are driven by the change in *type* of Player *B* implies that leadership decapitation owes its success to the mechanical act of simply 'getting rid' of a corrupt politician. Investigations and arrests are one way to achieve this though are certainly not the only way, which lends itself to a broader 'menu' of policy choices. An

alternative strategy would be to offer corrupt politicians a ‘golden parachute’: a chance to walk away from their leadership posts free from shame with the promise that they will not be pursued. This may be less edifying than exposing their corrupt behaviour, but could be cheaper and more effective than a protracted investigation campaign, which is not guaranteed to deliver results. Of course, there are moral hazard issues endemic to this strategy, though these can be mitigated through a careful design of incentives (see Klaas, 2016).

The final policy recommendation relates to something not addressed until now: the economic meaningfulness of these results. Despite being statistically significant, the fact that each point estimate amounts to less than a dollar implies that the arrest did not make it substantially cheaper to ferry goods through Burkina Faso, curbing the possibility of any salutary general equilibrium effects. The small scale involved suggests that governments may be better off focusing on ‘big ticket’ corruption associated with public spending or foreign investment, where general equilibrium effects are likely to be several magnitudes larger (Lambsdorff, 2006). However, this is not to say that targeting petty roadside bribery is pointless. The fact that removing just one official led to a 28% reduction in bribes suggests there is some ‘low hanging fruit’ when it comes to tackling this type of corruption. Moreover, as Kelling and Wilson (1982) note, visible signs of crime (i.e. ‘broken windows’) may breed a cycle of anti-social behaviour and disorder. Thus, while petty roadside corruption should not be at the forefront of a government’s anti-corruption agenda, it certainly should not be neglected, either.

Though I hope this paper offers an interesting attempt in broaching the empirical study of hierarchical corruption, there are a number of ways it can be built upon. The most obvious limitation is that my dataset ends in September 2012, just 9 months after Guiro’s arrest. This is particularly frustrating, given the indication that the results were beginning to fizzle out towards the end of the post-event window. Whether or not this fizzling out continued has strong implications on the long-term effectiveness of leadership decapitation as an anti-corruption strategy. There exists an extension to this dataset continuing until September 2013 which would provide novel insights to this question, but unfortunately I was not able to access it for the purpose of this essay. Nonetheless, this task remains open to anyone that is able to get access.

Another interesting extension to this study would be to test for heterogeneous treatment effects *within* the sub-set of Burkinabé customs officials. My framework would suggest that officials in Ouagadougou (where *B* is located) may have been *more* affected by leadership change than those in the hinterlands, since the effectiveness of *B*'s monitoring technology may plausibly dissipate with distance. If so, this would corroborate the findings of Bates (1983) and Herbst (2000), who argue that the penetration of the African state is inversely related to the distance from the capital. I omit these further avenues due to space constraints, but would be keen to revisit them in the future.

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