Chapter 1

Introduction

On the afternoon of September 7, 2001, in the central Nigerian city of Jos, a fight erupted between a Christian woman and a Muslim security guard outside a prominent mosque in a commercial area. Accounts of what exactly happened between them vary, but eyewitness observers agree that, at some point during the course of their conversation, the woman began shouting at the guard. Friday prayers were underway, and hundreds of mosque attendees lined the road. At that time, city rules dictated that traffic could be stopped on such occasions, and it was customary for Christians, particularly women, to take alternative routes on Friday afternoons.

After repeated attempts to persuade the woman to change her route, the fight became physical. The woman fled the scene and rumors that a Christian woman had been “slapped” by a Muslim man spread like wildfire throughout the immediate neighborhood. Within the hour, a street fight erupted on the main road in front of the mosque, involving dozens of Christian and Muslim men from the area. By midnight, the neighborhood surrounding the mosque was engulfed in its first Christian-Muslim riot. As surprising as this was to many Jos residents, they were utterly unprepared for what would follow—seven days of full-scale rioting, a death-toll of over a thousand people, and the burning of dozens of churches and mosques across town.¹

¹This story was repeated to me, with minor variations, in interviews with four eyewitnesses to the
Around eight o’clock that same Friday evening, a young Christian man, whom I will call Saidu, living in the predominantly Christian neighborhood of Ali Kazure, heard screaming outside the front door of his house. Running outside, he met a close friend who was well known as a member of a youth organization in the neighborhood. At that time, Saidu was 23 years old. He was working part-time in his uncle’s stationery shop, trying to finish secondary school. Saidu’s friend had been running, and spoke between gasps for air about some “trouble in town” near a mosque up the hillside in the adjacent Muslim neighborhood of Ungwan Rogo. “They are killing Christians up there and we have to do something about it, or we are next!” After a few minutes of discussion, Saidu decided to see for himself what was going on. That night, Saidu explained, he helped the youth leaders in his area organize people to “protect” their neighborhood. “We Christians were a minority next to the people in Ungwan Rogo, and I knew I could help get the youths out.” Throughout the course of the evening and the following day, Saidu fought alongside friends and neighbors in and around Ungwan Rogo and Ali Kazure.²

Jonathan is another Christian from Ali Kazure. At the time of the riots, he was 20 years old and was working in a local barbershop. He had recently completed secondary school and, although he had hopes of further study, he needed to work to earn money first. Like Saidu, Jonathan had lived in Ali Kazure for much of his life. Unlike Saidu, however, he did not hear about the riots until the early hours of Saturday morning, when fighting began in earnest on the outskirts of his neighborhood. Jonathan woke up around 5 a.m., when his father came into his room with the news that there was fighting going on in Ungwan Rogo. He ran outside immediately and saw a mosque on fire up the hillside. “I knew that the crisis would be terrible from that Saturday morning,” he explained. But he did not fight that day, or on any of the following days.³

outbreak of the Jos crisis. (Interviews 1229, 1723, 1528 and 1566, Jos, July 2008.) Nearly 40 anonymous field interviews were conducted in 2008. Each was assigned a random number after completion. All names used in the book have been changed to protect respondent anonymity.

²Interview 1602, Jos, July 18, 2008.
³Interview 1507, Jos, July 17, 2008.
These short narratives illustrate the central puzzle of this study. Saidu and Jonathan are similar in many respects—in their religion, age, gender, and neighborhood of residence, to name a few. They both had Muslim friends prior to the riots, and neither had a history of violent or criminal behavior. In both cases, the decision to riot was voluntary, even though this choice was made under stressful conditions. But Saidu participated in the riots and Jonathan did not. This book centers on two fundamental questions about ethnic violence inherent in this puzzle: Who are the people who take to the streets and commit acts of violence and destroy property during the chaotic and frightening chains of events we know as ethnic riots, and why do they ultimately decide to riot? Are there systematic differences between the group of people we can call “rioters” and those who chose not to participate in the violence? The best way to answer these questions, I will argue, is by interviewing comparable groups of former rioters and non-participants and asking them to explain their choices and behavior prior to and during concrete riot episodes. Although ethnic riots are typically highly chaotic events, the chapters that follow build a case that participation can be studied systematically across individuals and across riot episodes.

This book explores riot participation decisions from close range, using the microscope as much as the wide-angle lens. The book’s main empirical approach draws on in-depth interviews and systematic micro-level survey data collected by the author from two Nigerian towns that have repeatedly suffered deadly communal riots in the recent past. The research presented here is the first large-scale survey of riot participants in the developing world and, as such, offers new insights into the dynamics of mass participation.

4The vast majority of rioters interviewed were not coerced into participating, suggesting an important degree of individual agency at work during the riots. This contrasts with strategies of abduction used by rebel groups in civil wars in Uganda, Sierra Leone, Liberia, and other civil conflicts, or accounts of coercion exerted by the Interahamwe and local authorities across the country during the Rwandan genocide to induce participation.

5Following Horowitz (1985, 2001), I use the term “ethnic” to refer to groups in which membership is based on actual or perceived descent, and I thus classify violent events in which Christian and Muslim groups are the main actors as ethnic violence. Brass (1997), Horowitz (2001), Varshney (2002), Chandra (2004) and Wilkinson (2004) use this logic in defining Hindus and Muslims in India as ethnic groups.
in localized violence.

1.1 The Puzzle of Participation

The question: “why join a riot?” touches in a dramatic way on fundamental puzzles about collective action. The risks and potential costs of participation are high in most places where riots occur, while the benefits are typically highly uncertain. Across a range of contexts, communal riots occur in localities with relatively balanced ethnic populations. As a result, it is often unclear which side will prevail in battles on the street, and the risk of death to participants is very real. Equally threatening is the prospect of being caught by the police or even the military, if they are called in to intervene to quell the violence. On the benefits side, the potential gains of rioting are often unclear. Field interviews and survey responses from Nigeria suggest that selective incentives associated with rioting (such as payment for participation) are rare or are obtained at great personal risk (for example, goods from looting).

Despite this puzzle, the literature on ethnic conflict tends to presuppose rather than explain mass participation in violence (for example, Gagnon 1995, Hardin 1995, de Figueiredo and Weingast 1999, Snyder 2000, Wilkinson 2004). This branch of research has been invaluable in pinpointing a range of elite motivations to instigate or foment violence in contexts as diverse as 1930s Germany and contemporary India. However, these studies often rely implicitly on the assumption that elites are highly strategic, if not implausibly cunning, while masses are non-strategic and easily manipulated. In doing so they overlook basic and important questions about why ordinary people would ever decide to participate in such risky collective behavior.

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6 Examples of ethnic riots in locations with relatively balanced group size include riots in Libreville, Gabon in 1981, in Nouakchott, Mauritania in 1989, and in Poso and the Moluccas, Indonesia in 1998 and 1999.

7 Horowitz (2001) describes an impressive range of cases in which central government military forces were called in to stop fighting during ethnic riots because the local police were unable to restore order on their own.
For example, in his rich ethnographic study of Hindu-Muslim riots in India, Paul Brass describes violence as an “anticipated by-product of election campaigns” in which faceless crowds are cunningly used by political elites to serve electoral ends (Brass 1997, pp. 13–18). Similarly, Steven Wilkinson proposes that ethnic riots in India are “planned for a clear electoral purpose... to change the salience of identities among the electorate in order to build a winning political coalition” (Wilkinson 2004, p. 1). Arguments such as these, which suggest citizens fall prey to the rhetoric of political elites, are also common in the popular literature on ethnic violence (for example, Philip Gourevitch’s writings (1998) on the Rwandan genocide), but are hard to defend in contexts where violence erupts repeatedly. Why would ordinary people, particularly in places where politicians fail to deliver on their promises, not update their beliefs over time and ignore top-down ethnic entrepreneurship as empty noise?

In a sweeping cross-national study of ethnic conflict, Donald Horowitz raises this question, urging conflict scholars to develop theories that ”answer the insistent question of why the followers follow” (Horowitz 1985, p. 140). Horowitz’s agenda points us in the right direction, but in formulating the question this way, he makes an important assumption. It is reasonable to ask the prior question: do “followers” actually follow political elites at all when they participate in ethnic riots? As I discuss below, this book looks closely at participation dynamics in two Christian-Muslim riots in Nigeria that occurred in political contexts where incentives often discouraged politicians from attempting to deepen the ethnic divide or stir up ethnic violence. Events that unfolded before and during religious riots in the northern Nigerian cities of Kaduna and Jos (described later in this chapter) suggest that the eruption of violence caught political elites largely by surprise. More generally, asking “why do followers follow?” rules out the possibility that leaders who appear to be instigating ethnic violence may in fact be reacting to mass behavior as much as directing it.

Several recent studies point to a promising focus of attention in the violence literature
on decision-making by ordinary people, typically focused on cases of full-blown warfare. Scholars in comparative politics have offered a wide range of individual-level explanations for acts of anti-regime rebellion (Petersen 2001), participation in insurgent groups (Wood 2003, Kalyvas 2006, Humphreys and Weinstein 2008), genocide (Verwimp 2005, Straus 2006), and non-battle violence during civil wars (Kalyvas 2006), bringing agency back to the people who choose to fight. In spite of the richness of these studies, however, the incentive structures and dynamics at work in civil and interstate wars are quite different from those we observe in shorter, more localized but often no less deadly episodes of ethnic rioting.

For instance, decisions made by participants during riots are typically made rapidly and the forms of collective action that occur are more weakly structured than what we would expect to observe during civil wars. Rioting does not involve joining a formal organization and, crucially, it involves a much more limited commitment of time and effort than joining a rebel group. These differences have potentially important theoretical implications for a study of the motivations for violence.

Take as an example the argument often made in the literature on civil wars about the “opportunity costs” of fighting for potential rebels. Marketable or gainfully employed individuals, the argument goes, should be less likely to forgo earnings and join a rebel movement than those without secure jobs or attractive job prospects (Collier and Hoeffler 2001, 2004, Miguel et al. 2004, Verwimp 2005). When applied to riots, however, the opportunity costs logic strains against the fact that riot participation typically involves a time commitment of hours or days, and participants can, and often do, return to work relatively quickly after the fighting ends. In contrast, rebels often travel far from home for months, or even years, after they make the decision to join. While the micro-level civil wars literature offers a wide array of hypotheses that we can test in the context of riot participation, we should expect some of them to fit poorly with the decision-making calculus of potential rioters.
The theory developed here draws on findings from two additional sources—a large body of ethnographic work on the kinds of individuals present in the crowds out on the streets during ethnic riots (Engineer 1987, Tishkov 1995, Tambiah 1996, Horowitz 2001), and scholarship on the processes of mobilization for high-risk collective action from the literature on social movements (McAdam 1986, McAdam and Paulsen 1993, della Porta 1995). These literatures focus squarely on the micro-contexts in which individuals decide to participate in collective behavior that may be fraught with extreme risk.

Ethnographic studies of riots have yielded consistent findings about the socioeconomic composition of rioting crowds, suggesting that rioters tend to be relatively poor, but are not typically criminals or “fringe elements” in society. Rather, rioters are often drawn from the working poor: people with jobs and connections in their communities, rather than the utterly marginalized (Tambiah 1996, Horowitz 2001). These studies offer powerful suggestive evidence on the question of who riots (the poor), but have less to say about why people ultimately riot. For example, what is it about poverty that makes riot participation more likely? As a further problem, the inferences we can draw from their findings are typically limited by the absence of non-rioters in their samples.

The social movements literature is similarly useful in its focus on the ways in which ordinary people come to participate in risky collective behavior. Several decades of scholarship on anti-regime protest, for example, indicates the importance of prior social relationships or informal social networks in pulling potential activists onto the streets (Kuran 1991, Lohmann 1993, 1994, Schussman and Soule 2005). Much like the ethnographic work on riots, however, studies of social movements have been more successful in answering the question of who participates (people with prior social ties to other joiners) than in uncovering the mechanisms through which social networks might work to increase the likelihood of participation.

Beyond academic concerns, the scale of the fatalities and destruction wrought by ethnic riots makes them worthy of scholarly attention in their own right. In each of
the riots investigated here, Christian-Muslim clashes killed at least two thousand people, and probably many more (Kaduna State White Paper 2001, Tertsakian 2001, 2003). In addition to the death toll, dozens of churches, mosques and private properties were burned to the ground. Country experts estimate that more than 5,000 people have been killed in Christian-Muslim riots in Nigeria since 1999 alone. This much destruction and killing demands an explanation.

1.2 What is an Ethnic Riot?

In spite of often deep disagreements over their causes and consequences, there is generally little controversy within the ethnic riots literature on the nature and scope of the event under study. In his longitudinal analysis of Hindu-Muslim riots in India, Ashutosh Varshney and Steven Wilkinson used the following two criteria to classify an event as an ethnic riot for inclusion in their dataset of Hindu-Muslim riots in India: “(a) there is violence and (b) two or more communally identified groups confront each other/members of the other group at some point during the violence” (Varshney 2002, p. 309; Wilkinson 2004, p. 255). The crucial feature of this definition is that rioters from one communal group attack members of another communal group. This excludes violence directed against state institutions or state property, for example. In fact, this dataset deliberately omits riots in which one ethnic group fights only with the police.

In an in-depth investigation of five communal conflicts in contemporary India, Paul Brass defined an ethnic riot as “an event involving large numbers of massed persons from opposing ethnic groups engaged in assaults on persons, lives and property” (Brass 1997, p. 4). A few features of this definition are worth underlining. First, although it is difficult to define how many people are necessary to count as “large,” Brass’s definition rules out

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8Data on conflict onset in Nigeria between 1999 and 2005 can be found in Lewis and Albin-Lackey, 2006.

9For a somewhat dissenting view, see Beth Roy’s account of Hindu-Muslim violence in rural Bangladesh (1994).
routine violent crime in which the attacker happens to have a different ethnic identity than the target. To take examples from this project’s research sites, a fight outside a local tavern between a Christian and a Muslim in Kaduna or Jos, for example, would not count as an ethnic riot. These kinds of events may trigger a riot, but are not riots themselves. Similarly, Brass’s term “massed” evokes crowd behavior. Small groups of Christians or Muslims fighting one another, for example, in the form of gang fights, would not count as riots under this definition.

Donald Horowitz offers a similar but slightly narrower definition of an ethnic riot as “an intense, sudden, though not wholly unplanned, lethal attack by civilian members of one ethnic group on civilian members of another ethnic group, the victims chosen because of their group membership” (Horowitz 2001, p. 1). The term “intense” evokes a similar image as Brass’s term “massed,” suggesting that riots need to assume at least a reasonably large scale in order to be classified as such. Horowitz adds the term “sudden” to differentiate rioting from violence involving higher levels of organization and advance planning, such as civil or interstate warfare. Most importantly, Horowitz emphasizes the logic of deliberate targeting—participants in ethnic riots do not simply happen to fight along ethnic lines but explicitly seek out members based on their ethnic identity, often going to great lengths to ensure that their victims are in fact members of the opposing group.

The riots that took place in Kaduna in 2000 and Jos in 2001 easily fit within the bounds of these definitions from the literature. They were undoubtedly large in scale and highly lethal in effect. They began suddenly, without advance planning, and there is ample evidence that targeting during the violence occurred deliberately along ethnic lines. But why are these definitional considerations important? Why should we attempt to distinguish features of riots from other types of violence, such as civil wars?

I advocate a “splitting” rather than a “lumping” approach to the study of ethnic violence for the simple reason that, in considering many different forms of violence to-
gether, we may miss important differences in the motivations for participation across types. In contrast, if we first look carefully at the micro-dynamics of riots, we will be in a better position to investigate whether and under what conditions our findings on riots can generalize to other types of violence. Further, the more narrow we are in our focus, the less likely we are to be led down irrelevant empirical paths. For example, a widely accepted argument in the literature on insurgency suggests that rebel recruitment will be easier in localities with “rough terrain” (such as mountains or forests), because these geographic conditions thwart anti-insurgency efforts on the part of the state (Fearon and Laitin 2003). Anticipating this lower risk of being caught, potential rebels will be more likely to take up arms. While plausible for the study of insurgency, arguments about rough terrain map poorly onto the conditions under which riots take place—typically in urban areas, often in plain sight of state authorities.

1.3 The Argument

The explanation advanced in this book rests on an important background condition: a state bureaucracy weak enough that local authorities cannot guarantee protection for citizens, either during peacetime or when violence breaks out. Contexts where state institutions like the police cannot respond effectively to routine crime, let alone violence on the scale of the Kaduna and Jos riots, may produce conditions akin to the classic security dilemma described in the causes of war literature (Herz 1951, Jervis 1978, Snyder 1985), particularly when polarizing social cleavages already exist (Posen 1993). Absent competent state security provision, ordinary people may be pulled into rioting because they fear they will be attacked if they do not riot.

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10This condition holds in a many developing countries where communal riots routinely occur. For example, in Kyrgyzstan, ethnographers have described the local police not only in urban but in rural areas as “paralyzed” in the face of both ordinary criminal activity and large-scale violence between ethnic Kyrgyz and Uzbeks (Tishkov 1995). Examples of inefficient or incompetent police in periods prior to the outbreak of ethnic riots abound in the case study literature: Senegal (which experienced riots in 1981) and Zanzibar (1932) are two examples. In contexts like these, ordinary people cannot count on the police to protect their lives and property, even in times of peace.
In northern Nigeria’s Middle Belt, for example, the weight of evidence suggests that state authorities, including the police, are typically neutral with respect to Christian-Muslim conflicts. In spite of this neutrality, however, the weak capacity of the police leaves ordinary people highly vulnerable to attack once a riot has begun. As the book’s main empirical chapters will show, the inability of the state to offer protection, and the subsequent fear of being targeted, can motivate large numbers of people to participate who might not otherwise choose to riot. Interviews with riot participants in both Kaduna and Jos make clear that many ordinary people, particularly those living in the cities’ poorest neighborhoods, took to the streets in anticipation of attacks from members of the other side of the religious divide.

But not everyone will be equally vulnerable to attack once a riot breaks out. When state authorities are unable to maintain public order, poor people in particular are vulnerable once a riot trigger occurs. Unlike wealthier people, who have access to alternative security measures than those ostensibly provided by the state, such as private security guards or high compound walls, poor people need to organize to defend their families and property during the chaos of an ongoing riot. As such, conditions of poverty serve as a powerful motivation to fight before one is attacked by members of the other group. That said, given the risks involved in joining a riot, one might still prefer to stay home, while others go out and defend the neighborhood from attack.

The book’s main empirical implication, then, is that poverty will increase the likelihood of riot participation for people who are embedded in social networks linking them to other potential participants. Poverty may make people more willing to riot, in order to defend their property, their families and themselves, but it is social networks at the grassroots level that help to transform potential into actual rioters. Networks of informal social ties may pull rioters out onto the streets through a variety of channels. I argue that they are particularly important as conduits of peer pressure. In addition to providing basic information about the riots (where the fighting is taking place, or who else from the
neighborhood is going to participate), refusal to participate when recruited by a prior contact can carry with it non-trivial social costs. Together, the motivating “push” of poverty and the “pull” of local social ties to other potential rioters make an explosive combination. Empirically, the main implication of the argument is that we should see a particularly strong joint effect of individual poverty and local social embeddedness on the likelihood of riot participation.

1.4 Case Selection: Two Nigerian Riots

In the pages that follow, this book investigates dynamics of participation in deadly religious riots that took place in two northern Nigerian cities, one in Kaduna in February 2000, and one in Jos in September 2001. Kaduna city, the capital of Kaduna state, is typically considered one of the ten largest cities in northern Nigeria, and was the capital of the Protectorate of Northern Nigeria under the British colonial administration. The north of Nigeria is predominantly Muslim, but Kaduna state has a large Christian population, drawn from over thirty tribal groups mostly based in the southern half of the state. The majority of people in the northern half of the state are Hausa Muslims. Although ethnic demography is controversial in Nigeria and reliable census data is unavailable for the post-colonial period, country experts estimate that Muslims comprise a slight majority (estimates typically range between 55 and 60 percent) in Kaduna state and Kaduna city (Abdu and Umar 2002, Sani 2007).

Jos is the capital of Plateau state, located approximately four hours by road to the southeast of Kaduna. Plateau state is generally considered to have a slight Christian majority, and a substantial Muslim minority. Most Christians living in the state belong to one of nearly a hundred small tribal groups considered indigenous to the state. Jos itself is

\[11\] Unfortunately, no reliable nationwide census has been conducted in Nigeria since 1963. Results from the 1991 census were “corrected” in the wake of violent protests that led to the deaths of several people in southwest and northern Nigeria. As a result, tribal and religious identification were not recorded in the most recent 2006 census.
Figure 1.1: Kaduna and Jos, Federal Republic of Nigeria
relatively balanced in its Christian and Muslim populations, with local experts estimating the Christian population at roughly 55 percent and the Muslim population approximately 45 percent (Bagu et al. 2004). Although Kaduna and Plateau are technically part of different administrative regions, they both fall within the swath of territory known as the “Middle Belt,” straddling the center of the country, separating the Muslim “far north” from southern Nigeria.

While not as poor as the northernmost region that borders Niger, the Middle Belt is considerably poorer than southern Nigeria, with higher levels of unemployment, lower average per capita household income, and worse performance on a range of other socioeconomic indicators (Nigerian Annual Abstract of Statistics, 2001, 2005). The Middle Belt is most widely known for its relatively even Christian-Muslim balance and, internationally, because it has been the site of a greater number of religious riots than any other Nigerian region in recent years. A dataset of violent events across Nigeria compiled by Human Rights Watch indicates that, of the 36 Christian-Muslim riots that occurred between 1999 and 2005, as many as 22 of them took place in the Middle Belt (Albin-Lackey and Lewis, 2006).\textsuperscript{12} Jos in particular has experienced large-scale Christian-Muslim riots even more recently, in November 2008 and in January 2010.

The fact that both Kaduna and Plateau states have Christian and Muslim populations of relatively equal size is relevant because of an important institutional feature of electoral politics in Nigeria. Under what is described in the current constitution as the “federal character” principle, both gubernatorial and presidential candidates must not only win more votes than any other candidate, but must also win at least one quarter of the vote in at least two-thirds of the local government areas (LGAs) in that state.\textsuperscript{13} The governors of

\textsuperscript{12}I use the terms “religious riot” and “Christian-Muslim riot” synonymously, as these are the only types of religious riots that occur in Nigeria. Only a tiny fraction of the population could be classified as belonging to a religious group other than Christian or Muslim. For example, only one out of 798 survey respondents classified himself as holding animist beliefs. All others identified themselves as Christians or Muslims.

\textsuperscript{13}Local Government Areas are the smallest administrative unit for which public officials are elected in Nigeria.
both Kaduna and Plateau states, which have large populations of the minority religious
group scattered throughout their states’ territories, thus have incentives to reach across
the religious divide, and have little to gain from polarization. As we will see in the
description of the riots below, there is no evidence to suggest that politicians at the state
level acted to foment violence in Kaduna and Jos. Elected officials appear to have been
genuinely caught off-guard when the riots began.

1.4.1 Kaduna Riots, February 2000

Between February 21 and 25, 2000, Kaduna city erupted in the worst inter-religious
violence ever witnessed in Nigeria. A judicial commission of inquiry established by the
Kaduna state government in the aftermath of the riots reported 1,295 deaths, although
other sources have suggested the true numbers may be far higher (Tertsakian 2003). The
fighting began after several weeks of public debate about the possibility of introducing
Shari’a law into the criminal code in Kaduna state. Although Shari’a provisions had
long been incorporated into “personal” or domestic law for Muslims throughout northern
Nigeria, the debate raised concerns in religiously heterogeneous states that Shari’a would
be imposed on Christian communities (Abdu and Umar 2002).

In October 1999, the governor of Zamfara, an overwhelmingly Muslim state bordering
Niger, decided to re-open the debate, in a move that was popular across his state, and
in December Zamfara did incorporate Shari’a into its criminal code. Over the next few
months, state legislatures across the northern region began to follow suit, with the excep-
tion of states like Kaduna and Plateau, which contain large Christian populations. The
issue was particularly contentious in Kaduna and its governor, Ahmed Makarfi (himself
a Muslim), strongly opposed introducing Shari’a. In January 2000, however, under pres-
sure from Muslim civil society organizations, the Kaduna State Assembly established a
commission to examine the “applicability” of Shari’a criminal law for Muslim residents of
the state. The action divided the assembly along religious lines, and sparked increasingly

On Monday February 21, the Kaduna branch of the Christian Association of Nigeria (CAN), a nationwide confederation of Christian civil society groups, organized a demonstration against the government’s investigative committee on the Shari’a question. According to participants in the rally, announcements were made in churches during the prior week, urging parishioners to attend the demonstration, and the event attracted hundreds of demonstrators (Haroon 2002). The rally began in the southern part of Kaduna’s town center, as protesters made their way north along the city’s main thoroughfare, Amadou Bello Way, toward Lugard Hall, the seat of the governor and the state assembly. Most observers agree that the protest itself was peaceful and that protesters were unarmed, although some elements of the crowd carried signs marked with provocative slogans, such as “Shari’a is not Y2K Compliant” (Maier 2001). Accounts of how exactly the fighting began differ, but it is clear that at some point during the morning of February 21, small numbers of Christians and Muslims began throwing stones at each other as demonstrators made their way past Kaduna’s crowded central market. At this point, the fighting spread quickly, and within the next hour, the market was engulfed in a Christian-Muslim riot, as Christian protesters fought with Muslim traders in the market, using simple weapons, such as stones and wooden planks.14

The fighting in the largely commercial center of town spread outward to residential areas across Kaduna metropolis, as rumors circulated about atrocities committed on either side. Interview respondents from neighborhoods across Kaduna recalled seeing smoke rising from the center of town, and hearing stories about churches and mosques being set on fire (Interviews 1558, Kaduna, July 28, 2008 and 1735, Kaduna, July 29, 2008). The riots lasted for four days, and were only put to rest when the central government ordered the military to intervene.15

15The military was sent in late on the second day of the riots and stopped much of the fighting. Order
Although sparked by the debate over Shari’a in the state assembly, the violence appears to have caught Kaduna’s politicians entirely by surprise. Governor Makarfi himself was out of the country, undergoing medical treatment, when the riots began, and there is no evidence of other state-level politicians attempting to foment violence through the use of inflammatory public rhetoric. The commission debating the Shari’a issue had barely begun its work by the time of the demonstration and the governor’s opposition to the changes in the criminal code, even for Muslims, remained strong. Interviews with witnesses to and participants in the riots revealed that politicians did not use the radio or television to incite violence, and there is no evidence to suggest collusion between Christian politicians at the state level and protest organizers. In combination with the fact that the Kaduna riots did not occur in proximity to local, state or national elections, this strongly suggests that the riots were not engineered or encouraged by political elites.

Given the poor regard in which most ordinary Nigerians hold their politicians, even if members of the state assembly had been making public speeches designed to stoke inter-ethnic tensions, it is unclear that such efforts would have been effective. In a telling moment, one interview respondent who was involved in fighting during the Kaduna riots, when asked if the actions or words of politicians in the days leading up to the riots had led him to consider fighting, responded: “What? Those guys are criminals. Honestly, if we had been able to find them, we would have killed them, too!” When asked why, he answered, “They never do anything for us. Nobody trusts them. They don’t bring us development... Nobody saw them before the riots and during the riots, they just hid behind their gates.”  

Turning to the reaction of the state bureaucracy, observers of and participants in the riots uniformly agree that, once the riots began, the police were quickly overwhelmed by the situation. In neighborhoods across Kaduna, calls to the police for help went was fully restored across Kaduna city on the fourth day.

16 Interview 1473, Kaduna, July 28, 2008.
unanswered and police units that did arrive at the scene of battles between Christians and Muslims found themselves unable to maintain order. One interview respondent from a Kaduna neighborhood that experienced heavy fighting during the first two days of the riots laughed when I asked him about the response of the police: “They really could not do anything. They were not serious. There were hundreds of people fighting at the Tudun Wada junction, and I think they came with five or six men, without guns! They were scared and they just ran away!”\textsuperscript{17} Residents of neighborhoods across Kaduna during the riots confirm that the police were unable to stop the fighting, due to insufficient manpower and inadequate weapons. Several interview respondents described rushing to nearby police barracks to report riot-related events, only to find officers “hiding” inside their offices. Interviewees only rarely reported outward signs of police bias in favor of one side during the fighting, but the conclusion that the police were simply not equipped to deal with a large-scale violent riot is inescapable.

\subsection*{1.4.2 Jos Riots, September 2001}

As described at the beginning of this chapter, the immediate trigger to the Jos riots was a fight between a Christian woman and Muslim security guard in front of a crowded mosque in the Muslim neighborhood of Ungwan Rogo during Friday prayers on September 7, 2001. The fight appears to have been the spontaneous result of a disagreement about the appropriate route for a Christian woman to take on her way home from work while prayers were in session at the mosque. Once the fight became physical, however, it engulfed the neighborhood with a speed and intensity that caught both ordinary residents and political elites in Jos by surprise. Within hours, the neighborhoods surrounding the mosque where the riot trigger took place were involved in fighting on a scale that had never been seen before in Jos. By the next day, Christians and Muslims were fighting in pitched battles across the city, as violence spread from the center of the town to

\textsuperscript{17}Interview 1872, Kaduna, July 31, 2008.
residential neighborhoods across its outskirts. Precise estimates vary, but most accounts suggest that well over a thousand people were killed during the riots (Albin-Lackey and Lewis, 2006).

Although, unlike in Kaduna, the Jos riots did not occur at a time when issues related to religion featured prominently in public debate, some have pointed to a contentious political appointment as contributing to the outbreak of the crisis (Tertsakian 2003, Interviews 1483, Jos, July 16, 2008, and 1229, Jos, July 19, 2008). Several weeks before the riots, in August, a Hausa Muslim named Mukhtar Muhammad had been appointed by Plateau’s governor to chair a “poverty eradication program” for Jos North local government area (LGA), the northern half of the city. Muhammad’s appointment was opposed by Christian civil society organizations in Jos, not only because a high-profile post had been given to a Muslim not originally from Plateau state but because he had been forced to abandon another local government post in Jos in 1998, after being accused of falsely portraying his credentials (Tertsakian 2003). In the weeks following the appointment, several Christian groups launched a public advertising campaign against Muhammad, with one organization going so far as distributing leaflets urging him to stand down from office, while others wrote letters of complaint to the governor (Sani 2007).

Much like the Kaduna riots, the violence on September 7 caught the state authorities by surprise and the scale of the fighting quickly outpaced the capacity of the police to maintain order. Police squads were unable to keep large numbers of Christians and Muslims from fighting in battles across the city. One interview respondent from Ungwan Rogo, a neighborhood close to the initial outbreak of the violence, was present when a group of rioters surrounded a police unit and easily and quickly disarmed them.\(^\text{18}\) Fighting continued until the military intervened on the second day of the crisis and began shooting into the rioting crowds, and then erupted periodically, in pockets across the city, for nearly a week afterwards. As in Kaduna, there is no evidence to suggest that

\(^{18}\)Interview 1602, Jos, July 18, 2008.
political elites planned or hoped for violence in Jos. The riots did not occur during an election year, and there was no discernible spike in inflammatory public rhetoric prior to the riots.

1.4.3 Why Study These Riots?

Beyond the intrinsic interest in understanding why people took to the streets in events with such stark humanitarian consequences, I chose to study participation in these two riots for several substantive and social scientific reasons. First, and most importantly, this book makes the case that the riots in Kaduna and Jos were relatively spontaneous in their early stages and their organization remained decentralized as the fighting unfolded over the course of several days. Political (or other) leaders did not order ordinary citizens to riot, nor is there evidence that they manipulated the public prior to the outbreak of the fighting in either city. What leadership did arise was extremely localized, and largely reactive to events and perceived threats on the ground once the actual fighting had begun.

After a breathtakingly broad review of dozens of ethnic riots across Asia and Africa throughout the 20th century, Donald Horowitz concluded that “...most riots seem to be unorganized, partially organized and partially spontaneous, or organized by the ephemeral leadership that springs up to respond to events as they happen, often suddenly.” The Nigerian riots under study here correspond well with Horowitz’s final characterization. The riots in Kaduna and Jos were by no means entirely chaotic or atomistic. As we shall see, participants rioted in groups, more often than not alongside people they knew before the fighting began, such as neighbors and friends. There are also surprisingly predictable patterns in the profiles of participants and the locations where the most intense fighting took place. It would be wrong, however, to suggest that these riots were organized in a top-down fashion or planned in advance.

Suppose we imagine a one-dimensional spectrum of riot centralization, running from

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19Horowitz 2001, p. 225
the extreme of tightly hierarchical riots—where elites can induce rioting on command—
on the left-hand side to the extreme of purely atomistic riots—with no leadership or social
organization—on the right-hand side. Thinking in these terms, I would argue that much
of the most compelling recent work on the dynamics of ethnic riots has been skewed to
the left side of the spectrum. For example, Paul Brass’s investigations of “organized riot
systems” and Steven Wilkinson’s study of the electoral logic of riot onset in contemporary
India, have led scholarly attention away from unplanned, relatively spontaneous riots in
recent years.20 These scholars have produced an impressive amount of evidence to suggest
that, under certain conditions, politicians representing extremist ethnic parties may have
both strong motives and capabilities to instigate riots for their own electoral gain. This
book does not question these findings. Instead, it suggests that an important scholarly
contribution can be made by focusing on less centralized riots, such as the Nigerian
examples investigated here.

A second reason that led me to study the Kaduna and Jos riots is that they were
sufficiently large to make it possible to locate former riot participants to include in a
large-scale survey, and to conduct statistical analysis. As described below, the book’s
main empirical chapters draw on a survey of nearly 800 respondents, over 200 of whom
directly participated in the Kaduna and Jos riots, as well as nearly 40 in-depth interviews
with residents of Kaduna and Jos during the riots. Third, the riots were recent enough
that even detailed survey and interview questions did not place unrealistic demands on
respondent recall.

Fourth, and perhaps most importantly from a methodological standpoint, prior to
2000 and 2001, neither city had experienced a religious riot for more than ten years,
mitigating problems in determining the arrow of causal direction between important
explanatory variables and riot participation. The riot before the 2000 crisis in Kaduna
occurred in 1987, and was small in scale. The Kaduna riots in 1987 began in the town

20Brass 1997, Wilkinson 2004
of Kafanchan, in southern Kaduna state, about two hours outside of Kaduna city, and spread in a limited fashion to Kaduna several days later. Across both cities, a total of 25 people were killed. In Jos, no Christian-Muslim riot had ever occurred before 2001 (Yakubu 2005, Sani 2007). As such, these two research sites offer a reasonably clean way to isolate individual motivations for participation independent of past riot events.

1.5 Looking Ahead

In the chapters that follow, I develop the book’s central argument — about the motivating “push” for the poor of the fear of being attacked and the “pull” of social ties to other rioters — in greater detail and test its central claims using a range of primary data sources and a variety of social science methods. Chapter 2 outlines the logic of this argument for the decision calculus of the potential rioter. Poverty and embeddedness in local networks may interact in a number of other ways to increase the likelihood of individual riot participation, however, and Chapter 2 outlines a series of alternative explanations that might account for a joint effect of the two variables.

Rather than serving as a proxy for vulnerability to attack, poverty may increase the likelihood of riot participation through several other channels, including the *lure of material benefits* (such as goods from looting) during rioting that are more valuable to the poor than to wealthier individuals, or through an *expressive logic* (Berkowitz 1962, Gurr 1970), in which poverty produces such high levels of discontentment that poor people resort to violence out of frustration.

On the networks side of the argument, chains of informal social ties may work in other ways aside from the exertion of peer pressure to increase participation in high-risk collective behavior. In particular, they may play a *purely informative role*, by circulating information about the location and timing of ongoing riot events and by signaling a group of other people willing to fight. In a very different argument, beyond mobilizing already willing rioters, it is possible that local social ties may actually work to *activate grievances*
prior to the riot itself. In this explanation, networks serve as what qualitative researchers of social movements have described as “workshops where grievances and strategies of resistance are constructed.”

This theoretical discussion is followed by four empirical chapters. Chapter 3 tests the argument’s main observable implication—that the interaction between poverty and social embeddedness should dramatically increase the likelihood of riot participation. The chapter first describes the design and implementation in 2007 and 2008 of an original survey of nearly 800 Kaduna and Jos residents, over 200 of whom actively participated in the 2000 and 2001 riots. I then present a set of novel sampling and question-design strategies to overcome three empirical challenges for a micro-level study of riot participation: (1) On the basis of what sort of behavior can we distinguish riot participants from non-rioters?; (2) How can representative members of the hidden population of rioters be found?; and (3) Once found, how can we obtain reliable responses about their participation in violence? Using the survey data, the chapter then tests the argument about poverty and social networks against alternative explanations from the violence literature.

In order to convince the reader that my interpretation of the relationship between poverty, social embeddedness and rioting is correct, Chapter 4 draws on a combination of data from the riot participation survey described above and a set of 37 semi-structured interviews with riot participants and eyewitnesses in Kaduna and Jos. Interview subjects were recruited from a diverse set of neighborhoods, in order to build a fine-grained picture of riot events as they unfolded in different micro-contexts across the two research cities. Chapter 5 focuses on the importance of neighborhood-level social ties in mobilizing both willing and somewhat less willing young men to riot. This chapter is largely qualitative, and uses the interviewees’ stories to trace the way in which particular people were recruited to fight in a series of large-scale street battles across Kaduna and Jos. Chapters 4 and 5 turn our attention from the initial question of who riots to an exploration of why

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rioters and non-rioters made the decision to fight or stay on the sidelines.

Chapter 6 looks at the question of riot participation through a somewhat broader empirical lens. This chapter provides an additional test for the book’s central hypotheses, with the help of a Nigeria-wide dataset on a similar but not identical outcome: participation in violent demonstrations. Individual involvement in localized violence is explored using survey responses from the Nigerian Living Standards Survey (NLSS), data on self-reported participation in violent demonstrations collected by Afrobarometer, and data on local government characteristics from a random sample of over 130 of Nigeria’s 773 Local Government Areas (LGAs). Strong support is found linking poverty, centrality in local social networks and participation in violent demonstrations, suggesting that the argument travels beyond Kaduna and Jos.

I conclude the book with a discussion of the scope of the argument. I consider the way in which three particular features of the Nigerian Middle Belt research context—the weakness of state institutions, the relative balance in size between the ethnic groups in conflict, and the intermingling or segregation in residence patterns of ethnic groups at the neighborhood level—inform the generalizability of the book’s theory. I probe these claims about scope by discussing several examples of riots beyond Nigeria, drawn from varying geographic regions and time periods.