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Understanding the Causes and Consequences of Refugee Movements

by

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Abstract for:

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As refugee populations continue to grow, understanding their relevance to international politics becomes increasingly important. In this project, I explore how refugee populations shape and are shaped by conflict. First, I develop a model of the causes of refugee flight emphasizing how individuals evaluate their prospective risk of victimization from a variety of direct and indirect experiences with violence. I find evidence that refugee flows are larger when violence takes place in heavily populated areas (direct exposure) and when a rebel group experiences significant battlefield defeats (indirect exposure). These results, and the finding that poor performance by rebel organizations is correlated with the presence of co-ethnic refugee populations, point towards the possibility of connections between refugees and militant groups. To further explore this connection, in the second section, I propose a model of the spread of civil conflict through refugee populations. I argue that refugee camps provide useful resources to improve militant’s warfighting capabilities and recover strength after suffering strategic defeats. When these groups seek to use refugee populations and camps for their benefit the risk of civil conflict in the refugee-hosting states increases due to the presence of violent actors. I find support for these expectations using data on conflicts and refugee flows in Africa and further explore the validity of these ideas by looking closely at a small set of conflicts from across the globe. Finally, after examining the ways that refugee populations can influence the spread of violence abroad, I consider how states may respond to the risks associated with hosting refugees. Specifically, I argue that states hosting large refugee populations are more likely than others to intervene in the refugee-producing conflict – especially when the state would otherwise be unaffected by the conflict due to its distance or lack of ethnic ties. This expectation is supported by data on African conflicts and interventions and supports the idea that refugee flows drive intervention by creating security risks for host states. Altogether, these findings offer compelling evidence for the importance of refugees to international politics and civil conflict and provide an impetus for further study of the dynamics of refugee flows.
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Chapter 1: Introduction

Each day, millions of people find themselves surrounded by violent conflict. Gang violence, civil wars, interstate conflict, and state repression are but a subset of the forms of violence that an individual may encounter (Sambanis 2004). Quite obviously, individuals in such circumstances can respond in a variety of ways and are not simply passive observers of such conflicts. Individuals face varying incentives and opportunities to engage in behaviors along a spectrum from full participation in to outright flight from these violent activities. Though this multitude of options greatly complicates our attempts to understand the way civilians behave in times of conflict, such an understanding is critically important for any attempt to prevent, anticipate, and mitigate the effects of such conflicts.\(^1\) While this concern can motivate the study of any number of empirical phenomena related to war-time civilian behavior, the focus of this piece is on the causes and consequences of cross-border refugee flight.

The United Nations High Commissioner for Refugees (UNHCR) identifies refugees as individuals “unable or unwilling to return to their country of origin owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group, or political opinion” (UNHCR 1951). Recent work estimates that over 65 million people were displaced at the end of 2015. Out of this total, around 21 million individuals were classified as refugees with internally displaced persons (IDPs) making up the remainder. While refugees have been a persistent feature of the post-World War II world, the magnitude of current refugee migration is widely considered to be reaching “crisis-levels” and is the highest officially-recorded refugee count in at least two decades (UNHCR 2016a). As the current crisis continues

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\(^1\) While this statement likely comes as no surprise, research into how individuals make these decisions, the factors that influence the available choices, and the way these choices impact other conflict dynamics has developed relatively recently. For a selection of models which examine the strategic choices faced by civilian actors during civil conflict see for example (Barter 2014, Kalyvas 2006, Kalyvas & Kocher 2007)
to grow, policymakers in a number of countries have recently proposed various types of legislation aimed at protecting their constituents from the harms associated with hosting refugees. These concerns are often delicately balanced with the existing international norms that call for protection of refugees (UNHCR 1951). Further, both governmental policymakers and international organizations like the United Nations have been challenged by the current crisis as they work to allocate precious resources to efficiently provide relief to both refugees and the countries that host them.

Altogether these and other events have caused refugees to become a topic of much discussion among scholars, policymakers, pundits, and everyday citizens. At the core of many of these discussions are questions about why the current crisis has become so severe and how host states will be impacted. In this project, I seek to explore these two aspects of refugee behavior, and aim to make three distinct but interrelated contributions. First, I will re-examine the causes of refugee flows and attempt to expand the pool of known determinants of refugee flight. In doing so, I hope to provide insight into how the decision to flee as a refugee is made and further develop existing theoretical models of refugee flight.\(^2\) My second objective is to develop and evaluate a theoretical model linking refugees to violence in countries that host them. While a number of mechanisms linking hosting refugees to civil violence have been proposed, few have been examined using quantitative measures. My plan is to use recently available data to both refine and evaluate a model of refugee-related violence that directly links refugees, rebel groups, and host governments together. The third and final goal of this project is to examine how host

\(^2\)Note here that I refer to refugee flight as a “decision.” This is an important point as the debate over whether to treat refugee flight as a (“rational” or “considered”) decision or to emphasize the “forced” aspect of the term “forced migration” is an ongoing one. Within political science, it is the norm to treat refugee flight as an individual decision. I follow this tradition and emphasize that flight is a choice – weighed against other options – and that this choice is made by individuals and not by groups.
states respond to refugee inflows by examining their propensity to intervene in the conflicts that produce said refugees. In doing so, I hope to expand the types of outcomes examined by refugee studies scholars to include direct policy responses by host states and to broader the field of theoretical concepts linked to refugees.

Throughout this project, I plan to use a combination of statistical models and qualitative evidence to support my theoretical arguments and enhance the validity of my specific findings. In my opinion, the nature of refugee studies (and the associated quantitative data and historical records) necessitates such an approach. My goals are to provide fresh insight into specific areas within refugee studies and to facilitate the expansion of what scholars consider relevant to refugee flows. Indeed, I argue that scholars have overlooked, for a variety of reasons, a number of important dynamics that, when properly considered, would not only enhance our scholarly understanding of refugees but also help policy-makers respond to refugee crises.

Aside from providing additional insight into what has become a popular and much-debated topic, this research project can be justified on a number of other theoretical and policy-oriented grounds. First, one of the most obvious reasons to better develop models of refugees is the potential for increased accuracy in predicting large-scale refugee flows. A host of international organizations and agreements are unequivocal in their assessment that large refugee flows are an international humanitarian crisis that impacts countless stated beyond those directly “sending” and “receiving” such individuals. Guarding against regional destabilization, preventing harmful economic competition, and providing humanitarian aid are but a few of the concerns faced by entire regions facing a refugee crisis. Obtaining reliable predictors of when refugee flows, and these subsequent concerns, will reach “disaster levels” is helpful not only for planning when and where to devote precious resources but also allows humanitarian
organizations and interested states to potentially undertake strategies aimed at reducing refugee flows.

In addition to increased predictive accuracy, perhaps the most compelling policy-based reason to examine refugee dynamics is found in the plethora of security concerns that surround refugees post flight. Large refugee populations, whether hosted in camps or dispersed throughout an asylum country, face difficulties over the allocation of basic resources, employment opportunities, disease control, security from persecuting forces and a number of other concerns (Jamal 2000, Agier 2011). Failure by the host state to adequately rise to these challenges can lead to protests, riots, and even large-scale violent attacks (Salehyan & Gleditsch 2006, Lischer 2006).

Finally, studying refugee migration during civil conflict can also provide insight into a broader class of individuals and a larger set of conflictual settings than those directly studied in this project. For instance, studies of refugees can provide insight into issues of social trust and the relationship between citizens and their governments by examining which conflict dynamics lead to larger refugee flows than others. Such theoretical connections can even provide additional insight into how conflicts play out after significant migration has occurred. Consider for example how war-torn countries might change after individuals with low risk tolerance and a lack of strong social trust have fled. The rise of protest movements, pro-government militias, and increased recruitment by rebel actors may follow large refugee flows as a process of self-selection leaves only those willing to take risks and resort to arms remain. Research into the timing and causes of these changes would likely benefit from a deeper understanding of refugee migration as we develop a deeper understanding of the types of people who flee and, by extension, the types who remain.
The above discussion should make clear that studying both the causes and consequences of refugee flows can aid in findings solutions to practical problems by facilitating advanced planning and a deeper understanding of the contexts in which refugees may be associated with violent events. Further, our scholarly understanding of civilians in war-torn countries will also benefit from increased attention to refugee flows as we develop models of civilian choice and begin to understand the characteristics of both individuals who flee and those who remain.
Chapter 2: A Prospective Risk Model of the Refugee Decision

The war is like a subject endowed with a will and a power of its own: it comes, it arrives, it bursts into their life and drives them hastily into the ‘bush’

- Michel Agier *Managing the Undesirables*

A large proportion of firsthand accounts of refugee flight often depict one of two scenarios surrounding the decision to flee. In one case, an account of direct victimization is offered in which the subject or the subject’s possessions are violently assaulted by a conflict actor and flight is chosen in order to escape from imminent danger. Survivors of such violence describe troops “indiscriminately killing” civilians as they “stole…cattle, destroyed…property, and decimated the whole village” (quoted from Smock 1982; see Vickery 1984, Agier 2011, and Van Arsdale 2006 for similar accounts).

In the other narrative, subjects recount a decision to flee that is based more on the prospects of future loss than on present circumstances and direct experience with violence. For instance, Agier (2011) recounts the story of Liberian refugees who fled their homes long before rebels arrived in their village because they belonged to a “group targeted by [rebel leader Charles] Taylor” and were afraid they would be victimized if his forces arrived. Similar accounts also demonstrate that many refugees take flight not because of current violence but because of the prospects of future turmoil and victimization.¹

¹ See Zolberg et al. (1989) for instances where it seems many individuals fled their home country due to fear of future events. Of particular interest are the Cuban Revolution and the events in many Southeast Asian states during the 1970s where invasions from outside actors, the fall of capital cities, and the success of revolutions all drove refugee flight among those indirectly impacted by violence. Also note that the UNHCR definition of a refugee does not explicitly require being directly victimized in order to be considered a refugee - a well-founded fear is sufficient (UNHCR 1951).
These two impetuses of forced migrant flight involve different, though not incompatible, calculi based on distinct inputs. In this project, I offer a comprehensive model of refugee flight in times of civil conflict in which both of these dynamics are fully integrated as immediate causes of refugee flows. In doing so, I move beyond simply identifying previous violence as the cause of flight and instead focus on how individuals take into account their current and future levels of exposure to risk when deciding to flee or not. In this way, I present a prospective model of the refugee decision wherein future expectations about the course of the conflict are more influential than retrospective evaluations of a conflict’s intensity. Using newly available data, I demonstrate how factors that increase one’s prospective risk of exposure to violence have a larger impact on refugee flight than the associated, retrospectively evaluated intensity of violence. After discussing my results and their implications for research and practice, I conclude by offering directions for future work on the causes of refugee flight.

2.1 Existing Research on the Causes of Refugee Flight

There is little doubt that violence is the primary cause of refugee flight. Indeed, the very definition of a “refugee” demands that violence (or a fear of violence) be the cause of flight. That being said, appealing to general “violence” as a complete explanation is unsatisfying as it provides no means to differentiate between refugee crises where millions of people undertake flight and small-scale crises where only a select number of individuals flee. This basic shortcoming, and the motivation to build useful predictive models of refugee flight, has motivated scholars of refugees to consider deeper, more immediate causes of flight.

The dominant model of the causes of refugee flows used in political science begins by considering the refugee decision through the lens of a decision-theoretic framework wherein
civilian actors decide whether to flee or not by estimating a probability that they will be victimized by the conflict.\(^2\) This approach has its roots in early, largely descriptive work examining refugee crises that linked certain types of conflicts – namely civil wars – to larger refugee flows (Weiner 1996). Building on this distinction, scholars began to develop a framework were individual citizens evaluate the level of violence in their home country and chose whether or not to flee based in this consideration.

The expectations derived from this framework have been straightforward and empirically robust: violence creates refugees and different forms of violence create variation in the number of refugees (Moore & Shellman 2004; Moore & Shellman 2007; Melander & Oberg 2006; Melander & Oberg 2007). Specifically, variables such as the presence of an international war on a state's territory, the intensity of a civil conflict, and the level of state repression have all been conceptualized as indicating high chances of victimization and thus predicting high levels of refugee flows (Schmeidl 1997; Melander & Oberg 2007).\(^3\)

Moving beyond general violence as a cause of flight, a central theme among many of these previous works is the identification of the previous intensity of violence as a critical determinant of the size of refugee flows. The presence and *intensity* of government repression, genocide, terrorism, and armed conflict (where intensity is often measured via the number of associated deaths) are all linked, to varying degrees, to the creation of more refugees.

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2 See Moore & Shellman (2004) for the clearest formulation of this theoretical model. Note that many examples of empirical models of refugee flows which predate this model can be found e.g. Apodaca (1998), Schmeidl (1997). However, these previous models rely on a similar logic and argue for similar conclusions.

3 Coupled with a small body of research on the costs of forced migration – thus far focused on how trans-border ethnic kin and information networks can reduce such costs and facilitate migration (Ruegger & Bohnet 2015; Moore & Shellman 2007; UNHCR 2016b; UNHCR 2016c) – these findings form the core of our current understanding of the causes of refugee migration.
This focus on conflict intensity as a key driver of displacement highlights a key feature of this framework in that it focuses heavily on retrospective evaluations of conflict dynamics. In this framework, citizens decide whether or not to displace based solely on a general sense of how severe the conflict has been in the past. As a result, hypothetical citizens do not take into account questions about whether violence has ever been directed at ordinary citizens (as opposed to soldiers), whether they belong to a group (ethnic, political, etc.) that is likely to be victimized by a conflict actor, or even whether violence is taking place nearby and is actually likely to impact them. In this way, said citizens are entirely retrospective in their evaluations of the conflict in that they express concern over the previous course of the conflict and flee as a result.

In contrast to this retrospective model, the model presented here focuses on how citizens evaluate the current and previous course of the conflict in order to make prospective evaluations about the future of the conflict and, in particular, whether they will be victimized by it. This new focus on prospective decision making allows me to easily incorporate and examine a number of new theoretical concepts into a revised model of the causes of refugee flows. For instance, I explore how allegiances to conflict actors can impact decisions to flee when one actor is experiencing strategic losses and, as a result, defeat and the victimization of the group’s supporters seems likely. This idea is notable absent from the current literature simply because previous scholars have chosen to focus on retrospective evaluations of the conflicts severity as the primary driver of displacement.

In addition to focusing attention on the role of prospective risk assessments, this project provides insights largely absent from the current literature by examining and fully incorporating concepts such as ethnic ties between conflict actors, the different motivations for internal and external (or refugee) displacement, and the way different types of battles and different battlefield
outcomes impact refugee flight. Before detailing all of these ideas and the numerous related hypotheses however, it will be useful to fully specify the underlying theoretical model of prospective risk assessment and refugee flight.

2.2 A Prospective Theoretical Framework

The framework set forth in this study relies on much of the same logic used by the previous literature. The basic assumption that individual citizens evaluate their risks of victimization and decide to flee when said risks outweigh the costs of flight is retained. In addition, the idea of a refugee as one who flees abroad because of a “fear of persecution” is also maintained (UNHCR 1951). As a result, the key theoretical contribution of my model then is interpretive in that I develop a novel understanding of the method and information used in evaluating their risk of harm. That being said, while the overarching framework remains similar, the expectations I develop are novel and, in some cases, opposed to those developed in the existing literature.

Stated succinctly, the key distinction between the theoretical model I employ and that of the previous literature is in the way violence is evaluated as an indicator of risk. Under the existing framework, violence is largely seen as immediately threatening regardless of its nature (e.g. the type of attack or its outcome) or its location (e.g. near of far from any specific individual). As a result of its nearly universally threatening nature, violence is evaluated almost exclusively by its intensity; i.e. since violence is always threatening to every individual, its

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4 Indeed, it is hard to imagine a framework that doesn’t rely on this logic. Potential alternatives may include models based on groups making decisions (e.g. city or village officials promoting flight) or on individuals receiving some significant benefit from flight (e.g. greater economic opportunity). I find neither of these frameworks appealing; the former because it does a poor job in explaining locale-specific variation in flight and the later because it seems to be rarely the case that flight yields positive benefits for those who would flee during times of conflict (who must sacrifice much of their property/wealth to flee).
impact on overall threat is measures by its severity. I refer to this interpretation as a *general retrospective model* since the severity of all previous violence is used to evaluate current risks when making the choice to flee or not. Here, if violence is especially intense and many people are being killed, then the risks of victimization are seen as high and flight is more likely.

In contrast, I propose to consider the refugee decision from what I call a *particular prospective model* wherein current conflict dynamics are viewed with an eye towards evaluating the chances that future violence will negatively impact the individual. Under this framework, conflict intensity is less important than conflict *exposure* – that is, whether violence is likely to continue to or eventually harm the individual. Evaluating this chance of violent exposure involves considering short term dynamics (i.e. is violence taking place near me or towards me such that I am likely to be immediately harmed – the first narrative described in the introduction) or long term (i.e. do ongoing conflict dynamics signals that I am likely to be victimized in the future – the second narrative). In the proposed framework, not only does the attention shift away from indicators of a conflict’s intensity towards measures of its impact on civilians’ lives, but a whole new set of potential causes of flight come into consideration – those dynamics which signal a high risk of victimization in the distant future.

### 2.2.1 Short-Term Evaluations of Risk and Exposure to Violence

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5 To my knowledge, Melander & Oberg (2007) is the only existing study to examine exposure as a critical cause of refugee flight in their effort to “relax the assumption that armed conflict affects the population of a country uniformly.” This similarity does not reduce the uniqueness of the current project for several reasons. First, their theoretical model only discusses immediate exposure (wherein one has already been victimized) and does not argue from a prospective viewpoint that included a chance of future victimization. Second, unlike the current model, the authors retain a strong reliance on intensity based arguments (even though they note – correctly in my opinion – that intensity does not “necessarily imply a significant threat” and thereby partially undermine their expectation of a positive link between intensity and refugee flows). Finally, regardless of their theoretical framework, their findings regarding the link between exposure and refugee flows are complicated by their use of an indicator of exposure that seemingly includes a measure of the severity of displacement in its construction (see [http://www.systemicpeace.org/inscr/PTTFProbSetCodebook2015.pdf](http://www.systemicpeace.org/inscr/PTTFProbSetCodebook2015.pdf) and the section on the “MAGAREA” variable for a description of the relevant independent variable).
The distinction between how violence is evaluated has a significant impact on our understanding of the immediate causes of refugee flight. For example, consider how the intensity of a conflict is incorporated into the existing, general retrospective model. Under this framework, measures of intensity such as battle deaths, the amount of property damage, and a whole host of other factors are taken to mean that victimization and, as a result, flight are both likely. This is because high levels of previous battle deaths, for example, are taken as a sign that the level of violence in the country is high and, subsequently, that each individual’s risk of being killed is also high.

Consider now how a model emphasizing prospective risk of victimization would interpret a large number of deaths from conflict. In the model I present, general conflict intensity is expected to have little influence on expectations of victimization and, as a result, the overall flow of refugees from a conflict. This is because generic battle deaths, lacking information about whether the associated violence is likely to impact any specific person, does not inform anyone of their future risks of victimization.⁶ To gather this type of information about future risks, citizens focus on other aspects of violence. In particular, one straightforward indicator of a high risk of victimization in the short term is found in the location of violence and, specifically, whether the area where the individual resides has been recently exposed to violence.

When violence occurs in immediate proximity to an individual, it is, without a doubt, a common response for the individual to flee from said violence. The reason, I would argue, behind such behavior is simply that the individual in question recognizes that his or her short-

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⁶ Indeed, I expect that, net of the other important covariates I will examine, indicators such as the number of battle deaths associated with violent events will have no impact on refugee flow patterns. I do not deny that such measures of intensity will often correlate with refugee flows as they often also correlate with the important measures of exposure and future risk I emphasize. However, I contend that once these other causes of flight are brought to the forefront we will see that the mechanism exists that directly links conflict intensity to refugee flows.
term risk of being harmed is high. Especially in the contexts of civil conflicts, since violence is taking place near the individual, the chances that the individual or the individual’s property will be purposely or inadvertently harmed in the short term is indeed likely very high (Eck & Hultman 2007; Humphreys & Weinstein 2006; Wood 2010). This perception of increased risk is not due to any regard for the intensity of the violence, but instead due to the proximity of the violence and the fact that, much like those individuals noted in the introduction, the citizens have been directly exposed to violence. As a result, when seeking to explain variation in refugee flight, it is important to think about where violence is taking place and, relatedly, the number of individuals directly impacted by the violence.

The above idea is critical to understanding the first hypothesis of this project. I have already stated that refugee flight is driven by a heightened perception of risk and have now argued that direct exposure to violence will increase citizens’ short-term risk estimates. In order to use this logic to explain variation in refugee flows, I propose using information about the location of violence, in particular the number of citizens in the immediate area surrounding the event, as an indicator of the number of individuals who experience this increase in perceived short-term risk and who, as a result, the number of individual who flee as refugees.

Put simply, I argue that violence in heavily populated areas will create many refugees because more people are exposed to violence and more people update their risks expectations. Note how, in this case, the individuals fleeing in these situations are not necessarily those who have already been subject to bodily harm. While such individuals are indeed likely to flee in these circumstances (as they obviously have a high risk of future victimization), it is also the case that many other individuals, responding simply to the proximity of the violence, will also undertake flight. These ideas form the basis for my first hypothesis.
**Hypothesis 1:** Violence taking place in heavily populated areas will produce more refugees than violence in less populated areas.

To see this type of dynamic in action, consider the difference between events in Sierra Leone in the 1990s and Mali in the 2000s. While both countries experienced the onset of civil conflict during these periods and both began with relatively similar levels of conflict intensity, the location of the violence combat had, I argue, a notable difference on subsequent refugee flight. In the case of Sierra Leone, violence in the Eastern Province in and around the heavily-populated provincial capital of Kenema caused over three-hundred thousand individuals to displace in the first years of the conflict. In contrast, recurrent violence in the Timbuktu Region of Mali – where various rebel organization and government forces have repeatedly fought for control of the major cities in the area – has resulted in the displacement of half as many in the five years since the conflict began.

I attribute these differences in refugee flight patterns largely to the vast differences in the populations of the impacted areas. In one conflict, a large number of individuals live within close proximity to violent conflict and thus perceive the immediate risks of harm as high. In the other conflict, violence is taking place in remote areas with relatively few individuals seeing themselves as having a high short-term risk of victimization.\(^7\) As a result of these differences in the number of people exposed to violence, the number of refugees seen and expected from these conflicts varies considerably.

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\(^7\) Similar arguments can be made about a number of other conflicts. The violence in Syria could be an example with its focus on heavily populated (or at least heavily populated before the conflict) cities. It is also worth noting that the size disparity between Mali and Sierra Leone is also not likely to be the primary driver of this difference as similar examples of violence in heavily populated areas causing large refugee flows can also be seen in the likes of (the relatively large state of) Sudan where conflict in the populous southern region (an area now part of South Sudan) in the 1980s created large refugee flows.
These arguments and examples motivate my first hypothesis and typify how the theoretical model I employ can be used to generate expectations about refugee flight based on prospective decision making and evaluations of short-term risks of victimization. In a way, the previous hypothesis highlights the current model’s major departure from that of the previous literature by shifting the focus from intensity of combat to exposure to combat. However, the current model also produces additional unique expectations based on its allowance for long term expectations about loss. These additional dynamics are the subject of the next section and subsequent hypotheses.

2.2.2 Conflict Dynamics and Long-Term Risks of Victimization

Allowing for individuals to develop long term expectations about their risks of victimization means that the model presented here can incorporate conflict dynamics previously unaddressed in the existing literature. To do so, I also make additional arguments about citizens’ preferences over conflict actors and outcomes and the possibility of internal displacement instead of cross-border, refugee flight.

Existing research on civil conflicts makes clear that, in many cases, citizens are divided into “sides” due to explicit or implicit political allegiances, ethnic characteristics, or geographic location. Research on who conflict actors target for violence, how rebel groups create resource networks in conflict zones, and the flow of information to conflict actors from civilians, all demonstrate that civilians are often more than passive bystanders in times of conflict and instead have clear interests in supporting the success and, importantly, fearing for the failure of their preferred actor.
Accepting that a large number of individuals, in addition to the conflict actors themselves, have defined preferences over conflict events, it becomes clear that certain conflict events, particularly those that bode poorly for an individual's preferred conflict actor, should increase the individual’s expected long term risk of victimization and, as a result, lead to an increased number of refugees. In one sense, the relationship here is simple; the impending defeat of one conflict actor should lead to an increase in refugee outflows due to a fear of future loss (be it physical, economic, exclusion from politics etc.). This expectation flows directly from the current model’s incorporation of long term risks. However, such expectations are complicated by two interrelated considerations: the possibility of internal displacement and the way one theorizes “impending defeat.”

To see how important conflict events impact refugee flows consider how individual battles inform citizens’ expectations about the success of their preferred actor and their own prospects of future loss. Two aspects of individual engagements are important here: the location and the outcome.

Consider first, individual battles from the perspective of a government supporter. For these individuals, the major concern is that the government will be defeated and replaced with a rebel-controlled regime that pursues an agenda biased against former government supporters. As a result, violent combat that directly endangers the government’s control over the country will result in an increased perception of risk and larger refugee flows. Such risk-producing conflict events must have more than simply a large number of casualties and their overall impact cannot simply be restricted to a limited geographic area. This last consideration is critical. If a government supporter notices that the government is losing ground in a particular area, said
individual can simply displace internally and move to one of the many other areas within the
country where the government still retains control.

Due to the preceding considerations, for battles to significantly influence refugee flows
by forcing government supporters to flee, these battles must put the central government at a clear
risk of collapse. To generate this study’s third hypothesis, I argue that battles that take place
close to the capital – the seat of the central government – and result in the government losing
territory are likely to fulfil this requirement. Such battles signal that the rebel group has the
power to hurt the central government at its center and, potentially, overthrow the government’s
control of the state. As a result, government supporters are more likely to perceive refugee flight
as the only form of displacement that can guarantee their safety and thus high levels of refugee
flows are likely. Such capital-proximate violence is distinct from violence in distant areas that –
though it may harm the government – does not signify an impending failure of the central
government and does not require cross-border refugee flight to escape.

**Hypothesis 2:** Conflict events closer to the capital will produce more refugees than
similar events further from the capital.

Having detailed a way in which specific conflict dynamics would drive refugee flows
among those who support the government, I will now turn my attention to a similar discussion of
how battlefield events can drive rebel supporters to flee as refugees. In some ways, doing so is
simpler than deriving expectations for government supporters. The difference is straightforward
but has meaningful implications: since rebel groups often only control small segments of
territory, the feasibility of internal displacement is small and, as a result, even small defeats,
especially those resulting in the loss of territory, will produce refugee flows.
To see this dynamic in action, consider that many rebel groups are only able to exert control over a selection of towns or a small stretch of territory (as is often the case in secessionist movements). In these areas, the rebels may have a high degree of control over daily operations and may provide public goods, recruit combatants, and generally integrate into society at large (Kalyvas 2006; Heger & Jung 2017; Stewart 2017). When this is the case, it is often also true that the majority of citizens who remain in this territory support the rebel group’s political agenda and provide some specific resource to them (e.g. information, manpower, material, facilities) (Lyall et al. 2015; Kalyvas & Kocher 2007). While the vast majority of these individuals are unlikely to engage in direct combat when violence takes place, they do have a clear interest in the success of the rebel group and are indeed likely to suffer losses if the group is defeated.

As with the expectations for government supporters, to turn these ideas into a specific hypothesis we must again think about which conflict events would cause individuals to raise their expected risk of victimization and cause them to flee as a result. As noted above however, since internal displacement is unlikely for these individuals, any event which leads to a significant loss for the rebel group should create displacement. Furthermore, I argue that refugee flows should be greatest after engagements that see rebel groups lose territory to government forces. In these cases, refugee flight among rebel supporters is attractive not only because they may find themselves in government-seized territory but also because it may seem more and more like the rebel group is likely to be defeated and that they, as a rebel sympathizer, will be victimized by the government.

**Hypothesis 3:** Battles that result in territorial losses by the rebels will produce more refugees than other battles.
In addition to expecting an increase in refugee flows when rebel groups suffer strategic losses, the discussion above makes clear that, in many cases we can expect the refugees produced by these losses to be from a particular region or ethnic background – as such a characteristic is what connects them to the rebel group in the first place. As such, it is possible to generate an additional hypothesis building on Hypothesis 4 but linking the ethnic composition of the refugee flows to those of the rebel groups.\(^8\)

**Hypothesis 4:** The more territorial losses a rebel group suffers in a conflict, the more likely it is that the group will share ethnic ties to the refugee population.

This final hypothesis is perhaps the most distinct from much of the previous work on refugee flows and conflict. While ethnicity has previously been discussed in the literature, it has largely been seen as a factor capable of reducing the costs of flight by facilitating adjustment to the host country and by providing information about how to flee safely. In this study, I instead incorporate ethnicity as something that informs the long-term risks of victimization; since ethnic identification is what connects one to a rebel group and also what provides government forces a clear way to identify rebel-supporters in order to victimize them.

In sum, by retaining the focus on individual decision but adding considerations for allegiances to conflict actors, long and short term victimization risks, and a clear focus on exposure to violence, the current model produces significantly different expectations while relying on the same underlying logic. This means that refugee crises are still the result of a large number of individual decisions: individuals evaluate their risks and respond with refugee flight when those risks rise too high. The current study moves further in this individual-based direction

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\(^8\) While it is not currently possible to accurately discern the source region of any particular refugee population, it is sometimes possible to identify the ethnic composition of a refugee movement. Thus, I am only able to generate one additional, testable hypothesis from this insight.
by thinking clearly about how specific conflict events will lead specific individuals to see victimization as more likely.

2.3 Data and Analysis

In advancing a new theoretical approach to the causes of refugee flows, it is also necessary to use newly available data on conflict and refugees. While previous work has focused on aggregate, state-year measures, the current project utilizes a combination of spatially and temporally disaggregated datasets. This section proceeds in two parts with the analysis of the first two and last three hypotheses treated separately.

2.3.1 The Location of Violence, Prospective Risks, and Refugee Flight

In order to evaluate Hypotheses 1 & 2, data on the timing of refugee flows in relation to specific conflict events is necessary. For the independent variables relating to conflict events, recent advances in events data make tracking and analyzing the necessary events relatively straightforward. In this case, data from the ACLED project can be used as it provides, among other useful pieces of information, the geographic coordinates and the date of a variety of violent events (ACLED 2018). These data can be coupled with the PRIO-GRID dataset that divides the world into individual grid cells and provides specific geographic information about these cells including population and the location of capital cities – two key concepts for the above hypotheses (Tollefsen et al. 2012).
While the data sources discussed thus far are well-known and provide a great degree of freedom with respect to the level of aggregation used for analysis, most datasets on refugee flows do not have the same level of flexibility. In most cases, analyses of the causes of refugee flows (and most other topics related to refugees) have utilized the state-year data provided by the UNHCR. While these data are certainly useful in some situations – for instance, the spatial and temporal coverage is excellent and is useful when paired with similarly expansive datasets, as I shall demonstrate later in this piece – it is difficult to pair such data with the highly disaggregated data necessary to evaluate the preceding hypotheses. Fortunately, as part of the UNHCR’s efforts to collect, analyze, and disseminate more data on refugee crises, a small number of temporally disaggregated datasets of refugee flows have recently become available.

As the UNHCR monitors ongoing crises in Mali, Burundi, and the Central African Republic it has provided monthly data on refugee flows from these countries at regular intervals. While these data still do not indicate the specific local origin of any refugee movements, they do provide insight into the size and timing of the movement and thus will be useful as the source of the dependent variable for the first analysis.

Combining the data sources above using R and its GIS tools leads to a dataset at the state-month level recording the number of refugees fleeing the country in a given month as well as information about the number of people impacted by violent events and the distance from the capital of violence in a given month. To estimate the number of individuals impacted by an event I construct a variable called *Affected Population* using the logged sum of the populations of all grid cells impacted by violence in a given month and expect that this measure will be positively associated with the dependent variable. To estimate the violence-capital distance, I use the network with a resolution of 0.5 x 0.5 decimal degrees, covering all terrestrial areas of the world (Tollefsen et al. 2012).
minimum distance from the capital of violent events in a given month (*Minimum Capital Distance*) and expect that this measure will be negatively associated with refugee flows (i.e. events further from the capital will produce fewer refugee flows). Since I am interested in the impact of violent events on subsequent refugee flows, I lag all of my independent variables one month to avoid counting refugee flows produced before the event in question.

Given that the dependent variable to be analyzed is a count of the number of refugees produced in a given state-month and the fact that several months have zero refugee flows, I use hurdle negative binomial regression models in my analyses. To ensure that conflict intensity is not actually driving the exposure effects emphasized in my hypotheses I also control for the total number of *Battle Deaths* each month. Additionally, I control for the time since the conflict episode began (*Duration*) and the total refugee population (*Total Refugees*) to account for the idea that refugee flows change over time and as a function of previous events (e.g. how many individuals have already fled). Finally, I also introduce fixed effects for each country to account for other country or conflict specific attributes.

### 2.3.2 Results for Hypotheses 1 & 2

Table 1 displays the results of a hurdle negative binomial model of refugee flows as a function of my primary independent variables (*Affected Population* and *Minimum Capital Distance*). The results of this model are mixed with respect to supporting Hypotheses 1 & 2. First, note how, in line with Hypothesis 1, the *Affected Population* variable is positive and

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11 The operationalization of this variable is complicated by the fact that events taking place close to the capital may signify an imminent threat to the government in some situations but not others. To address this issue, I estimate models using three different operationalizations of this concept. I discuss these different measures in more detail later but will note ahead of time that the statistical significance of my findings does not change regardless of the measure used.
statistically significant. This indicates that violent events taking place in heavily populated areas will produce more refugees than events in less populated areas.

This result is consistent with my argument that individuals choose refugee flight as a result of viewing proximate violence as creating a short-term threat of victimization. Note also how this result holds after controlling for the number of Battle Deaths caused by these violent events – a further indication that prospective evaluation about future victimization are more important than retrospective assessments of past victimization or concerns about the general intensity of a conflict.

While the results in Table 1 are supportive of Hypothesis 1, they do not provide support for Hypothesis 2 concerning the role of violence near capital cities. I have argued that violence near capital cities should produce more refugees than violence occurring far from the capital due to government supporters raising their expectations that the government will soon be defeated by the rebel groups and, as a result, they will be victimized under the new rebel-controlled regime. The results in Table 1 do not support this argument as the Minimum Capital Distance variable, is, contrary to my expectations, both positive and not statistically significant. These results indicate that capital proximity as measured here has little to no impact on refugee flows.

These results are concerning as they cast doubt on my argument that long-term expectations of victimization are relevant in the refugee decision. Further, different operationalizations of capital-proximate violence using the average distance of all events and the
minimum distance of events that result in government losses do not change the substantive conclusions suggested by this model.\textsuperscript{12}

\begin{table}[h]
\centering
\begin{tabular}{lcc}
\hline
\textbf{Table 1: The Location of Conflict and Refugee Flight} \\
\hline
 & \multicolumn{2}{c}{
\begin{tabular}{c}
\textbf{Coefficient} \\
\textbf{Standard Error}
\end{tabular}} \\
\hline
Affected Population & 0.464\textsuperscript{**} & (0.147) \\
Minimum Capital Distance & 0.316 & (0.176) \\
Battle Deaths & -0.291\textsuperscript{*} & (0.129) \\
Total Refugees & -0.498\textsuperscript{*} & (0.214) \\
Duration & -0.064\textsuperscript{**} & (0.013) \\
Central African Republic & 2.318\textsuperscript{**} & (0.359) \\
Burundi & 0.313 & (0.610) \\
Constant & 8.946 & (3.378) \\
\hline
\end{tabular}
\end{tabular}
\caption{The Location of Conflict and Refugee Flight}
\end{table}

Observations & 73 \\
Log Likelihood & -584.7 \\
\hline
\end{table}

Note: Standard errors in parentheses
*p<0.05; **p<0.01
All continuous variables lagged and logged
Zero component omitted for presentation

\textsuperscript{12} Indeed, these alternative specifications only cast further doubt on the role of capital-proximate violence as a cause of refugee flight. None of the three specifications result in statistically significant results and the sign of the estimated coefficients vary – a further indication that no effect exists or that the impact of these events can be both positive and negative depending on other circumstances.
This consistency in the results leads me to believe that aspects of Hypothesis 2 are incorrectly specified. Indeed, in setting out Hypothesis 2, I have assumed that most individuals support the government in times of civil conflict. If this is assumption is correct then the imminent failure of the government will indeed naturally lead to a marked increase in the number of refugees overall. However, this may not be a valid assumption as it is entirely possible that many people are either rebel supporters or have tried to stay relatively un-aligned during the conflict. This possibility implies that Hypothesis 2 should be reformulated to focus on the composition of the refugee flows and not on their size; in the same way that Hypothesis 4 emphasizes how rebel losses can cause citizens co-ethnic with rebel groups to flee. While this is an interesting adjustment to my hypothesis, I am currently unable to link the ethnicities of the refugee populations and governments involved in this sample as the ethnicity data used for Hypothesis 4 is unavailable during the time period examined in this data.

Even with these concerns however, it is still not possible to completely rule out the idea that poor government performance in a conflict can result in refugee flight. Future research using other metrics of government performance or using alternative methods to assess civilian identification with the government (e.g. utilizing better ethnicity data, surveys of support for the government, or proxy variables such as the quantity and quality of goods provision in an area) is required in order to better assess this relationship.

Altogether, the preceding results thus provide mixed evidence for my theoretical arguments. While exposure to violence does seem to impact refugee flight, the impact of capital-proximate violence on long-term risk evaluations is unclear. However, before turning to a complete evaluation of my theoretical argument, I must still evaluate Hypotheses 3 & 4.

2.3.3 Battlefield Events, Citizen Allegiances, and Refugee Flight
Hypotheses 3 & 4 deal explicitly with the idea that long-term expectations of the prospects of victimization can be a significant cause of refugee flight. The information needed to evaluate these hypotheses can be drawn from the ACLED data used previously as well as two other datasets on refugees. Since the required refugee data are at the yearly level and the ACLED data is restricted to the African continent, combining the two results in a state-year dataset focused on African conflicts. I evaluate these hypotheses in two parts.

First, I use additional data from the ACLED project to determine the number of times rebel groups involved in a conflict have suffered a loss of territory as the result of a battle. I combine this data with UNHCR refugee data to determine how many refugees fled the country in question in a given year. As before, I use a hurdle negative binomial model to account for the large number of years in which a country produced zero refugees and use the natural logarithm of the number of territorial losses as my primary independent variable.

In these models, I control for the logged number of deaths from combat violence (Battle Deaths), a measure of democracy (Polity) to account for the general political stability of the country, and a logged measure of the total population of the country (Population). These controls are designed to account for alternative causes of refugee flight that may also correlate with how successful rebel groups are at competing against the central government in a conflict. For instance, a large number of battle deaths could potentially be driving both rebel defeats (through loss of manpower) and refugee flows (through concerns over collateral victimization). In a similar way populous countries may imply both a strong, well-manned military as well as a large number of individuals susceptible to internal displacement. Finally, as an additional control, I also include a measure of the refugee flows in the previous year (Refugee Flows \( t-1 \)) to account for the possibility that historic flows influence current ones.
Additionally, I also estimate the subsequent models with a measure of the *Geographic Scope* of the conflict (as a percent of the total land area of a country) as a further means to evaluate Hypothesis 1 and demonstrate the importance of accounting for exposure to violence and how doing so can greatly alter the perceived impact of conflict intensity (measured by battle deaths).\(^\text{13}\)

Beyond evaluating how rebel losses lead to an increased number of refugees, I also evaluate whether rebel losses lead citizens co-ethnic with the rebel group to flee. I use the Ethnicity of Refugees (ER) dataset to determine the major ethnic groups fleeing from a country in a given year and combine this information with data from the Ethnic Power Relations (EPR) and Armed Conflict Data (ACD) to link the ethnicity of the refugees to the ethnic affiliations of the rebel groups in a given conflict year (Gleditsch et al. 2002, Vogt et al. 2015).

For the dependent variable, I create a binary indicator of whether any of the three largest refugee populations fleeing from a conflict in a given year are co-ethnic with the rebel groups involved. For the independent variable, I continue to use the logged number of territorial losses suffered by groups in a conflict year. I also include the same set of controls as with the previous count models. While models of refugee flow composition are relatively new and no “standard set” of controls exists, these specific variables should again prove useful as they relate either to the prospects of success for rebel groups (e.g. *Polity* and *Battle Deaths*) or to things that would make shared ethnic ties more or less likely (e.g. *Refugee Flows_{t-1}* and *Population* respectively). Since the dependent variable for this model is a binary indicator, I use logistic regression to

\[^{13}\text{Despite the expectations set forth in Hypothesis 1, it is unlikely that this variable will be statistically significant in the count regressions below due to the aggregate nature of these models. In some sense, this model serves as another evaluation of Hypothesis 1 in a more difficult setting. Regardless of the resulting significance however, I do expect that including a measure of the scope of the conflict will impact the statistical and substantive significance of the battle deaths variable due to the way battle deaths can, in the absence of other measures of exposure, act as a proxy for the number of individuals exposed to violence.}\]
evaluate how battlefield losses impact the probability that a refugee population is co-ethnic with a rebel group.

### 2.3.4 Results for Hypotheses 3 & 4

Table 2 displays the results from a series of regressions aimed at evaluating Hypotheses 3 & 4. The first two columns estimate the impact of Rebel Losses on the size of refugee flows and, as expected, show that an increase in the number of rebel losses is associated with an increase in the size of refugee flows. This finding supports Hypothesis 3 and the idea that rebel losses can produce larger refugee flows as a result of rebel supporters seeing their prospects of victory as low and their long-term risk of victimization by a victorious central government as high.

Notably, the measure of the number of rebel losses is statistically significant and positive even after controlling for the intensity of the conflict and for the level of exposure to violence (as estimated by the measure of the geographic scope of the conflict). This finding is in line with my argument that specific battlefield events can impact the size of refugee flows beyond that expected from exposure to violence itself and points to a major shortcoming of previous models which focus solely on immediate risks and retrospective evaluations of intensity. Also of note is how Geographic Scope can be seen to play a statistically and substantively significant role in the size of refugee flows. This finding offers further support to Hypothesis 1 that exposure to violence increases perceptions of risk and subsequent refugee flight.

Table 2 also displays, in the second column, results from a logistic model estimating the probability that a refugee population is co-ethnic to a rebel organization as a function of the number of territorial losses suffered by groups in the conflict. In line with Hypothesis 4, I find that more territorial losses are associated with an increase in the probability that a rebel group
shares ethnic ties to a refugee population. This finding lends support to the idea that specific conflict dynamics can cause specific individuals to flee as refugees. In this case, we see support for the idea that when rebel groups are doing poorly in the conflict, individuals who support the group are more likely to flee as refugees.

<table>
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<tr>
<th>Table 2: Battlefield Losses and Refugee Flows</th>
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<td>Flow Size (HNB)</td>
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<tr>
<td>Rebel Losses $\log$</td>
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<td>Battle Deaths $\log$</td>
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<td>Geographic Scope</td>
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<td>Population $\log$</td>
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<td>Refugee Flow$_{t-1}$</td>
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Note: *p<0.05; **p<0.01
Standard Errors in Parentheses
HNB = Hurdle Negative Binomial
Zero components of hurdle models omitted for presentation
This result is consistent with the theoretical model I present above where these individuals are assessing their long-term risks of victimization using information gleaned from observing the successes and failures of the group they support. In fact, the results of this model indicate that none of the other covariates help explain variation in co-ethnicity between rebel and refugee groups. This too is consistent with the theoretical framework I present in that none of the other covariates offer specific information about which group – rebel or government supporters – should be fleeing from the conflict.

2.4 Discussion and Conclusion

In total, the results presented here provide support for Hypotheses 1, 3 & 4 and for the prospective risk framework I present. From the regression models, we see substantial evidence that prospective evaluations about future risks of victimization can play a significant role in refugee flight. Within this framework, citizens pay close attention to conflict dynamics and make decisions based on events that imply future consequences for them directly. These consequences can be in the short-term and determined by exposure to violence or can be estimates of long-term risks determined by conflict events which bode poorly for the citizen’s favored conflict actor.

These findings are a clear departure from previous work and demonstrate that citizens in conflict zones pay attention to more than the presence or intensity of conflict. Instead of focusing exclusively on retrospective evaluations of the general severity of the conflict, the model I have developed and evaluated in this project focuses on factors influencing prospective evaluations of specific risks of victimization. Doing so has allowed me to examine new and important determinants of refugee flight and to examine both the size and composition of the refugee populations.
While these findings are important, they are just the beginning of a series of possible projects focused in evaluating the role of particular prospective risks. First, as noted above, the results pertaining to Hypothesis 2 indicate a need to reexamine the way capital violence, refugee flows, and flight among government supporters are considered in my model. While violence near the capital should indeed have some impact on risks perceived by government supports, it will be necessary to better isolate – like Hypothesis 4 – who is fleeing and what specific negative events they are fleeing from.

In addition, it may also be fruitful to examine how one-sided violence targeted directly at civilians influences refugee flows. This would likely be another place where the specific prospective model diverges from the general retrospective model. In particular, while one may expect one-sided violence to cause refugee flight in all cases, it is more likely that refugee flight is only caused by one-sided violence that is targeted directly at a large, identifiable group and/or inescapable via internal displacement. Finding, for instance, that only violence perpetrated by the central government (with its inescapable force-projection capabilities) at a clearly identifiable ethnic group caused large refugee flows would be an interesting result consistent with the model I present.

Finally, the model presented here could also be extended to generate hypotheses directly concerning internally displaced persons (IDPs). While I have incorporated internal displacement into my theoretical considerations, I have not generated explicit hypotheses about variation in IDP populations. Though data on internal displacement is less readily available than similar data on refugee flows, this is a largely unexplored area of research and one where the specific prospective model could be quite useful in specifying the situations in which internal displacement is more attractive than refugee flight.
Altogether, the results and discussion presented here have offered evidence that a model of refugee flight based on prospective evaluations is both empirically supported and theoretically useful. I argue that continued research utilizing this framework has the potential to offer many more insights than those I have mentioned above. By focusing on prospective risk assessments and specific conflict dynamics we can determine not only how many individuals flee as refugees but also which specific individuals flee. This information could prove invaluable in planning for the arrival of large refugee populations or ensuring that populations co-ethnic with militant organizations do not become violent themselves. These long-term, policy-oriented aspects of this research are exciting and will hopefully drive more scholars to approach the refugee-violence nexus with an eye towards helping to create refugee policies that ensure a safe environment for both the refugees and the citizens around them.
Chapter 3: Refugee Violence and the Performance of Rebel Organizations

A large body of evidence suggests a link between the spread of conflicts and refugee flows. Hosting refugees has been linked to the onset of civil war, increased incidences of terrorism, and even interstate disputes (Lischer 2006; Salehyan & Gleditsch 2006; Milton et al. 2013). However, previous studies are unclear about how these refugee populations can actively create violence in host countries (see, for example, the various explanations offered by Salehyan & Gleditsch as well as the drastically differing models of Krcmaric 2014 and Onoma 2013). After all, the conventional picture of a refugee population is one composed of dispossessed individuals with few economic or social resources that have been relegated to camps in the asylum country (Jamal 2000, UNHCR 2016). The question then becomes: How then can individuals with so few resources ultimately have the capability to facilitate armed violence?

I propose that the answer to this question is found in linkages between rebel groups in origin countries and refugees in asylum countries. Specifically, I develop a model of refugee violence focused on how rebel groups can use the security and protection offered to refugee populations to regroup and re-initiate conflict after suffering strategic defeats at the hands of the origin country’s government. By detailing the specific contexts in which refugee populations can be expected to be associated with violence and by explicitly focusing on the connections between rebel groups and refugees, this new model provides several key insights and, most importantly, begins to explain why some refugee populations are associated with violence while others are not.

Before detailing and evaluating my own argument, I first provide an overview of existing research on refugee-related violence. This review motivates the central puzzle of the current
paper – how can refugees produce violence despite being marginalized and deprived of resources – and identifies key areas where existing explanations and studies are deficient. Subsequent sections outline my own model of how rebel groups can use refugee populations and the protection offered by refugee status to recuperate from battlefield defeats and re-initiate conflicts. To evaluate the expectations of this model, I conduct a statistical analysis of data on conflicts in Africa. However, while the results from this empirical model are suggestive, they provide only limited evidence for the utility of my model. As a result, I also discuss a number of illustrative examples from conflicts that feature significant refugee populations in an attempt to illustrate the hypothesized relationships and determine areas where the model can be improved. I close with a discussion of the implications of my findings and the importance of understanding rebel-refugee connections for individual states and international organizations such as the United Nations High Commissioner for Refugees.

3.1 Do Refugees Create Violence?

Debates over whether migrant populations can cause negative outcomes in the countries they settle in have pervaded political discourse for many years. Generally speaking, much of this discussion has focused on the economic consequences of an influx of foreigners into a country with opinions divided over how migrants influence, for example, prices and unemployment (see Zolberg 2006 for a historical account of perspectives on immigration in the US context and Borjas 1999 and Powell 2015 for multiple perspective on the economic consequences of immigration). This focus on economic issues makes sense considering that many (if not most) voluntary migrants are pulled towards a specific country in their quest for economic opportunity and engagement with their newfound local economy (Greenwood 2005). Refugees, on the other hand, are more often pushed towards an asylum county by a violent conflict in their home
country (Weiner 1996, Davenport et al. 2000, but see Moore & Shellman 2006b for an analysis of the idea that refugees can be “pulled” to host countries). In light of this observed pre-existing connection to violence, debates about the consequences of hosting refugees have come to include questions about whether refugee populations bring violence along with them when they enter host countries.

Research in political science, history, anthropology, and sociology indicates that hosting refugee populations is associated with various forms of violence in asylum countries including the onset of civil war, increased incidences of terrorism, and even interstate disputes (Zolberg et al. 1989; Muggah 2006; Salehyan 2008; Lebson 2013). Much of this literature has developed out of work highlighting the spatial clustering of civil violence and identifies refugee flows as a potential pathway through which violence can be “transmitted” from one country to another (Wood 1994; Weiner 1996; Salehyan & Gleditsch 2006).

Unfortunately, while many scholars agree on a connection between refugees and host-country violence, these models vary greatly in the exact mechanism by which refugees are associated with violence.¹ In some models, refugees are still regarded as the victims of violence and are said to be targeted by militants in host countries that resent the economic burden refugees impose on their state (Onoma 2013). In other models, refugees are said to, perhaps inadvertently, upset the “ethnic balance” of the host state and exacerbate existing ethnic tensions to violent levels (Krcmaric 2014 and see Bosker & de Ree 2014 and Cederman et al. 2013 for more on the broader idea of ethnicity and the spread of conflict). Finally, other models argue that refugee populations can directly and purposefully engage in or facilitate violence by supporting rebel

¹ The focus on the negative outcomes refugee populations create is not entirely ubiquitous in the extant social science literature. See Jacobsen (2002) for a discussion of the potential benefits refugees can bring to host countries.
groups in the origin country with men and material, conducting terrorist attacks, or rioting against the host government or international aid organizations (Lischer 2006; Lebson 2013 Choi & Salehyan 2013).

Models built on the last mechanism – where refugees are active participants in the resulting violence – have perhaps become the most common in recent years and have been highlighted repeatedly in political discourse on the risks of hosting refugee populations (for a recent example of the discussion of refugees and refugee policy in the media see Frum 2015, for insight into longstanding debates over state responses to refugee crises see Weiner & Munz 1997, Khosravi 2009, Posen 1996). Drawing on the literature on cycles of violence and deprivation, these models view refugees as facilitating or perpetrating violence as a result of the victimization that drove them to flee and/or the poor conditions they experience after displacing (Widom 1989; Moore & Shellman 2004; Moore & Shellman 2006a). Much of this work offers as the canonical example the case of the Great Lakes Refugee Crisis in 1994 wherein refugees fleeing from the Rwandan Genocide incite violence in host countries, particularly in Zaire (now Democratic Republic of the Congo). In this particular case, the violence of the Rwandan Genocide and the squalid refugee camp conditions in Zaire serve as the impetuses for refugee violence that ultimately produces more genocide, riots in Zaire, cross-border attacks on Rwandan forces, and, ultimately, the First Congo War in 1996.

While the preceding narrative that “violence begets violence” may have intuitive appeal, it constitutes a poor theoretical explanation of refugee violence in host countries given that it fails to explain how refugee groups, in their dispossessed and victimized state, are actually capable of carrying out the processes necessary to incite violence. Put another way, most existing models of refugee violence, while effectively providing an account of the “willingness” of
refugees to use violence, fail to explain the contexts in which refugees have the necessary “capability” to create violence.²

A model that carefully considers both of these issues is important to the study of refugee violence not simply because it more realistically models the mechanism of violence but because it provides insight into the specific contexts in which refugee violence can be expected. Indeed, a central critique of many existing works is that they fail to account for the clear variation in the tendency of refugees to produce violence. For example, while refugee flows following the Rwandan Genocide and the 1979 Vietnamese invasion of Kampuchea (now Cambodia) had clear implications for the security of surrounding countries, other major refugee crises, including the recent influx of refugees into Europe, have failed to result in significant regional destabilization and/or the spread of civil conflict abroad.

This shortcoming can be explained in part by the “unconditional” nature of the existing models and the lack of specificity over when exactly refugee populations can be expect to be associated with violence. To address this issue, and explain how refugees can be associated with violence despite their dispossessed and marginalized position, I introduce a theoretical model focused on the connections between refugees and rebel groups and the incentives rebel groups have to use refugee populations as a cover to conduct their military operations.

3.2 Rebel Performance and Refugee Violence

² While I use the specific terms “willingness” and “capability,” similar terminology using, for example, “motive and opportunity” could also be used. The general idea is the same and is meant to draw attention to the fact that all actions, and especially those that are resource-intensive (like perpetrating widespread civil violence), take both a desire to undertake the action and the ability to do so. Refugee violence is, of course, no different and the desire to coherently model each of these two pre-requisites is a primary motivation of this paper.
The first step to understanding how refugee flows can produce violence is to realize that refugees are not always entirely divorced from the conflicts that produce them or the militant actors that fight around them. In the previous chapter, I set forth a model of the causes of refugee flows that emphasizes the connections between refugees and conflict actors. In this section, I build on this model by examining how these connections can allow rebel groups to conduct violent activities outside of the origin conflict.

Perhaps the most novel feature of the model I present is that, in explaining how refugee populations are associated with violence, I focus on the incentives faced by rebel groups and how they mobilize refugee populations. As a result, the following model is perhaps focused more on rebel groups than on refugee populations. Indeed, I argue that this balance of attention is a necessary consequence of attempting to model both the willingness of refugees to use violence and their ability to do so – as the later comes directly from their connections to rebel groups.

To understand the origins of refugee violence, I begin by considering rebel groups in their competition with the government of the origin country. While there is little doubt that civil conflicts are initiated for a wide variety of reasons, the central similarity between all conflicts is that rebels groups are trying to achieve some policy change from the government and are attempting to use military force to coercively produce these changes. Regardless of whether mediated settlements or the complete defeat of the central government is being demanded, the central concern of rebel groups is their military capability and ability to do harm to, and thus conceivably force change from, the government.

For these rebel fighters, maintaining successful military operations is of paramount importance and, when given the chance, these actors can be expected to pursue a variety of means to continue to fight effectively. Violence against civilians, terrorism, establishing external
bases, and forcing external intervention are just a few of the unconventional ways that rebel groups are known to pursue victory against government forces. In many of these instances, rebel groups are often taking advantage of some existing political situation and using it to advantage them in their struggle with the government. Research has shown, for example, that the presence of civilians with easily identified political allegiances is conducive to violence against civilians while neighboring states rivalrous to the rebel group’s opponent facilitate external bases or intervention. The preceding examples should make clear that a model of rebel groups and refugees must explain how refugee militarization aids rebel groups and when these groups can be expected to support such militarization.

The answer to the first question is largely found in the nature of refugee camps and refugee populations. Using the model detailed in the previous chapter, one can assume that, at specific times during a conflict, refugee populations hosted abroad are composed of individuals with some allegiance to rebel actors. Having left their home country for shelter abroad, these individuals are often met with a level of safety and humanitarian assistance that was unavailable in the conflict country. However, the relative safety offered by host countries and UNHCR assistance is likely considerably less appealing than the policy changes and new order that would be brought about by their rebel group emerging victorious from the origin conflict.

For these individuals, the protection of refugee status is not always an appealing end to the political struggle they support but could instead provide a means to further their political agenda. Under the present model, the willingness of refugees to engage in violence comes from

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3 More specifically, the analysis in Chapter 1 demonstrates, for example, that battles resulting in rebel defeats produce more refugees than would be expected given the number of individuals killed. My model argues that this is due to rebel supporters fleeing in fear that their preferred group (the rebels) is performing poorly and is unlikely to win the war — a result they find undesirable for a number of reasons including for fear that they, as former rebel supporters, will be killed by the victorious government.
their connection to rebel groups and their desire to see such group succeed. When refugees are connected to rebels in this way, they are likely to be willing to engage in a number of activities aimed at supporting the rebel group. These populations and the safety provided by camps could facilitate the transport of arms, provide recruiting grounds, serve as bases to launch attacks on opposing forces abroad and at home, or can simply be used as a refuge for combatants.

While, individually, these forms of militarization may not amount to a full-scale civil war in the refugee hosting country, there are reasons to suspect that, in many cases, these events do result in substantial levels of violence. For example, activities like forced recruitment and the theft of aid resources can result in significant violence that may be met with resistance from the local government, aid organizations, or native civilians in the area. Indeed, some of these activities run a serious risk of drawing the host state into direct conflict with the rebel group and the associated refugee population. This risk is likely especially large in the face of cross-border attacks from the militant groups. In these situations, even though the initial violence is directed outward, the host state may be compelled to respond militarily in order to avoid a larger conflict with the neighboring state. Additionally, militarized populations may find some level of violence directly aimed at the host government as productive if it furthers the cause of the rebel organization they support. Examples might include assassinating leaders who oppose the group, fighting to expel government forces from refugee settlements, and wider efforts to mobilize co-ethnics or other similarly marginalized native groups to fight against the host government.

The preceding discussion should make clear that a refugee population’s discontent and willingness to fight can be used by rebel groups to further their (shared) goals. However, to fully understand this process, I now examine exactly when rebel groups would be most prone to undertake this type of behavior. After all, mobilizing refugee populations takes resources and
runs the risk of causing outside actors – including the host state – to enter the conflict against the rebel group. As a result, it is important to think about exactly when rebel groups might face strong incentives to spend resources mobilizing refugee populations.

In line with previous work on the behavior of rebel groups, the model I present focuses on how a group’s performance in conflict influences its desire to pursue unconventional military strategies. I argue that rebel groups are more likely to pursue refugee militarization when they find themselves performing poorly in the conflict. During these times, rebel groups, having attempted conventional combat and faced defeat, will be prone to pursue a variety of methods aimed at increasing their fighting capability. As noted before, refugee militarization provides this desired increase by facilitating recruitment, combat, and recovery out of the enemy’s reach.

Two specific components of this model are worth discussion in greater detail. First, I argue that the costs of mobilizing and militarizing refugee populations are non-negligible. The costs of transporting weapons, conducting recruitment, and diverting the attention of rebel leaders to these refugee populations can be quite high and are likely to be avoided by groups who are performing well against the government. For these groups, no extra support is necessary and paying the costs of mobilization simply takes resources away from conventional combat operations. This consideration is key to understanding how poor rebel performance motivates paying these costs. For those groups that are doing somewhat poorly in their conflict, paying the costs of mobilization may be the only way to regain a footing against the government and continue to fight.

However, the costly nature of mobilization also implies a second key feature: that groups performing especially poorly are likely to be unable to pay the costs of mobilization and, as a result, refugee violence is unlikely. Considered in this way, it is clear that the military
performance of groups indicates more than simply their chances of emerging victorious from a conflict; performance also acts as a proxy for the amount of resources a group has available. This is because groups suffering strategic defeats are undoubtedly simultaneously suffering the loss of combatants, territory, and material. For these groups, no expendable resources exist with which to facilitate refugee militarization. Such groups have, for example, lost too much territory or had too many of their resources destroyed to be able to effectively facilitate refugee rebellion. Groups in this situation are simply focused on staying alive, perhaps while doing as much harm to the government as possible, and simply do not have the resources to reach into neighboring states to mobilize refugee populations.

The preceding discussion thus points to a non-monotonic relationship between the performance of rebel groups and the militarization of refugees. Specifically, while intermediate levels of poor performance motivate rebels to mobilize refugees, an excessive number of defeats will limit the group’s ability to do so. As a result, refugee mobilization will be more likely at low levels of defeat compared to high levels. This is the core result from my model and motivates my primary hypothesis:

**Hypothesis 1:** The relationship between group performance and refugee militarization is non-linear such that low levels of battlefield defeats increase the risk of militarization whereas high levels reduce the risk.

This hypothesis captures the non-linear theoretical relationship between the performance of rebels and refugee militarization and can be evaluated empirically. Unfortunately however,
perfect measures of rebel performance and refugee militarization are both exceptionally difficult to come by. To help remedy this issue, in the following sections, I evaluate my argument using a combination of statistical models and case studies. I first examine these relationships with a statistical model using a count of the number of battles rebel groups lose and the presence of civil conflict in refugee hosting countries as measures of performance and militarization respectively. Next, I use a series of case studies to explore whether refugee mobilization follows from rebel groups suffering battlefield defeats.

3.3 Statistical Evidence Linking Refugees, Rebels, and Civil Conflicts

As indicated above, precisely evaluating the complex relationship between rebel groups, refugee populations, and civil conflicts is a difficult task. Properly identifying and measuring refugee and rebel populations is often difficult by itself and, when coupled with difficulties in identifying militarized refugee populations, makes for a substantial empirical challenge. However, as the literature review above makes clear, through various approximate measures, case studies, and surveys, a large body of scholarship has developed in this area. In line with this previous literature, this section approaches evaluating the model presented in this paper using a combination of statistical modeling and case studies. By combining both of these approaches, I am able to demonstrate the wide applicability of my arguments (from the statistical model) and the accuracy of the mechanism I propose linking refugees to conflict in host countries (from the case studies).

3.3.1 Data

The central logic of the above model is that battlefield losses by rebel groups lead to refugee militarization abroad. While this logic is straightforward, a number of concerns are
present when attempting to model this process empirically. I begin my analysis using a sample of states in Africa with nearby civil conflicts. Refugee militarization in these states is the outcome of interest in my models. However, since no perfect measure of refugee militarization exists, I turn to a direct measure of the presence of civil wars in these countries as drawn from the UCDP dataset on armed conflicts.

As noted before, while refugee militarization does not necessarily result in the onset of civil war in a state there are reasons to suspect, at the very least, that the probability of civil conflict onset is higher under conditions conducive to refugee militarization. Assassinations, forced recruitment, resource appropriation, and conflicts with natives in the region could all produce direct conflict between the militarized groups and the central government. In some of these cases, the resulting conflict is largely unintentional – as might result from the government stepping in to stop resource theft. In others cases, such as when the militant groups attempt to mobilize co-ethnics, direct conflict with the central government is part of the overall goal. Both of these patterns of conflict onset are consistent with the model I posit in that both are seen as resulting from attempts by the (poorly performing) militant group to bolster its capabilities. Whether this is done through risky resource appropriation and forced recruitment or through mobilization of a broader fighting force and the assassination of opposing leaders is of little consequence. What is important empirically is that the availability and attractiveness of these strategies will result in an increased probability of observing civil conflict when a rebel group is suffering battlefield defeats, even if such defeats do not deterministically produce this type of civil conflict.

To capture instances of battlefield losses by rebel groups, I turn to the ACLED data on African conflicts and determine how many times rebel groups in surrounding states have
suffered a loss of territory as the result of a battle with the government. I use the count of all of these events in a single year and the square of this count as the independent variables in my model.

Modelling the successes of rebel groups in this way is one of the more challenging aspects of this research design for two reasons. First, this procedure makes it impossible to differentiate between one rebel organization suffering multiple defeats and multiple organizations suffering single defeats. For conflicts that have a large number of rebel groups in conflict with the government, this could lead to an overestimation of the degree of failure being experienced by rebel groups and their supporters. Second, because this measure is being used to proxy for both the performance of rebel groups and the presence of rebel-supporting refugee populations, it is entirely possible that this measure improperly estimates the core mechanism specified by the theoretical model. Specifically, it is possible that a high number of rebel defeats may not correspond to a rebel-supporting refugee population in the country under examination.

These concerns are real, and represent real limitation on the reliability of the results obtained from these analyses. However, there are reasons to believe that these concerns are not great enough to completely invalidate the subsequent analysis. For one, the fact that this measure aggregates defeats from various groups may not be much of a concern as doing so still adequately captures the performance of the government in relation to rebel groups. If government performance overall is considered to be negatively related to the performance of all rebel groups, then we can still expect these defeats to impact the behavior of rebel groups and their supporters. Additionally, further evidence for the validity of this modelling strategy can be drawn from my previous work indicating that rebel defeats do indeed create substantial refugee flows. While I am still unable to determine exactly who these individuals are, I can have
confidence that the events I am measuring do indeed produce refugees and, using the logic of my previous work, do so by causing rebel supporters to fear for the defeat of their group.

Aside from the key variables to be used in the statistical model, I also incorporate a number of control variables aimed at isolating the effect of rebel defeats on civil violence in host countries. In essence, the models I run are models of the initiation of civil conflict in a country with an emphasis on the spillover of conflict from one state to the next. As a result, I use control variables focused in two specific areas. First, I control for factors making a host state more or less susceptible to being influenced by nearby conflict including the country’s GDP, population, and the state’s Polity Score and its square. Second, I also control for factors making the conflict more likely to impact, and thus potentially spread to, host states including the total number of battle deaths and a measure of the geographic scope of the conflict. In addition, I also incorporate cubic polynomials of time at risk to account for duration dependence.

Finally, I also include a measure of the total number of refugees in the host country. This variable is critical to assessing my argument, since its effects on the likelihood of civil conflict in host states is widely noted. I employ this measure in two ways. First, I model the onset of civil conflict in host states using all of the control variables without my key independent variables. If my statistical model is consistent with existing research then the results should indicate a positive and significant effect of refugee populations on the risk of civil conflict. After running this model, I incorporate my measure of rebel success with the expectation that the refugee population variable will no longer be significant.

The reasoning behind this argument is that the above model makes no explicit reference to the size of the refugee population and instead focuses on its composition. While it may be true that larger refugee populations are more likely to contain refugees supporting rebel groups, there
is nothing in the above model implying that a refugee population must be large for it to become associated with violence. I argue that, in previous research, *refugee population* has acted as a proxy for *refugee composition*, a concept I capture more effectively using data on the performance of rebel groups.

The empirical approach detailed above produces a dataset covering African states from 1997 to 2008. Since my dependent variable is a measure of the presence of civil conflict in host states, I employ two Bayesian logistic regressions to evaluate my hypothesis. I perform my analysis in R using the *rstanarm* package and use the default, uninformative priors used by the package.

### 3.3.2 Analysis

Table 1 displays the results of the two models. Column A displays the results of the “Refugees Only” model and indicates that, consistent with previous research, large refugee populations are significantly associated with the onset of civil conflict in host countries. The coefficient for the refugee population variable is positive with a 95% credible interval that does not include 0. In addition, the coefficient for neighboring civil war is also significant according to these criteria. These results are not surprising given the previous literature and both are consistent with my argument that refugees can provide a pathway for the spread of civil conflict from one state to another. However, I believe this model and the associated results do not adequately capture the process through which conflict spreads through refugees.

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4 Initial frequentist regression models produced results consistent with those presented here yet exhibited signs of complete separation when using the variables I have identified. A common suggestion to alleviate concerns over separation is to employ Bayesian models (see Gelman et al. 2008 and Rainey 2016 for discussion of the issue, proposed solutions, and remaining concerns). Beyond comparing the results of the frequentist and Bayesian models, I also examine diagnostics for my primary models to ensure that they are estimating properly.
### Table 1: Rebel Defeats and the Spread of Conflict

<table>
<thead>
<tr>
<th></th>
<th>A</th>
<th>B</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Refuges Only Model</td>
<td>Full Model</td>
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<tr>
<td>Total Rebel Losses</td>
<td>0.0916</td>
<td>0.0976</td>
</tr>
<tr>
<td></td>
<td>[0.0145, 0.1726]</td>
<td>[-0.0380, 0.2419]</td>
</tr>
<tr>
<td>Total Rebel Losses^2</td>
<td>-0.0021</td>
<td>-0.0001</td>
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<tr>
<td></td>
<td>[-0.0040, -0.0005]</td>
<td>[-0.0004, 0.0000]</td>
</tr>
<tr>
<td>Total Refugees</td>
<td>0.1300</td>
<td>0.0976</td>
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<tr>
<td></td>
<td>[0.0034, 0.2709]</td>
<td>[-0.0380, 0.2419]</td>
</tr>
<tr>
<td>Battle Deaths</td>
<td>-0.0001</td>
<td>-0.0001</td>
</tr>
<tr>
<td></td>
<td>[-0.0004, 0.0000]</td>
<td>[-0.0004, 0.0000]</td>
</tr>
<tr>
<td>Conflict Scope</td>
<td>0.0083</td>
<td>0.0904</td>
</tr>
<tr>
<td></td>
<td>[-0.7634, 0.8058]</td>
<td>[-0.7737, 0.8839]</td>
</tr>
<tr>
<td>Civil War in Neighbor</td>
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<td>0.9476</td>
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<td></td>
<td>[0.0651, 2.3268]</td>
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<td>RGDPPC</td>
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<td></td>
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<td>[-0.0974, 0.6692]</td>
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<tr>
<td>Polity</td>
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<td>0.0112</td>
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<td></td>
<td>[-0.1025, 0.0980]</td>
<td>[-0.0865, 0.1121]</td>
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<tr>
<td>Polity^2</td>
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<td>0.0008</td>
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<td></td>
<td>[-0.0184, 0.0257]</td>
<td>[-0.0219, 0.0235]</td>
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<tr>
<td>Time at Risk</td>
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<td>-0.1638</td>
</tr>
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<td></td>
<td>[-0.6469, 0.3962]</td>
<td>[-0.6598, 0.3564]</td>
</tr>
<tr>
<td>Time at Risk^2</td>
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<td>-0.0075</td>
</tr>
<tr>
<td></td>
<td>[-0.0924, 0.0796]</td>
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<tr>
<td>Time at Risk^3</td>
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<td>0.0005</td>
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<td></td>
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<td>[-0.0047, 0.0056]</td>
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<tr>
<td></td>
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</tr>
<tr>
<td>Observations</td>
<td>473</td>
<td>473</td>
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</table>

Note: Bold estimates are statistically significant
Accordingly, Column B of Table 1 incorporates the measure of rebel battlefield losses to better investigate the process through which conflict spreads and to evaluate my primary hypothesis. The results of this model indicate that the data support Hypothesis 1 and demonstrate a relationship between the number of battlefield defeats a group experiences and the probability of civil conflict in neighboring states. In accordance with my argument, the impact of rebel losses is non-linear such that the unit effect of rebel losses on the spread of civil conflict decrease as the number of losses increases and eventually becomes negative.

To more explicitly examine the impact of rebel losses on the probability of conflict, Figure 1 displays the predicted probability of conflict across values of the number of rebel losses (and, implicitly, its squared term). Figure 1 clearly indicates that the probability of conflict rises as a function of the number of rebel losses until around 20 losses whereupon the probability begins to decrease. To be sure, this figure displays significant uncertainty about the size of the effect of rebel losses on conflict onset. However, the curvilinear nature of the relationship is clear and an initial increase is clearly visible.

Beyond highlighting the significance of rebel losses, the results in Table 1 indicate that, after incorporating a measure of rebel losses, the total number of refugees in a state is no longer a significant predictor of the onset of civil conflict. I argue that this result provides additional support for my argument that a refugee population’s connection to a rebel group is critical for the spread of conflict. Indeed, this result indicates that a large refugee population is not sufficient to for conflict to spread and that, in fact, a large refugee population will have no effect on conflict once links to rebel organizations are better accounted for.

These findings are the key results from the statistical analysis and support the theoretical model I have developed. Specifically, these results suggest that, as rebel groups experience
strategic defeats at the hands of the government, the risk of civil war spreading to neighboring states increases. Even though, as mentioned before, this model does not precisely explore the chain of events that would facilitate the spread of conflict through rebel-supporting refugees, it does provide evidence that rebel losses are associated with conflict spread.\(^5\) Indeed, while there are potentially other reasons why this might be, my theoretical model provides one such explanation that is coherent and connected to previous findings. The significance of battlefield losses and the finding that refugee population size is rendered insignificant in light of a measure of rebel losses, coupled with my previous analysis of the causes of refugee flight – specifically that rebel losses can induce flight – all provide significant (if only tentative) support for my

\(^5\) As noted before, while I believe there are sufficient reasons to focus on state-based conflicts that result from refugee flows, I do acknowledge that this measure is only a proxy for the theoretical outcome of interest. In an alternative empirical model, I have combined data on state-based and non-state conflicts to evaluate my hypotheses. These models lead to roughly the same conclusions with the exception that the Rebel Loss variable is no longer statistically significant; though it comes very close and the squared term is still significant.
arguments. To further bolster this argument, the next section offers a series of illustrative examples of several refugee crises and explores the links between refugees and conflict in these cases.

### 3.4 Illustrative Examples of the Link between Refugees and Rebels

The quantitative evidence presented in the previous section, while generally supportive of my theoretical model, is limited in what it can tell us about the validity of my hypothesis and the mechanisms through which conflict spreads. To further bolster my argument, this section examines several examples of refugee crises in an effort to illustrate the reality of the refugee-violence nexus, highlight empirical examples of important aspects of the theoretical argument, and provide the foundation for future analyses of the refugee-rebel model.

In what follows, I examine cases of civil conflict and focus on the way refugee militarization did or did not materialize. I begin with a detailed account of the conflict in Sierra Leone and the spread of refugee violence to Guinea towards the end of the war. This example offers the clearest evidence for the connections between rebel groups, refugees, and violence that I posit above and serves as the primary case under analysis. After discussing the evidence from Sierra Leone, I also offer (more-abbreviated) discussions of conflicts in Cambodia, Rwanda, and Somalia that point to interesting aspects of my argument that need further development – namely the way that conflict can spread through government defeats and the way the spread of conflict may not occur.

Altogether, these discussions serve illustrative and exploratory roles in supporting my statistical analysis. The first example offers direct evidence that, in some cases, the causal process I set forth in the theoretical model does indeed operate. In this sense, this discussion is
largely illustrative and focused on providing a clear example of my argument – while not necessarily attempting to “prove” that the mechanism I propose is necessary or sufficient to create refugee-related violence. Indeed, this is an especially useful exercise given the aforementioned limitations of the preceding statistical model. On the other hand, the other cases I examine fill a more exploratory role and help to provide insight into both the limitations and possible extensions of the model I have set forth. To fulfill this purpose, these secondary cases were chosen precisely because they do not exactly follow the patterns I have emphasized. For instance, the Rwandan and Cambodian examples serve to illustrate how the process I examine can take place even when it is government forces that suffer defeat and are displaced, while the discussion of Somalia provides interesting insights since, while one might expect widespread refugee militarization to occur, the conflict produced little refugee militarization. Altogether, examining these cases helps support the empirical findings in the previous section while also pointing to the need for further research and theory-building in the study of refugees and violence.

3.4.1 Sierra Leone, Guinea, and the Violence of the RUF

In September 2000, soldiers aligned with the Revolutionary United Front (RUF) – the primary rebel group engaged in the Sierra Leone Civil War – attacked a number of Guinean cities and villages bordering Sierra Leone (IRIN 2000, Muggah 2006). The proximity of the attacks to a number of refugee camps and the RUF’s history of forced recruitment both pointed to a connection between the camps and the RUF’s attacks. Indeed, UNHCR officials, the refugees, and even the Guinean government all noted that the rebels had infiltrated the camps (Muggah 2006, Human Rights Watch 2001, United States Committee for Refugees and

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6 That being said, I will offer some comments about why the arguments I propose offer a better explanation of the rise of refugee violence when compared to other models.

In many ways, the regional instability surrounding the Sierra Leone conflict and the spillover of violence into surrounding states was unsurprising and indisputably tied to longstanding hostility between the governments and dissident groups from Sierra Leone, Guinea, and neighboring Liberia (International Crisis Group 2005). Nonetheless, while the spread of violence seemed likely, the connection between refugee groups and violence was not a forgone conclusion. Indeed, while over 200,000 Sierra Leonean refugees resided in Guinea as early as 1996, little cross-border violence took place until the end of the decade (Global Security, United States Committee for Refugees and Immigrants 1997, Onoma 2013). With this in mind, I argue that a critical factor in explaining why the spread of violence followed only after nine years of conflict and why refugees played such an important role in the ensuing violence is the performance of the RUF and its ability to use refugee camps to carry out violence against its enemies (Muggah 2006).

Two aspects of the Sierra Leone Civil War – and particularly the Revolutionary United Front – are notable for understanding the rise of refugee-related violence. The first aspect is the

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7 Indeed, disputes between the governments and rebel groups within each of these countries created various violent encounters throughout the 1990s and 2000s. One especially interesting dynamic, similar to the one I examine, arose during the Second Liberian Civil War when Liberian refugees fleeing to Guinea assisted, voluntarily and after coercion, the group Liberians United for Reconciliation and Democracy (LURD). Reports indicate that these refugees fought and transported arms for the LURD in their struggle against the Liberian government (Human Rights Watch 2002). Interestingly, in this case the Guinean government explicitly supported the LURD and the militarized refugees in their struggle against the Liberian government of Charles Taylor (Florquin & Berman 2005). Thus, while this example fits with the theoretical argument about refugee militarization, it also shows how using resulting state-based civil conflict as a proxy is troublesome in that it fails to account for cases where militarization is ignored by or even supported by the host state.
one critically identified by the model of refugee violence I present above: the repeated strategic
defeats of RUF forces in Sierra Leone and the overall weakening of the group due to
international interventions (United States Committee for Refugees and Immigrants 1997).
Indeed, by late 2000 military forces assembled by the Economic Community of West African
States (ECOWAS), the United Nations, and individual states (most notably the United Kingdom)
had rebuffed two RUF advances on the Sierra Leonean capital of Freetown, facilitated the
signing of the Lomé Peace Accord, demobilized thousands of former fighters, and through
cooperation between UK and UN forces retook significant ground from the rebels (Ucko 2016,

Thus, it is clear that by the time cross-border violence exploded into Guinea and the
refugee camps in September of 2000, the RUF was under considerable pressure to recruit new
soldiers and recuperate from its losses. While estimating the size and strength of the RUF during
this period is difficult, it is certainly true that the organization had shrunk in the preceding years.
Indeed, after the Lomé Peace Accord, many soldiers had demobilized and RUF leader Foday
Sankoh seemed content to accept the ceasefire – and the valuable government position it offered
him – with only second-in-command Sam Bockarie continuing to mobilize RUF forces against
the government (Abdullah 2004).\footnote{There is some debate over the intentions of the various RUF leaders during this time. While it is clear that Sankoh and Bockarie had differing, sometimes violently, opposite views on the peace negotiations, it is unclear how genuine Sankoh efforts for peace were and whether he did indeed stop actively supporting the violent activities of the RUF (Gberie 2005, Abdullah 2004). Regardless of the ultimate motives of the leaders however, it is clear that internal disputes, coupled with strategic defeats and the ongoing negotiations, substantially hindered the capabilities of the RUF during this time – setting the stage for subsequent refugee militarization.}

Consistent with my theoretical argument, only in this atmosphere of rebel defeat and a
need to recoup losses did the RUF begin to consistently reach beyond its borders and incite
violence in and around refugee camps. That being said, as noted previously, it is only when rebel
groups still retain the capacity to mobilize refugees and create violence abroad that we should expect conflict to spread in this way. This is the other critical aspect of the conflict useful for understanding its spread: the RUF’s historic ability to recruit – sometimes forcibly – fighters for its cause and the support it received from outside actors – namely Charles Taylor of Liberia.

Perhaps the best known aspects of the conflict in Sierra Leone were its overall brutality and the role diamonds and the “resource curse” played in the conflict. While the RUF certainly wanted political regime change, it is also clear that its members and leaders benefited greatly from the extraction of alluvial diamonds across sections of the country (Gberie 2005). These diamonds were used to buy weapons and materials for the conflict and, perhaps most importantly, provided a source of income for combatants and became a major recruiting tool for the RUF (Baines 2001). While this method surely attracted many soldiers, it was not the only way the RUF bolstered its ranks. Indeed, consistent with the perception of the conflict as especially brutal, the use of forced recruitment – sometimes of young children – by RUF forces was also especially common (Amnesty International 2001).

This ability to hire or forcibly recruit soldiers would prove to be essential to the conduct of RUF violence as international forces moved in on the group. As cross-border violence escalated, reports of the presence of militants in refugee camps began to appear and the UNHCR began to implement screening programs to separate “refugees” from “rebels.” In some cases, the RUF would utilize “spies” to reconnoiter the refugee camps and other villages in efforts to assess the threat level in the area and the availability of recruits (Onoma 2013, Richards 2005). One Sierra Leonean refugee in Guinea even reported that rebels would simply stand on the border and “shout to [them] from there” telling them to join the rebels in Sierra Leone or face the consequences – presumably forced recruitment or death (Human Rights Watch 2001).
Eventually, similar incursions by RUF forces into refugee camps would cause the camps to empty with occupants either fleeing further into Guinea or back into Sierra Leone – sometimes into rebel-controlled areas, providing further evidence to observers of the connections between the groups (Afrol News 2000).

Even considering the RUF’s ability to recruit new members, it may be hard to imagine that this recruitment mechanism would result in significant violence in Guinea. Indeed, in this case, RUF soldiers committed widespread violence in and around refugee camps as well as in other villages and towns in Guinea. To understand why the attacks took on such a broad character, it is critical to understand the connections between the RUF and Charles Taylor of Liberia. Consistent with the discussion of sources of refugee-related violence and the rise of civil conflict in host countries discussed above, it is clear that part of the motivation for the violent incursions into Guinea was to undermine the Guinean government and its opposition to Taylor and his forces (LCHR 2002, Gerdes 2013).

While fully examining the longstanding conflict between groups in Sierra Leone, Liberia, and Guinea is beyond the scope of this chapter, it is important to note that, in this case, the expansion of violence beyond the borders of Sierra Leone was likely directly connected to support from outside actors and, most importantly, an explicit desire to harm a neighboring government. These connections help explain how, despite recent setbacks, the RUF was able to execute these cross-border incursions. With Taylor’s support and encouragement, attacking Guinean settlements, seeking refugee supporter and using camps as refuge became a significant strategy for the beleaguered group (Afrol News 2000).9 Indeed, as the links between the refugees

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9 These connections also help explain how the refugee-related violence rose to such high levels during this period as the RUF aimed not only to recuperate and gather resources but also to do directly damage to the Guinean government.
and militants grew during this period, the distinction between refugees and rebels became increasingly unclear and ultimately resulted in widespread violence against refugees throughout Guinea as the government and general population sought to expel all refugees (Muggah 2002, Onoma 2013).

Altogether, the case of Sierra Leone and the Revolutionary United Front seems to offer a compelling example of how refugees and conflict can become intertwined. Facing a series of strategic defeats yet still capable of recruiting and equipping new fighters, the RUF incited violence along the Guinean border in an attempt to strengthen its numbers and further destabilize Guinea and undermine its involvement in the war in Sierra Leone. The case is especially illustrative as it counters the argument that simply hosting a large refugee population will cause conflict as Guinea hosted tens of thousands of refugees for nearly a decade before conflict erupted. Only when the Sierra Leonean government and its supporters began to make significant inroads against the RUF did refugee-related violence come to prominence.

Similar cases where strategic defeats appear to create incentives to facilitate refugee rebellion can be found throughout studies of refugee crises. The history of the Allied Democratic Forces (ADF) in the Democratic Republic of the Congo (DRC) and Uganda provides another illustrative example. Founded from the remnants of defeated Ugandan regional resistance organizations, the ADF has expanded its operations from Uganda into the DRC where its members once fled to avoid extermination at the hands of Ugandan forces (Fahey 2015, Mwakikagile 2012) Now, some 30 years since the breakdown of its progenitor organizations, the ADF continues to terrorize both countries – with many attributing its success to its extensive multi-state networks of supplies and personnel (UN 2015).
These cases share notable similarities over the resources of the rebel organizations and the role defeat plays in the spread of conflict while differing on the nature of the refugee–related violence and the scale of the refugee crises. Further, in case one is tempted to view these cases as idiosyncratic to African states, note also Lischer’s (2005) analysis of the case of rebel groups in Pakistan resisting the Soviet occupation of Afghanistan. In this setting, the overwhelming military might of the Soviet army drove nearly 3 million refugees into Pakistan which, in turn, created a hotbed of guerilla activity that would ultimately outlast the Soviet forces (Roy 1986).

Clearly, while circumstances can vary considerably, the incentive to militarize refugees and to use the camps as zones to recuperate and plan further attacks is real and faced by groups in a variety of conflict. That being said, it is worthwhile to note that rebel groups are not necessarily the only groups capable of facilitating refugee-related violence. Indeed, while I have, thus far, evaluated my arguments about refugees and violence by linking rebel groups to the spread of conflict, as I note above, my theoretical argument makes no distinction between the types of groups in conflict. For the purpose of this analysis, I have chosen to focus on rebel groups simply because data on their battlefield losses are readily available and because I believe such data provides a reliable proxy for the performance of the rebel group. In the next section, I briefly discuss cases of government-induced refugee rebellion stemming from conflicts in Rwanda and Cambodia.

3.4.2 When Governments Become Rebels: Rwanda and Cambodia

\[10\] Indeed, I have argued throughout my works that battlefield defeats do not matter as much to government forces as they do to rebel groups. While I have detailed the specifics of this argument elsewhere, I will reiterate that defeats matter more to rebel groups because they control less land and associated resources (on average) than governments, do not have established infrastructure to support them across the country, and must rely (more heavily) on local recruitment to strengthen their forces.
While successive battlefield defeats are enough to compel rebel groups to pursue refugee militarization, a more decisive set of events is required to induce the same behavior by government organizations. For governments, the prospects of militarizing refugees to act as a paramilitary force carries significant risks that established governments will be unwilling to take lightly. Indeed, the act of arming a group (other than the state’s military forces) carries with it concerns about control over the group’s violent behavior, whether the group will demobilize at the end of the conflict, and whether the group will turn against the government after receiving benefits from the state.\footnote{While little research exists examining how refugee populations interact with those who facilitate their militarization, one can draw on the logic of principal-agent problems to understand some of the concerns. See especially the work on relationships between pro-government militias and governments (e.g. Mitchell et al. 2014) for a sample of the associated risks.} However, governments on the verge of total defeat at the hands of rebel groups and their supporters may be willing to take this risk and mobilize refugees and other civilians to reclaim control from the rebels.

The oft-cited example of refugee militarization during the Great Lakes Refugee Crisis following the Rwandan Civil War and the resulting Rwandan Genocide follows this pattern closely with the ousted Hutu government and its supporters creating a “state in exile” in surrounding states (Lischer 2005).\footnote{It is worth noting that the Rwandan case actually provides two instances of government-induced refugee rebellion: the conflict in the 1990s and the one in the 1960s where the Tutsi-led government was ousted by Hutu forces and resorted to forming militant groups in refugee camps (see Zolberg et al. 1989 for a discussion of this earlier case).} Particularly in the case of refugee violence in Zaire (now the Democratic Republic of the Congo) the connections between the refugees, the Hutu-led former government of Rwanda, and the perpetrators of the Rwandan Genocide were so clear that many aid agencies (notably excluding the UNHCR) quickly withdrew their support for the refugee camps citing a desire not to support perpetrators of genocide (Lischer 2005, Betts & Loescher 2011).
While the sequence of events that created this situation are well-documented in many studies of refugee militarization, the critical aspect for this analysis is how the strategic defeat of the Hutu-dominated government created the conditions for government-led refugee rebellion (Muggah 2006). Specifically, the success of the Tutsi forces in overtaking the Hutu government and the resulting flight of former government and military leaders along with hundreds of thousands of civilians to refugee camps in Zaire, Tanzania, and Burundi created a hotspot for anti-Tutsi activity. In the camps, Hutu leaders could rely on support from co-ethnic refugees, protection by the host country’s forces, and, at least for a short time, the assistance of a vast number of humanitarian agencies. With all of these resources before them, the displaced Hutu leaders were able to effectively mobilize the various refugee populations to undertake violence against a host of targets in both Rwanda and Zaire. Ultimately, the violence of these Hutu militants would be instrumental in causing the First Congo War (and arguably the Second Congo War).

While the complexity and severity of the Great Lakes Refugee Crisis is largely unmatched by any other crisis, it is again important to note that this form of government-induced refugee militarization is not bound solely to Africa. The 1978 Vietnamese invasion of Democratic Kampuchea (now Cambodia) provides an excellent example of this process outside of the African context. In this case, Vietnamese forces invaded the newly formed and tumultuous Kampuchea, quickly removed Pol Pot and the Khmer Rouge from power, and began a decade-long occupation of the country. In response to the Vietnamese invasion, many former government leaders and their supporters fled westward towards Thailand where they focused on

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13 In fact, examining this case reveals similarities between my model and Lischer’s (2005) arguments about violence among “state in exile” refugee populations. The critical distinction between the two models however is that I identify the conditions under which a refugee population will be representative of a “state in exile” or even a “rebel in exile” population.
building up their forces and securing international support in order to reclaim their country from the Vietnamese (Vickery 1984). Refugee militarization along the Thai border led to repeated clashes between militant refugees (i.e. supporters of the Khmer Rouge), the Thai military, and the Vietnam-backed forces of the People’s Republic of Kampuchea (Associated Press 1983, Campbell 1983, Reuters 1984). Once again, the militarization of these groups would eventually become a contentious issue among aid organizations and disputes over whether aid should be provided to known perpetrators of genocide and other atrocities (Zolberg et al. 1989).

This dynamic of the near-complete defeat of a central government resulting in refugee militarization is a useful extension of the model I present above. Violence in this situation clearly stems from political allegiances among refugees and the (former) government’s willingness to pursue any strategy to defeat its enemies. These cases are also notable as avenues to explore how governments can retain power, control, and resources even after being deposed. These particular cases suggest a link between a receptive and facilitative host state and successful refugee-rebellion by pro-government forces. For example, the support of Chinese and Thai forces was critical to sustaining the Khmer Rouge refugees in their struggle against the Vietnamese (Zolberg et al. 1989). Though doing so is beyond the scope of the current piece, this analysis indicated that expanding on the idea that host states may play a role in refugee rebellion would be a productive avenue for future research.

In sum, while these two cases do not directly evaluate the hypothesis presented earlier, they are coherent with the overarching logic and provide supplemental evidence both that refugee militarization does occur and that, when it does occur, it is often the result of one side in the conflict suffering strategic – though not entirely debilitating – defeats. Additionally, these cases point to additional aspects of the broader arguments I have made about the key role of
allegiances to conflict actors among civilians and refugees and point to a need to further develop my model to account for the role of host states and to better explicate the conditions that facilitate government-sponsored refugee militarization. However, before concluding, it will be fruitful to examine situations in which refugee militarization did not occur – despite being potentially expected. The benefit of examining such a case is the ability to determine whether an absence of militarization corresponds (at least in some cases) to an absence of the capability and willingness to pursue it or whether significant additional changes must be made to the theoretical model.

3.4.3 Somalia: Insecurity without Militarization

The dire situation in Somalia that has persisted for the past several decades surely needs little introduction. Organized rebellion beginning in the 1980s culminated in the overthrow of Siad Barre’s regime in 1991 and has produced numerous international interventions, infighting between competing proto-governments and warlords, and, combined with repeated famine and drought, has created one of the largest refugee crises ever known (Fergusson 2013). While the number of displaced persons has fluctuated considerably since the start of the Somali Civil War, both Kenya and Ethiopia have consistently hosted over 200,000 refugees each with tens of thousands of other refugees dispersed into other countries (Refugee Council USA 2016).

Despite this context of violence and political turmoil, little evidence exists pointing to a strong link between Somali organizations and refugee violence in the decade following the fall of the Barre regime. While forms of insecurity consistently plagued Somali refugees in Kenya and Ethiopia, such violence appeared largely to be related to criminal activity and sexual violence. For instance, UNHCR initiatives and reports consistently indicate that, throughout the 1990s, the Dadaab refugee camps in Kenya featured a large degree of criminal violence but lacked a clear
component of military recruitment or the utilization of the refugee camps as staging areas for attacks (Crisp 1999, Kagwanja 2000, Newhouse 2015). Additionally, despite the Kenyan government’s ill-treatment towards the refugees, organized violence against the host government largely failed to materialize during this period (Horst 2006, Campbell 2006, Patinkin 2014). Reports from Ethiopia bear similar sentiment and indicate that, while community-based violence sporadically took place, full-scale militarization like that seen in other refugee crises did not (IMC 2011).

These facts make clear that, in the Somali case, militarization did not follow from a large refugee crisis, from the defeat of a central government, or from the defeat of specific rebel groups at the hands of other groups and international forces (e.g. after the defeat of Mohamed Aidid – head of the United Somali Congress and the target of UN operations in Mogadishu) (Fergusson 2013, UN 1997). In this case, while it would seem that conflict actors, whether supporters of the former government or of the rebel groups, had numerous reasons to desire refugee militarization, they never had the capability to do so.

Indeed, after both the defeat of the Somali government in 1991 and the strategic defeats of the United Somali Congress (USC) in the later part of the decade, neither groups’ supporters had sufficient resources or organizational capacity to effectively promote refugee militarization. For the few remaining supporters of Said Barre’s government, there were simply too many rebel organizations to effectively combat and Barre’s government policies had debilitated the economic ability of the state and had left him and his clan with more enemies than friends. Similarly, for supporters of the USC (or indeed any of the factions competing for control after the collapse of Barre’s regime), the situation was similar. Despite several major setbacks, the opportunity to effectively utilize the Somali refugee populations simply never materialized. For
all of these groups, the lack of widespread legitimacy, coupled with a lack of resources to fund rebellion and recruit refugee combatants, meant that refugee militarization was unlikely. Indeed, in the absence of some additional facilitating factor like diamonds, co-ethnicity, a strong recruitment network, or a receptive host state, groups from the Somalian Civil War largely failed to militarize the surrounding refugee populations; despite the great number of refugees and the general instability of the region.

This case bears on the theoretical model I present in several ways. First, it makes clear that refugee militarization is not always a natural consequence of refugee flows or civil war more generally. While this point may sound obvious, it is worth noting that previous research – with its emphasis on the size of refugee flows – would likely expect Somali refugees to be major vectors of conflict given their sheer numbers. Second, especially when compared to the previous cases, the study of Somalia offers evidence that the capabilities of conflict actors does indeed matter for the prospects of refugee militarization.

The basis of this logic has been present in my arguments all along: groups cannot provide the opportunity for rebellion to refugees if they themselves barely have enough power to continue fighting. My theoretical argument and empirical tests have focused on the role battlefield performance plays in this dynamic and how too many battlefield losses reduce the risk of victimization because militant groups simply no longer have the resources or ability to facilitate refugee militarization. However, evidence from the case studies suggests that using battlefield performance as an indicator of capacity to militarize has its limitations. As a result, it is clear that a more fully developed theoretical model of refugee-related violence will need to account for the various sources of power and resources available to militant groups. Indeed, no matter how many refugees are present or how much a militant group wants to mobilize them, the
absence of coordination, leadership, and resources will prevent them from being able to do so. Thus, while the evidence from the case studies indicates that my theoretical model can be improved upon, the core logic of the model is supported: refugee militarization is tied to the goals and actions of existing militant groups and will only occur when such groups desire and have the ability to facilitate militarization.

Indeed, the above conclusion, is the key takeaway from all of the case studies (and from the theory more generally); while refugee militarization is often possible, it is only likely when the conditions of willingness and capability are both met. While the statistical analysis provide a general sense of the wide applicability of the model, the case studies provide a “reality check” on the basic validity of the model, highlight its key features, and draw attention to some avenues for future development. To see all of these things more clearly, in the closing section that follows, I will review the theoretical model I have set forth, draw together the evidence evaluating the model, discuss pathways for future work, and consider the policy implications of the findings.

3.5 Discussion, Extensions, and Implications

In this project, I have set forth a model of the spread of violent conflict that connects the militarization of refugee populations with the behavior of militant groups during civil conflict. I argue that groups face an incentive to mobilize refugee populations to fight alongside them when such groups are underperforming relative to their enemies. However, poor performance also has the effect of reducing the group’s ability to facilitate refugee mobilization by decreasing the group’s strength and available resources. As a result, I ultimately propose a non-linear relationship between the likelihood of militarization and battlefield performance such that lower levels of strategic defeats increases the risk of mobilization whereas higher levels decrease it.
To evaluate this argument, I examine conflicts in Africa and show that states with neighbors involved in civil wars where rebel groups are suffering numerous battlefield defeats are more likely to experience civil conflict themselves. Additionally, I demonstrate that the number of battlefield defeats matters and that when groups experience too many defeats the probability of conflict spreading decreases. I interpret these findings as suggesting that poor performance by militant groups can them to militarize refugee populations abroad – but only when this performance has not become so bad as to deprive the rebels of the resources required to mobilize the refugees.

While these statistical findings are suggestive, I also examine a set of specific conflicts in more detail and document how refugee militarization of the kind I describe in my theory actually takes place. Though all of the examples offer some evidence for the validity of my argument and empirical findings, the discussion of the Sierra Leone Civil War is especially instructive as it offers clear evidence for a well-funded rebel organization seeking to mobilize refugees abroad (in this case, in Guinea) after suffering a series of strategic setbacks including battlefield defeats and the deepened involvement of foreign actors.

These results are especially informative when considered against alternative, unconditional models of refugee-related violence. Instead of simply offering evidence that a link between refugees and violence exists, the analyses in this project have demonstrated that the link between hosting refugees and experiencing conflict is conditional on other factors – namely the behavior of militant groups. The evidence provided by both the statistical models and the illustrative cases suggest that population size is of little importance when compared to other conflict dynamics. That being said, these results also indicate that refugee-related violence associated with strategic defeat is also not a forgone conclusion. The evidence from Somalia in
particular points to instances where large refugee populations (at least partially) displaced by the defeat of a violent group does not lead to the spread of violence.

Finally, despite the evidence given here, some might suggest that the relationships I identify are strictly due to the actions of militant groups and have little to do with refugee populations. Indeed, this is a fair critique of the statistical analysis presented here as it does not take into account the perpetrator of the violence. However, this alternative explanation is less compelling when considered in light of the illustrative examples. In these examples, the refugee populations are clearly associated with the violence to an extent that has been widely recognized by scholars, governments, and non-governmental organizations. For example, the case of refugee-related violence after the Rwandan Genocide has been thoroughly documented and there is little questions that both “legitimate” refugees and former combatants came to participate in the violence. Indeed, the model I set forth makes a strong case that the source of refugee militarization is the interaction between refugees and rebel groups – implying that, in almost all cases, former combatants are at least partially responsible for the violence. However, this is different from saying that the refugee population is entirely separable from the resulting violence. This later position is, I believe, untenable and does not provide a compelling explanation for the results of my statistical model or for the events in the illustrative examples.

Taken together, the evidence from the illustrative examples and the statistical models offer substantial evidence that the theoretical model I propose is valid and has wide applicability. That being said, the theoretical model I present is far from the last word on the topic of refugee militarization. One clear area where more work is needed deal with how to conceptualize and measure the performance of the central government during civil conflict and to relate these concepts to the risk of refugee militarization. While some of the anecdotes discussed above
demonstrate that governments can be the source of refugee mobilization, the overall model above lacks a clear measure of the willingness of these governments to pursue this strategy.

Another key area for research highlighted by the examples relates to the role refugee hosting states play in the process of militarization. One thing the case studies demonstrate very clearly is that militarization has many forms. While in some cases, militant refugees launch attacks on targets inside the host state, in other settings attacks are solely focused on targets in the origin country. In these latter cases, it may be that the implicit or explicit support of the host state is required for this type of behavior to take place. Even in the former cases, where attacks are focused on both host and origin countries, the role of third-party supporters may also be important. While I have discussed to some degree the advantages given to refugee militants by aid agencies, it may be the case that more support is required in the form of another state channeling resources to the militants in their new refuge. All of these considerations point to the need to potentially incorporate more concepts from conventional interstate relations into the model of refugees and civil war. Insights from the study of rivalries, interstate territorial disputes, or regional politics may provide leverage on the problem of refugee militarization by highlighting when other state actors will be willing to facilitate rebellion.

The evidence presented here makes a compelling case that refugee militarization is not only real but that it has certain preconditions that can be reliably understood. Indeed, the most novel aspect of this model is that it provides a way to understand why and when militarization occurs and does not simply “black box” the militarization process. These findings are especially useful in two particular, policy-relevant ways. First, these findings can aid in determining when refugee militarization, and the spread of violence, are most likely. When a militant group is suffering strategic defeats, such as being rebuffed from a major offensive or driven back to a
small section or territory, militarization may be likely—especially if the group is still well-funded and can reach refugee populations. Equipped with this knowledge, policymakers—including the UNHCR—will be able to devote resources and attention to strategically important populations when the risk of militarization is highest. For example, by spending extra time screening refugees and meeting basic needs during periods of underperformance by a rebel group, the UNHCR may be able to prevent militarization of a particular camp or population.

In addition to helping policymakers determine when militarization is likely, these results also help military forces understand what steps they can take to ensure militarization does not occur. Specifically, these results indicate that even though groups may want to militarize a population, they will not be able to unless they have the resources to do so. This is useful information for military planners who may decide to prioritize de-funding (in various ways) the rebel groups they are pursuing in an attempt to further decrease the risk of the spread of violence. Indeed, these findings point to the need to pursue a multi-pronged approach to combatting rebel organizations that not only repels attacks, but also seeks to destroy supplies, cut off networks, and prevent external backers from providing funds. While this approach is likely already common, these findings surely provide additional reason to pursue it.

In total, these finding provide new insights into the process of refugee militarization and provide the basis for continued research into the topic. Indeed, the final message seems to be that militarization is real and is directly linked to the incentives and behaviors of militant organizations. In some ways, it has always been clear that refugee populations can become violent and spread conflict to other states. However, these concerns should not be exaggerated. Just as civil war itself is rare, refugee militarization also seems to be rare and the product of at least two specific conditions being met. Further, even when these conditions are met, refugee
militarization is not a given; it simply become more likely. In the end, I hope these results fuel more optimism about refugees than fear. Indeed, my argument does not say that all, or even most, refugees are bound for violence. Instead, it suggests that militarization is rare and that appropriate responses by refugee hosting states and aid organizations can ultimately prevent the spread of violence among refugee populations.
Chapter 4: Humanitarian Crisis or Unwanted Burden: Examining the Link Between Refugees and External Support in Civil War

The first years of civil conflicts are often directly associated with large-scale outflows of refugees. The violence associated with civil wars is a leading cause of refugee flows worldwide as are many potential precursors of civil war such as state repression, military coups, and low-level political violence (UNHCR 2016). Historically, these refugee flows have often been considered an indicator of the severity of the overarching humanitarian crisis engulfing the country of origin. Viewed in this way, refugee crises are often said to motivate a variety of behaviors from concerned actors responding to the crisis. In the specific case of third-party support for conflict actors, scholars have argued that third-party states are motivated to intervene when a refugee crisis occurs in an effort to mitigate the humanitarian consequences of the crisis. While a link has indeed been found between refugee flows and external intervention, the existing scholarship has been unclear about the motivations behind intervention and has not provided a method to evaluate which states will ultimately intervene. This project aims to provide insight into both of these issues.

Specifically, I argue that the logic of intervention underlying many previous models of intervention - that states intervene for humanitarian reasons - stands in contrast to the logic behind other models of the determinants of third-party support or intervention. These alternative models eschew humanitarian concerns and focus on the self-interest of third-party states who intervene to prevent suffering the negative externalities of the conflict. For example, allies of a state experiencing a civil war are said to have an incentive to intervene because, if they do not and their ally is defeated, then they will lose a military ally and thus lose some level of power and/or control. Similarly, researchers now largely agree that

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In this project, I use the terms “external support” and “intervention” interchangeably. This is due to my focus on the provision of any form of military-focused support to a conflict actor by another state. While much of the previous literature on intervention and refugees only looks at the direct combat involvement of outside states, I see no reason to restrict my model in such a way and thus use the term “intervention” to cover a set of behaviors more commonly called “external support.”
contiguous states consistently intervene in their neighbor’s civil conflicts in part to prevent regional destabilization and conflict “spillover.” I argue that a similar set of concerns are directly applicable to refugee flows when one considers not only the number of refugees produced by a conflict but also the number of refugees hosted by a potential third-party supporter.

To be clear, this project is not the first to examine the link between refugees and conflict intervention. Indeed, I will discuss a large number of studies that have included a measure of refugees in some form or another in their analysis (Regan 1998; Lemke and Regan 2004; Findley and Teo 2006; Kathman 2011). My concern in this project deals specifically with how these models theorize and empirically evaluate the link between refugees and intervention. In some cases the theoretical justification is one where refugees signal a humanitarian crisis and states intervene to put an end to this crisis. In other cases, scholars argue that refugee outflows have some type of general destabilizing effect (mostly through the economic and security concerns they create) which similarly motivates third-party actors to become involved. The fundamental issue with much of this existing literature however is that, regardless of the theoretical justification provided, the empirical measure of refugees used is consistently an indicator of the total number of refugees the conflict has produced. Thus, even when previous scholars have put forth theoretical models of intervention motivated by self-interest, they have evaluated empirical models more consistent with an explanation based on altruism. Thus, in an effort to better clarify the link between refugees and intervention, this project looks directly at how many refugees a potential intervenor hosts from a state involved in civil war and expects that as the number of hosted refugees increases, so too does the probability of intervention.

In addition to providing a clearer link between the theoretical model used and the empirical evaluation, this project also provides additional leverage to scholars of conflict intervention by identifying states hosting a large number of refugees as the specific states most
likely to intervene. As noted above, while previous research identifies a link between refugee flows and intervention, existing models do not tell us which states will intervene. By using an actor-centric approach to the link between refugees and intervention, I hope to improve our ability to identify the specific actors most likely to intervene in a conflict.

In the subsequent sections, I provide more detail on the existing literature linking refugees and conflict intervention, explicate my own model of this link, and provide an empirical assessment of my argument using data on refugee flows and external support in Africa. Along the way, I will highlight mitigating factors that modify the way hosting refugees impacts intervention behavior. All together, my findings suggest a robust link between refugees and third-party involvement in civil war and provide an impetus for reassessing the way scholars think about the motivations of potential intervenors.

4.1 Existing Studies of Refugees and Intervention

To understand the contribution of the current project, it will be important to clearly delineate the different components of the existing literature with a focus on the current understanding of the motivations behind intervention. Further, I will distinguish between “phenomena-centric” and “actor-centric” approaches to third-party involvement in civil conflict as well as demonstrate how, despite considerations about the pressures refugees place on host countries, the existing literature consistently treats refugees as a “phenomena-centric” explanation for intervention (Findley and Teo 2006).
4.1.1 Models of Intervention and External Support

The earliest empirical models of conflict intervention take a “phenomena-centric" approach wherein certain conflict dynamics cause third-parties to be more or less interested in intervention (Regan 1998). In these cases, intervention is undertaken by an unspecified state in response to concerns that the civil conflict may lead to state failure, regional destabilization, or some other disruption in the regular workings of international affairs. Using information such as the number of battle deaths, the duration of a conflict, and the time period (e.g. during the Cold War or not) during which the conflict took place, scholars have shown that phenomena-focused explanations of intervention can indeed provide some insight into which conflicts third-parties will become involved in.

In contrast to phenomena-centric approaches, more recent work has focused on incorporating “actor-centric" explanations of intervention. In these models, motivations for intervention are derived not only from attributes of the conflict under study, but also from the specific attributes of potential intervenors and their connections to the country experiencing conflict. The core insight from these models is that intervention is inherently not a monadic event and that by examining the attributes of potential intervenors one can specify which specific states are more likely to intervene. Empirical scholarship using this approach has argued that third-parties are often motivated to intervene in a conflict due to self-interested concerns about the negative impact the conflict can have on their own national interest. For example, states have been shown to be more likely to intervene in civil conflicts afflicting one of their allies, presumably in order to ensure the survival of a valuable ally. Similarly, scholar have argued that states involved in an international rivalry with the conflict state are likely to intervene on behalf of the opposition group in an attempt to destabilize the rival country.

Perhaps the most consistent finding across this later strand of literature concerns the
general classes of variables which are said to influence intervention behavior. For the most part, these two classes can be defined theoretically as those concerning “power” and those concerning “interest.” Power variables are just what they sound like; measures of major power status and a higher level of relative capabilities are both strongly associated with intervention behavior. The logic behind these findings has consistently been that states become involved in foreign civil conflict when they are actually capable of directly influencing the outcome of the conflict through their military might or vast wealth of resources (Balch-Lindsay and Enterline 2000; Findley and Teo 2006; Gent 2008; Kathman 2011).

In contrast to power variables, the importance of interest variables has been less clearly established. This is not to say that understanding why a state would intervene in a conflict is has a strategic interest in is difficult - surely states seek to protect their interests and may use interventions as a means to do so. The issue is instead with determining which of a host of potential determinants of interest are actually significantly associated with intervention behavior. For example, while the presence of an alliance between a third party and a conflict actor is significantly associated with intervention, shared democratic institutions are only sometimes found to drive intervention behavior. Similarly conflict intensity, measured in a variety of different ways, is often found to be unassociated with third-party behavior, even though such measures of intensity are often thought to capture the conflict’s “spillover potential.”

To a large degree, the previous approaches dominate the study of third-party involvement in civil war. Even when scholars do not explicitly invoke the terminology used above, they often separate the variables in their theoretical and empirical models into multiple sections roughly corresponding to the above scheme. That being said, the existing scholarship on refugees and intervention is less clear about whether refugees should be considered from an actor or phenomena-centric approach. Specifically, while it is clear that refugees help determine a third-party’s “interest” in intervention, it is unclear what motivates this interest.
Before outlining my own (actor-centric) model of refugees and third-party support, I will detail the existing literature with a focus on the disconnect between the proposed theoretic motivations for intervention and the empirical assessments performed.

### 4.1.2 Refugees and Third-Party Behavior

The treatment of refugees as a determinant of intervention behavior has varied throughout the past few decades. The earliest empirical work on the topic suggested a relationship wherein altruistic third-parties would feel motivated to intervene when the total refugee outflows reached some threshold signaling a humanitarian crisis. While this view was supported by anecdotal evidence about intervention behavior and humanitarian suffering, empirical examination provided mixed results. For example, Regan (1998) argued explicitly that humanitarian concerns were a clear motivator of direct, military intervention in civil conflict. Using United Nations High Commissioner for Refugees (UNHCR) data on refugee flows, Regan (1998) constructs an binary indicator of a humanitarian crisis and shows a positive link between the existence of a refugee crisis and military intervention.\(^2\) This logic of humanitarian intervention is repeated in subsequent studies using different estimation strategies and a host of other control variables (Regan 1998; Lemke and Regan 2004; Gent 2008). Generally, these studies proffer mixed results on the link between refugee outflows and intervention, with different empirical models alternating between showcasing a significant or insignificant relationship.

\(^2\)Though this link is not statistically significant.

In contrast to models highlighting the humanitarian crisis explanation, a small set of theoretical models and case studies detail a impetus for intervention based on the economic and security strains placed on host states by refugees. The clearest statement of this viewpoint is found in Dowty and Loescher (1996) who present a case that the presence of large refugee flows should be considered legitimate grounds for foreign intervention. Through small-scale
case studies and an examination of international laws, the authors argue that intervention to prevent or stop refugee flows should be (in a legal sense) and is (in a practical sense) a central component of the intervention decision. In this framework, flow-mitigating interventions are to be undertaken both by host states and by other third-party actors in order to mitigate a host of security concerns (Zolberg, Suhrke and Aguayo 1989; Salehyan 2014).

This later framework is perhaps the one most commonly adopted in the theoretical literature today. Numerous manuscripts in the academic and policy spheres have recently detailed the economic and security dangers associated with hosting refugees. Building on these findings, scholars of intervention have seemingly accepted that intervention in the face of refugee flows should be common.\(^3\) This perspective on refugees and intervention is perhaps most clearly articulated in Findley and Teo (2006) where a measure of the number of refugees emanating from a conflict is seen as an important indicator of conflict intensity and the likelihood that the potential intervenor will be negatively impacted by said conflict. This theoretical justification is the closest to the one employed in this project; refugees create problems for hosts and intervention can be a method to stop refugee flows and thus mitigate these problems. What makes the current project unique however is the direct evaluation of this argument in an actor-centric perspective wherein the act of actually hosting refugees is the driver of the decision to intervene.

4.2 Host State Self Interest and Intervention Behavior

The basic assumption underlying my model linking refugees to conflict intervention and third-party support is that one primary motive for intervention is the presence of a clear and immediate security concern. This simple assumption helps us make sense of many existing

\(^3\)Indeed, while I have not conducted a formal survey, my review of the literature indicates that a “Refugees” variable is almost always included in models of intervention. This pattern of consistent usage underscores the acceptance of refugees as closely linked to intervention motivations.
findings in the intervention literature. Under this framework, alliances with conflict actors signal a clear danger of the loss of an ally and a reduction in the third-party’s power, while the presence of shared democratic institutions does not necessarily create such an immediate concern. This perspective also explains the behavior of conflict-bordering third-parties - who fear a direct conflict spillover - as well as the limited role of increasing battle deaths - which, in themselves, provide only limited information about the potential for spillover. In addition to helping clarify existing findings, this framework suggests a particular link between refugees and intervention wherein *actually hosting refugees* produces such a security concern; while the fact that a conflict is producing many refugees does not. To understand why hosting refugees creates such a concern, and thus motivates intervention, we must examine the literature linking refugees to negative, and often violent, outcomes in the states that host them.

Refugees, though clearly the product of victimization, are also directly connected to negative, and sometimes violent, outcomes in the countries which host them. Beginning with Zolberg, Suhrke and Aguayo (1989) scholars have increasingly recognized the risks host states face when they accept large numbers of refugees (Salehyan 2014). Research has linked the act of hosting refugees to an increased risk of civil conflict onset, terror attacks, interstate disputes and a variety of forms of lower-level violence including protests and riots (Salehyan and Gleditsch 2006; Salehyan 2008; Choi and Salehyan 2013; Milton, Spencer and Findley 2013; Onoma 2013). Within this literature, various mechanisms, including upsetting the “ethnic balance," creating an economic strain through resource and employment competition, and militarization by rebel groups have all been detailed as potential causes of refugee-related violence in asylum countries (Salehyan and Gleditsch 2006; Lischer 2006; Krcmaric 2014). Indeed, the analyses of the previous chapter build on this final mechanism and provide evidence that certain conditions can lead to militant groups utilizing refugees and refugee camps to increase their warfighting capabilities and can ultimately lead to the spread of conflict to refugee-hosting states. While the model I present makes clear that
refugee militarization is the result of a specific set of circumstances - and will thus be relatively rare - the broader concern that hosting refugees may lead to the spread of violence is well documented in both academic and policy circles as is likely to be a major concern among refugee-hosting states (Salehyan 2014; Frum 2015). Overall, the evidence I present, combined with the results from previous studies, offers substantial support for the idea that hosting refugees can adversely affect the overall level of security in a states. As a result, it is clear that host states, recognizing this fact, may be compelled to take action to reduce the threat and ensure their security.

Given these security concerns, it is clear that states that host, or are likely to host, a large number of refugees face serious policy questions about how to respond to a refugee crises in a way that ensures their own security. The argument I set forth in this project is that direct involvement in the source conflict is one method through which host states can mitigate the security concerns associated with hosting refugees. While I focus on intervention and support as a response to hosting refugees, it is clear that hosting refugees can motivate a variety of other behaviors on the part of host states. In addition to direct involvement in the conflict, border closings, quotas, and anti-refugee legislation all come to mind as methods by which host states can limit the negative impact of refugees. The critical question that remains to be answered is why exactly the intervention option is pursued in spite of its obvious costs (e.g. in lives and resources). While this option may not seem viable at first, several consideration about conflict intervention and its alternatives will help explain why intervention may in fact be a preferable policy choice.

The primary reason why intervention, despite all of its potential costs, can be seen as a viable and perhaps preferable policy choice is that intervention does seem to “work” in the sense that it causes wars to terminate faster than would be expected without intervention (Balch-Lindsay and Enterline 2000; Gent 2008). While there exists some debate about
precisely when intervention is effective (i.e. whether intervention on the side of the conflict state is as effective as intervention on the side of rebel groups), the existing research makes clear that many states undertake intervention and provide other forms of support because the historical record indicates it can be a useful tool for ending conflicts in a favorable manner.

Since most refugees are directly fleeing from the violence associated with civil wars, it makes sense then to argue that interventions that succeed in ending conflicts will have the additional effect of reducing, or even reversing, the flow of refugees into a host country. In this way, intervention is an obvious policy choice because it directly alleviates the security concerns associated with hosting refugees by either slowing the influx (facilitating adaptation to the stresses of hosting refugees) or by ultimately removing the refugees from the host state completely (in which case states face no added security threats). Indeed, Salehyan and Gleditsch (2006) argue that the risks of hosting refugees increase directly with the number actually hosted. Thus, if intervention does in fact result in the rapid end of a conflict, it is a clearly viable policy choice for states seeking to mitigate the threats posed by hosting refugees. In this situation, states can, through ending the conflict, directly reduce the number of refugees in their country and, potentially, prevent future influxes, thereby directly reducing their own security risks.

Aside from being effective, intervention and support also provides a method of helping to stem the tide of refugees without running the risk of radicalizing the existing refugee population and thus inadvertently producing the very security crisis one sought to avoid. For example, while border closures can help prevent refugees from entering, and potentially destabilizing, a country, they can also lead to illegal migration, the “back-up” of refugees along the border in squalid camps, and widespread resentment towards the state among refugees who have already crossed the border. As a consequence of these events, migrants both inside of and on the border of the third-party state may eventually become violent as
they try to force their way across the border or protest for better treatment. In contrast, intervention by itself does not run any of these risks and instead simply aims to bring the conflict to a close and lead to the repatriation of the refugees. Thus, states can eventually alleviate the security concerns surrounding refugees without applying political pressures that can ultimately exacerbate such concerns.

Taking the comparison between intervention and alternative policy options even further, it is also true that conflict intervention does not necessarily require massive commitments in terms of men or material to be effective. Specifically, since intervention behaviors run a broad gamut from providing military forces and bases to supplying intelligence and funds, it is conceivable that states can pursue intervention at relatively low costs. Because of this, it is not obvious that potential intervenors will always have cheaper and more effective policy options suited to handle the security concerns refugees create. As a result, I argue that intervention, defined in this way, is at less of a comparative disadvantage than one might first presume when comparing it to alternative policies.

In addition to the above ideas, we can also consider how domestic audiences within the host country may actually push for some form of intervention after refugees arrive in their country. Indeed, if intervention is regarded as (a) historically effective at ending conflict, (b) capable of stemming the influx of refugees, and (c) relatively low cost when pursued in a particular way, it may be the case that domestic audiences would advocate for intervention so that the host state “does something” to prevent the consequences of refugee flows. Citizens or interest groups may press for intervention for a number of reasons including to alleviate the security concerns noted above, to mitigate the potential economic burden refugees can place on local communities, or even to indicate that, if the state does not take action, the civilians may resort to violent action to expel or marginalize the refugees. All of these dynamics are

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4Indeed, one could argue that, in some cases, states might undertake intervention in an effort to prevent the destabilization that would follow from their own citizens attacking hosted refugees. In this way, intervention
well-documented in the existing literature on refugees and all, at some level, can be linked to the overall security threat that refugees place on host states (Jacobsen 2002; Salehyan and Gleditsch 2006; Onoma 2013). Thus, while intervention may often by motivated by security concerns by elite decision-makers we can see that it is also possible for citizens and other domestic groups to play a role in motivating intervention.

Finally, note that while I focus on intervention, nothing in my theoretical model holds that intervention cannot be undertaken in conjunction with other policy options if a state sees reason to do so. Such an approach may in fact be the most common response of states faced with large refugee populations. For example, states pursuing this strategy can focus on limiting the immediate dangers of refugee populations by instituting strict quotas while simultaneously working to stop the flows at their source by intervening in the conflict. In this way, third-party intervention seems likely not because it is the only or best option states have, but because it is a useful one which, when combined with other strategies, can holistically address the security concerns which refugees present.

As a result of these considerations, it is clear that we should expect a link between refugees and conflict intervention. Most notably, it should be clear that refugee populations should most directly impact the states who are actually hosting them. While generic concerns about regional destabilization, humanitarian crises, and the economic costs of providing humanitarian aide to refugee-burdened countries may exist, such concerns do not present clear security threats. As such, to truly understand how refugees matter as a determinant of conflict intervention and the support of civil conflict actors, one must examine refugees from a definitively "actor-centric" perspective. In this framework, it is clear that hosted refugees is still motivated by security concerns surrounding refugees but these concerns arise in a more indirect way not directly associated with the potential for refugees themselves to become violent. Such an argument is difficult to address empirically given how little we know about the actual instigators of refugee-related violence but nevertheless demonstrates that intervention can be motivate from a variety of pressures all ultimately associated with host state security.
are an "attribute" of potential intervenors which drives these states to favor intervention. This approach motivates the primary hypothesis of the current project:

**Hypothesis 1:** The more refugees a country hosts from a state currently involved in a civil conflict, the more likely it is to intervene in some manner in said conflict.

The expectation set forth in Hypothesis 1 varies from existing scholarship on refugees and intervention in several ways. First, as noted previously, I do not restrict my examination of support to the direct provision of military forces. In my model, hosting refugees does not necessarily bind a state to intervene using their own military forces; these countries can supply weapons, training, intelligence and a host of other militarily useful resources to conflict actors in an attempt to facilitate the end of the conflict. Second, Hypothesis 1 also deviates from the existing literature by specifying the specific type of refugees that matter most for third party intervention: those hosted by the third-party state. While the existing literature consistently employs "phenomena-centric" measures of refugees (e.g., a binary indicator of whether the conflict has produced 50,000 refugees), this study focuses on the way refugees directly impact host states and drive up the likelihood of intervention - an approach necessitating a more actor-focused approach. Finally, it is worth clarifying that Hypothesis 1 does not offer an expectation about the "side" taken by the intervenor. Indeed, while some models separate factors that lead to intervention on the side of rebel groups from those that lead to intervention favoring the conflict state, my model makes no such prediction.5

The general theoretical model of the impact of refugees noted above makes clear that hosting significant refugee populations serves to sensitize host countries to the potential costs of the associated civil war and prompts action to mitigate these costs. Two additional

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5I would argue that while it may be possible to predict the "side" of an intervention from refugee flows using more information about the composition or perhaps timing of the flows, doing so from aggregate numbers alone would be difficult.
considerations are critical here. First, the marginal impact of refugees should be decreasing as the number hosted rises since the change in security implications from hosting 0 refugees to 1000 refugees is much larger than the change caused by moving from 10000 to 11000. This decreasing marginal impact is traditionally captured by taking the logarithm of whichever refugee measure is employed in a given study - a tradition I continue in my own subsequent empirical examination. The logic behind this assertion is that the security risks of hosting refugees are driven not simply by the large numbers of refugees hosted, but by the mere presence of such an aggrieved and politically vulnerable population. Subsequently, the marginal impact on the likelihood of intervention by a third party should be less at higher levels of refugees simply because the host state is already aware of the risks it faces and is already inclined to pursue some type of intervention.

This same line of reasoning suggests more than simply a specific form of the influence of refugees; it implies a model of the impact of refugee hosting wherein states which already face substantial security concerns resulting from another state’s civil conflict will be less impacted by hosting refugees than those which otherwise would have no immediate security concerns. This second implication is critical to understanding the next set of hypotheses. Essentially, I argue that the impact of refugees on intervention is largest when no other security factor has already brought intervention, or some other form of involvement, to the forefront of the third party’s priorities. For example, states which border a conflict zone already face security implications involving, among other factors, terrorist attacks, cross-border raids, and the incitement of popular uprisings in their own country. For these states, refugees do not make the nearby conflict significantly more salient as the probability of in-

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6 Both the natural and base-10 logarithm are used in such studies and this usage pattern goes beyond models of intervention to, for instance, models of civil conflict in hosts countries. It is worthwhile to note that few studies ever appeal directly to this theoretical reasoning to justify using the logarithm but instead often simply take the logarithm to accommodate the skew of the refugees measure. While there is nothing inherently wrong with this justification, I believe that ignoring potential theoretical justifications inhibits one’s ability to draw out additional hypotheses and theoretical refinements; as evidenced by my interactive hypotheses.
tervention is already high and the presence or absence of refugees does not meaningfully increase the overall risk the third-party state faces. In contrast, non-contiguous states are often largely unaffected by ongoing conflicts and, when faced with large refugee inflows, are thrust into a high-risk situation that they must respond to. For these states, the impact of hosting refugee populations is considerable and increases their likelihood of intervention to a significant degree above its previous, virtually non-existent, risk (due to a lack of security threat). This conditional relationship motivates Hypothesis 2:

**Hypothesis 2:** The impact of refugees on potential intervenors will be greater for non-contiguous states than for contiguous ones.

Continuing to pursue this logic, I also argue that shared ethnic ties can create a similar situation for potential intervenors. When states share ethnic connections to a state in conflict they are faced with either direct concerns about supporting those with shared ethnic ties or preventing domestic uprisings from similarly oppressed groups. For these states, intervention to prevent either of these outcomes will likely appear especially attractive and hosting a large refugee population is unlikely to have a substantial impact on this consideration. Even further, in some cases, providing refuge to fleeing members of one’s own ethnic group may even be seen as an important step in ensuring your own group’s survival. In these cases as well, hosting increasing numbers of refugees is unlikely to significantly impact the prospects of involvement in the civil war as such considerations are dominated simply by the shared ethnic tie and the various associated motivations for intervention. Again, this all contrasts to states which do not share ethnic ties to the state in conflict. For these third-party actors, the ongoing civil conflict has little relevance until they begin to experience refugee inflows and the associated security side-effects of the conflict. Empirically, we should expect these state to be much more influenced by the size of the hosted population - an expectation formalized
in Hypothesis 3:

\textit{Hypothesis 3: The impact of refugees on potential intervenors will be greater for states not sharing ethnic ties to a conflict country than for those sharing such ties.}

4.3 Data and Methods

The theoretical models and hypotheses set forth thus far readily lend themselves to empirical evaluation. Indeed, data on third-party behavior and refugees has become increasingly fine-grained over the past several years and we are increasingly able to ask specific questions on these topics. As noted above, while existing empirical research has broached the topic of refugees and intervention, no analysis exists that can answer the specific, “actor-centric” relationships proposed in this project. Thus, the next section outlines the data sources and estimators I use to evaluate my hypotheses and ultimately provides a discussion of my findings.

Before turning to a discussion of the variables and methods, I will note right away that the present study is limited to African conflicts and African intervenors. While the variables used in my analysis are in fact available for all states, I have chosen to restrict my analysis to African countries for two primary reasons.\footnote{A third reason for limiting the sample is simply due to computational constraints. While it is possible to gather the data and run analyses using a more universal set of cases, doing so takes much longer.} First, the refugee data used for this analysis, while available for all countries starting around 1960, is less reliable further back in time and outside of Africa. This may seem odd at first, as it would seem to be easier to document refugees outside of conflict-prone Africa, but follows from the increased attention accorded by the UNHCR and other organizations to African refugees. Indeed, a cursory look at the data suggests that in earlier years and in countries outside of Africa the specificity of the refugee
data is correlated with the severity of the flows and the related conflict. Thus, in order to use only the most accurate data, I rely on the more limited African sample. In addition to concerns about the accuracy of the data used, I also restrict my analysis to African states in order to focus on a sample of states where intervention is always available as a policy response on a practical level. If, as an alternative, I were to use all countries in my analysis I would need to clearly differentiate, using my control variables or a particular estimation strategy, those states for which intervention is not a practical possibility. While doing so may be possible, I believe using a restricted sample is sufficient for the current analysis.

Overall, while restricting my analyses to African states has some drawbacks, there are a number of reasons why doing so does not significantly reduce the quality or applicability of my findings. First, with respect to the generalizability of my findings, it is most certainly the case that the conclusions I draw can inform our understanding of and decisions about African states in the future. Given how tumultuous the continent has been historically, the chances that conflicts, refugees, and interventions will not be prominent in Africa going forward are low and the ability to use current work to meet future challenges is of paramount importance. Additionally there is little in my theoretical model to indicate that any relationships I find will not operate outside of the African context. Any area subject to frequent civil conflicts and refugee flows would be theoretically equivalent to Africa under my theoretical model and would be subject to the same theoretical expectations. Indeed, as previously noted, the primary reason why I limit the sample to African states has nothing to due with the theoretical argument and everything to do with the quality and reliability of the data used in the empirical analyses. In this sense, generalizing to other geographic contexts is straightforward and can be done with few reservations about the risk that the effects I describe are strictly limited to the sample I analyze.

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8This concern is similar to that seen literature on the deterrent effect of alliances where only states which are likely to actually target an alliance member are included as potential challengers. In this literature, various strategies including the use of politically-relevant dyads or other sample restrictions are employed to avoid grouping together cases where the likelihood of a challenge is essentially zero.
4.3.1 The Dependent Variable

To evaluate the three hypotheses above I require data on involvement in civil conflicts by third-party actors as well as information on how many refugees these third-parties host from the state in conflict. As noted before, this project considers intervention in a broad sense such that a third-party is considered an intervenor if they directly provide military forces (i.e. soldiers) as well as if they merely provide military resources (e.g. weapons, funds, intelligence). Further, intervention in this study can be on the side of the government in conflict or the rebel group opposing them. The reasoning behind this level of generality is derived from the model posited above. Given that intervention is undertaken in order to protect a state’s interest and, I argue, to mitigate concerns about hosting refugees, states are not limited to a single form of involvement and can choose the support strategy that they believe will be most productive.

As a result of the above considerations, the dependent variable for this study is constructed using the UCDP External Support dataset in combination with a dyad-year dataset of all states in Africa and codes whether a support link exists between a third-party and any actor in the conflict. The dataset is structured such that every African civil conflict in the period under study is included in the data with one observation each for every possible third-party intervenor. If the potential intervenor has provided support (regardless of its form) the dyad is coded as having experienced intervention (i.e. the third-party has provided some form of external support to one of the conflict actors). This yields a dyad-year dataset providing a binary indicator of whether a third-party has intervened in a civil conflict.\(^9\)

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\(^9\)To be clear, the support must be targeted at increasing military capabilities. Thus, development aid or investment that occurs during conflict years is not counted as a form of intervention.

\(^10\)Note that I only examine dyad-years in which one of the members is engaged in civil conflict, that is, I do not include pairs where no civil war was ongoing as intervention is not possible in such cases.
4.3.2 The Independent Variables

The key questions proposed in this project require measures of the number of refugees a country hosts from a conflict zone as well as information on the contiguity of the third-parties and conflict states and the ethnic ties the two states share. This data is available from separate datasets and can be combined into the dyadic form required with some adjustments. For example, I utilize UNHCR data on refugee populations that provides information on the size of a refugee population in a particular state as well as where these refugees originated from. This data is easily aggregated into a dyad-year framework and provides the key independent variable for this study. In particular, for a given potential-intervenor/conflict-state pair, I have a record of the number of refugees from the conflict state hosted by the potential intervenor. When using this variable in my analyses I take the natural logarithm to account for both the empirical skew in the variable as well as the theoretical observation that the marginal impact of refugees decreases at higher levels.

In addition to the refugees measure I also have information about the contiguity of states as well as whether they share ethnic ties. Contiguity data is drawn from both the COW Direct Contiguity dataset and the CShapes dataset. While the COW data provides a 5-category indicator of the degree of contiguity between two states (ranging from direct land contiguity to separation by up to 400 miles of water) the CShapes data allows for the calculation of absolute minimum distance between two states and offers more precise estimates.\footnote{Minimum distances were calculated using R and the \textit{cshapes} package.} I will use both indicators to check the robustness of my model.

Finally, to establish whether two countries share ethnic groups, I use the TEK dataset and simply code whether the potential intervenor has any trans-border ethnic kin living in the conflict state. This provides me with a binary indicator of shared ethnic ties. Though I currently cannot be sure whether the refugees in question are part of this shared group,
or not, my theoretical model does not strictly depend on this being the case. Recall from the theoretical model that shared ethnic ties may lead third-parties to intervene in order to protect a preferred group or to prevent an unfavored group from entering their country. In either case, involvement of the third party is likely and the interaction with refugee flows (wherein refugee populations matter less when countries share ethnic ties) should hold either way.

In addition to the above key independent variables I also record information about both the conflict under examination and the potential intervenor including data on battle deaths and the size of the conflict, and the strength of democratic institutions in the potential intervenor.\textsuperscript{12} In addition I also include a variable measuring the (logged) number of refugees the conflict has produced in an effort to evaluate the usefulness of the "humanitarian crises" arguments previously discussed. Finally, in an attempt to model the temporal dependency in the intervention process I include cubic polynomials of time at risk for intervention as control variables.\textsuperscript{13} Table 1 provides summary statistics for all of the variables used in the model.

\subsection*{4.3.3 Results and Interpretation}

To examine the relationship between refugees and intervention in detail, I use a variety of regression and graphical representations. Before moving to more complicated models however, it is fruitful to simply examine the bivariate relationship between the two measures. Figure 1 graphs the refugee measure against the binary indicator of intervention and displays a fitted logistic curve modeling the association. One can easily see from the graph that there does appear to be a relationship between the two concepts such that increasing the number of refugees a country hosts increases the probability that the country involves itself in the

\footnote{I use the UCDP Battle-Related Deaths dataset as well as the PRIO Conflict Site data for the former and the Polity project for the later.}

\footnote{To be more specific, the first-order time at risk variable begins counting when a conflict starts and will continue counting until either intervention takes place or the conflict ends. This allows us to determine, for instance, if it is true that states only intervene in a conflict after it has been ongoing for several years.}
Table 1: Descriptive Statistics of Key Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>N</th>
<th>Minimum</th>
<th>Mean</th>
<th>Median</th>
<th>Maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hosted Refugees_{log,lag}</td>
<td>10,306</td>
<td>0</td>
<td>1.193</td>
<td>0</td>
<td>13.797</td>
</tr>
<tr>
<td>Support Provided</td>
<td>10,306</td>
<td>0</td>
<td>0.009</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Binary Contiguity</td>
<td>10,306</td>
<td>0</td>
<td>0.091</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Continuous Contiguity_{log}</td>
<td>10,306</td>
<td>0</td>
<td>6.935</td>
<td>7.649</td>
<td>9.053</td>
</tr>
<tr>
<td>TEK</td>
<td>10,306</td>
<td>0</td>
<td>0.111</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Battle Deaths</td>
<td>10,306</td>
<td>0</td>
<td>1,478.080</td>
<td>142</td>
<td>36,250</td>
</tr>
<tr>
<td>Polity</td>
<td>10,306</td>
<td>−10</td>
<td>−495</td>
<td>−1</td>
<td>10</td>
</tr>
<tr>
<td>Polity^2</td>
<td>10,306</td>
<td>0</td>
<td>31.783</td>
<td>25</td>
<td>100</td>
</tr>
<tr>
<td>Relative Scope</td>
<td>10,306</td>
<td>0.002</td>
<td>0.423</td>
<td>0.309</td>
<td>0.996</td>
</tr>
<tr>
<td>TAR</td>
<td>10,306</td>
<td>1</td>
<td>4.611</td>
<td>3</td>
<td>20</td>
</tr>
<tr>
<td>TAR^2</td>
<td>10,306</td>
<td>1</td>
<td>39.932</td>
<td>9</td>
<td>400</td>
</tr>
<tr>
<td>TAR^3</td>
<td>10,306</td>
<td>1</td>
<td>473.351</td>
<td>27</td>
<td>8,000</td>
</tr>
<tr>
<td>RGDPPC</td>
<td>10,306</td>
<td>132.820</td>
<td>2,291.896</td>
<td>1,245.580</td>
<td>20,259.400</td>
</tr>
<tr>
<td>Total Refugees_{log}</td>
<td>10,306</td>
<td>0.693</td>
<td>10.934</td>
<td>11.605</td>
<td>14.630</td>
</tr>
</tbody>
</table>

Notes: TEK = Trans-Border Ethnic Kin; TAR = Time At Risk; RGDPPC = Real Gross Domestic Product Per Capita; Battle Deaths, Population, and RGDPPC are unlogged.

conflict from which the refugees originate. This relationship can also be seen in Column 1 of Table 2 which reports the coefficient estimates for the bivariate logistic regression.

While the above result is suggestive of a relationship between hosted refugees and intervention, the inclusion of the control variables noted above will help us avoid attributing undue influence to hosting refugees and help us parse out the specific impact of refugee hosting in conjunction with the other theoretically relevant variables. Column 2 of Table 2 reports the results of the logistic regression examining the link between refugees and intervention in the face of the control variables. Consistent with my theoretical expectations, the results of this second model also suggest a positive relationship between hosting refugees and intervention.\(^{14}\)

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\(^{14}\text{This relationship is also supported by a regression model controlling for the interaction terms I focus on later. In these models, the main effect of refugees still remains positive and significant - though at a greatly diminished magnitude. This is to be expected given my argument that these other variables essentially determine whether the refugees measure has any effect at all.}\)
Figure 1: Bivariate Relationship Between Hosted Refugees and Intervention
<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Support Provided</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Hosted Refugees log, lag</td>
<td>0.341***</td>
</tr>
<tr>
<td>(0.014)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Binary Contiguity</td>
<td>3.070***</td>
</tr>
<tr>
<td>Continuous Contiguity log</td>
<td>-0.436***</td>
</tr>
<tr>
<td>TEK</td>
<td>2.677***</td>
</tr>
<tr>
<td>Battle Deaths</td>
<td>0.00004***</td>
</tr>
<tr>
<td>Relative Scope</td>
<td>0.653*</td>
</tr>
<tr>
<td>Polity</td>
<td>-0.117***</td>
</tr>
<tr>
<td>Polity^2</td>
<td>-0.001***</td>
</tr>
<tr>
<td>Population log</td>
<td>0.214**</td>
</tr>
<tr>
<td>RGDPPC log</td>
<td>0.150</td>
</tr>
<tr>
<td>TAR</td>
<td>-1.025***</td>
</tr>
<tr>
<td>TAR^2</td>
<td>0.127***</td>
</tr>
<tr>
<td>TAR^3</td>
<td>-0.004**</td>
</tr>
<tr>
<td>Total Refugees log</td>
<td>-0.107***</td>
</tr>
<tr>
<td>Hosted Refugees:Binary Contiguity</td>
<td>-0.281***</td>
</tr>
<tr>
<td>Hosted Refugees:Continuous Contiguity</td>
<td>0.040***</td>
</tr>
<tr>
<td>Hosted Refugees:TEK</td>
<td></td>
</tr>
<tr>
<td>Akaike Inf. Crit.</td>
<td>1,947.602</td>
</tr>
</tbody>
</table>

Note: *p<0.1; **p<0.05; ***p<0.01; Standard errors of terms of substantive interest in parentheses

Table 2: Refugees and the Probability of Intervention
Though Table 2 is clear about the significance and direction of the relationship, a clearer conception of the specific impact of refugees on the probability of intervention can be gained by calculating the predicted probabilities of intervention at multiple levels of hosted refugees. Further, since I incorporated the time polynomials into my regression, it will be possible to show how a discrete change in the number of refugees hosted impacts the probability of intervention across time.\textsuperscript{15} Figure 2 provides graphic representations of these effects.

![Figure 2: Impact of Number of Refugees Hosted](image)

Figure 2: Impact of Number of Refugees Hosted. Panel A displays the predicted probability of intervention across levels of hosted refugees with all other values held at their median or mean. Panel B displays the predicted probability of intervention across time (0-20 years) when the number of hosted refugees is 0 (solid) and 1000 (dashed), with all other values at their median or mean.

Figure 2 provides two different representations of the effects of refugees on intervention. First, Panel A of 2 provides a simple look at the predicted probability of intervention across the range of the hosted refugee variable. Most notably, this graph provides clear support

\textsuperscript{15}This approach is similar to examining the impact of a covariate on the hazard rate in a survival model (e.g. a Cox model).
for Hypothesis 1 and demonstrates that the more refugees a country hosts, the more likely it is to intervene. Panel B takes this comparison further by examining how the probability of intervention changes across time. Specifically, Panel B plots the predicted probability of intervention across time (from 1 to 20 years of conflict) when the potential intervenor hosts zero refugees and when it hosts 1000 refugees.\textsuperscript{16}

Panel B of Figure 2 provides us with two interesting pieces of information. First, we can see that the likelihood of intervention is non-monotonic in time with the probability of intervention declining from a peak at the start of a conflict before rising to another peak after 10 years of ongoing conflict. While not the focus of the current study, this finding is telling about the drivers behind intervention as they suggest that by and large intervention is either near-immediate or delayed until after the conflict has proven intractable. In some ways, this finding is entirely consistent with the underlying logic of the model of intervention I posit above: states are, roughly speaking, either immediately motivated to intervene due to readily apparent concerns for their own interest or are “drawn in,” after some delay, as the potential consequences of the ongoing conflict grow and threaten to negatively impact the potential intervenor.

Beyond the general time trend, we can also clearly see how refugees can impact intervention behavior. Most obviously we see that, given the change from 0 to 1000 refugees hosted, the probability of intervention jumps considerably. While intervention is indeed a rare event and, as a result, the overall probability of any particular state intervening is rather low, hosting a large number of refugees has the potential to double or even triple the likelihood of intervention (depending on the values of the other covariates). Additionally, while Panel B displays predicted probabilities for 0 and 1000 refugees, historically the number of refugees hosted in a state has been seen to vary from zero to over one million. As a result, it should

\textsuperscript{16}All other variables in the model are held at their mean value for this calculation.
be clear that, when a major refugee crisis occurs, the probability that a third-party state becomes directly involved can rise drastically.

While the preceding results are generally supportive of Hypothesis 1 and a link between hosting refugees and intervention, these results have all focused explicitly on the unconditional impact of hosting refugee on the likelihood that a country intervenes. Hypotheses 2 and 3 however argue that the link between refugees and intervention is conditioned by other motivations for intervention. Specifically, when features of third-party states - such as contiguity (Hypothesis 2) or shared ethnic ties with a conflict state (Hypothesis 3) - create pre-existing security threats that motivate intervention the impact of hosting more refugees will be mitigated when compared to states that do not have such features. Columns 3-5 of Table 2 evaluate these expectations using data on contiguity (binary measure in Column 3, continuous measure in Column 4) and information about the presence of trans-border ethnic kin (Column 5).

In line with my expectations, Columns 3-5 of Table 2 indicates that the interactions between the number of hosted refugees and each of my proposed indicators of pre-existing security threats are significant. For example, the Hosted Refugees term in Column 3 indicates that, when states are non-contiguous, the impact of hosting more refugees is positive and statistically significant. In addition, the interaction term indicates that when states are contiguous this effect is moved towards zero (i.e. no effect).\footnote{Note that Table 2 only indicates that the impact of Hosted Refugees is less when states are contiguous. We cannot tell from these results alone whether the effect of refugees becomes statistically indistinguishable from zero when states are contiguous; a graphical representation will be better for addressing this question.} A similar effect can be seen using the continuous measure of contiguity. In this case, due to the coding of the contiguity variable, the Hosted Refugees term indicates that, when states are contiguous (i.e. the minimum distance between them is 0), hosting more refugees has no effect on the probability of intervention, while, as the distance between the two states rises, so too does the impact of
hosting refugees. Finally, a similar effect is clear in Column 5 where states without shared ethnic kin experience a positive and statistically significant increase in their probability of intervention as they host more and more refugees. Further, as before this effect is mitigated by the presence of ethnic kin such that refugees may no longer impact the probability of intervention at all when ethnic kin are present.

As with the unconditional results, the effects noted in Columns 3-5 of Table 2 can be better understood using the graphical depictions in Figures 3 and 4. Panel A of Figure 3 displays the predicted probability of intervention across the number of refugees hosted when the states in question are non-contiguous (right panel) and when they are contiguous (left panel). These graphs are particularly telling in that they directly show that the impact of refugees on intervention is statistically insignificant when states are contiguous. This result is supportive of Hypothesis 2 and is largely mirrored in Panel B of Table 3 which shows the impact of refugees at varying distances between states. While the results from Panel B are somewhat less clear than those of Panel A (a product of the continuous nature of the interaction), they are still largely supportive of the idea that refugees have the strongest impact when states are non-contiguous (the top right frame) and have essentially no impact when states are contiguous (the bottom left frame).

Finally, to better evaluate Hypothesis 3, Figure 4 displays the predicted probability of intervention across the number of refugees hosted for the case when the two states in question do not share ethnic ties and when they do. Again, consistent with Hypothesis 3, the graph indicates that, when a potential intervenor does not share ethnic ties with a conflict state (the left frame), hosting more refugees significantly increases the probability of intervention. In contrast, when third-party states already have ethnic ties to a conflict state, hosting more and more refugees has a much smaller impact on the probability of intervention.\textsuperscript{18}

\textsuperscript{18}Note that we cannot say that refugees have no impact when states shares ethnic ties. This is not a problem as I do not specify that the relationship will disappear; simply that it will become smaller.
Figure 3

Refugees and (binary) Contiguity

Refugees and (continuous) Contiguity
4.4 Discussion of Results

Altogether, the results from the regression models and the associated graphical representations all provide support for my three hypotheses and indicate that, in general, the act of hosting refugees from a conflict country leads to a significant increase in the probability that the host will become involved in the source conflict. This effect, while moderated by other variables, is consistent and, viewed through the lens of my model, indicates that the security threats refugees create for host countries are sufficient to motivate intervention by hosts. While aspects of the argument presented in this project can be found in other research, two aspects of these findings are particularly notable.
First, these findings stand in stark contrast to the argument that third-party intervention is motivated by altruistic concerns connected to the number of refugees a conflict has produced. Indeed, while this later argument is the one most commonly assessed in the literature, I find no connection between the number of refugees a conflict has produced and the likelihood that a country intervenes in the conflict. Indeed, most of my models include a term accounting for the (logged) total number of refugees a conflict has produced. In most cases the effect associated with this term is not statistically significant and, when it is significant, is negative. These results indicate that, at the very least, we can conclude that the proposed link between intervention and the total number of refugees produced is inconsistent when considered in light of the number of refugees a country hosts. In a similar way, these results offer substantial evidence that models based on altruistic motivations for intervention are suspect.

In addition to offering evidence against altruism-based models, the results presented here also provide evidence against the idea that intervention is solely the product of a state's proximity to a conflict or ease of intervening. In particular, the results of the interactive models demonstrate that, while intervention can indeed be motivated by proximity, refugee flows have a distinct impact on the likelihood of intervention. From these results, it is clear that proximity and ease of intervention cannot be the sole explanation of intervention behavior as it is precisely those states furthest from the conflict that are most strongly impacted by hosting refugee populations. If proximity was in fact the sole determinant of intervention, then one would expect the impact of refugees to diminish as distance increases - if a meaningful relationship existed at all. This is not to say that proximity plays no role in intervention. Indeed, the argument I present is that intervention is strongly determined by security concerns faced by third-party states. While proximity to a conflict can induce such concerns, shared ethnic ties, and, most importantly, refugees also play an important role. This result further bolsters the argument that intervention is a purposive activity undertaken largely by
states seeking to avoid the negative security implications associated with an external conflict.

4.5 Conclusion

In this project, I have argued that the link between refugees and conflict intervention is better viewed as driven by a process where the act of hosting refugees creates clear security threats for host countries who, in turn, intervene in the source conflict to mitigate these threats. I contrast this explanation with models that focus on the number of refugees a conflict has created and argue that states respond to the humanitarian crises that these refugees represent. Indeed, both of these arguments have been set forth in the existing literature, most empirical models of intervention have focused exclusively on the number of refugees a conflict produces, thereby missing the opportunity to evaluate the link between hosted refugees and third-party support. By formulating clear theoretical and empirical models focusing on this later dynamic, I argue we can gain several key insights.

First, by developing a model of hosted refugees and conflict intervention, we can focus on the idea of intervention as a response to security threats. This approach is beneficial both because it is consistent with existing literature on the risks associated with hosting refugees and because it forces us to consider how emerging threats, such as a growing number of hosted refugees, interact with existing threats, such as contiguity or shared ethnic ties. In arguing for an interactive link between these two sources of threat I have done more than motivate additional hypotheses, I have posited a general model of the way security threats motivate intervention. Specifically, I argue that pre-existing security concerns, as a driver of intervention behavior, operate in a largely binary fashion wherein states that have such pre-existing conditions are largely uninfluenced by emerging security concerns - such as refugees. In contrast, states without pre-existing threats are strongly influenced by these emerging
threats. Existing scholarship linking refugees and intervention has failed to explore this link directly and has largely avoided treating refugees as one of the many indicators of security threats.

Aside, from the additional theoretical insights, this model also allows us to better predict conflict intervention and, more specifically, which third-parties will ultimately become involved in a civil conflict. In contrast, previous models have only indicated that conflicts which produce more refugees will attract the support of outside actors - without any indication of which state will intervene. Thus, using this revised model allows for more precise predictions about the process of intervention allows scholars and policy makers to formulate clearer expectations about the course of a conflict.

In total, I believe this project makes several contributions to the existing literature while also pointing towards future research on refugees and intervention. For example, future work could focus on disaggregating the “type” and/or “side” of intervention that occurs. While this project argues that the motivation of mitigating the consequences of refugee flows could lead a third-party to become involved in any number of ways, future research may be able to demonstrate that particular forms of intervention are more likely than others. Additionally, the arguments about shared ethnic ties could also be extended to focus on the ethnic composition of the refugee population as well as the state of ethnic power relations between the relevant ethnic groups. Either of these additional pathways, and likely many others, could be fruitful extensions of this work. Generally, I hope the model and evidence I have presented serve to motivate clear examinations of these dynamics and push us to better clarify how refugees impact states and their behaviors. Ultimately, I hope that by understanding the many linkages between refugees and international relations we will be able to understand, predict, and prevent the negative consequences of refugee crises.
Chapter 5: Conclusion

The last five years have seen the world refugee population surpass 20 million as civil wars and other security crises around the globe have continued to deteriorate. While conflict in the Middle East has produced the bulk of the world’s refugees in this time, crises in Africa, South America, and Asia have also caused millions of individuals to flee from their homes to the shelter of another country. In this context, with the world refugee population continuing to expand, a well-developed understanding of the causes of refugee flight and the consequences of hosting millions of refugee is critical for advanced planning, resource allocation, international cooperation, developing refugee-related policies, and a variety of other decisions by states and non-governmental organizations alike.

Though no one project can answer all of the relevant questions at once, this dissertation examines some of the most pressing issues relating to the flow of refugees by directly examining the “refugee-violence nexus” and modeling the numerous connections between refugee populations and violence. In this final section, I discuss the core findings of my research and present a discussion of how this work matters for policymakers and scholars as well as how future research building on my arguments and findings should proceed.

Any proper understanding of the political implications of refugee flows must begin with a firm understanding of why people flee from one country to another. While past research has linked refugee flight to violence, the first section of my dissertation moves beyond this generic understanding of the causes of flight and examine the role of specific conflict events. By laying out a model in which individuals use current events to estimate their future likelihood of being adversely affected by a conflict I draw attention n to two types of conflict activities that should
heavily impact refugee flows: the location of violence and the identity of the group being harmed. Using disaggregated data on the location of violence and the actors involved, I demonstrate that violent events taking place in heavily populated areas generate significant refugee flows and that violence against specific groups (notably successful attacks against rebel groups) appears to cause supporters of these groups to flee abroad.

This last finding – that some refugees flee because of their allegiance to poorly performing groups – is an especially significant finding as it provides leverage to understand the oft-cited link between refugee populations and violent activity in the states that host them. Building on this finding, the second section lays out a model of refugee-related violence wherein the performance of rebel groups in one country impacts the prospects of violence in a neighboring state. I argue that militant groups have an incentive to militarize refugee populations as a source of recruits and resources when these groups are in danger of being defeated by their opponents. Using statistical analysis, I demonstrate that the onset of violence in a refugee hosting state is more likely when militant groups in the home state are experiencing severe losses. Further, using detailed case studies, I demonstrate that some refugee populations do in fact have close ties to militant organizations and engage in violence in coordination with these groups – especially when these groups are struggling to combat their opponents.

The model of refugee-rebel connections I present and evaluate offers a novel perspective on a long-recognized problem. Specifically, whereas others have proposed a multitude of non-specific ways that refugees may bring violence to states that host them, I isolate a thoroughly examine a single mechanism through which violence can spread. Additionally, by emphasizing the role rebel groups play in the militarization process, this model also turns our attention to understanding how refugee-hosting states can act to prevent militarization. Indeed, the idea that
defeating rebel groups can sever the links between refugees and militants and effectively prevent militarization motivates the third and final section of the dissertation.

The increasing global refugee population has brought with it a number of serious concerns about the safety and stability of refugee hosting countries. Indeed, these debates have a long history and state responses to hosting refugees have varied considerably. In light of the analyses presented in the first two sections, I argue that states have long understood the dangers of hosting refugee populations and recognize that putting an end to the source conflict is often one of the best ways to prevent refugee-related violence in their own country. Using this idea, I argue that refugee hosting states are more likely to undertake military intervention into a conflict when they are experiencing a large influx of refugees. The motivation behind this intervention is clear in light of the previous section – refugees create a security concern for host states and terminating the refugee-producing conflict can alleviate these concerns by decreasing or even eliminating the refugee population. Additionally, in modeling the way refugees create this security threat, I consider which states would be most responsive to this stimulus and argue that non-contiguous states and states not sharing ethnic kinship ties to the source state will experience a greater increase in the likelihood of intervention due to hosting refugees. My statistical models bear out these expectations and demonstrate that hosting large refugee populations increases the likelihood of intervention – especially among states without a pre-existing security-related reason to pursue intervention.

Altogether, the three preceding analyses offer a coherent, though complex, perspective on the link between refugees and violence. The models I present offer novel and informative perspectives on both the causes and consequences of refugee flows. Indeed, by focusing on the way conflict dynamics and allegiances to militant groups affect refugees, I have developed
expectations not only about the causes of refugee flight but also the way refugees impact host countries and the way host countries respond to refugee crises. The benefits and implications of these new models are numerous.

Broadly speaking, the model of refugee flight I set forth is especially notable in that it establishes a new framework for understanding why specific individuals ultimately flee. By continuing to explicitly consider the connections between militant groups and supporters, future scholarship can develop stronger expectations about the timing of refugee flight, the compositions of the flows, the destination country, the potential for voluntary repatriation events, and a number of other dynamics that have been under-examined in the literature. These opportunities are also rich with political implications in that they can help support governments and non-governmental organizations seeking to predict, respond to, and prevent emerging refugee crises. The ability, for instance, to accurately predict the onset of major periods of refugee flight during a civil conflict would greatly benefit organizations like the UNHCR as they organize their response.

Similarly, one of the advantages of the models of refugee-related violence I present is that it provides a clear mechanism through which refugee militarization occurs. The specificity of my argument sets it apart from many other extant explanations and provides clear “warning signs” for policymakers to pay attention to when considering the risk of militarization. Additionally, future research can examine circumstances that could increase or decrease the possibility of refugee militarization. Such factors might include the relationship between the host and source country, the ease of arming militant supporters abroad, and the number of militant groups involved in the conflict. All of these factors may alter the relationship between the performance of violent groups and the spread of conflict through refugees.
As the preceding avenues of scholarship develop, additional findings will surely point to steps states can take to reduce the risk of refugee militarization in the same way that I argue intervention has been used to mitigate the risks of conflict. This is perhaps the biggest benefit provided by my research. By identifying the specific causes of refugee violence, policymakers can develop strategies to prevent refugee violence by, for example, targeting specific militant organizations or further developing and implementing screening procedures to isolate militant refugees from the general populations. The process of refining these insights into a form useful for decision makers will certainly be a long one. However, the foundational research laid out in this dissertation provides a starting point and points to numerous avenues of future research.

Altogether, the research I present addresses a topic of ever-growing importance and provides fresh insight into problems that have long been the topics of heated discussion in both academic and policy spheres. These advances are only possible through the development of new theoretical perspectives and hypotheses and the novel use of data to assess my arguments. My hope is that this research lays the foundation for a substantial increase in our collective knowledge of refugees and refugee crises and, ultimately, helps us uncover ways to prevent, mitigate, respond to, and terminate refugee crises. Indeed, despite the occasional connections between refugee populations and violent activity, the protection of those involuntarily displaced from their homes and forced to seek refuge in another state should continue to be a dominant concern among academics and policymakers across the globe.
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