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Recession, then Repression? Protests, Human Rights, and the International Monetary Fund

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Bachelor of Arts, University of North Carolina Asheville, 2016

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An abstract of
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Abstract

Recession, then Repression? Protests, Human Rights, and the International Monetary Fund
By Gray M. Barrett

How do international institutions affect domestic politics and policy? In particular, how does the International Monetary Fund (IMF) condition the behavior of citizens and the state, and to what extent can it effect human rights? One side of this debate posits that the IMF should only improve human rights by improving access to markets, financial stability, and resolving balance of payments crises. On the other hand, being under a program with the IMF – particularly a program with adjustment terms attached – could make human rights worse by leading to painful, structural changes that displace labor and disrupt livelihoods. In this thesis, I use several techniques and improvements on existing work, including matching and monthly data, to assess these arguments. I find evidence for the latter claim: that being under an IMF program leads to increased protests and repression. However, this result is driven entirely by the inclusion of conditionality terms, economic policy adjustment conditions dictated by the IMF, which lead to worse human rights performance by inducing protest and repressive action by the state in return. Moreover, citizens may anticipate the effects of a program by protesting – and the state may respond by repressing – even before a program's onset, though this result is not robust. Examining this issue is of normative and empirical importance, probing the extent to which large international institutions can lead to domestic political strife and human rights violations and offering lessons for future IMF program design.

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For Lia, Mom, Dad, and Bryn

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1 Introduction

1.1 Motivating case and questions

In April 2019, Pakistan negotiated a large loan package with the International Monetary Fund (IMF), an international financial body that loans money to countries in financial crisis, to relieve a balance of payments crisis (Findlay and Bokhari 2019). Despite earlier criticism of the lending body, this is Prime Minister Khan's second such IMF loan since he took office, following the nearly $7 billion package that his government negotiated in 2013. Khan is not new in seeking assistance from the IMF – Pakistan has negotiated over 20 programs with the IMF since its membership within the organization (Khan 1999, Javed 2016), with some critics claiming that the country is fundamentally unable to reform its economy in a structural, meaningful sense (Zafar 2017) and faces little pressure to do so from the IMF or the countries that finance it (Ahmad and Mohammed 2012, Fund 2019).

Despite the fact that Pakistan has little incentive to implement structural reforms, many domestic interest groups still fear a deal with the IMF. In response to the specter of another loan, union, civil society groups, and opposition party members throughout Pakistan organized protests against the "slavery of the IMF" (Report 2019, Mashaal 2019). Citing the Universal Declaration of Human Rights, these leaders argue that dealing with international financial institutions (IFIs) like the IMF has led to increased privatization, the gutting of unions, and the violation of fundamental and workers' rights. Such protests against the IMF are common. In other states, protesters have been met with repression by the state (Stubbs and Kentikelenis 2017), as these protesters decry increases in privatization, austerity, and loan terms that dictate tough, structural changes in the state's economy. So far, Pakistan has held off on repressing this particular set of demonstrators, but as tensions increase will this remain the case?

In previous years under IMF programs, Pakistan has not held off on repressing its people. The figure below shows trends in protests and repression during the most recent IMF programs, marked by the light blue.

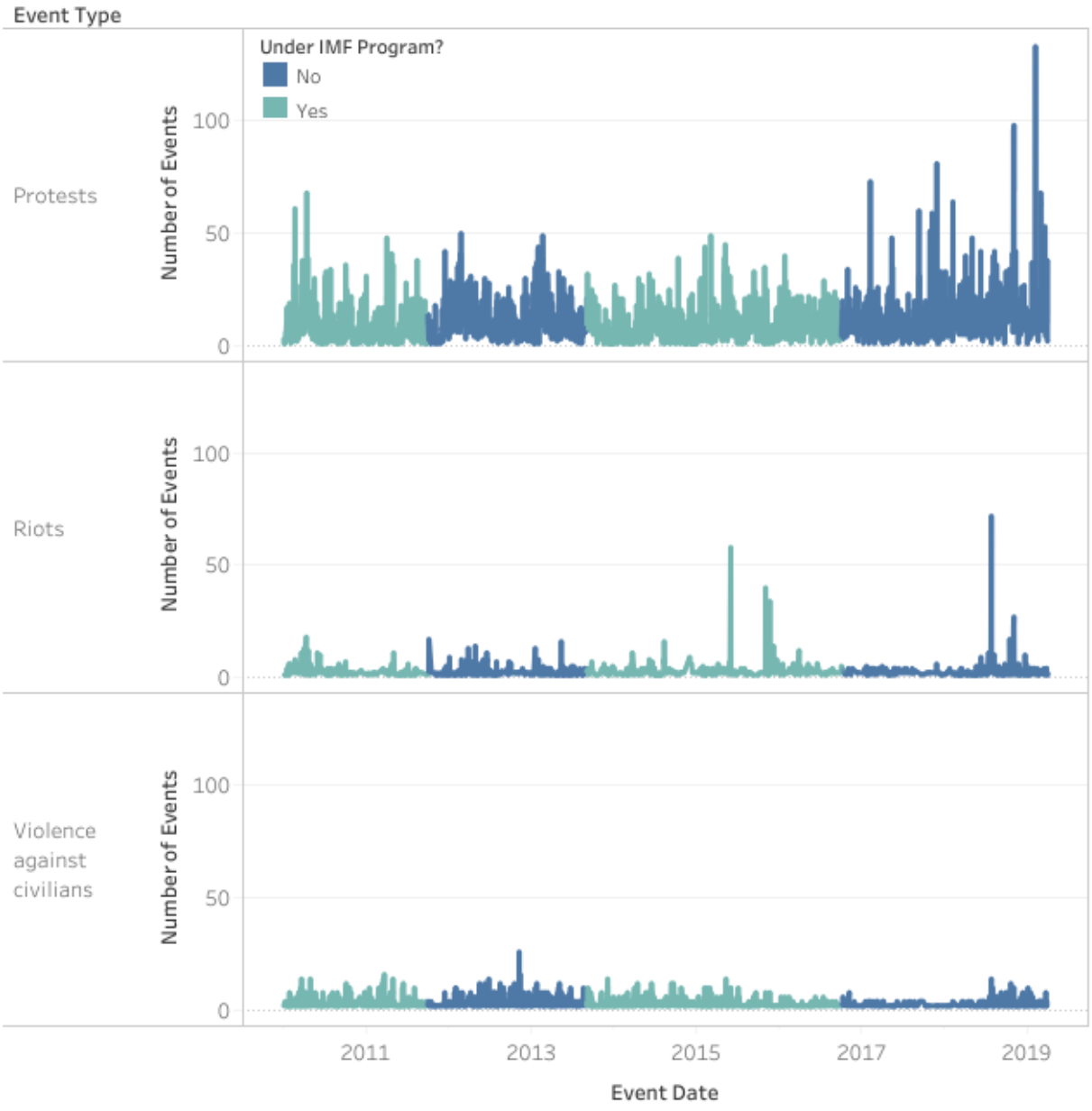

Figure 1: Recent Trends in Protest and Repression in Pakistan

However, a simple criticism of this point is that protests and repression during the years under IMF programs could simply be correlated to other, outside events; the programs themselves may not be what is driving the protests or the repressive behavior by the state in return. How do we know that it's the institution that is leading to changing human rights outcomes? Is the dealing with an IMF that bad for a country and its citizens, and is the upcoming deal between the IMF and Pakistan likely to produce similar results?

More broadly, how do international financial institutions affect human rights? Does borrowing from one of these institutions induce positive changes, such as making a state less likely to repress its own citizens? Or does interaction make a state worse off, as critics of globalization would contend? Despite continued interactions between states and IFIs, the degree to which agreements with IFIs affect repressive behavior – and how they do so – are not well understood. This paper attempts to probe deeper into the mechanisms at work.

1.2 International Financial Institutions

IFIs are large inter-governmental organizations that provide development assistance, stabilization through temporary loans, and foreign aid. The two primary international financial institutions today are the International Monetary Fund and World Bank (WB), two products of the Bretton Woods agreements at the end of World War II. The IMF is tasked with providing international financial stability, doing so by providing loans, grants, and implementing programs in heavily indebted, unstable, or economically-troubled states. The IMF's sister institution, the World Bank, primarily works on economic growth and development issues, helping states by providing grants, loans, and aid for a variety of infrastructural and developmental projects.

I will only focus on the IMF in this paper, because I believe the mechanisms through which this institution affects behavior are unique and unlike those that condition the relationship between states and the World Bank. For example, the IMF often serves as the "lender

of last resort," and its sole mandate is to provide worldwide financial stability. The aims of the World Bank are focused on development and improving human capital. The two organizations have important differences that condition when, to whom, and why they give out funds, as well as different normative reasons for doing so and for following up on the consequences of these loans. As I detail below, the IMF often fails to take human rights into account – either on the front or back end of a loan package. This in turn can condition what happens in a loan-receiving state.

To directly affect states' behavior, the IMF can stipulate terms into agreements or loan packages that require states to fulfill certain criteria in order to maintain eligibility for financial incentives. Additionally, I argue that the IMF can also unintentionally change state behavior. The IMF frequently makes states implement austerity measures when the partner state signs onto an agreement. Signing onto a loan with such provisions may mean steep cutbacks in social services or welfare. This in turn could lead to backlash, protest, and repression as a response. I argue that this could be the case even when conditions on the loans may not be enforced by the IMF – for example, in the case of Pakistan, the IMF has little incentive to promote strict reforms because of that country's important security alliance with the US. However, domestic interest groups still protest the loans and conditions.

Conversely, receiving a much-needed cash infusion could improve a regime's popularity and make it less likely to resort to repression, or it could inadvertently improve the efficiency of the state's repression apparatus if these funds are funnelled into the military or police. Thus, the results of signing onto an agreement can take many forms. This paper focuses on how interaction with international financial institutions affects one specific issue: human rights. I do so by examining how being party to an agreement with the IMF affects a state's propensity to repress and the citizens' willingness to protest.

Previous work has focused on the instruments that states and international organizations have to improve human rights, as well as the effectiveness of those instruments. Recent

scholarly attention has focused on IFIs like the World Bank and IMF, specifically regarding the fact that the agreements or loans from these institutions may have substantial domestic effects. However, previous work has left substantial questions on the causal pathways that may link involvement with the IMF to changes in protest and repressive behavior. I attempt to shed light on these mechanisms below.

My findings in this study are that being under an IMF program leads to more protests and more repression. In addition, I hold that the mechanisms and results discussed in this paper speak more broadly to the study of how international institutions – not just financial institutions – can shape and condition domestic state and citizen behavior, even unintentionally (Lake 2009). By using new methods, such as matching, and critically examining the literature by attempting to replicate previous results, I make the argument that interactions between states and these institutions can sometimes lead to worse results for states and their citizens. This implies that scholars and policymakers should critically examine the impact of these interactions and programs, especially when dealing with fragile and developing states, to see whether they may be working against their intended aim or causing negative and unexpected externalities.

In order to motivate this study and to focus broadly on the mechanisms and tools that international institutions can wield to affect behavior, this paper will first briefly examine the literature discussing how third parties can influence human rights within a state. Then it will focus specifically on IFIs and human rights, exploring potential mechanisms through which these bodies could affect state behavior. This review will provide a platform for further study of the research question at hand: whether and to what extent IMF intervention affects human rights outcomes.

2 Literature review

2.1 Affecting Human Rights

What mechanisms can international bodies wield to improve human rights? Previous scholarship has worked to address this question from several angles. For example, work focused on the impacts of nongovernmental organizations (NGOs) and international organizations highlights the ability of states to create change either indirectly by empowering domestic actors, e.g. Risse-Kappen et al. (1999); naming and shaming governments and working with domestic and international actors, such as in Murdie and Davis (2012); or by getting them to ratify treaties (Neumayer 2005). All of these mechanisms involve complex networks of international actors working with domestic actors and thereby shifting a regime's preferences toward repression – either positively or negatively. These mechanisms are applicable to both states and international institutions that want to change the human rights behavior of a target state.

A somewhat less-studied area is that of economic mechanisms for improving human rights. One of the leading ideas in this area is "trade linkage," meaning that states can use conditionalities within agreements in order to lead to some desired outcome. Some scholars, such as Peksen (2009) find that preferential trade agreements (PTAs) between two parties may make repression worse. Other scholars argue that mechanisms such as conditionalities are effective (Hafner-Burton 2005) or depend on how much leverage the "norm supplier" holds over the target (Donno and Neureiter 2018). That is, if a state wants another state to change its behavior, the degree to which it can do so depends on their relationship and alliance status: strong allies or aid partners hold leverage over their dependent partners, allowing the state supporting human rights to change the behavior of its partner.

Though these authors are talking about agreements between states, I posit that similar dynamic may play out between IFIs and debtor states. Hafner-Burton claims that PTAs are

"among the only existing international institutions with some capacity to enforce compliance, and they may prove to be one of the more effective available means of implementing very basic human rights values into practice, although partial and imperfect" (Hafner-Burton 2005, p. 624). Thus, adding stipulations to agreements may lead to *actual changes in state behavior*. These stipulations can take several forms.

While Hafner-Burton finds that coercion is the optimal technique, Milewicz et al. (2018) and Postnikov and Bastiaens (2014) find the opposite result. Milewicz et al. examine why non-trade issues (NTIs) are increasingly included in PTAs and find that the inclusion of a few of these issues into one agreement leads to the inclusion of such issues in further agreements. Thus, the driving force resulting in the expanding use of NTIs is not coercion but rather soft power – persuasion in Hafner-Burton's framework – created by the attractiveness of joining agreements. An unanswered issue in this literature, however, is implementation. The aforementioned authors do not address whether the inclusion of more NTIs actually leads to acceptance and implementation of these NTI provisions. States or international bodies may just add NTIs to agreements or ratify those with NTIs in order to satisfy domestic or international audiences, never actually intending to put the measures into effect. Neumayer (2005) indicates that the extent to which states' human rights records improve after they ratify treaties depends on the extent of democracy and the strength of civil society. Even if NTIs are included in agreements, their implementation is far from guaranteed. Thus, when examining whether agreements with IFIs have any effect on human rights, it is important to control not only for selection bias, but also for underlying levels of democracy and civil society, since these factors can influence the latent amount of repression.

One final piece of this puzzle is why repression would change at all due to the terms of PTAs, sanctions, agreements, or due to other instruments. One could expect an effect in either a positive or negative direction, or no effect at all. The shaming and pressure mechanisms discussed above could work to change the behavior of the leadership; citizens

and NGOs may hold a government to account for not complying with the terms of the treaty; or the threat or actual removal of the benefits all may affect state behavior. On the other hand, the increased financial capabilities of the state may allow it to repress dissidents more efficiently. Citizens may be dissatisfied with regime for dealing with another state or international body (Smith 2014) and the state may crack down in order to quell dissent.

This section has presented a number of linkages that may bind states to one another and to IFIs. The purpose of this broad literature review was to first consider whether conditions or strings attached to programs can actually produce any meaningful changes in state behavior. Another point was to consider how issues beyond those tied explicitly to economic conditions can factor into the relationships between institutions and states. The next section will focus in on and consider IFIs specifically, building off of the literature discussed so far. Keeping in mind the insights from the broader overview, I will use this specific literature to develop several falsifiable hypotheses about what we might expect from relationships between the IMF and states.

2.2 International Financial Institutions

The literature on the topic of economic tools for affecting human rights tends to blend the work on economic sanctions, PTAs, foreign aid, and other trade issues that IFIs and states can use to change behavior. In the above paragraphs I have done the same in order to think broadly about the tools that might be at a body's (i.e., state or IFI) disposal. Logically, it makes sense to first think about the broad questions, themes, and tools that might be at work. Now, I focus specifically on IFIs with this knowledge in mind.

International financial institutions are large, powerful, and diverse actors on the world stage (Koremenos, Lipson and Snidal 2001). States apply for loans or programs in order to tap into the wealth and stability offered by such packages, often hoping to use them to deal with domestic fiscal troubles. Aside from the effectiveness of the loans at restoring economic

balance and stability within a recipient country, what externalities arise from these loans? Specifically, what positive or negative externalities arise and what is their effect on citizens in that country, particularly with respect to physical integrity rights (e.g., freedom from torture, disappearance, political murder, and extrajudicial killing)? According to sources such as the United Nations Office of the High Commissioner for Human Rights (OHCHR), IMF policies do not consider human rights and may contribute to worse outcomes.[1] This is problematic, given that IFIs such as the IMF and World Bank hold sway over many developing states, and because their actions may have direct, measurable impacts on the lives of the everyday citizens in recipient countries.

In the next section, I review more of the existing literature, which I think points to two major driving trends: IMF programs induce grievances and a disregard for human rights by this body leads to few mitigating responses by the organization. I derive these trends from the literature and then present several falsifiable hypotheses after that.

2.2.1 Grievance mechanism

A growing body of work focuses on IFIs and human rights; specifically, it focuses on how the agreements that these institutions make influence human rights. The two prominent publications are Abouharb and Cingranelli (2009) and Abouharb and Cingranelli (2006) – both of which focus on how IMF or World Bank programs respectively influence human rights outcomes. Abouharb and Cingranelli (2009) focus primarily on the relationship between the length of time a government was under an IMF program and changes in its respect for human rights, finding that a longer tenure under such a program led to increased government use of torture and extrajudicial killings.

[1] https://www.ohchr.org/en/NewsEvents/Pages/DisplayNews.aspx?NewsID=22256LangID=E

However, this work is sparse in terms of theory and does not posit we might observe these results specifically. The key explanatory mechanisms offered by Abouharb and Cingranelli is that IMF loans could have caused greater repression by either reducing the government's reliance on its citizens for tax revenue, raising the ire of citizens who believe that their government is beholden to foreign powers, or a relative deprivation argument in the vein of Gurr (1970), wherein citizens' expectations about what they are entitled to (increased by the expected benefits of an IMF package) are not met by the government. Abouharb and Cingranelli's book on the same topic does little to elucidate this argument, focusing on case studies and positing the same basic mechanisms as their papers (Abouharb and Cingranelli 2007). The net result is that the authors argue against submitting to the will of these enormous financial bodies. Here, then, is one potential argument and causal mechanism: being under an IFI (IMF) agreement could increase grievances and drive protests or riots.

Another issue that the authors raise, but do not satisfactorily address in this author's view, is the IMF reforms of the late 1990s. These reforms are cited in the paper as the Heavily Indebted Poor Countries Initiative (HIPC) in 1996 and Poverty Reduction Strategy Papers (PRSP) in 1999. The authors indicate that they expect to see a result of these changes in terms of human rights. However, Abouharb and Cingranelli's dataset only ranges from 1981 to 2003 – a short time period in which to expect major effects. It is not surprising that the authors did not find a positive relationship between IMF grants and human rights when examining such a short window after the supposed reforms – that is, Abouharb and Cingranelli find that the reforms were ineffective. Future work should attempt to more rigorously test these claims, though I will not in this paper.

As a brief point of comparison, the World Bank also went through reforms during the mid to late 1990s. These changes primarily came from an Inspection Panel and Compliance Officer/Ombudsman, which serve as dispute-settlement mechanisms for human rights issues. Papers such as Graham et al. (2018) indicate that these mechanisms were effective

at improving respect for human rights, and I posit that they could condition the extent to which the institution takes human rights into account when providing assistance. However, Abouharb and Cingranelli do not find this result. Notably, the IMF did not make such improvements.

The other major study about IFIs by Abouharb and Cingranelli examines the effects of World Bank structural adjustment agreements (SAAs) on human rights during a similar time period to the IMF paper, finding that such SAAs were correlated with a decrease government respect for human rights. In general, SAAs force the host government to rapidly liberalize their economies (Abouharb and Cingranelli 2006). Newer works, such as Eriksen and De Soysa (2009) find the opposite result, instead suggesting that such agreements can improve human rights by improving economic freedom. Still, the authors caution that loan "dry-ups" (i.e. when a loan expires and the host country must begin paying back the loan) may be marked by less respect for human rights. In a more recent work, De Soysa and Vadlammanati (2013) find that free market policy reforms, such as those pushed by the IMF, have a positive effect on government respect for human rights, pushing back against the claims of those such as Donnelly (2013) who caution against liberalization as a cure for repression. Thus, the literature on IFIs and human rights remains inconclusive as to results for human rights.

Abouharb and Cingranelli push back against the advocates for liberalization, suggesting that acceptance of SAA conditions leads to the enactment of harsh, unpopular policies, leading in turn to protests and increased repression by the state (though they do not comment on whether repression or protest comes first – a key dynamic Ritter and Conrad (2016) and others have raised. Specifically, and without explanation as to the mechanism, Abouharb and Cingranelli find that World Bank SAA implementation leads to increased torture and political imprisonment; for example, they state that the probability that torture would occur in a year when a SAA was entered into and implemented was 31%. They posit that because

torture and political imprisonment are the most common types of repression (Cingranelli and Richards 1999), we should therefore expect an increase in the frequency of these types. This is an unsatisfying result, namely because it does not elucidate why these types of repression, specifically, should increase. If liberalization is supposed to increase the use of repression as a reaction to citizen backlash, might we not expect extrajudicial killings to increase instead?

I believe that a more specific method is called for to explain the mechanisms at work – otherwise we are simply commenting on statistical relationships, which do not enhance our understanding of the underlying mechanisms and dynamics at work. What specifically about these adjustments would cause torture to increase? This is not addressed in either paper.

In a critique of the work of Abouharb and Cingranelli's research, Eriksen and De Soysa (2009) attempt to carefully define the mechanisms at work. They are more agnostic about who exactly will be hurt or affected by SAAs (Structural Adjustment Agreements, denoted as Structural Adjustment Programs or SAPs in their paper), claiming that it could be the poor or people most affected by austerity, or it could also be the rulers or elites who may revolt against SAPs because such policies could benefit labor groups or farmers. The authors cite the canonical Heckscher-Ohlin, Ricardo-Viner-Samuelson model that emphasizes factor endowments to support this alternative hypothesis, namely pointing to how exposure to such forces can lead to increased economic anxiety and grievance.

In doing so, the authors' argument leads to an important point: when comparing the results of IFI agreements, it is important to keep in mind that states make these agreements when experiencing economic difficulty. It is important to control for economic stress and uncertainty, since these play into the initial need for an IFI agreement and may also endogenously influence the level of respect for human rights. That is, states that are under economic stress may be more likely to repress their citizens, even without the extra influence of IFIs. Thus, we can further refine the hypothetical grievance mechanism pointed out ear-

lier; citizens who are most at risk from the program may be the most likely to protest and underlying economic conditions may be important for parsing out the results of the program.

Still, Eriksen and De Soysa (2009) leave a few questions unanswered. Most importantly, they make no further strides in elucidating the exact process by which IFIs influence human rights. For example, they indicate that SAPs could perhaps lead people to riot (as could economic strife), but they do not include any measures of protest or protest frequency – measures that perhaps could have addressed this potential mechanism. Additionally, they question whether elites may revolt against a regime as well, but they provide no measure of this either. The results of their paper do show that when states take on larger loans, they have greater respect for human rights. The authors argue that this implies that the borrowing government believes that the IFI cares about their human rights record. Indeed, studies such as Jensen et al. (2014) show that governments are wary of violating property rights when experiencing economic crises because they are worried about their reputation with IFIs and other financial backers. However, this may not be the case for physical integrity rights.

2.2.2 IFIs and disregard for human rights

While these large IFIs have affected human rights in meaningful ways by changing states' behavior, it is not necessarily the case that these changes were intended. This is where the IFI literature departs from that discussing third party economic tools and human rights more broadly. According to NGOs such as Human Rights Watch, many international financial institutions claim that they do not have obligations to look after human rights. For example, some of these institutions may claim that they have a "non-political" mandate (de Zayas 2017), insinuating that human rights are not in their jurisdiction – and, by extension, not their concern. A UN-commissioned report on the human rights effects of loan conditionalities indicates that the World Bank has taken steps to improve its human rights record, but

that the IMF has taken few steps in that direction, aside from producing a few internal documents questioning the value and potential negative externalities of enforcing liberal economic reforms upon those receiving loans (Furceri, Loungani and Ostry 2016).

Thus, regarding the point raised by Eriksen and de Soysa, it does not necessarily make sense that states would seek to improve their human rights record in order to receive loans. In their results, the authors find that recipients of loans from the IBRD and IDA need to have low levels of "dissent" in order to get the loans, but they find a null result for other organizations such as the IMF (Eriksen and De Soysa 2009, 493). Unfortunately, the authors do not dwell on why these IFIs might be different – perhaps because the IBRD and IDA are development-focused and therefore more likely to want to negotiate with less repressive states for normative reasons – but it is an important point of consideration.

Finally, the authors posit that having access to IFI loans eases the fiscal burden of heavily indebted states and that when these states do not get new loans – and must start paying back – repression increases. The authors state that their results indicate that "(economic) crises rather than the stringency with which conditionalities are applied drive human rights violations" (Eriksen and De Soysa 2009, p. 494). But this is a small distinction. If paying back the loans is what causes repression to increase due to the threat of austerity, then wouldn't austerity imposed by the IFIs also lead to increased repression? The authors attempt to exonerate the critics of globalization but on this point make a less-than-convincing argument.

The main takeaways from the literature point to a few important trends. First, the authors discussed in this IFI-specific section all agree that interactions with IFIs should matter for human rights. The direction and intent of this influence is debatable, but it is clear that these institutions are not powerless.

Second, the literature suggests a few potential mechanisms for influencing human rights:

- **Being under an agreement with an IFI**: Citizens within a recipient state see that their government is working with the IMF in exchange for a loan and push back against this. They may due so due to fears of Western influence, a desire not to be "held hostage" by donor countries, or a desire to avoid "Washington Consensus" era policies (like austerity), which could reduce their overall welfare. These protests could occur when a state is in the negotiating process or most likely when a state has entered an agreement with the IMF.

- **Length Under Agreement**: Abouharb and Cingranelli's work suggests that the length of time a state is under the agreement matters. This could be due to decreasing accountability to the citizenry (because the state increasingly relies on the international body for cash), making a regime more likely to repress because it has the fiscal capacity to do so and will not face sanctions from the IFI. Or, if the agreement imposes austerity or other measures that decrease citizens' utility, the more time spent under this agreement could lead to increasingly salient grievances and an increased likelihood of protest – and backlash.

Regardless of the intent of these international bodies, it is normatively important to examine the effects that loans and structural adjustments have on human rights. Those states receiving these economic packages may be developing, fragile, under enormous economic stress, and may not have a strong rule of law tradition. It is important to investigate the extent to which these packages help or harm human rights in order to bring attention and ideas for possible reform to this issue. This investigation is important regardless of whether the human rights outcomes were specifically intended by the creators of the loan packages or programs or whether the repression outcomes were by-products or unintended consequences.

Given the issues at hand regarding IFIs and human rights, I have derived several hypotheses to more directly test how IMF programs can affect human rights. These attempt to address some of the issues that I raise in the review of the literature, above.

3 Theory

From the above literature, we can draw out a few potential causal mechanisms at work. In this section, I will draw on these mechanisms in order to suggest a few of my own and build my own theory. There are two particular aspects of interest, timing and endogeneity, which I will attempt to examine and address in the empirical section.

First, it is important to clarify how agreements with IFIs like the IMF might be different from PTAs. Given that I have examined the literature around PTAs most thoroughly and until this point assumed that this literature is applicable to IFIs, it is important to pause and take stock before proceeding. In the broadest sense, these literatures are connected because they are about using economic mechanisms to influence state behavior. In the case of PTAs, one state or bloc of states influences another state. Regarding IFIs, the institution seeks to change the behavior of another state. It is presumed in the PTA literature that states are actively trying to change behavior by incorporating human rights or labor rights clauses or conditions, for example. These actions represent a deliberate attempt by state A to change state B's behavior in A's preferred direction.

While this may be the case sometimes with IFIs, the key distinction that I argue for here is not that this is a deliberate attempt to influence another state – at least not to improve its human rights practices, since these practices are never mentioned in the agreement – but that changes in human rights practices are an *externality* of such agreements with IFIs. The IMF may want a state to liberalize, open its markets, decrease social welfare spending, and implement austerity measures; it may not consider human rights at all and yet human rights conditions may still worsen.

This is an important distinction to make because I do not mean to argue that the IMF is deliberately trying to push these policies and make human rights outcomes worse – I am not assuming agency on this matter – but the outcome is posited to be similar to one in which they did encourage repression. Unlike human rights provisions, which can be specified as hard or soft, IMF programs are a blunt instrument, built for another task, and yet still can affect human rights.

Another question is of effectiveness. The literature discussed above, along with new work from Davis, Reinhardt and Barrett (N.d.) indicates that we are not really sure whether these economic mechanisms are effective for improving human rights. The most likely result of this investigation is a null finding, especially when compared to "the usual suspects" – factors such as level of democracy, youth bulges, and civil war that have a profound effect on human rights (Hill and Jones 2014).

Why would an agreement with the IMF change a citizen, state, or leader's behavior, and under what conditions? I posit that there are a few possible scenarios under which this can happen. First is via the mechanism described by Abouharb and Cingranelli (2006; 2007; 2009): citizens dislike an IMF deal, they protest and dissent, and the state cracks down on them. Setting aside the specific types of repression that these authors find to be correlated with IMF packages, this argument makes intuitive sense. Citizens may take to the streets to protest against policies they dislike. State leaders can be challenged when facing mass protest, and repression is a useful and straightforward tool for controlling such dissent. In this paper, I will not delve into the debate surrounding responses to nonviolent versus violent forms of dissent (see Davenport, Johnston and Mueller (2005), Chenoweth and Stephan (2011), Chenoweth, Perkoski and Kang (2017), Ritter and Conrad (2016) and many others for a discussion of this issue). Instead, I assume that dissent will lead to repression by the state, as Abouharb and Cingranelli assume.

3.1 Backlash

Timing is a key facet of this explanation for a few reasons, but it is difficult to nail down exact expectations for protest behavior. On the one hand, protests could be most likely after a deal is being struck and implemented. This may be because negotiations with the IMF may go through back channels and citizens may not be aware that their government is about to strike a deal until it is nearly completed. I also assume that even if the media informs the public that a state government is doing a deal with the IMF, citizens may not immediately head to the streets. Because many of these packages involve austerity terms, which reduce social service spending, it is not unreasonable to assume that citizens will take to the streets. Citizens may also protest because they dislike the idea that their government is about to be beholden to international creditors or to the IMF's financiers, like the US and EU – we could observe more protests even in those situations without conditionalities. States that have colonial ties to one of the financial backers of the institution may see this agreement as a return to restricted sovereignty. We therefore have a few theoretical reasons to expect protest in the time surrounding the implementation and signing of a package. Still, it is tricky to parse out this dissent out from dissent about the dire economic situation that led to the need for a bailout in the first place, but I will attempt to do so in the methods section.

It could be the case that citizens protest when the terms of a deal (austerity, decreased social spending) really start to bite and affect their livelihood. The previous mechanism was anticipatory protest; another likely mechanism could be reactionary protest. Currently, tens of thousands of Argentinians have taken to the streets to march against the austerity caused by their third deal with the IMF. The Argentinian government's hands are tied, given that its ability to use its economic levers of power are limited by the agreements it

has reached with the IMF.[2] This suggests that once citizens start to feel the negative policy effects of an IMF agreement, they may be more likely to take to the streets. While the first mechanism might have included the assumption that citizens realize and predict the negative consequences of undergoing an IMF deal, this second mechanism is not as presumptuous. That is, citizens may not protest when first reaching an agreement because they may have had little previous experience with the IMF or because the state government may conduct a positive PR campaign to assuage any worry about deals with the IMF. However, when the deal starts to kick in and the degree of the IMF's leverage over the state becomes clear, citizens could take to the streets. Studies such as those by Rickard and Caraway (2018) indicate that governments face and respond to domestic pressure when implementing cuts to public spending, but they do not directly test this mechanism.

As an agreement is reached between the host government and the IFI, protests may start or continue against this deal. Actually reaching an agreement would indicate that the government is serious about reforms, which may only strengthen the desire to protest and dissent against the government. During a state's tenure under the IMF, the state will likely implement some – if not all – of the reforms required in order to receive the stabilizing funds. Protest is likely to occur due to a build-up of economic grievance from exposure to hardship and cuts in program budgets. This leads to my first hypothesis:

***H_{1A}**: Protest frequency will increase in the months a state is under an IMF program.*

Eriksen and De Soysa (2009)'s hypothesize that repression will not increase when states agree to the loans, conditioned on the size of the loan. However, I expect that the state will respond to dissent forcefully. Furthermore, once the state has reached an agreement, the impetus to stay on "good behavior" would dissipate, allowing the state to repress without fear of repercussion and perhaps enhanced by extra rents from the loan. Given the anecdote

[2]https://www.aljazeera.com/news/2019/02/tens-thousands-march-austerity-argentina-190213160623152.html

in the previous section about the IMF's (lack of) focus on human rights, I see little reason why the IMF would pull out of a deal or withhold funding if repression increased. States are often concerned about their reputation, particularly with international financial institutions (see Jensen et al. (2014), for example), but generally this is the case when states are concerned about expropriation or damaging property rights – not human rights. Graham et al. (2018) find that the World Bank has dispute mechanisms (an Inspection Panel and a Compliance Advisor/Ombudsman) for addressing human rights misdeeds. However, the IMF has no such body to monitor states' repressive behavior. It may be the case that states come under pressure from the international community to change their behavior, but it seems unlikely that pressure would come from the lending institution itself.

State governments might repress for a number of reasons. First, they may do so simply due to the threat posed by dissent and protest. That governments respond to dissent is a commonly-cited mechanism (see Ritter and Conrad (2016) for example), but there could also be other factors at work. Given that the IMF does not necessarily have a desire to monitor human rights, then the state may be more willing to use its repressive power to crack down on dissidents and given that the economy is in decline, it may be able to deem some of this repression as necessary to restore order.

However, one important caveat to this hypothesis is how the loans affect state capacity. It may be the case that since the government gets an influx of cash from the IMF, then it may feel like it has the license to use this to increase its repressive capacity. In this situation, the state would repress more because it not only is under less scrutiny but also has more capital to do as it pleases.

An alternative is that undergoing a program may decrease state capacity. This could result because of the terms of an agreement. That is, a deal could mandate that a state decrease spending in some policy area that would have otherwise allowed it to repress. This would mean that a deal with the IMF could have a negative relationship with repression.

By virtue of stringent spending terms, the state's hands may be tied and it may be less able to repress its own citizens. My baseline expectation is that the state will repress its citizens more:

***H_{1B}**: Repression frequency will increase in the months a state is under an IMF program.*

One final set of hypotheses looks specifically at variation within the program-months under the IMF. Conditionalities are terms on state behavior imposed by the IMF in return for program funding. Often, these conditionalities require that states reduce social spending, cut pensions, liberalize their markets, or encourage privatization of industries or sectors. Conditionalities, a controversial tool, made headlines during the recent financial crisis, primarily because they required states to impose austerity measures in order to get their economies back on track (Rodrik 2003). However, do programs with conditionalities lead to worse human rights outcomes? The IMF data in this study does not specify the type of conditionality for each loan (discussion of the limitations this poses for inference is in the empirical section), but since there are 140 programs out of 278 with conditionality, my data give me enough counterfactual cases to examine the importance of these conditions. Additionally, given the motivating example of Pakistan in which protesters were rallying against creeping privatization and attacks on workers' rights, it seems reasonable to think that programs with conditionalities will lead to increased protests and repression. I fold the following two hypotheses under the Backlash section, since they are fundamentally getting at the same problem: the fact that there might be negative externalities experienced by those under an IMF program.

***H_{1C}**: Protest frequency will increase in the months a state is under an IMF program with conditionalities, compared to those programs without such conditions.*

***H_{1D}**: Repression frequency will increase in the months a state is under an IMF program with conditionalities, compared to those programs without such conditions.*

The next set of hypotheses examines an alternative argument suggested by the literature – that protesters and the state will anticipate the negative effects of an IMF program.

3.2 Anticipation

An alternative argument would hold that as soon as citizens catch wind of an agreement – particularly when they hold unfavorable views of the IMF – they will protest to try to prevent a deal. This could certainly be the case, particularly when a state has received multiple packages from the IMF. In this case, the citizens would have to not only be aware that a deal was being struck, but also that the deal was worth protesting against. Thus I expect that the month leading up to the implementation of an agreement will have increased protest frequency. This may be due to reasons posited above: a general backlash against globalization, adjustment by international institutions, (i.e. cuts or changes to spending), or the feeling that the state's government is about to become beholden to international creditors. It is beyond the scope of this paper to understand the specific underlying reasons for protests or repression; I leave this to future work. My goal is simply to establish the degree to which these deals spark protest and repression and to examine the timing of these events. However, it is clear that such protest does occur (and is occurring currently). One example from ACLED (the Armed Conflict and Location Event Dataset, a source discussed below) highlights such an instance of protest in Pakistan:

> "Awami Action Committee (AAC), National Trade Union Federation (NTUF), lawyers, students, and political party activists organized a rally to protest against IMF loans, contract labor system, and privatization on Aug 11 (2018) in a march that began at the Arts Council of Pakistan and ended *[sic]* at the Press Club in Karachi."

It seems reasonable to think that similar events could have occurred many times in the timeframe that I focus on in this study, particularly when taking into account the many

agreements and loans that have been implemented and widely publicized. The protesters in this quote are rallying against making a deal with the IMF, rather than protesting an ongoing loan package. From the literature and the nature of the loan negotiation and approval process, it is unclear which window of time would be best to examine. That is, how long before a deal is implemented do citizens hear about the deal, and when do they decide to protest? I use one month in the hypotheses below as an appropriate window, though I am ambivalent about this timeframe.

To look at this issue from the lens of the state, it seems reasonable to assume that repression may follow protests as regimes seek to crack down on dissent (Davenport, Johnston and Mueller 2005). Some states, such as Argentina opted for an explicit "no repression" policy, in which case protest but not a forceful government response (Blustein 2005). However, it is unclear how many agreements with IFIs did not result in backlash by the state, but my theory and existing work predicts that the state will crack down against these protests through force – by using repression. If we expect that protests will increase in the short span of time before a program is implemented, then repression should increase as well.

This discussion leads to my first hypotheses, listed below:

***H_{2A}**: Protest frequency will increase in the month leading up to the implementation of an IMF program.*

***H_{2B}**: Repression frequency will increase in the month leading up to the implementation of an IMF program.*

The first of these hypotheses, backlash protest and backlash repression, are easier to test because we have a direct comparison case: I can look at the time period under a program versus a valid counterfactual case. The second mechanism, anticipatory protest, is trickier to examine. In the next section, I detail my data sources and methodological approach used to tackle these hypotheses, as well as the improvements I attempt to make on existing work.

In order to have an even number of hypotheses between the two categories, it would seem logical to also examine the effect of conditionalities on anticipatory protest and repression. I initially attempted to do so, leaving me with only 105 pre-program country months. However, the data availability was so poor for the matching variables – the subset of months was highly lacking in terms of data, that I did not feel that I could implement reasonable tests for these hypotheses. Thus, I leave it to future work to examine whether the months before a country is about to implement an IMF program with conditionalities is correlated with increased protests and repression, as the literature would suggest. Because the missingness in key covariates may lead to biased results, I digress on this potential mechanism. The next sections show the empirical tests that I do perform in this paper.

4 Empirical Analysis

This section will proceed in the following way. First, I will discuss the independent variables derived from the literature as well as matching – the method I think is most suitable for addressing the above hypotheses. Then, I will discuss the choice of dependent variables – another area that departs from previous work.

My expected contributions are two-fold. First, the unit of analysis in this study is the country-month – a much finer-grained unit than other work, which has utilized annual time-series data. Working with monthly data is helpful because it breaks down each country unit into smaller windows, thereby allowing for a closer analysis and stronger claims about the relationships between IMF programs, protests, and repression. There are over 27,000 observations in the full dataset utilized in this study, which covers 1997-2017, and close to 4,800 observations in the dataset used for Hypotheses 1C and 1D. Reliance on annual data presents a host of potentially-problematic issues regarding duration and participation effects. For example, if I country is under an IMF program for exactly half of a year, then does that country count as "treated" or "control" for the purposes of comparison? Since

IMF programs are created for a set number of months, it is more appropriate to focus on monthly data.

The second contribution regards the research design of this paper. In order to better understand the dynamics at work and to establish a counterfactual case, I employ a matching technique (Hill Jr 2010).[3] Matching will help alleviate a problem inherent in this study: the conditions associated with eligibility for IMF intervention, such as economic distress, high debt burdens, and out of control inflation, may themselves induce greater dissent and repression by the state as well as protest. Matching allows the researcher to parse out how much of the effect (in this case, protest and dissent) is due to the "treatment" – in this case, the IMF package – versus the baseline values for each dependent variable. To paraphrase (Hill Jr 2010), we are interested in $\tau_{i,m} = y_{i,m}^{IMFDeal} - y_{i,m}^{NoDeal}$, where $\tau_{i,m}$ represents the difference in repression or protest across the treatment and control conditions for a given country-month. Matching works by selecting relevant covariates – in this case, predictors of financial crisis – in order to create pairs of cases, with one member of the pair in the treatment and the other in control, and comparing those cases. By doing so, I can attempt to isolate exactly how much of the unrest and repression may come from being under the added condition of the IMF package. The next sections discuss the variables I chose to use in this paper.

4.1 Independent Variables

4.1.1 Economic Variables

For my matching approach, the relevant financial covariates are the latent conditions that may be tied to crisis: GDP change from the previous month, real exchange rate (REER), unemployment, log of GDP, and foreign exchange reserves (Frankel and Saravelos 2012). I

[3] I use propensity score matching in the main results of the paper, but I also performed Mahalanobis distance matching as a robustness check and obtained similar results – see Appendix.

will use these variables to find comparable cases within the pool of states for a more accurate comparison. The World Bank's Global Economic Monitor provides the data for unemployment, real exchange rate, (REER), foreign exchange reserves, and GDP (World Bank 2019); IMF agreement data comes from the IMF's website (International Monetary Fund 2019).

The literature suggests that these indices are suitable covariates upon which to match for this study. Due to data limitations – mostly because data such as GDP is only available at the quarterly level, I needed to impute the values for the quarters within each month. I did so by splitting each quarter to three months in R and imputing the values. For example, if country A had 2.65% GDP growth in Q1, then the first three months of the year would each take on the value of 2.65% as well. Though this method is imperfect, I believe it is defensible for this study and not doing so comes at the expense of losing a potentially powerful explanatory covariate. I calculated GDP percent change as the change (as a percent) from the previous month. It would be ideal to have other variables, such as debt and balance of payment data in order to boost the matching analysis, but this data was unavailable for most of the countries in the sample.

4.1.2 Noneconomic Variables

In addition, I match on a few other non-economic variables. The human rights literature has developed a list of "the usual suspects" for use as control variables, which I will match on in this study. Namely, as laid out in Hill and Jones (2014), these are: level of democracy (Polity IV), civil war (UCDP/PRIO), and alliance status with the US/EU (voting record in the United Nations General Assembly) (Bailey, Strezhnev and Voeten 2017, Voeten, Strezhnev and Bailey 2009). I will use these variables as well in this analysis. Of particular interest is degree to which states vote with the United States in the UN General Assembly. Given the US' and Europe's disproportionate sway in determining the lending behavior of the IMF, it may be the case that being in an alliance with one of these bodies may make loan terms

more favorable, allow the state to repress its citizens more and avoid shaming, or shorten the window between crisis onset and implementation of an agreement. Other factors, such as youth bulges (Urdal 2006), may also be important for matching and explaining levels of repression, but due to data missingness these were not included in this study.[4]

Of potential concern regarding these variables is the fact that they are only available at the annual level. In order to conform the data to the country-month setup used in the rest of the analysis, I impute the values for a given month by using the value for the year. Thus, if a country received a score of "4" on the Polity IV (democracy) scale for 2010, then I give that country a "4" for all the months of 2010. This is not a perfect method, but I do so to try to take into account other variables that may affect human rights, repression, and protest outcomes.

Figure 2, below, is a balance test for the dataset used to test the backlash hypotheses. It shows the differences in means between these variables before and after propensity score matching. Summary statistics for each subgroup are available in the appendix (where C = Control, T = Treatment group). In order for matching to be an improvement on a simpler model, we want to have the absolute mean differences as close to zero as possible, meaning that the difference between the two samples regarding these relevant covariates is negligible[5]

After matching, I then examine two different time periods, depending on the hypothesis. For *H1* (protest and repression as a result of being under a program), I look at the frequency of protest and repression for those countries in the treatment group – under a program – versus those that are not under a program. For the second set of hypotheses (protest and

[4]For instances of missigness that were not systematic, I followed Green's standard operating procedure (http://alexandercoppock.com/Green-Lab-SOP/Green$_{L}ab_{S}OP.pdf$),
which uses an indicator variable for those covariates with NA values and replacing those NAs with an arbitrary value (performed with the WeightIt package in R

[5]Note that in the WeightIt package, NA values are indicated by a separate dummy covariate, as is standard operating procedure in Don Green's lab at Columbia, as described in the previous footnote. It is important to check the balance across the NA values of the covariates as well.

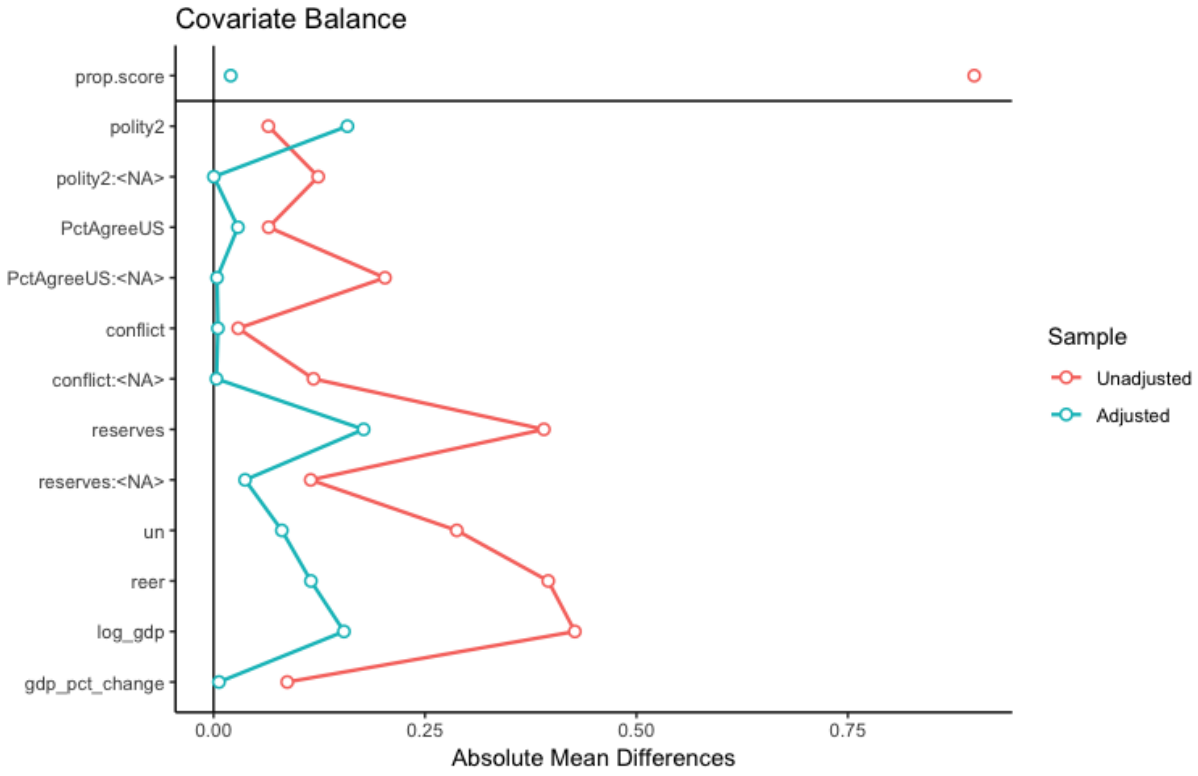

Figure 2: Balance Plot for Covariates, before and after Matching (Backlash Hypotheses)

repression around the time of a deal with the IMF), I created a dummy variable for the month prior to the start of an agreement and compared those months to the country-months for states not under a program. This method attempts to estimate how much behavior might change during a relevant time period when comparing a "baseline" or control case to those countries that strike a deal with the IMF. Next, I discuss the dependent variable I enlist in this study.

4.2 Dependent Variable

Traditionally, studies in this vein would use the Political Terror Scale (PTS) (Wood and Gibney 2010), Cingranelli and Richards (CIRI) (Cingranelli and Richards 1999), or Fariss (Fariss 2014) measures for human rights, all of which cover physical integrity rights – those

rights generally conceived to include freedom from torture, extrajudicial killings, and political imprisonment.

Each method naturally has its own set of drawbacks. Aside from the fact that each measure is based on yearly data, the PTS and CIRI are compiled from the US State Department's Human Rights reports, which could be biased in their own ways and comment more favorably on the human rights performance of their allies or partners under IMF deals (Qian and Yanagizawa 2009).

This paper will also introduce another way to measure human rights (more specifically, repression), as well as protests, by incorporating the data produced by ACLED – an initiative that produces high-quality, disaggregated and geo-located data on battles, conflicts, protests, riots, remote violence, and violence against civilians (Raleigh et al. 2010). These data also allow the user to disaggregate by perpetrator and target. For the purposes of this paper, this will allow me to examine specifically the actors and perpetrators of interest – in this case, the frequency with which the state committed human rights violations against civilians, since this is the mechanism suggested by previous work (Abouharb and Cingranelli 2006; 2009). ACLED will also be useful because it will allow me to use a more finely-tuned measure for examining the effects of loan implementation. Specifically, I will be able to see whether the month before and the months during implementation of the program were marked by significantly more unrest and protests (as predicted) and resulted in the predicted backlash by the repressive state.

If IFIs induce economic stress or grievance indirectly, however, these effects may not be limited to protests just directed at these institutions. This is why I focus on protest and repression generally. People may take to the streets to protest unemployment, an economic downturn, wealth concentration, globalization, or a host of other issues when programs put forth by international financial institutions could be (purposefully or inadvertently) at the

root of their collective problems. I do my best to control for these other underlying economic factors.

There are certainly downsides to using ACLED as a measure of repression and human rights. ACLED is funded by the U.S. Department of State's Bureau of Conflict and Stabilization Operations, along with the foreign ministries of several other prominent developed states. However, it is independent and is regarded as being a high-quality and consistent data source. The other potential downside with using ACLED is that it does not measure human rights exactly – it measures contention more broadly, including protests, battles, changes of territory, killings, riots, and violence against civilians. However, since most of the literature only focuses on a narrow subset of human rights (physical integrity rights, as opposed to the universe of economic, social, cultural, civil, and political rights), this seems like an appropriate use of the data. Also given the key mechanisms expected by the literature – primarily, state repression in response to or in advance of protests – ACLED provides a satisfactory data source, and it is one that has not been used in this type of study before.

In order to utilize the full power of this data source, I classify those events that involve state force against civilians as "repression" and those that involve protesters or rioters as "protests" using the interaction codes within the dataset. Given the information from the ACLED handbook, this seems like a logical approach – one that should not pick up state violence against rebel or terrorist groups, for example (ACLED 2019).

A few plots display the dominant trends in the dependent variable, with protests and repression generally rising in the first months under a program and declining after around two years. Figure 3 shows the full dataset limited to just those countries under the program as well as the trends for protests and repression. Repressive events include killings, beatings, violence, and brutality by state forces.

Figure 4 is a map of the countries covered in my final sample. On first blush, it might appear as if the data is limited, since the map only indicates coverage for the some of Europe,

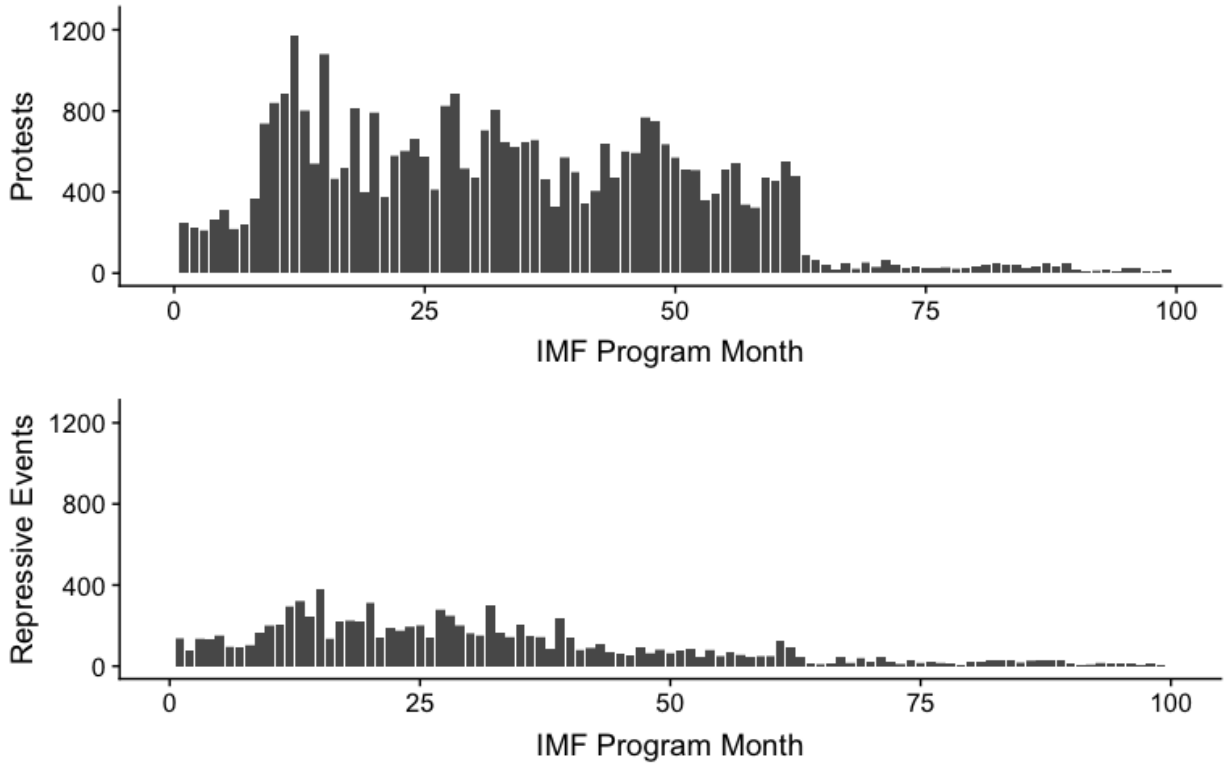

Figure 3: Trends in Dependent Variable

the continent of Africa and most of South and Southeast Asia. This is due to two factors. First, only a subset of countries apply for and receive IMF loans, and these countries are clustered in the developing world. Second, ACLED's data coverage is presently limited to the most conflict-stricken regions of the world, although it is currently expanding and will eventually provide worldwide coverage. For example, ACLED released the data on Europe on March 29, 2019, and the organization has plans to expand to cover South America as well. A full list of countries in this study as well as a set of summary statistics for each variable is in the Appendix (again, C = Control, T = Treatment group).

For the subset of the hypotheses discussing the effects of conditionalities on protests and repression, I limit the dataset to just the countries receiving IMF programs. Luckily, this data provides enough variation – by providing counterfactuals – to examine the marginal effect of conditionalities on human rights. a description of the data for the C and D hypotheses

is in the appendix. The following section will detail each of the models utilized in the study and will discuss the results.

Figure 4: Country Coverage

4.3 Models

I investigate these hypotheses in three ways: with standard OLS regression, OLS with fixed effects, and matching. Observations are indexed by m indicating month and i indicating country. The OLS models resemble the following formulae:

$$NumberofProtests_{i,m} = \gamma_0 + \gamma_1 * Polity2_{i,m} + \gamma_2 * UNPctAgreeUS_{i,m} + \gamma_3 * Conflict_{i,m} + \gamma_4 * ForeignReserves_{i,m} + \gamma_5 * Unemployment_{i,m} + \gamma_6 * RealExchangeRate_{i,m} + \gamma_7 * log_G DP_{i,m} + \gamma_8 * GDPpctChange_{i,m} + \epsilon_{i,m}$$

$$NumberofRepressiveEvents_{i,m} = \gamma_0 + \gamma_1 * Polity2_{i,m} + \gamma_2 * UNPctAgreeUS_{i,m} + \gamma_3 * Conflict_{i,m} + \gamma_4 * ForeignReserves_{i,m} + \gamma_5 * Unemployment_{i,m} + \gamma_6 * RealExchangeRate_{i,m} + \gamma_7 * log_G DP_{i,m} + \gamma_8 * GDPpctChange_{i,m} + \epsilon_{i,m}$$

Second, I add two-way fixed effects (country and month) to these OLS models, which is represented by a series of vectors added to the model, coded 1 for each individual country and month in the dataset. This has the result of isolating any noise that may be due to the specific country and month used in the model. The formulae for the fixed effects regression do not have the intercept (γ_0) term, but overall resemble a lengthier version of the above OLS formulae, with an added term to represent each individual country-month found in the data.

Lastly, I implement matching. First, I estimate the models using the WeightIt R package to calculate propensity scores for each estimate through the following equation:

$$Pr(IMF_{i,m}) = \alpha_0 + \alpha_1 * Polity2 + \alpha_2 * UNPctAgreeUS + \alpha_3 * Conflict + \alpha_4 * ForeignReserves + \alpha_5 * Unemployment + \alpha_6 * RealExchangeRate + \alpha_7 * log_G DP + \alpha_8 * GDPpctChange$$

I extract the propensity scores from the WeightIt package in order to weight the observations in the subsequent models. I then use the Match package to match each observation

in treatment with one in control using the propensity scores. Match uses Ordinary Least Squares (OLS) regression to model the following equations within each of the pair:

$$NumberofProtests_{i,m} = \beta_0 + \beta_1 IMF_{i,m} + \epsilon_{i,m}$$
$$ProtestOccurrence_{i,m} = \beta_0 + \beta_1 IMF_{i,m} + \epsilon_{i,m}$$
$$NumberofRepressiveEvents_{i,m} = \beta_0 + \beta_1 IMF_{i,m} + \epsilon_{i,m}$$
$$RepressionOccurrence_{i,m} = \beta_0 + \beta_1 IMF_{i,m} + \epsilon_{i,m}$$

These models conceptualize protest frequency and human rights violations as a function of tenure under an IMF agreement, examining whether being a recipient has an adverse effect on human rights in a given month. The next section displays the results of these models.

5 Results

5.1 Backlash Hypotheses

A simple regression of protests on being under an IMF program in a given month yields the result, showing that a nave estimate indicates that being under an IMF program will result in 12 more protests and 5 more repressive events per month on average versus those months not under a program. This result is also statistically significant, pointing in the direction that the critics of globalization theorize. The second model has country and month level fixed effects, and the result remains statistically significant; albeit the estimate decreases from 12 to 2.5 protests and from 5 to 1.6 repressive events per month.

I run these models to show the results without the use of matching and in order to illustrate the extent to which the estimates for protests and repression may be biased due to the lack of a valid counterfactual case. The models in Table 1 essentially produce the average increase in the number of protests or repressive events in those country months under IMF

programs, versus all of those country months in which states were not under IMF programs. Though the models take into account relative covariates – the ones discussed in the previous sections – they do not use those covariates to inform the estimates writ large. That is, we are not necessarily making an appropriate comparison between groups and our estimates may be biased as a result.

	Dependent variable:			
	Protests		Repression	
	OLS	*Fixed Effects*	*OLS*	*Fixed Effects*
	(1)	(2)	(3)	(4)
IMF program	12.018***	2.529***	5.044***	1.687***
	(0.472)	(0.670)	(0.161)	(0.245)
Polity2	−0.055**	−0.045	−0.038***	0.066***
	(0.025)	(0.062)	(0.009)	(0.023)
UN Voting record	−1.314*	10.276***	−0.617***	2.890***
	(0.701)	(1.800)	(0.239)	(0.657)
Ongoing conflict	1.730***	3.104***	0.087	0.467**
	(0.410)	(0.634)	(0.140)	(0.232)
Foreign Exchange Reserves	−0.040	0.004	−0.005	−0.052***
	(0.033)	(0.050)	(0.011)	(0.018)
Unemployment Rate	−0.062***	0.0001	0.001	−0.0003
	(0.024)	(0.039)	(0.008)	(0.014)
Real Exchange Rate	0.049***	−0.011	0.016***	−0.005
	(0.012)	(0.013)	(0.004)	(0.005)
log GDP	−0.433***	1.429***	−0.054	0.299***
	(0.118)	(0.303)	(0.040)	(0.111)
GDP Monthly Percent Change	−0.059	0.010	0.003	0.018
	(0.042)	(0.039)	(0.014)	(0.014)
Constant	1.420		−0.424	
	(1.580)		(0.539)	
Observations	3,069	3,069	3,069	3,069
R^2	0.227	0.427	0.326	0.427
Adjusted R^2	0.225	0.421	0.324	0.421
Residual Std. Error	5.855 (df = 3059)	5.060 (df = 3035)	1.997 (df = 3059)	1.848 (df = 3035)
F Statistic (df = 9; 3059)	100.079***		164.270***	
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 1: Backlash Protest and Repression

The next tables show the matching analysis – the results of which are presented simply since the estimation technique is a simple OLS regression between matched pairs to deliver one estimate.

	Dependent variable:	
	Protests	*Repression*
	(1)	(2)
Estimate	2.9918***	0.77427***
	(0.83559)	(0.21222)
T-stat	3.5805	3.6485
P-value	0.00034293	0.00026383
Original N	27,130	27,130
Original N, Treated	4,791	4,791
Matched N	4,791	4,791
Matched N, (unweighted)	227,667	227,667
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 2: Results from Matching Models, Backlash Hypotheses

These results indicate a slightly more nuanced result than the simple OLS model, but a very similar result to the fixed effects model. The dependent variables in the matching models indicate number of protests and number of repressive events. Being under an IMF program in a given month is correlated with around 3 more protests and roughly 0.8 more repressive events. Thus, estimates from matching are very similar to those estimated using other techniques. In order to visually display the differences between these approaches, I have included a coefficient plot displaying the change in estimates gained by employing matching.

This figure indicates that the estimates of repression derived from matching are much more conservative than for any of the other methods, but that estimates of protest frequency are similar to that stemming from a fixed effects model. What this means is that we have strong evidence of the correlation between being under an IMF program and increased protest

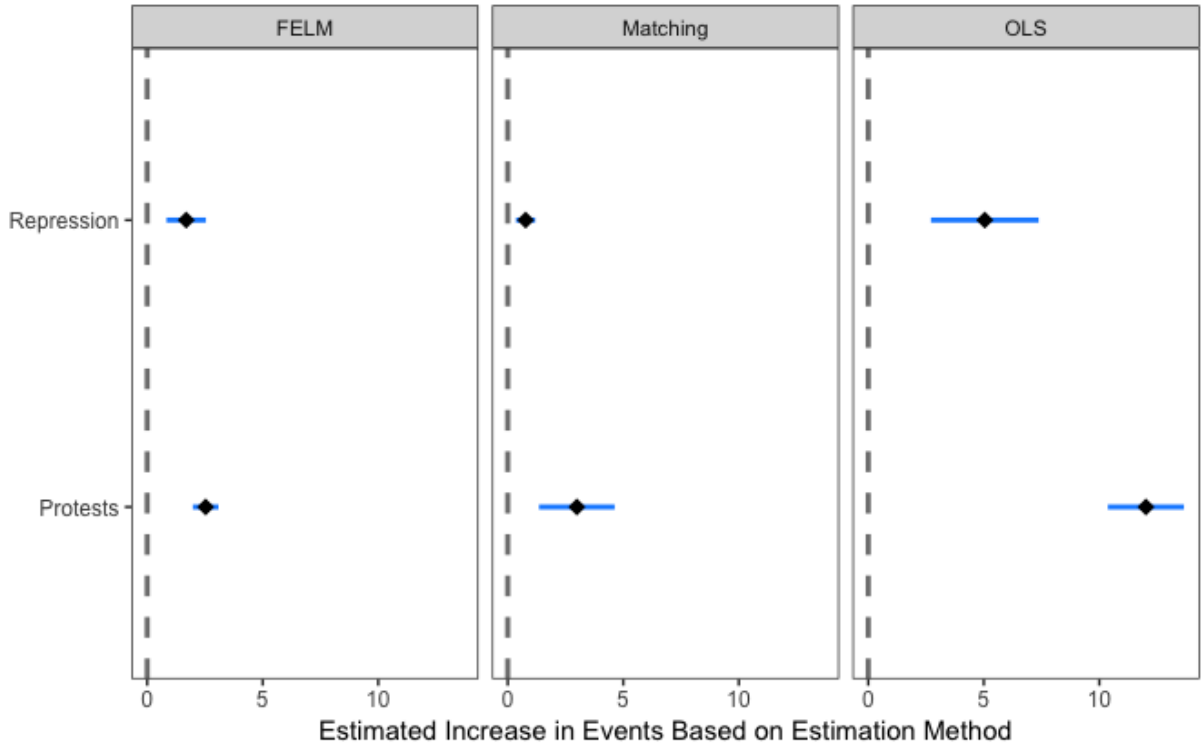

Figure 5: Model Comparison, Backlash Hypotheses

frequency and repression. This means that I fail to reject Hypotheses 1A and 1B – protests and repression are more likely in the months that I state spends under an IMF program.

Next, I isolate just the country-months during which states are under IMF programs. I use this subset of the data to examine Hypotheses 1C and 1D: that protests and repression increase during months under programs with conditionality versus those without. Again, I employ matching in several different phases to test the robustness of the result.

	Dependent variable:	
	Protests	*Repression*
	(1)	(2)
Estimate	10.045***	2.3055***
	(0.90523)	(0.22116)
Conditionality?	✓	✓
T-stat	11.096	10.424
P-value	< 2.22e-16	< 2.22e-16
Original N	4,791	4,791
Original N, Treated	3,097	3,097
Matched N	3,097	3,097
Matched N, (unweighted)	3,097	3,097
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 3: IMF Programs with Conditionality

The results from these models indicate that programs with conditionality are correlated with more protests and repressive events than those without conditionality. These results are highly statistically significant. More specifically, programs with conditionalities are correlated with 10 more protests and 2.3 more repressive events per month. It is important to note several caveats about the data at this point, which limit the amount of inference that can be drawn from this result. First, the data is drawn from the IMF website and the conditionality variable is a simple indicator for each program, denoting that a particular agreement has or does not have "strings attached." This means that it is difficult to say exactly which strings could have produced the result in the table – was it that cuts to social security programs and pensions caused citizens to riot, or was it demands for market liberalization and the gutting of unions? The data simply does not reveal what aspect produced this result.

More work is needed on this topic in order to understand exactly why conditionalities would make human rights worse. It is easy to speculate – as critics of globalization do –

that austerity or any number of factors leads to worse human rights, but it is not possible to make any of these aspects in particular lead to worse human rights outcomes. For now, the best I can do is to test whether conditionalities in general lead to worse outcomes, which I attempt in the next section.

Parsing out the effect of Conditionality

In order to test whether conditionalities are to blame for increased protests, I run two more matching analyses: **1)** a matching analysis with just the program-months with conditionalities versus the control months and **2)** matching with those program-months without conditionalities versus control months. In the previous section, I stated that those programs with conditions attached were marked by more protests and repression than those programs without. By differentiating these models, I can effectively parse out what may be driving the result. The results of these two matching analyses is below, and balance plots are in the Appendix:

	Dependent variable:			
	Protests	*Repression*	*Protests*	*Repression*
	(1)	(2)	(3)	(4)
Estimate	6.9584***	1.3258***	-5.9273***	-1.3208***
	(1.2707)	(0.34679)	(0.59523)	(0.16148)
Conditionality?	✓	✓		
T-stat	5.4761	3.823	-9.9579	-8.1795
P-value	< 4.2483e-08	0.00013185	< 2.22e-16	< 2.22e-16
Original N	25,436	25,436	18,648	18,648
Original N, Treated	3,097	3,097	1,694	1,694
Matched N	3,097	3,097	1,694	1,694
Matched N, (unweighted)	136,739	136,739	187,028	187,028
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 4: IMF Programs with and without Conditionality

First, it is helpful to look at the sign of the coefficients. Programs with conditionalities are estimated to produce almost 7 instances of protest and 1.3 instances of repression per month – a positive result that aligns with the other previous results. Taking a look at the programs without conditionality, we see that the coefficients are negative. That is, the programs without conditionalities are actually correlated with 6 and 1.3 *fewer* protests and repressive events, respectively, than the control months. Does this mean that I have parsed out conditionalities as the causal agent for strife and repression in these cases? Since these programs (and conditionalities) are not randomly assigned, we cannot assume causal identification in this study. Conditionalities may be given as disincentives or required of states in order to induce behavior, for example. We cannot say that conditionality causes protests or repression. What we can say, however, is that there is a strong correlation – one that is not due to the IMF programs alone.[6]

The problem mentioned above – that the IMF data does not dis-aggregate by conditionality substance and purpose – precludes me from saying or speculating about which specific conditionality might be responsible for these changes. It could be that conditionality terms make states institute painful changes in order to repay the IMF or to make them eligible for future help or loans from the international community. More research is needed in this area, as well as more granular data.

On the other hand, the negative coefficient on programs without conditionality should give researchers pause as well; this is a counterintuitive result – one that goes against much of the literature. The reason for fewer protests could be that states use IMF funding to actually invest in the projects that increase citizen well-being and thereby decrease the need for protest. It could also be that states have a stronger repressive apparatus when they receive a cash inflow, though this hypothesis is counteracted by the fact that repression decreases

[6]I also performed a robustness check using Mahalanobis distance matching and obtained a similar result – see Appendix.

in addition to protests decreasing. More, case-specific research is needed to understand exactly how these programs might foster growth and decrease the propensity of citizens to protest. If programs lead to better economic outcomes, then this could perhaps be a key explanatory factor. Thinking about this issue is also important for the case study of Pakistan and our expectations as the deal-making process continues. If Pakistan is able to successfully negotiate for fewer (or no) conditionalities, then we might expect fewer protests and repression than would have occurred in the counterfactual world of conditionality. Next, I test my second send of hypotheses, about whether citizens and the state anticipate the effects of these programs.

5.2 Anticipation Hypotheses

Next, I test the hypotheses regarding anticipation, or the theory that protests and repression are more likely to occur in the month immediately prior to the beginning of the agreement. The mechanism in this case could be that protesters try to prevent a program from being implemented or try to get the government to renege on its deal with the IMF.

In order to examine these hypotheses, I first create an indicator variable that takes the value of "1" when the month is immediately prior to the start of an IMF program. I then subset my data to compare these "anticipatory" months with the rest of the control months (months not under an IMF program). The results from the OLS and fixed effects regression models indicate that the month immediately prior to the beginning of a program is correlated with more protests (23 or 10, depending on the model) and more repression (10 or 6.5 more repressive events). These results are significant at the 99% confidence level.

At first blush, it would appear that there is some evidence for the hypotheses that protests and repression increase in the month prior to the implementation of an IMF program, perhaps because citizens and governments anticipate the negative effects of that program. However, I also ran a matching model, as before, which produced starkly different results. There are

two caveats with using matching for testing this set of hypotheses. First, the sample size is much smaller because there are only 210 anticipatory months in the dataset, meaning that there are approximately a tenth of the number of matched pairs, compared to matching to test the backlash hypotheses. The second caveat is that covariate balance is much poorer for these hypotheses – see the figure below. Notice that though the blue line (representing the absolute mean difference between the observations for the adjusted pairs) is to the left of the red line (absolute mean difference for the unadjusted pairs), both lines are still far away from the target value of 0. This means that comparison the two groups may not be appropriate and may produced biased results.

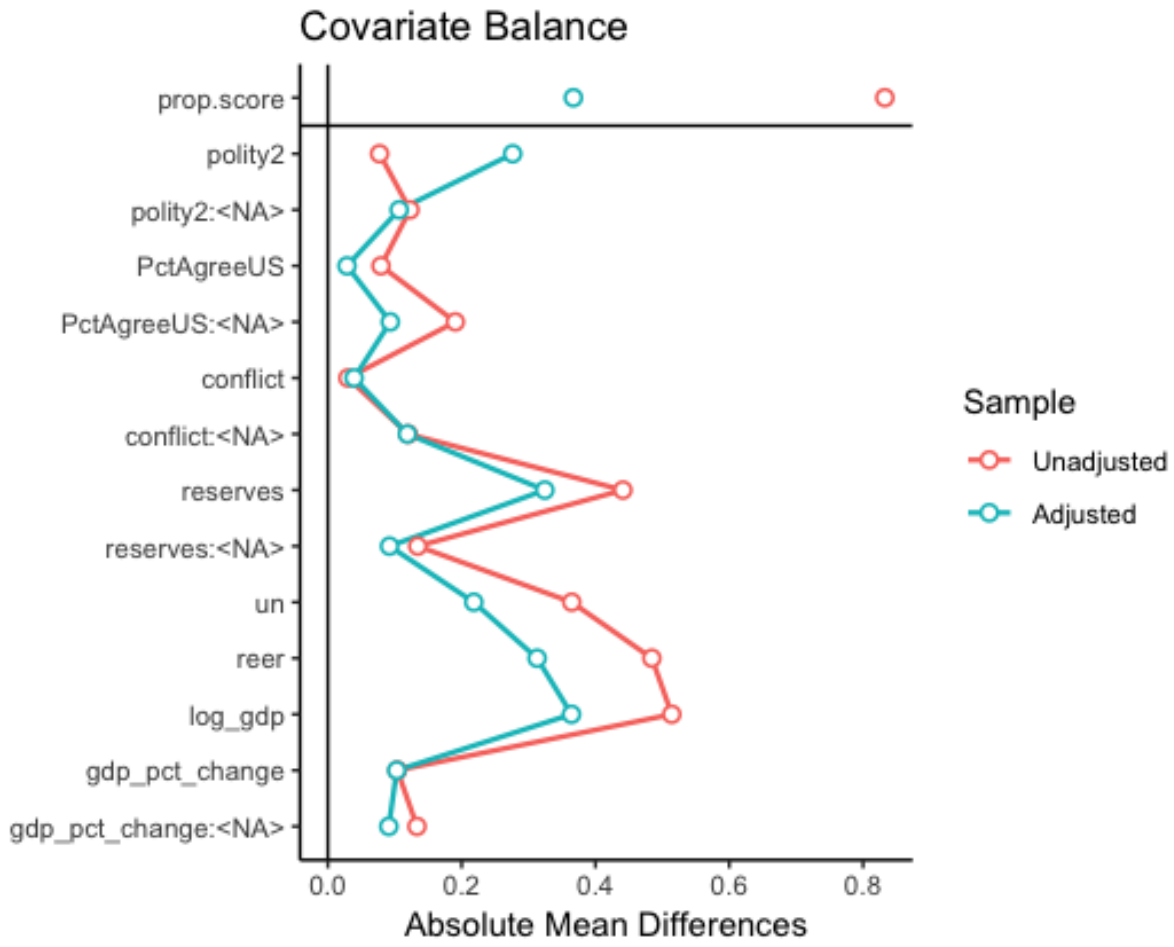

Figure 6: Balance Plot for Covariates, before and after Matching (Anticipation Hypotheses)

	Dependent variable:			
	Protests		Repression	
	OLS	*Fixed Effects*	*OLS*	*Fixed Effects*
	(1)	(2)	(3)	(4)
Month before program	23.523***	10.192***	10.095***	6.468***
	(2.016)	(1.993)	(0.451)	(0.454)
Polity2	−0.108***	−0.171***	−0.045***	−0.003
	(0.022)	(0.054)	(0.005)	(0.012)
UN Voting Record	−0.130	5.405***	−0.217	1.096***
	(0.592)	(1.591)	(0.132)	(0.363)
Ongoing conflict	1.093***	2.873***	−0.070	0.380***
	(0.346)	(0.547)	(0.077)	(0.125)
Foreign Exchange Reserves	−0.012	0.103**	0.0001	0.001
	(0.028)	(0.044)	(0.006)	(0.010)
Unemployment Rate	−0.107***	0.012	−0.013***	0.003
	(0.020)	(0.034)	(0.005)	(0.008)
Real Exchange Rate	0.025**	−0.0002	0.004*	−0.002
	(0.011)	(0.012)	(0.002)	(0.003)
log GDP	−0.174*	1.420***	0.015	0.229***
	(0.100)	(0.265)	(0.022)	(0.060)
GDP Monthly Percent Change	−0.046	−0.014	0.004	0.005
	(0.036)	(0.035)	(0.008)	(0.008)
Constant	1.492		0.120	
	(1.338)		(0.299)	
Observations	2,866	2,866	2,866	2,866
R^2	0.087	0.282	0.202	0.348
Adjusted R^2	0.084	0.274	0.199	0.340
Residual Std. Error	4.897 (df = 2856)	4.361 (df = 2832)	1.095 (df = 2856)	0.994 (df = 2832)
F Statistic (df = 9; 2856)	30.330***		80.220***	
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 5: Anticipatory Protest and Repression

	Dependent variable:	
	Protests	*Repression*
	(1)	(2)
Estimate	0.053018	0.14679
	(0.58057)	(0.25943)
T-stat	0.091321	0.56582
P-value	0.972724	0.57151
Original N	22,444	22,444
Original N, Treated	210	210
Matched N	210	210
Matched N, (unweighted)	10,040	10,040
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 6: Results from Matching Models, Anticipation Hypotheses

The end result is that there is mixed evidence for hypotheses 2A and 2B. Depending on the model specification, the month before an IMF program is implemented is correlated with more protests and more repression, or this month is not any different in terms of protester or state behavior. Why might I have gotten such different results, depending on the model selected? This could come down to a number of factors, including endogeneity problems, that are beyond the scope of this paper. For example, though I attempted to include all possible relevant covariates, there could be something about the pre-program months that led to biased results for the OLS and fixed effects models that led the results to indicate increased protest frequency. This is the perk of using matching – each anticipatory month is paired with a (more or less) closely related counterfactual country month. By doing so, the researcher can parse out how many more protests or events are likely to be a result just of the "treatment" – the anticipatory month in this case – versus background noise or random chance. It seems likely, then, that matching should have gotten us closer to the "true" effect of the anticipatory month, though it will take more research to confirm whether this is the case. Given the caveats articulated above – small sample size and covariate imbalance – I

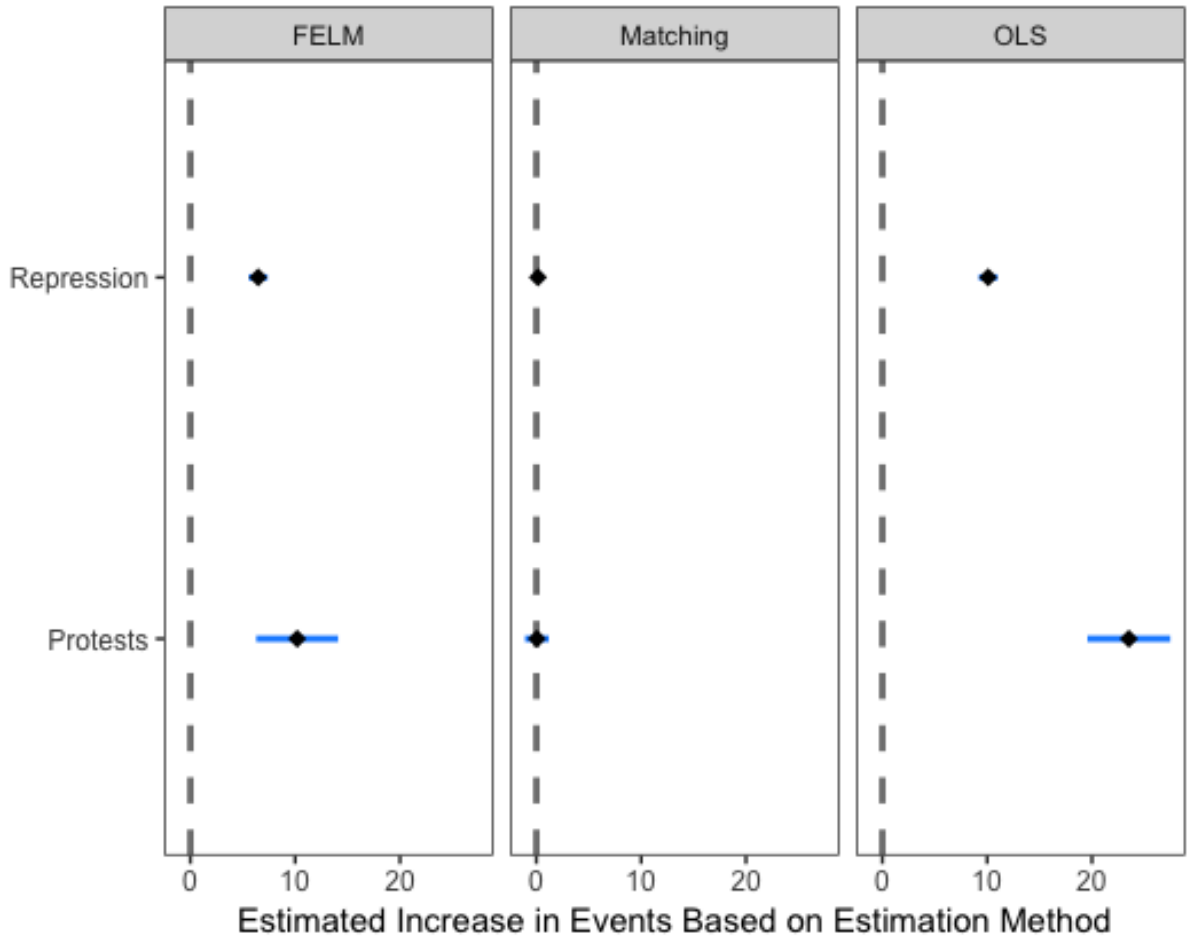

Figure 7: Model Comparison, Anticipation Hypotheses

refrain from making any strong claims about the effect of IMF programs on protests and repression the month before a program begins.

6 Discussion and Conclusion

In this paper, I sought to examine how and to what extent agreements with IFIs can affect state and protester behavior – specifically regarding human rights. This literature poses salient empirical and normative questions, particularly because these large lending institutions continue to loan money to struggling and developing states. This study focused on the largest institution, the IMF. I have attempted to address previous methodological issues and to contribute to this debate by advocating for a more rigorous approach – one that seeks to tease out the specific mechanisms at work.

My contributions to this area of study are two-fold: one methodological and one regarding data. I used the method employed in this study, matching, in an attempt to create more realistic counterfactual cases for comparison. That is, I used background information about each country-month in the dataset to find a comparable comparison case – with the only (substantive) difference between the two cases being that one country-month was under a program and one was not. Implementing such a technique hopefully allowed me to draw closer to the true result for each set of hypotheses. Using the technique of matching to try to find relevant comparison cases, versus comparing cases that differ widely on aggregate, is an improvement on this work and should allow for greater confidence in our estimates and greater clarity in policy prescription.

I also utilize monthly data in order to add a level of granularity not found in many papers in human rights (though see Beardsley (2011)). Using monthly data is a contribution to this area of study because it allows the researcher to implement a different dependent variables than the traditional CIRI human rights index or Political Terror Scale measures – both of which are aggregated representations of human rights in a country-year in general and provide little nuance as a result. Though ACLED is not a perfect measure – particularly for studying repression writ-large – it is useful in this case because it captures state-on-civilian

violence and protests: the two dependent variables that flow logically from the literature. Because I also have monthly data for many of the economic and noneconomic covariates, I can also harness more of the power of this data than would be possible if I had to rely solely on annualized measures.

The results of my analysis indicated that months under IMF tenure are correlated with more protests and more acts of government repression, indicating that previous work by authors such as Abouharb and Cingranelli (2006; 2007; 2009) and others points in the right direction. Indeed, given that I subjected these backlash hypotheses to a battery of tests, it seems reasonable to say that IMF programs are correlated strongly with increased protests and repression. What I cannot say, as Abouharb and Cingranelli and others do, is that programs are correlated with a specific type of repression. Moreover, I have no theoretical reason to believe that programs will be related to increased extra-judicial killings, torture, or any specific human rights violation, as they claim. Future work could determine, specify, and examine such claims, but I do not do so here.

Additionally, previous work did not explore the specific effects of program conditionality. In this work, I find that this conditionality underpins my entire result. That is, only IMF programs with conditions on the disbursement of funds are correlated with increased protests and repression; program-months without conditionality actually lead to decreased protest and repression. Unfortunately, data limitations do not allow more specific identification of *which* conditions lead to worse human rights outcomes – again I leave this to future work. However, by working to rule out specifically what it is about IMF programs that lead to worse outcomes, we might be able to then back-track and speculate about what led people to protest in the first place. If conditionalities are to blame, then potential causal mechanisms relying on grievances about being hamstrung by an international financial institution have less support. It may be the case that becuase conditionalities make states change their behavior, then these changes are what lead to observed human rights outcomes.

I also tested two hypotheses focused on the importance of the month before a program began. The hypothetical mechanism at work is that protesters know that the state is about to undergo a loan with the IMF, but they do not like this outcome for some reason – they fear the repercussions from paying back the loan, are worried about conditionality and cutbacks to public spending, or simply do not want their leaders to be beholden to a foreign financial institution run by the United States. Results for this mechanism were mixed. Two of the models – OLS and OLS with two-way fixed effects – showed that the month before the start of a program is correlated with more protests and more repression. However, once I used matching to isolate valid counterfactuals, this result disappeared. Though I speculated in the paragraphs above that this may be a data issue, there could be other factors at work as well. It is simply not clear whether the anticipatory hypothesis might be valid or not.

One way to investigate this particular issue further would be to gather data on the release of information by the IMF about the status of negotiations with states. That is, if I had information about how far in advance of a program start date it became clear that a country was going to enter a program with the IMF, then I examine the months before and after this point to see if these announcements changed the behavior of protesters or the state, perhaps implementing an event study (a la Wilf (2016)) or regression discontinuity design across a host of cases.

Another issue that deserves more exploration is conditionality. Do conditionalities, or perhaps even just the threat of conditionalities, make states more likely to repress or people more likely to protest? Why? This study has shown that programs with conditionality are correlated with increased protests and repression, but we do not know why exactly. We cannot make broader claims for two primary reasons. The IMF's data does not indicate what goes into their "conditionality" category, and it is likely that this varies from country to country and across time – perhaps due to that country's affiliation with the United States or for a whole host of other reasons. Secondly, and more broadly, we do not know how long

it takes for the conditionalities to be implemented and for them to have a substantial effect. Perhaps they do not actually hurt unions, as the case in Pakistan would lead us to believe, but we cannot tell this from the data. Enormous structural reforms, privatization, and austerity are likely to be highly controversial. What effect do conditionalities recommending such policies produce? Do they increase political turnover and unrest? Is this what is actually driving the increased number of protests and repressive events? We simply do not know.

Addressing the issues of this paper more broadly raises fundamental questions about state accountability, capacity, and behavior. What happens when a state becomes accountable to a third party? Does this make that state less likely to pay attention to its own citizens needs? If an IFI supplants a citizenry as a source of direction and legitimacy, what are the long-term effects for state development? Unaddressed in this paper are more questions: to what extent do states comply with conditionalities? Are there degrees of austerity, and how do these affect levels of protest, repression, and unrest? How does the negotiation process between debtors and the IFIs work – what are the internal political conditions that may change how quickly a loan deal is reached (McDowell 2017) or the terms of that deal? Such questions are beyond the scope of this paper, but I do attempt to add to this growing debate.

Turning briefly to the motivating case of Pakistan, it seems unlikely that entering into another IMF program will lead to positive human rights outcomes, though this result could be dependent upon the conditionalities that the IMF imposes on the country. And although the state has stayed its hand so far, will that be the case in the future and after a deal is reached? The negotiations with the IMF are highly public in this day and age and with the advent of social media – how will this affect the likelihood of protesters to organize, share, and express their grievances? How will the state respond, and will this be conditioned on the conditions of the loan? Will the state's repressive capacity also increase as a result of the IMF program, or will its hands be increasingly bound by the international organization? Will protests from labor unions and other activists dissuade the Pakistani government from allowing the IMF to

add in harsh conditionalities? This case demonstrates the policy relevance of this study and the importance of working to better understand how the interaction between international institutions and states plays out on a daily basis.

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Appendix

Variable	C Mean	T Mean	C SD	T SD	C Median	T Median	C Min	T Min	C N	T N
IMF programs	0.00	1.00	0.00	0.00	0.00	1.00	0.00	1.00	22339	4791
Polity2	1.93	2.03	6.35	4.55	4.00	3.00	-10.00	-9.00	18759	4616
UN Voting Record	0.18	0.14	0.13	0.07	0.15	0.13	0.00	0.00	16613	4534
Ongoing Conflict	0.33	0.26	0.47	0.44	0.00	0.00	0.00	0.00	19671	4785
Foreign. Ex. Res.	6.81	5.11	4.41	2.00	6.73	4.85	0.00	0.07	3624	227
Unemployment	8.92	11.47	5.21	3.61	8.50	9.62	0.45	4.40	3624	227
Real Exchange Rate	96.21	100.34	9.89	6.34	98.34	100.12	57.17	81.37	3624	227
GDP percent change	0.45	0.15	2.45	1.78	0.00	0.00	-21.74	-7.54	3599	227
log GDP	10.78	9.85	1.44	0.53	10.82	10.07	7.76	8.09	3600	227
Protests	12.88	7.37	93.51	40.82	0.00	0.00	0.00	0.00	22339	4791
Repressive Events	4.25	2.03	31.96	9.91	0.00	0.00	0.00	0.00	22339	4791

Table 7: Summary Statistics for Entire Dataset

Variable	C Mean	T Mean	C SD	T SD	C Median	T Median	C Min	T Min	C N	T N
Conditionality	0.00	1.00	0.00	0.00	0.00	1.00	0.00	1.00	1694	3097
Polity2	0.92	2.63	4.81	4.28	0.00	5.00	-9.00	-7.00	1624	2992
UN Voting Record	0.14	0.14	0.07	0.07	0.13	0.13	0.00	0.03	1628	2906
Ongoing Conflict	0.34	0.21	0.47	0.41	0.00	0.00	0.00	0.00	1694	3097
Foreign. Ex. Res.	6.37	4.46	1.98	1.69	6.85	4.39	0.56	0.07	77	150
Unemployment	9.98	12.23	3.12	3.61	9.27	9.93	5.50	4.40	77	150
Real Exchange Rate	96.21	102.46	5.82	5.51	97.85	101.18	81.37	88.01	77	150
GDP percent change	0.29	0.08	2.01	1.65	0.00	0.00	-7.54	-5.05	77	150
log GDP	9.83	9.86	0.53	0.53	10.05	10.11	8.09	9.23	77	150
Protests	0.86	10.93	3.33	50.35	0.00	0.00	0.00	0.00	1694	3097
Repressive Events	0.51	2.86	2.32	12.13	0.00	0.00	0.00	0.00	1694	3097

Table 8: Summary Statistics for IMF Program Country-Months

Country		
Afghanistan	Guinea	Oman
Albania	Guinea-Bissau	Pakistan
Algeria	India	Palestine
Angola	Indonesia	Philippines
Bahrain	Iran	Qatar
Bangladesh	Iraq	Republic of Congo
Belarus	Israel	Romania
Benin	Ivory Coast	Russia
Bosnia and Herzegovina	Jordan	Rwanda
Botswana	Kenya	Saudi Arabia
Bulgaria	Kosovo	Senegal
Burkina Faso	Kuwait	Serbia
Burundi	Laos	Sierra Leone
Cambodia	Lebanon	Somalia
Cameroon	Lesotho	South Africa
Central African Republic	Liberia	South Sudan
Chad	Libya	Sri Lanka
Croatia	Madagascar	Sudan
Cyprus	Malawi	Syria
Democratic Republic of Congo	Malaysia	Tanzania
Djibouti	Mali	Thailand
Egypt	Mauritania	Togo
Equatorial Guinea	Moldova	Tunisia
Eritrea	Montenegro	Turkey
eSwatini	Morocco	Uganda
Ethiopia	Mozambique	Ukraine
Gabon	Myanmar	United Arab Emirates
Gambia	Namibia	Vietnam
Ghana	Nepal	Yemen
Greece	Niger	Zambia
Guinea	Nigeria	Zimbabwe
Guinea-Bissau	North Macedonia	

Table 9: List of Countries in Dataset

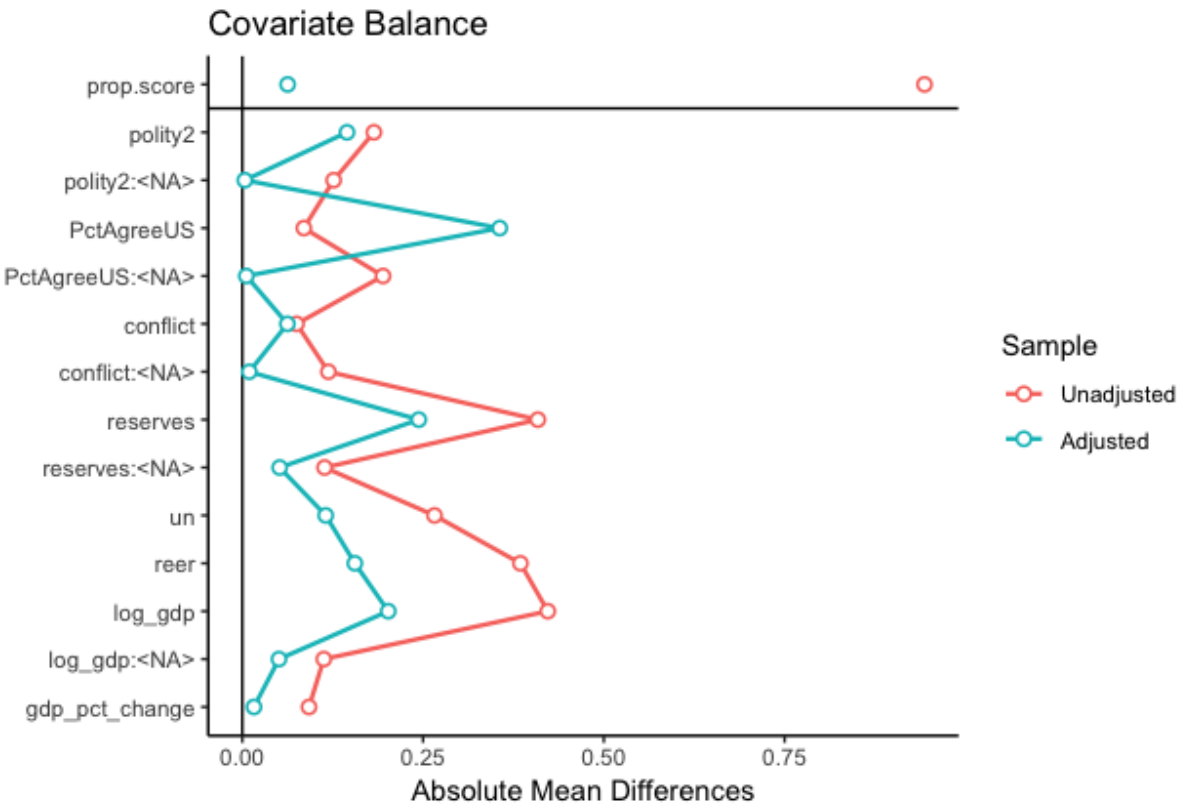

Figure 8: Balance Test, Conditionality Program-Months

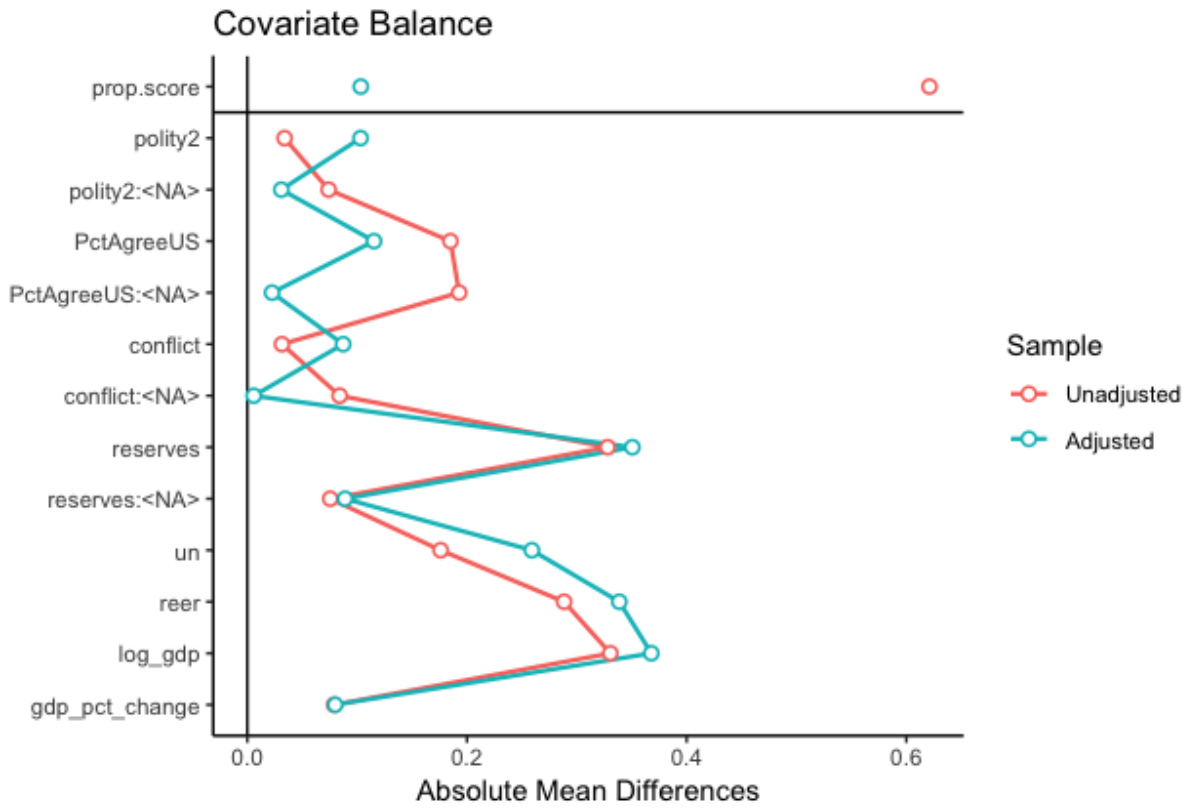

Figure 9: Balance Test, Non-Conditionality Program-Months

	Dependent variable:	
	Protests	*Repression*
	(1)	(2)
Estimate	2.392***	0.93759***
	(0.60693)	(0.16891)
T-stat	3.9411	5.5508
P-value	8.115e-05	2.8439e-08
Original N	27,130	27,130
Original N, Treated	4,791	4,791
Matched N	4,791	4,791
Matched N, (unweighted)	227,667	227,667
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 10: Robustness Check for Backlash Hypotheses using Mahalanobis Distance Matching

	Dependent variable:			
	Protests	*Repression*	*Protests*	*Repression*
	(1)	(2)	(3)	(4)
Estimate	6.0202***	1.5675***	-5.9226***	-0.76084***
	(0.85104)	(0.28943)	(0.97849)	(0.19116)
Conditionality?	✓	✓		
T-stat	7.0739	5.4161	-6.0528	-3.9801
P-value	< 1.5059e-12	6.0925e-08	1.4236e-09	6.886e-05
Original N	25,436	25,436	18,648	18,648
Original N, Treated	3,097	3,097	1,694	1,694
Matched N	3,097	3,097	1,694	1,694
Matched N, (unweighted)	136,739	136,739	187,028	187,028
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 11: IMF Programs with and without Conditionality – Robustness Check with Mahalanobis Distance Matching