

# Repression and Foreign Aid in Autocracies: Exploiting Debt Relief Negotiations in Post-Cold War Africa

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## **Abstract:**

Does dependence on development aid from Western sources constrain the use of repression among autocrats? To answer this question, I employ a novel dataset of Africa's post-Cold War autocracies in which the unit of analysis is the country-day rather than the country-year. This day-level dataset enables me to address three potential sources of bias that may obscure the relationship between Western aid dependence and repression. When the threat of financial sanction is credible, I find, Western donors have reduced the daily odds of repression in Africa's post-Cold War autocracies. Western aid dependence is constraining even during periods of sustained popular protests. The results suggest that modern autocrats who rely on Western donors for financial support lack the easy recourse to repression enjoyed by their Cold War era predecessors.

**Keywords:** debt relief, post-Cold War, Africa

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If we could just get 1,000 people in the streets, [President Denis Sassou Nguesso] could never open fire on them all. He is too reliant on the West.

– An anonymous opposition activist in the Republic of Congo.<sup>1</sup>

## 1. Introduction

There is mounting evidence that Western donors curtail foreign aid when recipient governments violate their citizens' human rights.<sup>2</sup> These financial sanctions, the pro-democracy activists quoted above believe, give Western donors leverage over the world's autocrats. Since money is fungible, Western aid – even in the form of public good interventions – enables autocrats to allocate scarce revenue to their internal security apparatus, longtime political allies, and opposition rivals, all of which are imperative for survival.<sup>3</sup>

Is the prospect of punishment by Western creditors sufficient to deter Africa's aid dependent autocrats from repressing their citizens? Scholars have long found reason for pessimism. While some scholars argue that foreign aid has little impact on political liberalization,<sup>4</sup> others attribute to it a range of pernicious effects. Foreign aid is thought to foster bureaucratic inefficiency,<sup>5</sup> diminish government responsiveness to citizens,<sup>6</sup> and strengthen the state's repressive apparatus.<sup>7</sup> Some scholars compare foreign aid to oil receipts, which, as a source of unearned income, may undercut the “social contract” that fosters accountability to citizens through taxation.<sup>8</sup> Foreign aid may also prolong civil wars.<sup>9</sup>

Recently, however, scholars have found evidence that Western aid can foster political liberalization. In post-Cold War Africa, Dunning (2004) finds that foreign aid is associated with democratization.<sup>10</sup> More modestly, employing a global sample, Bermeo (2015) finds that foreign aid may have inhibited democratization during

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<sup>1</sup> Interview with the author, 13 April 2012.

<sup>2</sup> Nielsen (2013). Note, however, that Stone (2004) and Lebovic and Voeten (2009) find that enforcement by bilateral donors and multilateral institutions may be subject to domestic political calculations.

<sup>3</sup> See also Bueno de Mesquita and Smith (2009a, 2011). For these reasons, Bueno de Mesquita et al. (2003) argue, Western aid may sustain the world's autocrats. Escribà-Folch and Wright (2015) make a similar theoretical argument.

<sup>4</sup> Knack (2004) and Regan (1995). Some find that foreign aid was driven as much by the domestic political calculations of donors than the needs of recipients; see Alesina and Dollar (2000).

<sup>5</sup> Burnside and Dollar (2000) and Remmer (2004).

<sup>6</sup> Brautigam and Knack (2004) and Rajan and Subramanian (2007).

<sup>7</sup> Bueno de Mesquita and Smith (2009a,b), Brautigam and Knack (2004), Kono and Montinola (2009), and Ahmed (2013).

<sup>8</sup> Morrison (2009), Djankov et al. (2008), Smith (2008), and Ahmed (2012). Note, however, that Altincekic and Bearce (2014) argue that aid is less fungible than oil revenue, and so shouldn't be a perfect substitute. Bermeo (2015) makes a similar argument.

<sup>9</sup> Nunn and Qian (2014).

<sup>10</sup> Note that Dietrich and Wright (2014), using a longer time-series, report less robust results, indicating that the effect in Dunning (2004) may be driven by the Third Wave in the early 1990s.

the Cold War, as critics argue, but not since.<sup>11</sup> These conflicting findings may turn on whether threats of Western aid reductions are credible and substantial. By rewarding democratic reforms with aid, Kersting and Kelly (2014) argue, Western donors have incentivized liberalization.<sup>12</sup> At the very least, the evidence suggests, foreign aid has not inhibited liberalization since the end of the Cold War, when financial sanctions have been most credible. At best, when donors condition aid on “good governance” reforms, they may be able to extract democratic concessions.

This paper advances existing literature by focusing on a single aspect of liberalization: repression. I do so partly because, by using Polity scores and other blunt measures of liberalization, it is difficult to determine precisely what outcomes aid conditionality affects. Consequently, it is also unclear through what channel aid conditionality might ultimately foster liberalization. It is possible, for instance, that aid conditionality compels autocrats to build genuinely democratic institutions. But it is equally possible that the threat of aid reductions compels autocrats to employ less repression, leaving them vulnerable to popular protests.

The effect of Western aid dependence on the probability of repression may be obscured by three sources of bias. First, aggregating day-level events into arbitrary temporal units, such as the year, may introduce ecological bias. Accordingly, since repression occurs at the day level, I study it at the day level. Second, since citizen protests are strategic, autocrats who are least constrained may seldom actually repress; their citizens may dare not protest in the first place. I refer to this as protest selection bias, and, if present, it will understate the constraining effects of Western aid. Finally, Western donors, driven by ethical concerns, may allocate scarce aid to governments who are committed to liberalization or least likely to repress. I refer to this as donor selection bias, and, if present, it will overstate the democratizing effects of foreign aid.<sup>13</sup>

To accommodate ecological bias and protest selection bias, I employ a day-level dataset of protest and repression in Africa’s autocracies. I restrict attention to Africa’s autocracies because of data limitations and theoretical interest: to Africa, because day-level records of protest and repression exist, and to its autocracies, because repression is regarded as a key instrument of survival. I accommodate the possibility of protest selection bias by estimating two sets of statistical models. One set focuses on an autocrat’s behavior the day

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<sup>11</sup> See also Bermeo (2011).

<sup>12</sup> See also Brown (2005). Focusing exclusively on economic reforms, Bearce and Tirone (2010) report similar findings. Wright (2009) shows that aid is particularly effective in fostering liberalization when its recipients expect to remain in power afterwards.

<sup>13</sup> Indeed, leaders who are most committed to liberalization may be most likely to accept aid with political conditions. Notably, two related works attempt to overcome this with different identification strategies. Abouharb and Cingranelli (2008) employ a Heckman selection model to control for the probability that countries accept structural adjustment programs. Using an instrumental variables strategy, Aronow et al. (2012) find that increases in aid from the European Union are associated with more respect for human rights in recipient countries since the Cold War, though only in the short term.

*after* a protest. By restricting attention to country-days following protests, I ask how reliance on Western donors impacts an autocrat's response to protests once they emerge. In so doing, I remove from the sample country-days on which autocrats had no need for repression. The second set of statistical models includes all country-days in the sample, and simply controls for whether protests occurred on day  $t - 1$ . If protest selection bias is present, the effect of Western aid dependence in the first set of models will be consistently stronger than in the second set.

To account for donor selection bias, I isolate temporal periods when the international community's attention to human rights violations in Africa is focused and punishment is credible. In particular, I focus on debt relief negotiations with the International Monetary Fund (IMF) and World Bank that occurred as part of the Heavily Indebted Poor Country (HIPC) program. I exploit two features of the HIPC debt relief program. First, notwithstanding their rhetoric, the Bretton Woods institutions initiated debt relief negotiations with little regard for the recipient government's human rights record. Since 1996, of the 33 African countries that met the economic conditions for debt relief – that is, were sufficiently indebted and impoverished – the Bretton Woods institutions granted full, irrevocable debt relief to 30. Second, many recipient autocrats were in power long before HIPC negotiations began and remained long after they concluded. Consequently, in the lexicon of causal inference, each autocrat constitutes his own counterfactual: how he would have behaved in the absence of credible sanctions from Western donors.

These two features of the HIPC debt relief program enable a differences in differences estimation strategy. Controlling for a range of day- and year-level features, I find that during the final phase of debt relief negotiations, Africa's autocrats were substantially less likely to repress their citizens. The effect of HIPC debt relief negotiations is particularly strong on days immediately following popular protests. This suggests that, by not accommodating the possibility of protest selection bias, existing literature may understate the effect of Western aid dependence on repression.<sup>14</sup> In short, when the threat of punishment is credible, Western donors have constrained repression among Africa's post-Cold War autocrats.

This paper advances our understanding of modern autocracies by identifying new constraints on the instruments of survival. Drawing on the Cold War, scholars often view the chief threats to an autocrat's survival as emanating from disgruntled elites, who can engineer military coups. To facilitate credible revenue

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<sup>14</sup>Wood (2008), for instance, finds that US and UN sanctions are associated with higher rates of state repression between 1976 and 2001. Because his estimating equations are not well identified, it may be the case that citizens are more inclined to protest when their rulers are weakened by financial sanctions. If so, the net effect of sanctions may indeed be to undermine autocrats.

sharing agreements with their elites, scholars contend, autocrats construct political institutions: single parties or nominally democratic institutions.<sup>15</sup> When necessary, autocrats employ violence to suppress popular revolts.<sup>16</sup> This article joins a new wave of scholarship that suggests that modern autocrats confront new threats to their survival, and meet those threats with new constraints. Since the end of the Cold War, Western creditors have virtually required nominally democratic institutions in exchange for development aid and debt relief.<sup>17</sup> As a result, the institutional environment confronting modern autocrats is relatively fixed, particularly in Africa, where autocrats are most vulnerable to Western pressure.<sup>18</sup> These nominally democratic institutions are often destabilizing, for regular elections enable frustrated citizens to coordinate protests.<sup>19</sup> With Western creditors pressing newly ascendant military dictators to quickly organize democratic elections, the rate of coups in aid dependent countries has declined as well.<sup>20</sup> For both these reasons, threats from the street are now more salient than ever. This paper finds that Africa's modern autocrats must confront these threats without the easy recourse to repression enjoyed by their predecessors. Increasingly, to understand how autocrats survive, scholars must look beyond the tools of formal political institutions and repression.

This article proceeds as follows. Section 2 illustrates the central causal mechanism with qualitative evidence from the Republic of Congo. Section 3 introduces the day-level dataset, discusses ecological bias and protest selection bias, and confirms that dependence on Western aid is associated with less repression in Africa's post-Cold War autocracies. To address donor selection bias, Section 4 presents evidence that, during HIPC debt relief negotiations, Africa's autocrats curtailed repression dramatically. Section 5 concludes with suggestions for future research.

## 2. Qualitative Evidence: The Republic of Congo

This paper's central hypothesis is that *reliance* on Western donors constrains repression among Africa's post-Cold War autocrats. The Republic of Congo clearly illustrates the causal mechanism. After seizing

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<sup>15</sup>Geddes (2005), Arriola (2009), Svobik (2012), Gandhi (2008), Brownlee (2008), Blaydes (2011).

<sup>16</sup>Slater (2010) and Frantz and Kendall-Taylor (2014).

<sup>17</sup>Dunning (2004) and Levitsky and Way (2010).

<sup>18</sup>Bratton and van de Walle (1997) and van de Walle (2001).

<sup>19</sup>Tucker (2007) and Fearon (2011).

<sup>20</sup>Goemans and Marinov (2014).

power following the 1997 civil war, President Denis Sassou Nguesso inherited one of the world's most heavily indebted countries per capita.<sup>21</sup> Sassou Nguesso required financial support from Western creditors to facilitate post-conflict reconstruction. To earn it, he had to appear to be a democrat.<sup>22</sup> These concerns shaped Sassou Nguesso's political strategy ahead of the 2009 presidential elections. Opposition leaders observed Sassou Nguesso's sensitivity to Western pressure, and so became persuaded that he would not violently suppress protests.

In 2010 Sassou Nguesso's position vis-à-vis Western creditors changed. After securing HIPC debt relief, Sassou Nguesso turned to China for development aid and credit. By then, China's commitment to non-interference was well established among Africa's autocrats. China couched its policy of non-interference as a principled respect for state sovereignty. "We don't believe that human rights should stand above sovereignty," declared the Director of African Studies at the Chinese Academy of Social Sciences.<sup>23</sup> As the Chinese ambassador to Eritrea put it, "there are no rogue states."<sup>24</sup> By pivoting to China, Sassou Nguesso sought to inoculate himself against Western pressure. Figure 1 captures this discontinuity. Following debt relief in 2010, Sassou Nguesso virtually terminated his relationship with Western donors in favor of China, prompting the IMF to dramatically downsize its country office.<sup>25</sup>

This discontinuity makes Congo a uniquely appropriate case study, for the strategic environment that Sassou Nguesso confronted immediately prior to 2010 was identical to the one he confronted afterwards. Sassou Nguesso responded to his new autonomy from Western donors by sharply increasing repression.

## 2.1. Before Debt Forgiveness

Upon seizing power after the 1997 civil war, Sassou Nguesso faced severe financial constraints. Congo's debt to GDP ratio was roughly 260%, and debt service obligations to the IMF and other public creditors reached some 25% of total export value. Yet he also confronted several financial imperatives. Since the war was waged in Brazzaville, the capital, Sassou Nguesso had to rebuild the city's infrastructure, as well as

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<sup>21</sup> Sassou Nguesso first seized power in a 1979 coup, ruled as a single-party dictator in the 1980s, and then was forced to concede democratic elections in 1992. He lost, and reclaimed power following the 1997 civil war. Bazenguissa-Ganga (1997) remains the most complete account of Sassou Nguesso's reign during the 1980s.

<sup>22</sup> Clark (2008).

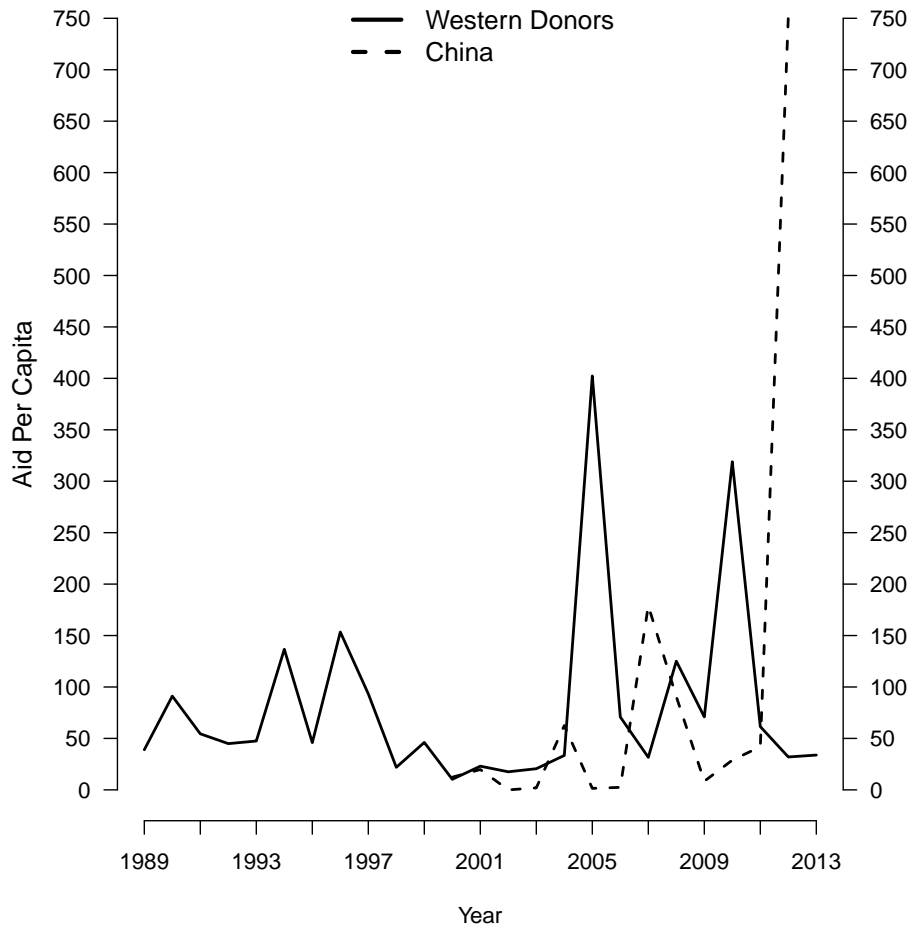
<sup>23</sup> Quoted in Taylor (2006).

<sup>24</sup> Quoted in Taylor (2006).

<sup>25</sup> Interview with senior IMF officials, December 2011.



**Figure 1**



**Notes:** Development aid to Congo from Western donors and the Chinese government, expressed in per capita terms. Data for Western aid is drawn from the World Bank's World Development Indicators; data for Chinese aid is drawn from the AidData project. Congo received HIPC debt relief in 2010.

the transportation routes that linked it with Pointe-Noire, seat of the oil industry. Meanwhile, Brazzaville's western edge was surrounded by loyalists of deposed president Pascal Lissouba.<sup>26</sup> Consequently, Sassou Nguesso had to quickly rebuild the military and pay off veterans of his civil war effort.<sup>27</sup>

Sassou Nguesso financed these expenses in two ways. First, he overhauled Congo's oil industry. Whereas previous governments entrusted marketing to foreign companies,<sup>28</sup> Sassou Nguesso decided that the government would henceforth market oil as well.<sup>29</sup> Second, Sassou Nguesso courted Western donors. As

<sup>26</sup>Clark (2008). For more on the civil war itself, see Yengo (2006)'s excellent work.

<sup>27</sup>Moutsila (2009).

<sup>28</sup>Le Floch-Prigent (2001).

<sup>29</sup>For more, see Cooke (2005) and Hariprashad-Charles (2007).

part of his campaign to attract aid and secure debt relief, Sassou Nguesso hired a team of lobbyists to persuade Western capitals of his democratic credentials.<sup>30</sup> Accordingly, he was forced to abide the nominally democratic institutions created during the National Conference of 1991. Although he managed to increase presidential terms from five years to seven, Congo's political institutions otherwise changed very little.<sup>31</sup> The public relations campaign ultimately succeeded. In addition to financing a series of demobilization programs for ex-combatants, Western donors rehabilitated Congo's road network.<sup>32</sup>

Sassou Nguesso's political strategy reflected his reliance on Western donors. Constrained to appear democratic, Sassou Nguesso attempted to avoid repression at all costs. To do so, he sought to prevent mass protests from emerging in the first place. Election seasons, scholars increasingly find, are dangerous for autocrats. By fostering common knowledge among citizens about shared frustrations and providing a single event around which to coordinate, regular elections facilitate popular protests.<sup>33</sup> Sassou Nguesso sought to counter these effects by forming electoral alliances with opposition leaders. Although these alliances served no electoral purpose – Sassou Nguesso could guarantee electoral victory with fraud – they rendered popular collective action far more difficult. Without opposition leaders mobilizing citizens, Sassou Nguesso reasoned, mass protests would be less likely and, therefore, repression unnecessary.<sup>34</sup> Second, rather than policing hostile southern regions with northern soldiers, Sassou Nguesso recruited southerners to do so. For although he regarded northern soldiers as more loyal, Sassou Nguesso recognized that they were more likely to prey on the southern population: to engage in economic extortion and to commit human rights abuses. In so doing, northern soldiers would provoke the sorts of popular grievances that foster mass uprisings, which Sassou Nguesso desperately needed to avoid.<sup>35</sup>

As the 2009 presidential elections approached, proof of high level corruption circulated in Brazzaville. In response, the regime assassinated journalist Bruno Ossèbi, who disseminated the evidence, and briefly suspended two independent media outlets that covered it.<sup>36</sup> Apart from these two incidents, however, outright repression was limited. Sassou Nguesso claimed some 80% of the vote in the July elections, which were

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<sup>30</sup>Narayanswamy (2009).

<sup>31</sup>Clark (2008). Unable to replace nominally democratic institutions with single party regimes, these marginal changes are common in Sub-Saharan Africa, as Levitsky and Way (2010) and Albaugh (2011) find.

<sup>32</sup>Clark (2008).

<sup>33</sup>Kuran (1991), McFaul (2005), Tucker (2007), Levitsky and Way (2010), Radnitz (2010), Bunce and Wolchik (2010, 2011), and Fearon (2011).

<sup>34</sup>Interviews with opposition leaders and civil society officials in Congo, August 2009. For more on the role of political entrepreneurs in fostering collective action, see Olson (1977) and Medina (2007).

<sup>35</sup>Interviews with residents of southern Congo, May 2013. For a similar argument in Kenya, see Hassan (2014).

<sup>36</sup>Committee to Protect Journalists (2009a) and Committee to Protect Journalists (2009b).

plagued by fraud.<sup>37</sup> Brazzaville residents took to the streets in protest. But since the the protests were covered by French media outlets, the government was careful not to use force; security forces employed tear gas instead of bullets. Cognizant that visual evidence would damage Sassou Nguesso's international reputation, the regime confiscated television cameras.<sup>38</sup>

## 2.2. After Debt Forgiveness

In 2010, after a series of “good governance” concessions and an intense public relations campaign, Sassou Nguesso secured debt relief from the Bretton Woods institutions. Congo's debt stock plunged to just over \$2b, from a high of nearly \$7b in 2004. From Figure 1, Sassou Nguesso quickly pivoted to China, which granted development aid and low interest loans without political conditions. Western development assistance reached near record lows by 2013, just as Chinese assistance began growing exponentially. Sassou Nguesso contracted with Chinese firms to revitalize Congo's agro-industrial sector and to finish the rehabilitation projects that Western donors started.

Sassou Nguesso formulated a new political strategy in response to this autonomy. Although he still sought to prevent mass protests from emerging in the first place, Sassou Nguesso signaled his willingness to employ repression in four ways. The costliness of these signals underscored the credibility of Sassou Nguesso's threats. First, he was much quicker to suppress journalists, even those active in Western media. Elie Smith was the director of the television station operated by Sassou Nguesso's elder brother, Maurice Nguesso. A native Cameroonian, Smith criticized Sassou Nguesso's efforts to secure an unconstitutional third presidential term. In response, in September 2014, the regime ransacked Smith's house, brutalized Smith and his family, and deported him. These human rights violations were reported by Sadio Morel Kanté, a prominent Reuters journalist who was born in Brazzaville – and hence a Congolese citizen – to West African parents. After receiving a series of death threats, Kanté was deported, despite her citizenship.<sup>39</sup>

Since 2010 the regime has been quicker to suspend independent newspapers as well. In September 2012 the regime suspended *Le Glaive* for six months and *La Voix du Peuple* for nine months. In June 2013, the government suspended *L'Observateur*, *Talassa*, and *Le Trottoir* for four months, and *Le Glaive* for an

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<sup>37</sup>Interview with Western diplomatic official, August 2009.

<sup>38</sup>Mampouya (2009).

<sup>39</sup>Committee to Protect Journalists (2014).

additional two months. In November 2013, the government issued nine month suspensions for *Sel-Piment*, *Le Glaive*, and *La Voix du Peuple*.<sup>40</sup> By this point, *Talassa* had ceased publication, nearly bankrupted by its earlier suspensions. “There is no more press freedom here,” the editor and publisher of one of these newspapers told me.<sup>41</sup> Even Reporters Without Borders (RSF) noticed the change. Between 2002 and 2009 Congo averaged 75th position on the RSF Press Freedom Index. Between 2010 and 2014 Congo’s position sharply declined, averaging 93rd position.

Third, the government has proven quicker to use force against political opponents. On March 4, 2012, explosions at a munitions depot rocked Brazzaville, destroying several neighborhoods and leaving several hundred dead. The regime responded by arresting outspoken critics. The most noteworthy target was Colonel Marcel Ntsourou, second in command of the *Conseil Nationale de Sécurité* and a key lieutenant during the 1997 civil war. Incarcerated in April 2012 and tortured in custody, Ntsourou was sentenced to five years’ hard labor in September 2013. Upon his early release, Ntsourou accused Sassou Nguesso of gross corruption on Radio France International (RFI). For this, on the morning of December 16, 2013, Ntsourou’s Brazzaville home was raided by Sassou Nguesso’s security detail. Four hours later nearly 40 of Ntsourou’s bodyguards had been killed and 55 arrested, including Ntsourou himself.

Finally, in April 2013, the government launched a police operation that deported as many as 250,000 citizens of the Democratic Republic of Congo (DRC) over four months, often brutally. Dubbed Operation “Smack of the Elders,” the government justified the operation with appeals to the crime rate. But the political opposition interpreted the violent campaign as a “show of force,” which was intended to highlight the government’s new willingness to employ violence.<sup>42</sup> Prior to 2010 the government was extremely sensitive to its reputation in Western capitals. This time, the government was not. One United Nations official declared:

I heard stories of children drowning in the river during their forced crossing. I saw a man injured by bullets and mothers who had given birth alone on the shore of the Congo River. All this has to stop. . . . This is unacceptable.<sup>43</sup>

Sassou Nguesso’s spokesman responded simply: “[We are] surprised by these rude comments.”<sup>44</sup>

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<sup>40</sup>Committee to Protect Journalists (2013).

<sup>41</sup>Interview, March 2014.

<sup>42</sup>Interview with opposition activist, July 2013.

<sup>43</sup>UN News Centre (2014).

<sup>44</sup>Chitera (2014).

## 2.3. Taking Stock

The Congolese experience suggests that Western pressure can constrain repression in autocracies. Prior to 2010, Sassou Nguesso sought to obviate the need for repression by discouraging popular protests. He allied with the opposition leaders who possessed the greatest capacity to mobilize citizens. He policed hostile southern regions with security officers drawn from the local population, the better to ensure that southerners would not be assaulted and extorted by northerners, who fought against them during the civil war. When the government deemed repression necessary, its use was circumscribed, and the government attempted to limit its international exposure.

After debt forgiveness and the pivot towards China, Sassou Nguesso revised his political strategy. The strategic environment that Sassou Nguesso confronted immediately prior to 2010 was virtually identical to the one afterwards, save for his reliance on Western donors. Newly unconstrained, Sassou Nguesso employed repression freely: against journalists, his domestic political opponents, and some 250,000 immigrants from Kinshasa, despite the UN's warnings.<sup>45</sup>

## 3. Baseline Estimation

### 3.1. Ecological Bias and Protest Selection Bias

To assess whether Western aid dependence is *associated* with less repression in Africa's post-Cold War autocracies, I combine day-level records of protest and repression with a range of day- and year-level characteristics.

Ecological bias arises from aggregating day-level events into larger temporal units, such as the month or year.<sup>46</sup> Ecological bias may be particularly salient when studying foreign aid and repression. If, for instance, foreign aid rises during election years – either because donors provide more electoral support<sup>47</sup> or because

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<sup>45</sup>Interview with anonymous opposition activist, April 2015.

<sup>46</sup>For more on ecological inference problems, see King (1997).

<sup>47</sup>Gearan (2014).

incumbents solicit aid as a means to purchase political support<sup>48</sup> – and if autocrats are more likely to employ violence during election seasons,<sup>49</sup> then an apparent relationship between foreign aid and repression could be a result of aggregating day-level records of repression into an annual indicator. By treating the country-day as the unit of analysis, I control for day-level characteristics that may vary across years and favor repression.

Probing the relationship between Western aid and repression is further complicated by the fact that both protest and repression are strategic. If, for instance, an autocrat does not engage in repression, it may be because he is constrained by the prospect of punishment by Western creditors. But an autocrat could also forgo repression because he has no need for it: because his citizens, knowing that he would repress their protests, choose not to protest in the first place. Having no need for repression, the autocrat forgoes it, and avoids whatever costs – from forgone financial aid or a besmirched reputation – it would entail. I refer to this as protest selection bias, and, if present, it will understate the constraining effects of Western aid dependence.

Protest selection bias is likely in post-Cold War Africa. Teodoro Obiang Nguema has ruled Equatorial Guinea since ousting his uncle in a 1979 coup. A leading oil producer, Equatorial Guinea is among Africa's least aid dependent autocracies. Few citizens doubt that Obiang would meet protest with repression, and so few risk it. When an intrepid citizen chooses to protest Obiang's rule, the population's expectations are quickly justified. In March 2015, the Obiang government arrested opposition leader Guillermo Nguema, abducted him to a village far from the capital, and instructed him to remain indefinitely. One citizen protested the abduction by distributing anti-regime leaflets. Days later, he too was forcibly abducted to the same village and instructed to remain indefinitely. The absence of blatant, widespread repression implies not that Obiang is constrained, but that he is unconstrained, and that citizens know it.<sup>50</sup>

To accommodate the possibility of protest selection bias, I estimate two sets of models. The first focuses exclusively on an autocrat's behavior the day *after* a protest. By restricting attention to days following protests, I ask how reliance on Western donors impacts an autocrat's response to protests once they emerge. This approach attends to protest selection bias by focusing only on those autocracies in which citizens are *willing* to protest. The second set of models features the full sample, and controls for whether protests occurred on day  $t - 1$ . As a result, it includes observations from autocracies where citizens may be unwilling to protest

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<sup>48</sup>Jablonski (2013).

<sup>49</sup>This may be either because elections facilitate protests or because repression has electoral benefits.

<sup>50</sup>For more on Obiang, see Klitgaard (1991) and Heilbrunn (2007).

because they *expect* to be repressed if they do. If protest selection bias is present, the effect of Western aid dependence from this full sample will be *weaker* than in the sample where protest occurred on day  $t - 1$ .<sup>51</sup>

### 3.2. Data and Descriptive Statistics

To construct the day-level dataset, I build on the work of others. Svolik (2012) provides a roster of the world's autocrats between 1960 and 2007; it includes the dates of their entry and exit, as well as the means by which they did so.<sup>52</sup> I draw data on state repression and popular protests from the Social Conflict on Africa Database (SCAD), introduced by Salehyan et al. (2012). SCAD records the daily number of repression and protest events throughout Africa since 1990. Based on an exhaustive search of the Lexis Nexis archive, Salehyan et al. (2012) employed a research team to code details about each repression and protest event. The result is the most detailed, complete record yet assembled. Although employing SCAD restricts attention to Africa's post-Cold War autocracies, I argue that the gains from day-level precision outweigh the costs in geographic scope, particularly since nearly half of the world's current autocracies are in Africa.

I draw data on Western aid from the AidData project, introduced by Tierney et al. (2011). The AidData project records project level commitments and disbursements by year from a range of donors to a range of countries. The AidData project defines aid *commitments* as the amount promised to a country in a calendar year, whereas aid *disbursements* reflect the amount donors actually transferred. Since the *commitment* measure more faithfully represents the *anticipated* subsidies an autocrat stands to lose if aid is revoked, I employ it. I define "Western" donors as the United States, all European countries and Anglo-Saxon offshoots, Japan, the Bretton Woods institutions, multilateral development banks, United Nations, and private organizations such as the Gates Foundation.<sup>53</sup> Following Goemans and Marinov (2014), I measure dependence on Western aid

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<sup>51</sup> As the results below make clear, I find some evidence of protest selection bias; the effect of Western aid dependence is consistently larger when conditioning on protests on day  $t - 1$ . Accordingly, future research might consider alternative estimation strategies that correct for protest selection bias, such as Heckman selection models, particularly when the literature is able to predict the day level probability of protest with greater certainty. I thank an anonymous reviewer for this insight.

<sup>52</sup> I update Svolik's roster to extend through 2012. Future research might consider the effect of Western aid dependence on repression in hybrid or competitive authoritarian regimes, as defined by Levitsky and Way (2010).

<sup>53</sup> The overwhelming share of aid to African countries is provided by the United States, European Union, the Bretton Woods institutions, multilateral development banks, and the United Nations. As a result, the results below are not sensitive to the inclusion or exclusion of particular countries or multilateral institutions.

by standardizing aid commitments by GDP, which I draw from the Penn World Tables<sup>54</sup>:

$$\text{Western Aid Dependence}_{i,s} = \frac{\text{Total Western Aid}_{i,s}}{\text{GDP}_{i,s}}$$

where  $i$  indexes country and  $s$  indexes year. Since Western Aid Dependence $_{i,s}$  is subject to skew, I employ its natural logarithm.<sup>55</sup>

According to AidData records, of the 110 autocrats in the dataset, Joseph Kabila of the Democratic Republic of Congo (DRC) is Africa's most Western aid dependent. Other relatively aid dependent autocrats include Kenya's Daniel Arap Moi, Zambia's Kenneth Kaunda, Cote d'Ivoire's Laurent Gbagbo, and Burkina Faso's Blaise Compaoré, all of whom acquired reputations for ruling with a relatively light fist. Many of Africa's least aid dependent autocrats are also among its most violent. The Online Appendix illustrates the number of days on which a repressive event occurred by country. Zimbabwe's Robert Mugabe ranks as Africa's most violent, consistent with anecdotal accounts. As Robinson and Torvik (2009) and Hafner-Burton et al. (2014) make clear, Mugabe routinely employs repression to discourage opposition voters. Liberia's Samuel Doe, Sudan's Omar al-Bashir, and Angola's José Eduardo dos Santos also acquired similar reputations in the absence of Western aid. In all, the dataset includes a total of 295,177 country-days in Africa since 1990. Of these, SCAD records a repression event on 2,277 days, or just less than 1% of total country-days.

Virtually all of Africa's post-Cold War autocrats govern with nominally democratic political institutions: presidential term limits, multiparty legislatures, and regular elections. They have little choice. As scholars observe, Western donors virtually require nominally democratic institutions in exchange for development aid and debt relief.<sup>56</sup> These regular elections constitute focal moments for popular protest. Since elections can help citizens coordinate otherwise dangerous anti-regime behavior, protests are far more likely during election seasons than otherwise.<sup>57</sup> I draw data on elections from the National Elections Across Democracy and Autocracy (NELDA) dataset. Introduced by Hyde and Marinov (2012), the NELDA dataset records the dates of every election around the world since 1960. To accommodate periods of tension before and after election day, I also control for whether day  $t$  in country  $i$  falls within the 30 days before and after election day.

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<sup>54</sup>Feenstra et al. (2013).

<sup>55</sup>Note that the statistical results below are substantively unchanged if Western Aid Dependence $_{i,s}$  is standardized by GDP per capita rather than GDP.

<sup>56</sup>Bratton and van de Walle (1997), van de Walle (2001), Dunning (2004), Levitsky and Way (2010), and Goemans and Marinov (2014).

<sup>57</sup>McFaul (2005), Tucker (2007), Radnitz (2010), Bunce and Wolchik (2010, 2011), and Fearon (2011).



I refer to this 60 day period as an *election season*.

I control for a variety of other day-level events that may be associated with both repression and Western aid dependence. Since one set of models conditions on protests on day  $t - 1$ , all country-days in this sample are subject to some degree of political instability. To accommodate other forms of instability that might render repression more attractive, I control for whether a rebel group engaged in a violent offensive as part of an ongoing civil war on day  $t$ , as well as whether the government engaged in a violent offensive on day  $t$ . I draw this information from the Uppsala Conflict Data Program's (UCDP) Georeferenced Event Dataset, introduced by Sundberg and Melander (2013). This dataset includes day-level conflict events that occurred as part of inactive conflicts: those that did not exceed the 25 battle death annual threshold. When deciding whether to protest, citizens may draw on the autocrat's history of repression up to day  $t - 1$ . Likewise, when deciding whether to repress, autocrats may draw on their citizens' record of protest up to day  $t - 1$ . These histories may also be associated with Western aid dependence. To accommodate this, I create the variables  $\text{Repression History}_{it}$  and  $\text{Protest History}_{it}$ , which record the proportion of days over the previous two years of the autocrat's tenure that have witnessed a repression or protest event, respectively.<sup>58</sup>

I control for country-year factors as well. Poor economic conditions may render country  $i$  a more attractive recipient of Western aid, may dispose citizens to protest, and may dispose autocrats to repress. To accommodate this, I control for the employment rate, real GDP, and GDP per capita during year  $s$ , all drawn from the Penn World Tables. Oil reserves may have a similar effect, particularly if resource wealth discourages Western creditors and renders power more valuable to autocrats. Finally, I control for country  $i$ 's reliance on Chinese aid in year  $s$ , which I measure as:<sup>59</sup>

$$\text{Chinese Aid Dependence}_{is} = \frac{\text{Total Chinese Aid}_{is}}{\text{GDP}_{is}}$$

where  $i$  indexes country and  $s$  indexes year. I draw  $\text{Total Chinese Aid}_{is}$  from the AidData project. Definitions and descriptive statistics for all variables appear in the Online Appendix.

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<sup>58</sup>Note that the results below are not sensitive to the duration of the temporal window.

<sup>59</sup>A range of scholars have suggested that the availability of Chinese aid may render autocrats less vulnerable to Western human rights norms. For more on China's engagement with Africa, see Brautigam (2009), McBride (2008), Power et al. (2012), Rotberg (2008), Tan and Bueno de Mesquita (2013), and Taylor (2006).

### 3.3. Model Specification

The baseline model estimates the probability of repression in country  $i$  on day  $t$  as a function of Western aid dependence in year  $s$ :

$$\begin{aligned} \text{logit} [\Pr (\text{Repression}_{it} = 1 | \text{Protest}_{it-1} = 1)] &= \alpha + \beta \ln (\text{Western Aid Dependence}_{is}) \\ &+ \psi X_{is} + \kappa Z_{it} + \gamma_j + \epsilon \end{aligned} \quad (1)$$

where  $i$  indexes country,  $j$  indexes autocrat,  $s$  indexes year, and  $t$  indexes day. The vector  $X$  gives country-year covariates, while the vector  $Z$  gives country-day covariates. This baseline model restricts attention to country-days where a protest emerged *the day before*. I also estimate a variant of (1) that does not restrict attention to days on which a protest occurred on day  $t - 1$ . If protest selection bias is present, the estimated effect of Western Aid Dependence <sub>$is$</sub>  in the conditional model should be stronger than in the full sample.

A range of unobserved factors may condition both Western aid dependence and the day-level probability of repression in Africa's autocracies. To account for these features, I employ a full set of autocrat-level random effects, given by  $\gamma_j$ . Since no autocrat presided over two countries, these autocrat-level effects render country-level effects redundant. This is important, for country  $i$  could be particularly well represented in the SCAD dataset because of unobserved factors that render its affairs of greater interest to Western readers.<sup>60</sup> Autocrat-level effects accommodate this.

Random effects estimators depart from fixed effects estimates in several respects. Like fixed effects estimators, random effects estimators let intercepts vary by unit: in this case, autocrats. Unlike fixed effects estimators, random effects estimators assume that unit intercepts arise from a normal distribution with finite variance. Because these unit intercepts are estimated directly from the data, random effects models can also estimate the effect of variables that are set at the unit level, such as Western aid dependence. Since equation (1) includes day- and year-level covariates, moving to a random effects estimator is imperative to avoid *overstating confidence*. Estimating (1) with a standard fixed effects estimator would impose an independence assumption across observations. For day-level variables, this may be appropriate. But because Western aid allocations are set annually, observations within country-years on the explanatory variable of interest are

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<sup>60</sup>For instance, these autocrat-level effects accommodate the possibility that country  $i$ 's affairs are over-reported in the news sources that Salehyan et al. (2012) employ to construct the SCAD dataset.

heavily correlated. Ignoring this dependence, as fixed effects estimators do, yields standard errors that are considerably smaller than they should be.<sup>61</sup>

### 3.4. Results

The results appear in Table 1. Model 8 corresponds to the baseline estimating equation in (1). Model 7 is identical to equation (1), but does not restrict attention to days following mass protests. Models 5 and 6 exclude Chinese Aid Dependence<sub>*is*</sub>, which is available only after 2000 and so halves the sample size. Models 1 through 4 make clear that the effect of Western Aid Dependence<sub>*is*</sub> is robust to specification choice.

The results in Table 1 confirm that autocrats who are more dependent on Western aid are less likely to employ repression on day *t*. The effect is substantively meaningful, as the odds ratios at the bottom of Table 1 make clear. In Model 7, a one percent increase in Western aid dependence implies that, on any given day, the odds of repression are but 52% as great as otherwise. In Model 8, a one percent increase in Western aid dependence implies that the odds of repression are only 26% as great as otherwise. Owing to the large sample size, these effects are precisely estimated.

The difference between Models 7 and 8 – indeed, across the Full Sample and Protest Response models – suggests the presence of protest selection bias. From Section 3.1, Models 2, 4, 6, and 8 include observations from countries like Eritrea and Equatorial Guinea, where Presidents Isaias Afewerki and Teodoro Obiang, respectively, have less need for outright repression because their citizens dare not protest. By conditioning only on days when citizens are willing to protest and then asking how autocrats *respond* the day after, Models 2, 4, 6, and 8 exclude these country-day observations. Accordingly, Models 1, 3, 5, and 7 suggest a much weaker relationship between Western aid dependence and repression than Models 2, 4, 6, and 8.

Again, the results in Table 1 are not causal. They could be driven by two factors. First, Africa's aid dependent autocrats may be less likely to employ violence against protesters because of the threat of Western punishment. Alternatively, the results could stem from donor selection bias. Western donors may direct development aid to autocrats who exhibit stronger commitments to human rights.

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<sup>61</sup>For more, see Gelman and Hill (2006).

**Table 1. Repression and Western Aid Dependence**

	Model 1 Full Sample Logit	Model 2 Protest Response Logit	Model 3 Full Sample Logit	Model 4 Protest Response Logit	Model 5 Full Sample Logit	Model 6 Protest Response Logit	Model 7 Full Sample Logit	Model 8 Protest Response Logit
$\ln$ Western Aid Dependence $_{i,t}$	-0.476** (0.035)	-1.542** (0.087)	-0.527** (0.037)	-1.797** (0.096)	-0.16935** (0.051)	-0.959** (0.102)	-0.651** (0.081)	-1.356** (0.178)
$\ln$ Chinese Aid Dependence $_{i,t}$							-0.327** (0.022)	-0.398** (0.035)
Election Season $_{i,t}$			1.979** (0.065)	2.424** (0.147)	2.093** (0.068)	2.815** (0.167)	2.834** (0.110)	2.652** (0.206)
Election Day $_{i,t}$			0.919** (0.309)	2.168** (0.499)	0.931** (0.316)	2.209** (0.509)	1.180** (0.491)	1.895** (0.706)
Protests $_{i,t-1}$	1.144** (0.052)		1.159** (0.053)		0.885** (0.059)		0.167† (0.101)	
Repression History $_{i,t}$					1.633** (0.218)	1.650** (0.380)	0.621* (0.309)	1.478** (0.530)
Protest History $_{i,t}$					3.423** (0.211)	3.863** (0.354)	1.660** (0.393)	1.262† (0.680)
Civil Conflict Event: Non-State $_{i,t}$					0.023 (0.072)	0.072 (0.132)	-0.060 (0.181)	0.434 (0.317)
Civil Conflict Event: State $_{i,t}$					-0.278 (0.083)	-0.182 (0.249)	-0.375** (0.107)	-0.247 (0.394)
$\ln$ GDP Per Capita $_{i,t}$					-5.116** (0.337)	-4.189** (1.002)	-7.394** (0.800)	-2.158** (1.825)
$\ln$ Real GDP Expenditure Side $_{i,t}$					0.805** (0.233)	3.691** (0.839)	1.639 (0.559)	-0.053** (1.375)
Oil Supply $_{i,t}$					0.277** (0.023)	0.145 (0.106)	0.035 (0.045)	1.863** (0.418)
Employment Rate $_{i,t}$					-16.873** (0.993)	-17.441** (2.103)	-35.975** (1.705)	-25.757** (4.212)
Constant	-3.127** (0.498)	7.957** (0.967)	-2.951** (0.528)	9.689** (1.065)	25.929** (1.665)	-0.325 (6.314)	49.302** (3.834)	27.399† (15.411)
Autocrat Effects	Random	Random	Random	Random	Random	Random	Random	Random
$N$	251,526	23,192	251,526	23,192	251,526	23,192	83,923	9,803
Significance levels:	† : 10%	* : 5%	** : 1%					
Odds Ratios [95% Confidence Intervals]								
$\ln$ Western Aid Dependence $_{i,t}$	0.621 [0.586 , 0.658]	0.214 [0.186 , 0.247]	0.590 [0.555 , 0.627]	0.166 [0.142 , 0.194]	0.844 [0.77 , 0.92]	0.383 [0.32 , 0.45]	0.521 [0.46 , 0.60]	0.257 [0.192 , 0.345]

## 4. Exploiting HIPC Debt Relief: Donor Selection Bias

To account for donor selection bias, I employ a differences in differences estimator. HIPC debt relief negotiations constituted a temporal shock that rendered repression particularly costly for Africa's autocrats. During these negotiations, Africa's autocrats *needed* the approval of Western donors, and the prospect of punishment for human rights violations was credible. Two features of the HIPC initiative make it particularly attractive for a differences in differences identification strategy.

### 4.1. Lenient Selection Criteria

The IMF and World Bank launched the HIPC Initiative in 1996 to “[ensure] that no poor country faces a debt burden it cannot manage.” In principle, the program grants debt relief to “[free] up resources for social spending” in the world's poorest, most heavily indebted countries. According to the IMF, countries under consideration for debt relief are required to meet stringent criteria, which ostensibly ensure that, after debt relief, governments direct money to anti-poverty programs that would otherwise have gone to debt service.

Debt relief negotiations begin with the publication of a “decision point” document. Qualification for this first stage is straightforward. Governments must have an ongoing relationship with the IMF and World Bank, as well as a Poverty Reduction Strategy Paper (PRSP). The decision point document identifies a set of “good governance” conditions that must be satisfied for debt relief. Once governments satisfy these conditions, they reach the completion point, when debt relief is full and irrevocable. As of May 2016, 30 African countries had qualified for debt relief under the HIPC Initiative, and another three had been identified as eligible – based on their indebtedness and GDP levels – to begin the approval process. The HIPC Initiative has provided debt-service relief worth greater than \$75 billion, or an average of roughly \$2 billion per country. Relative to GDP, this is a non-trivial sum. When Congo's debt was forgiven in 2010, for instance, its GDP was roughly \$10 billion. For countries that received relief, debt service payments declined by 2 percentage points of GDP between 2001 and 2013.<sup>62</sup>

The years between the decision point and the completion point determine whether a government is granted

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<sup>62</sup>International Monetary Fund (2014).

debt relief.<sup>63</sup> Accordingly, there is abundant evidence that Africa's autocrats modify their behavior during these years to persuade the IMF and World Bank of their commitment to good governance. Again, Congo is instructive. Sassou Nguesso siphons as much as \$300 million per year from state coffers, and so has acquired a reputation for gross corruption.<sup>64</sup> Still, the Congolese government reached the decision point on January 31, 2006, at which point negotiations commenced. The IMF and World Bank identified two central conditions for debt relief: that the government permit quarterly financial audits of the state oil company and that Sassou Nguesso's son, Denis Christel, be removed from his position atop its marketing branch. Sassou Nguesso conceded to both and, according to IMF officials, the proportion of oil revenue accounted for in the national budget increased from roughly 60% prior to the decision point to 80% afterwards.<sup>65</sup> After debt relief, Sassou Nguesso backtracked. Quarterly audits ceased, Denis Christel was named second in charge of the state oil company, and oil revenue accounted for in the national budget plummeted. But since Congo's debt had been forgiven, the IMF and World Bank had no leverage to intervene.

By focusing the international community's attention and credibly threatening sanctions, HIPC debt relief negotiations increased the expected costs to Africa's autocrats of corruption and repression. Accordingly, I construct the variable  $HIPC\ Negotiations_{it}$ , which assumes value 1 if day  $t$  in country  $i$  occurred between the publication of its decision point document and its completion point document. The dataset includes nearly 27,000 country-days for which  $HIPC\ Negotiations_{it}$  assumes value 1, encompassing 21 autocrats from 19 countries. The dataset assumes value 0 for nearly 270,000 country-days, encompassing 107 autocrats from 50 countries. For some countries, debt relief negotiations concluded quickly, with only 71 days elapsing between the decision point document and the completion point document. For others, negotiations required a decade to complete.<sup>66</sup>

Like development aid, HIPC debt relief is assigned by Western creditors, and hence potentially subject to donor selection bias. In practice, however, this is not a concern, as human rights activists observed.<sup>67</sup> Of the 33 African countries that qualify for HIPC status, only three have *not* received debt relief. The list of autocrats who have received debt relief is startling. It includes Blaise Compaoré of Burkina Faso, Paul Biya of Cameroon, Denis Sassou Nguesso of Congo, and Yoweri Museveni of Uganda, who have collectively ruled

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<sup>63</sup>Interview with senior IMF official, December 2012.

<sup>64</sup>Global Witness (2004, 2005, 2007).

<sup>65</sup>Interviews with senior IMF official in December 2012.

<sup>66</sup>For the start and end dates of all countries, see the Online Appendix.

<sup>67</sup>Easterly (2002), Thomas (2001), Birdsall and Williamson (2002), and Easterly (2009).

their impoverished countries for 130 years, and whose human rights violations are regularly decried.<sup>68</sup> It also includes Pierre Nkurunziza of Burundi and Paul Kagame of Rwanda, whose human rights violations were overlooked after the Rwandan genocide but increasingly generate international outrage.<sup>69</sup> Even Gambia's Yahya Jammeh was granted debt relief. In power since a 1994 coup, in 2011 Jammeh announced that he would rule for "one billion years, God willing."<sup>70</sup>

The *only* qualifying countries not currently being considered for debt relief are Eritrea, Somalia, and Sudan. Somalia has long lacked a functioning central government. Sudan is regarded as a state sponsor of terror and its president, Omar al-Bashir, is subject to an arrest warrant from the International Criminal Court (ICC). President Isaias Afewerki of Eritrea uses his country's compulsory, indefinite military service requirement to create a pool of slave labor, which the government uses for mining.<sup>71</sup> In short, the IMF and World Bank initiated debt relief negotiations with virtually *all* of Africa's autocrats. Donor selection bias is not a concern.

If the Bretton Woods institutions ultimately extended debt relief to virtually all of Africa's autocrats, was the threat of sanction really credible? The answer is yes. Africa's autocrats were eager to finalize debt relief negotiations, the better to escape the international community's scrutiny. The Bretton Woods institutions knew this, and so used the *duration* of negotiations strategically. As a result, Africa's most venal, abusive autocrats were subjected to long negotiation periods. In Guinea negotiations lasted some 12 years, in Guinea-Bissau 10 years, in DRC seven years, in Gambia seven years, in Cameroon six years, in Congo five years, and in Burundi four years. By contrast, Uganda's Museveni was granted debt relief after only two months of negotiations in early 2000, a reflection of his status as a Western darling during his early years. Virtually all of Africa's autocrats received debt relief, but the duration of negotiations varied dramatically according to the autocrat's economic and human rights record.

## 4.2. Substantial Human Rights Records

Many autocrats who received debt relief were in power long before HIPC negotiations began and remained long after they concluded. This constitutes the second feature of the HIPC debt relief program that favors

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<sup>68</sup> For Cameroon, see Le Nouvel Observateur (2009) and Albaugh (2011). For Angola, see Global Witness (2004) and Human Rights Watch (2012a). For Uganda, see Human Rights Watch (2012b). For Congo, see Global Witness (2004, 2005, 2007).

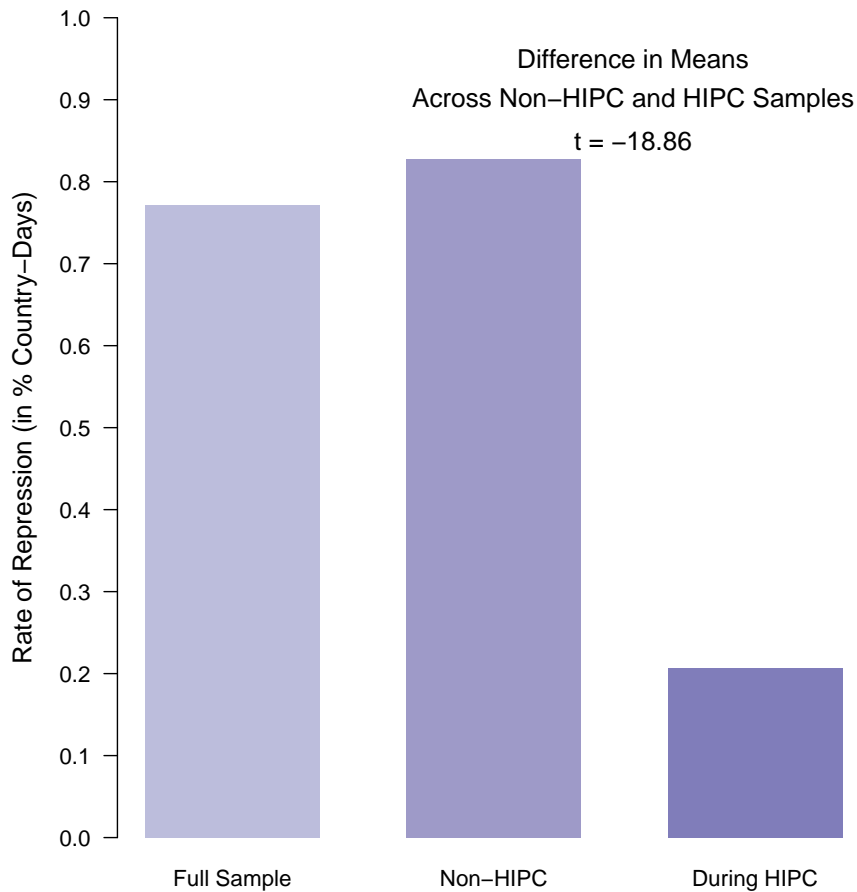
<sup>69</sup> For Burundi, see Human Rights Watch (2011a). For Rwanda, see Reporters Without Borders (2010).

<sup>70</sup> Ruble (2015).

<sup>71</sup> For more on Eritrea, see Amnesty International (2013) and Human Rights Watch (2011b, 2013).

differences in differences estimation. Because each autocrat accumulated a substantial human rights record both before and after debt relief, each autocrat constitutes his own counterfactual. As a result, the differences in differences estimator controls for any unobserved, autocrat-level characteristics that might be correlated with both HIPC debt relief negotiations and repression. Conditional on the range of day- and year-level control variables described above, there is no reason to expect that, during HIPC debt relief negotiations, an individual autocrat would have modified his rate of repression for any reason *but* Western pressure.

**Figure 2**



**Notes:** Daily rates of repression in Africa's autocracies since 1989. The *y*-axis measures the percentage of country-days on which repression occurred for the three samples along the *x*-axis. Outside HIPC negotiations, a repression event occurred on 0.8% of country-days. During HIPC negotiations, a repression event occurred on roughly 0.2% of country-days. The difference in means for the two samples is significant at the 0.001% level.



### 4.3. Model Specification

Visual inspection of the data makes clear that autocrats engaged in far less repression during HIPC debt relief negotiations than otherwise. Figure 2 gives the percentage of country-days on which Africa's autocrats employed repression for the three samples along the  $x$ -axis. In the full sample, with HIPC and non-HIPC days aggregated, repression occurred on roughly 0.78% of country-days. The second and third bars give the rates of repression when the full sample is divided into non-HIPC days and HIPC days, respectively. The difference, as the  $t$ -statistic for a difference in means suggests, is striking. During HIPC debt relief negotiations, the rate of repression among Africa's autocrats plummeted, to roughly 0.21%. Outside HIPC negotiations, the rate of repression was 0.83%, nearly four times greater.

The baseline estimating equation is

$$\begin{aligned} \text{logit} [\text{Pr} (\text{Repression}_{it} = 1 | \text{Protest}_{it-1} = 1)] &= \alpha + \beta (\text{HIPC Negotiations}_{it}) \\ &+ \psi X_{it} + \kappa Z_{it} + \gamma_j + \epsilon \end{aligned} \quad (2)$$

where  $X_{it}$  gives the vector of day-level controls from equation (1),  $Z_{it}$  the vector of year-level controls, and  $\gamma_j$  a full set of autocrat fixed effects. Again, by comparing autocrat  $j$ 's record of repression during HIPC negotiations with his record both before and after – that is, by focusing exclusively on within-autocrat variation – this fixed effects model ensures that  $\beta$  is not a function of donor selection bias. As long as the days of debt relief negotiations are, on average, identical in all salient respects to those on which HIPC negotiations did not occur, the estimated effect will have a causal interpretation. As above, I estimate both this conditional model and a second model, which does not condition on day  $t - 1$  protests. If protest selection bias is salient, then the estimated effect of  $\text{HIPC Negotiations}_{it}$  will be larger in the first model than the second.

This identification strategy focuses on the *beliefs* of Africa's autocrats about what *would* happen if they engaged in repression. Accordingly, whether Western creditors are actually more likely to punish human rights abuses during HIPC negotiations is immaterial. The salient point is that Africa's autocrats modify their repression strategies based on their beliefs about the likelihood of Western punishment, and hence about the costs and benefits of repression.

## 4.4. Estimation Results

The results for the protest selection sample appear in Table 2. As a baseline, Model 1 includes only HIPC Negotiations<sub>it</sub>. Model 2 adds the election season variables, Model 3 adds the day-level controls that capture political instability, and Model 4 includes the year-level controls. As robustness checks, Model 5 estimates the full model with a rare events logit, and Models 6 and 7 with random effects rather than fixed effects.

The coefficient estimate on HIPC Negotiations<sub>it</sub> is very similar across models. In each, autocrats curtail repression dramatically during HIPC negotiations. The effect holds regardless of prevailing political instability, the proximity of day  $t$  to an election, and the range of economic conditions that might induce popular grievances or render power more attractive to the autocrat. The associated odds ratios appear at the bottom of Table 2, along with 95% confidence intervals. They suggest that, during HIPC negotiations, the daily odds of repression are between 5% and 20% as great as otherwise. Owing to the large sample size, these estimates are relatively precise.

To accommodate protest selection bias, in Table 3 I reestimate Models 1 through 6 without conditioning on protests on day  $t - 1$ . As in Table 1, the coefficients across models are consistently lower than those in Table 2. The associated odds ratios suggest that, during HIPC negotiations, the daily odds of repression are between 50% and 60% as great as otherwise. The difference in the coefficients clearly reveals the importance of accounting for protest selection bias. Citizens are strategic. They refrain from protesting if they expect to be repressed when they do. This suggests that, by not accommodating protest selection bias, existing literature understates the constraining effects of Western aid dependence.

The coefficients on the control variables are substantively similar to those in Table 1 and equally instructive. Again, the rate of repression spikes during election seasons – the 30 days before and after election days – and on election day itself. The effect is similar in magnitude to HIPC negotiations. Whereas HIPC negotiations reduced the daily odds of repression by a factor of 10, election seasons *increase* the daily odds of repression by a factor of 10. This too is consistent with existing research. Students of autocratic politics increasingly find that the regular elections occasioned by nominally democratic institutions constitute focal moments for collective action, and hence prove destabilizing. During election seasons citizens are more engaged

in politics and more aware of their neighbors' discontent.<sup>72</sup> Opposition leaders have strong incentives to coordinate mass protests and alert citizens to electoral fraud.<sup>73</sup> As a result, scholars find, the rate of popular protest is significantly greater during election seasons than at any other time of year. Table 2 reflects this.

#### 4.5. When Restraint is Least Likely

During HIPC debt relief negotiations – when Western creditors enjoyed substantial leverage and credibly threatened punishment – Africa's autocrats were far less likely to repress their citizens than at any other point during their tenures. Given the magnitude of the effect, it is worth asking whether Western creditors have constrained Africa's autocrats *even* during moments of crisis.

To do so, I focus on periods of sustained protests.<sup>74</sup> Of the 113 autocrats in the dataset, 81 confronted protests that were sustained for a period of seven days. Unsurprisingly, these moments of crisis are relatively rare. Of the 295,177 total country-days in the dataset, only 18,239 were preceded by seven days of sustained protests.<sup>75</sup> The rate of repression on country-days preceded by a week of sustained protests is much higher than in the full sample. Figure 3 illustrates this. In the full sample, an episode of repression occurs on just less than 1% of country-days; this same descriptive statistic appeared in Figure 2. On country-days preceded by a week of sustained protests, however, the rate of repression spikes, to greater than 5% of country-days. This should be no surprise. As Aidt and Leon (2015) make clear, protests have consistently toppled Africa's post-Cold War autocrats. Sustained protests are especially threatening.

Has reliance on Western creditors constrained repression *even when* autocrats confront a week of sustained protests? Again, the descriptive statistics are highly suggestive. The final two columns of Figure 3 present the mean rates of repression on country-days preceded by a week of sustained protests, outside HIPC negotiations and during, respectively. The difference is striking. Confronted with a week of sustained protests, autocrats are far more likely to respond with repression, unless that week of protests occurred during HIPC debt relief negotiations. Then, constrained by Western creditors to at least appear democratic, Africa's

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<sup>72</sup>Kuran (1991), Tucker (2007), van de Walle (2006). Medina (2007) and Hollyer et al. (2014) also emphasize the role of common knowledge in protest.

<sup>73</sup>Beissinger (2002), Javeline (2003), McFaul (2005), Radnitz (2010), Bunce and Wolchik (2010, 2011), Fearon (2011), and Dobson (2012).

<sup>74</sup>I thank an anonymous reviewer for this insightful suggestion.

<sup>75</sup>For more descriptive statistics about this sustained protest sample, see the Online Appendix. Note that these periods of sustained protest are distributed relatively uniformly across autocrats. Of the 110 autocrats in the full dataset, 82 confronted a week of sustained protests.

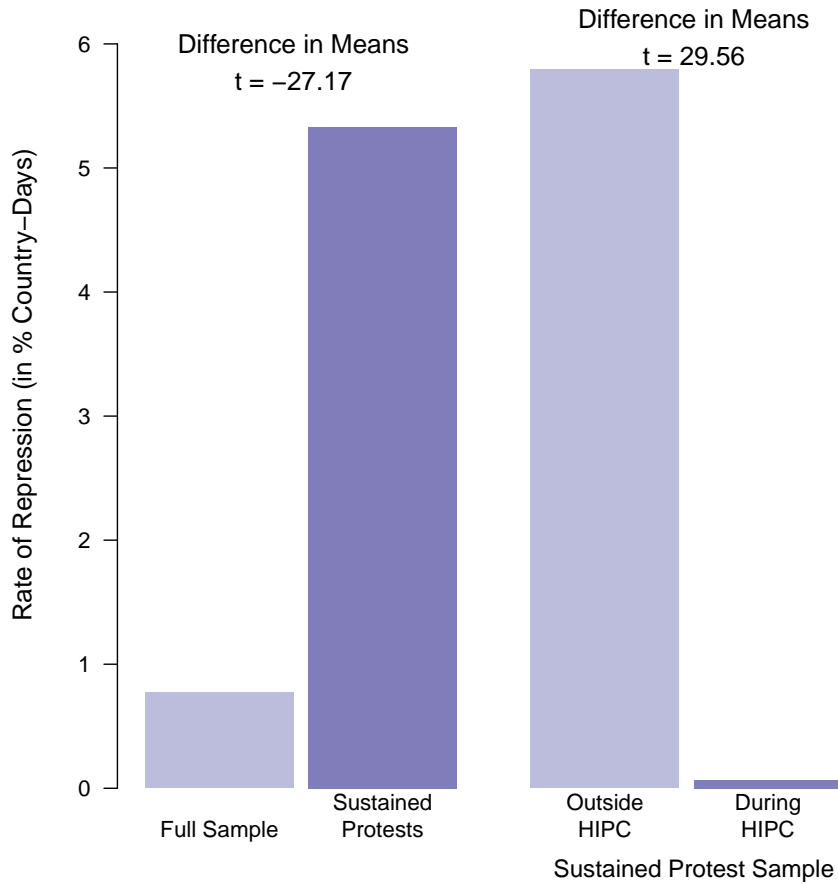
Table 2. Repression and HIPC Debt Relief Negotiations, Conditional on  $Protests_{it-1} = 1$

	Model 1 Logit	Model 2 Logit	Model 3 Logit	Model 4 Logit	Model 5 Rare Events	Model 6 Logit	Model 7 Logit
HIPC Negotiations <sub>it</sub>	-1.432** (0.519)	-1.829** (0.509)	-2.824** (0.541)	-2.932** (0.556)	-2.899** (0.541)	-1.642** (0.495)	-2.939** (0.552)
Election Season <sub>it</sub>		2.008** (0.132)	2.625** (0.148)	2.897** (0.170)	2.879** (0.169)	1.977** (0.131)	2.817** (0.166)
Election Day <sub>it</sub>		2.170** (0.479)	2.331** (0.508)	2.342** (0.520)	2.336** (0.518)	2.093** (0.470)	2.369** (0.521)
Protests <sub>it-1</sub>							
Repression History <sub>it</sub>			1.424** (0.363)	1.862** (0.396)	1.856** (0.395)		1.695** (0.395)
Protest History <sub>it</sub>			4.644** (0.316)	3.901** (0.358)	3.887** (0.356)		4.147** (0.360)
Civil Conflict Event: Non-State <sub>it</sub>			0.044 (0.138)	0.136 (0.127)	0.162 (0.126)		0.102 (0.131)
Civil Conflict Event: State <sub>it</sub>			-0.334 (0.255)	-0.284 (0.259)	-0.207 (0.258)		-0.302 (0.259)
ln GDP Per Capita <sub>it</sub>				-8.085** (1.084)	-8.094** (1.080)		-5.942** (1.024)
ln Real GDP Expenditure Side <sub>it</sub>				8.188** (0.796)	8.116** (0.793)		6.334** (0.796)
Oil Supply <sub>it</sub>				0.151 (0.123)	0.130 (0.123)		0.147 (0.109)
Employment Rate <sub>it</sub>				-20.649** (2.118)	-0.208** (2.110)		-19.105** (1.992)
Constant	-20.566 (2839.131)	-22.461 (2810.272)	-23.200 (2798.916)	-28.261 (2797.406)	1.064e+07 (2.786e+03)	-7.145** (0.050)	-24.230** (4.929)
Autocrat Effects	Fixed	Fixed	Fixed	Fixed	Fixed	Random	Random
N	26,164	26,101	26,101	23,537	23,537	26,101	23,537
Significance levels:	† : 10%	* : 5%	** : 1%				
Odds Ratios [95% Confidence Intervals]							
HIPC Negotiations <sub>it</sub>	0.239 [0.102, 0.561]	0.161 [0.070, 0.371]	0.059 [0.0244, 0.145]	0.053 [0.021, 0.133]	0.053 [0.021, 0.133]	0.193 [0.086, 0.437]	0.053 [0.021, 0.131]

**Table 3. Repression and HIPC Debt Relief Negotiations, Full Sample**

	Model 1 Logit	Model 2 Logit	Model 3 Logit	Model 4 Logit	Model 5 Rare Events	Model 6 Logit	Model 7 Logit
HIPC Negotiations <sub>it</sub>	-0.501** (0.164)	-0.655** (0.167)	-0.680** (0.172)	-0.645** (0.184)	-0.637** (0.184)	-0.637** (0.165)	-0.628** (0.183)
Election Season <sub>it</sub>		1.926** (0.063)	2.051** (0.066)	2.090** (0.068)	2.088** (0.068)	1.923** (0.063)	2.089** (0.068)
Election Day <sub>it</sub>		0.908** (0.306)	0.889** (0.311)	0.945** (0.322)	0.962** (0.322)	0.906** (0.299)	0.939** (0.318)
Protests <sub>it-1</sub>	1.218** (0.049)	1.237** (0.050)	0.863** (0.055)	0.889** (0.059)	0.889** (0.059)	1.244** (0.050)	0.891** (0.059)
Repression History <sub>it</sub>			2.743** (0.208)	1.575** (0.220)	1.574** (0.220)		1.643** (0.217)
Protest History <sub>it</sub>			2.090** (0.179)	3.568** (0.212)	3.561** (0.212)		3.483** (0.201)
Civil Conflict Event: Non-State <sub>it</sub>			-0.041 (0.076)	0.017 (0.073)	0.024 (0.073)		0.018 (0.073)
Civil Conflict Event: State <sub>it</sub>			-0.244** (0.077)	-0.270** (0.083)	-0.260 (0.083)		-0.275** (0.083)
ln GDP Per Capita <sub>it</sub>				-5.767** (0.409)	-5.756** (0.409)		-5.032** (0.309)
ln Real GDP Expenditure Side <sub>it</sub>				1.192** (0.274)	1.188** (0.274)		1.030** (0.224)
Oil Supply <sub>it</sub>				0.300** (0.025)	0.300** (0.025)		0.268** (0.022)
Employment Rate <sub>it</sub>				-17.796** (1.142)	-17.760** (1.142)		-16.103** (0.794)
Constant	-5.062** (1.010)	-6.789** (1.015)	-6.628** (1.015)	30.121** (2.372)	30.550** (2.371)	-8.547** (0.438)	21.516** (1.351)
Autocrat Effects	Fixed	Fixed	Fixed	Fixed	Fixed	Random	Random
N	295,177	294,337	294,337	261,746	261,746	294,337	261,746
Significance levels:	† : 10%	* : 5%	** : 1%				
Odds Ratios [95% Confidence Intervals]							
HIPC Negotiations <sub>it</sub>	0.606 [0.462 , 0.794]	0.520 [0.395 , 0.683]	0.507 [0.382 , 0.672]	0.524 [0.388 , 0.709]	0.529 [0.391 , 0.715]	0.529 [0.402 , 0.693]	0.534 [0.395 , 0.722]

Figure 3



**Notes:** The first and second bars give the rate of repression in the full sample and the sustained protest sample, respectively. From the first bar, in the full sample, an episode of repression occurs on only 0.8% of country-days; this same statistic appeared in Figure 2 as well. From the second bar, on country-days preceded by a week of sustained protests the rate of repression increases to just over 5% of country-days. The third and fourth bars focus exclusively on the sustained protest sample, where country-days have been preceded by a week of sustained protests. From the third bar, outside of HIPC negotiations, autocrats employed repression on nearly 6% of country-days. During HIPC negotiations, however, autocrats employed repression on less than 0.1% of country-days.

autocrats exercised restraint. This is consistent with the results in Tables 2 and 3: Sustained protests attract the attention of Western creditors as well, rendering repression *even more* costly for Africa’s autocrats.

To probe this more systematically, I estimate a variant of equation (2) above:

$$\begin{aligned} \text{logit} [\text{Pr} (\text{Repression}_{it} = 1 | \text{Protest}_{it-1:t-T} = 1)] &= \alpha + \beta (\text{HIPC Negotiations}_{it}) \\ &+ \psi X_{it} + \kappa Z_{it} + \gamma_j + \epsilon \end{aligned} \quad (3)$$

where  $T$  gives the number of days of sustained protest. When  $T = 1$ , equation (3) is identical to (2). When

$T = 2$ , equation (3) estimates the probability of repression on day  $t$  conditional on protests sustained over the preceding two days. When  $T = 7$ , I focus on the probability of repression conditional on seven days of sustained protests. When  $T = 14$ , I focus on the probability of repression conditional on a fortnight of sustained protests. As above,  $X_{it}$  gives the vector of day-level controls,  $Z_{it}$  the vector of year-level controls, and  $\gamma_j$  a full set of autocrat-level fixed effects. In short, I compare autocrat  $j$ 's record of repression on day  $t$ , after a period of sustained protests, during HIPC negotiations with his record both before and after.

The results appear in Table 4. Model 1 gives the results for  $T = 1$ , and is identical to Model 2, Table 2; I include it for comparison. Model 2 gives the results for  $T = 2$ , Model 3 the results for  $T = 3$ , Model 4 for  $T = 4$ , Model 5 for  $T = 7$ , and Model 6 for  $T = 14$ . Models 7 and 8 include the full set of control variables and are estimated with autocrat-level fixed and random effects, respectively, for  $T = 7$ . The results across models are consistent with the descriptive statistics in Figure 3. Conditional on a sustained period of protest, Africa's post-Cold War autocrats have been far less likely to repress their citizens during HIPC debt relief negotiations than otherwise. Owing to the large sample size, the effects are estimated with relative precision.

Comparing the magnitude of the coefficients across models is instructive. As a baseline from Table 2, Model 1 suggests that, conditional on protests on day  $t - 1$ , the odds of repression during HIPC negotiations are roughly 16% as great as otherwise. After two days of sustained protests, the odds of repression during HIPC negotiations fall to roughly 10% as great. The odds of repression continue to fall with the number of days of sustained protest until, at a full week, the odds of repression are but 2% as great. The list of autocrats who confronted a week of sustained protests during HIPC negotiations and *chose not* to repress includes some of Africa's most loathed, all of whom earned a reputation for human rights violations and gross corruption: Cameroon's Paul Biya, Burkina Faso's Blaise Compaoré, Chad's Idriss Déby, Central African Republic's François Bozizé, Burundi's Pierre Nkurunziza, Niger's Mahamadou Tandja, and Ethiopia's Meles Zenawi, among others. Each confronted a week of sustained protests outside HIPC negotiations. When they did, they responded with repression. During HIPC negotiations, however, when Western pressure was credible and consequential, they responded not with repression, but restraint.

**Table 4. Repression and HIPC Debt Relief Negotiations, Sustained Protest Sample**

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
	$T = 1$	$T = 2$	$T = 3$	$T = 4$	$T = 7$	$T = 14$	$T = 7$	$T = 7$
	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit
HIPC Negotiations <sub>it</sub>	-1.829** (0.509)	-2.308** (0.638)	-2.454** (0.725)	-3.816** (1.072)	-3.913** (1.073)	-3.616** (1.122)	-4.078** (1.176)	-4.307** (1.166)
Election Season <sub>it</sub>	2.008** (0.132)	2.043** (0.142)	2.049** (0.146)	2.035** (0.150)	1.948** (0.153)	1.893** (0.162)	2.749** (0.219)	2.709** (0.213)
Election Day <sub>it</sub>	2.170** (0.479)	2.416** (0.508)	2.507** (0.518)	2.730** (0.547)	2.778** (0.545)	2.616** (0.573)	2.971** (0.577)	3.024** (0.581)
Protests <sub>it-1</sub>								
Repression History <sub>it</sub>							4.296** (0.520)	4.057** (0.515)
Protest History <sub>it</sub>							2.529** (0.413)	2.779** (0.413)
Civil Conflict Event: Non-State <sub>it</sub>							0.253† (0.131)	0.253† (0.133)
Civil Conflict Event: State <sub>it</sub>							-0.120 (0.312)	-0.148 (0.315)
ln GDP Per Capita <sub>it</sub>							-11.047** (1.663)	-9.284** (1.557)
ln Real GDP Expenditure Side <sub>it</sub>							12.199** (1.177)	10.367** (1.136)
Oil Supply <sub>it</sub>							0.085 (0.205)	0.053** (0.188)
Employment Rate <sub>it</sub>							-22.577** (2.931)	-22.061** (2.742)
Constant	-22.461 (2810.272)	-22.517 (2929.639)	-22.516 (3012.831)	-22.601 (3292.447)	-22.514 (3697.038)	-22.460 (4432.595)	-41.067 (3696.909)	-44.015** (8.570)
Autocrat Effects	Fixed	Fixed	Fixed	Fixed	Fixed	Fixed	Fixed	Random
<i>N</i>	26,101	22,607	21,276	20,202	18,043	14,977	16,430	16,430
Significance levels:	† : 10%	* : 5%	** : 1%					
Odds Ratios								
[95% Confidence Intervals]								
HIPC Negotiations <sub>it</sub>	0.161 [0.070, 0.371]	0.099 [0.035, 0.284]	0.086 [0.026, 0.283]	0.022 [0.004, 0.128]	0.020 [0.003, 0.117]	0.027 [0.004, 0.170]	0.017 [0.002, 0.117]	0.013 [0.002, 0.092]



## 5. Conclusion

Modern autocrats confront new challenges. Unable to leverage Great Power rivalries, autocrats must increasingly abide the nominally democratic institutions insisted upon by Western creditors. This threatens their survival in two ways. First, they lack the advantages of single party regimes, which create incentives for elite loyalty despite temporary political and economic shocks to the regime. Second, the regular elections occasioned by nominally democratic institutions constitute focal moments for popular protest, when citizens can act collectively. Autocrats confront the threat of popular protests, this paper shows, with new constraints. Although repression is often regarded as a central feature of authoritarian regimes, dependence on Western donors has limited recourse to repression among Africa's autocrats. Using a differences in differences estimator and controlling for a range of day- and year-level features, I find that Africa's autocrats dramatically curtailed repression during HIPC debt relief negotiations. Moreover, I find consistent evidence that protest selection bias understates the effect of Western aid dependence on repression.

However striking, this evidence remains a function of the international environment that generated it. Among the salient features of the post-Cold War international order is that Africa's autocrats *expect* financial sanctions for human rights violations. This expectation has endured for some 25 years. But as China courts African allies with low interest loans and no political conditions, Africa's autocrats increasingly forgo Western aid in favor of Chinese support. In the competition to provide development aid – the *raison d'être* for many bilateral government agencies and multilateral institutions – Western sources are increasingly losing. In the medium term, this may force Western donors to abandon political and human rights conditions. If so, or if China continues its generous engagement, then the constraining effects of Western aid dependence may prove a unique feature of the immediate post-Cold War international order.

This paper suggests at least two directions for future research. First, although focusing on HIPC debt relief negotiations enables estimation of a plausibly causal effect of Western donor pressure, it comes at a cost. When employing repression, autocrats confront a tradeoff. Although repression may increase an autocrat's probability of survival – even if, *ex ante*, the magnitude of this marginal effect is extremely difficult for an autocrat to anticipate – repression also risks Western support that may be vital to fund a government, purchase political support, or pay the salaries of security officers. For the world's autocrats, in short, there is surely some marginal rate of substitution between repression and sanctions from Western donors. This

paper makes clear that Western donor pressure has constrained repression among Africa's autocracies, but it does not show *how much* leverage is required to purchase an additional unit of restraint. This marginal rate of substitution likely depends on a range of factors, which future research should elucidate.

Second, scholars have long found that autocrats ensure their survival by employing a range of political tools: patronage, repression, and formal political institutions that create incentives for elite compliance. Reliant on Western donors, Africa's autocrats lost the ability to make wholesale institutional changes following the end of the Cold War. This paper shows that many of Africa's autocrats have also lost the easy recourse to repression that their predecessors enjoyed. Even with two of the traditional tools of survival gone, however, Africa's autocrats are learning to survive. Between 1986 and 2000, the number of autocracies in Africa fell dramatically, from 45 to 30. Since 2000, however, Africa's autocrats have proven more resilient. Most broadly, this paper suggests that scholars should expand their search for the tools of autocratic survival in the modern world beyond repression and political institutions.

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