

Essays in the economics of violence

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Abstract

In this thesis, I analyse the impact of economic shocks on violence, and the mechanisms underlying them. I also investigate the long-term effects of violence on economic outcomes.

In my first chapter, I introduce this thesis, and broadly outline its aims and contributions.

In my second chapter, I examine lynchings of African Americans in the US South from 1882 to 1930, and show that cotton price shocks predict lynchings. Lynchings also predict more black out-migration from 1920 to 1930, and higher state-level wages. Using a simple model, I show that this is consistent with lynchings having labour market effects that benefitted whites: lynchings cause blacks to migrate away, lowering labour supply and increasing wages for white labourers. I then turn to the long-term effects of lynchings. I show that Mississippi counties with more violence during a 1964 Civil Rights campaign also had more lynchings during the earlier period. Finally, lynchings persist in labour market outcomes: present-day results show that lynchings predict higher black-white worker, family, and household income gaps.

In my third chapter, I turn to French West Africa, a co-authored work with James Fenske. We show that rainfall, temperature, and commodity price shocks predict unrest in colonial French West Africa between 1906 and 1956. We use a simple model of taxation and anti-tax resistance to explain these results. In the colonial period, the response of unrest to economic shocks was strongest in more remote areas and those lacking a history of pre-colonial states. In modern data spanning 1997 to 2011, the effect of economic shocks on unrest is weaker. Past patterns of heterogeneity are no longer present. The response of unrest to economic shocks, then, differs across institutional contexts within a single location.

In my fourth and last chapter, I find that favourable temperatures and favourable economic conditions predict more witchcraft trials in Early Modern Scotland (1563-1727), a largely agricultural economy. During this time, witchcraft was a secular crime, and it was incumbent on local elites to commit resources to trying witches. Consistent with this, I find that positive price shocks to export-heavy, taxable goods like herring and wool predict more witch trials, while price shocks to Scotland's main subsistence commodity, oats, do not.

Dedication

To Father Sean Connolly, Michael Lahey, and Terry Melnyk, outstanding teachers

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Chapter 1

Introduction

The economics of violence is a topical and germane subject for the 21st century. Although violence itself is as old as human beings (Pinker, 2012), the study of violence in an economic context is a relatively new discipline (Sandler, 2000). It is in the developing world that the terrible consequences of wars and unrest are chiefly felt, and this in turn has a profound negative effect on economic growth and development (Blattman and Miguel, 2010).

I am particularly interested in the underlying economic mechanisms that drive violent acts. Empirical literature already documents a relationship between economic shocks and conflict (Depetris-Chauvin, 2015; Dube and Vargas, 2013; Collier and Hoeffler, 2004; Miguel et al., 2004). Also, in a recent paper, Bazzi and Blattman (2014) stress the need for case study evidence to uncover channels that drive unrest.

In this thesis, I show the economic underpinnings and mechanisms behind three important historical events : lynchings in the postbellum US South, civil unrest in French West Africa, and witchcraft trials in Early Modern Scotland.

The common thread in my thesis is direct evidence that economic shocks cause unrest and violence. However, the channels by which this fundamental driver (i.e. economic shocks) affect violence and unrest are very different in each instance. For example, I find that negative shocks to cotton prices predict lynchings of African Americans in the US South, due to labour market competition between black and white cotton workers. On the other hand, favourable weather conditions result in more witchcraft trials in Scotland, owing to

the ability of local elites to finance a trial.

Examining three important and diverse episodes in our history allows me the twin advantages of both breadth and depth. Breadth of subjects allows me to test general principles. For example, do witchcraft trials in Scotland react similarly as unrest in colonial Africa, and are the mechanisms the same? My breadth of exploration also extends to other important reasons behind each of these conflicts: religious beliefs (witchcraft trials), racism (lynchings), and resistance to colonialism (French West Africa).

Depth of analysis allows me to focus on each event individually, discovering how, for instance, the political economy of poll taxation impacted civil unrest in French West Africa. By relying on good data and sound identification strategies, I can reveal causal channels of operation in each case.

History of course is not a static, staid and lifeless thing but rather a continuous, living and dynamic process that describes the human experience. Scholars can infer much about current day events by studying their historical antecedents. Thomas Pikettys recent (2014) book on wealth inequality is a work of history as much as a work on economics. Ben Bernanke, former Federal Reserve Chairman, was an economic historian whose expertise on the Great Depression helped in devising US monetary policy. Similarly, my research on violence and its economic mechanisms can inform our knowledge of the mechanisms behind historical and present-day conflicts and this in turn can potentially provide knowledge of means by which conflict mitigation and resolution can be brought about.

Related to this, a significant part of my thesis dwells on the persistence of history and of its long reach in certain instances, into the social and economic conditions of our present-day world. I find that lynchings between 1882 and 1930 in the United States strongly predicts a higher black-white income gap today. In my co-authored work on French West Africa, on the other hand, I find that patterns that predict unrest in the colonial period do not predict unrest today.

I ground my empirical analyses in theoretical underpinnings. My economic models serve

to simplify and clarify concepts and allow insights into each phenomenon. I show, for example, that negative economic shocks in French West Africa are more likely to lead to unrest, and with the use of my model, I am able to test for this empirically.

My thesis proceeds as follows. In Chapter 2, I examine lynchings of African Americans in the US South between 1882 and 1930. The South was heavily agricultural at the time; blacks and whites competed with each other as cotton workers on plantations. I show that negative cotton price shocks predict more lynchings, as whites seek to drive African Americans away from the labour market. Consistent with this, I also demonstrate that lynchings predict more black out-migration, and higher state-level agricultural wages. Turning to lynchings long-term consequences, I find that historical lynchings predict greater Civil Rights era violence, and higher black-white income and wage gaps today.

In Chapter 3, co-authored with James Fenske, we investigate civil unrest in French West Africa between 1906 and 1956. We find that negative environmental and commodity price shocks cause more unrest during the colonial period, and use a simple model of taxation and anti-tax resistance to explain these results. In the modern period, spanning 1997 to 2011, we find a weak impact of shocks on unrest. The response of unrest to economic shocks differs across institutional contexts within a single location.

Finally, in Chapter 4, I consider witchcraft trials in Scotland between 1563 and 1727. I find that favourable temperatures resulted in more trials. During this period of history, it was incumbent on local elites to finance witch trials – the more resources they had to do so, the more likely a trial was to take place. Consistent with this, I find that shocks to taxable, export-heavy goods, like herring and wool, predict more trials. Shocks to Scotland's main subsistence commodity, oats, do not. Persecution, then, can depend crucially on the ability of elites to finance it.

Major conflict and violence of diverse kinds no doubt has societal/religious (eg. Jha, 2013), racial (eg. Voigtlander and Voth, 2012), or territorial (eg. Michalopoulos and Papaioannou, 2014) drivers and these well recognized causes of violence have often been ac-

recorded prime places in the search for causation. However, behind many of these well recognized phenomena, it is economic shocks on both a local and national/international level that play important and fundamental roles in the causation and persistence of conflict and violence (Hsiang et al., 2013). My thesis contributes to the growing body of evidence that “the love of money” in many cases is indeed “the root of all evil.”

The pioneers of the scientific study of human conflict - Lewis Fry Richardson, Kenneth Boulding, and Anatol Rapoport - had one thing in common: they were all peace activists. Richardson was a conscientious objector during WWI, which cost him a steady academic post. Boulding and Rapoport organised the first teach-ins against the Vietnam War, well before popular opposition. Yet these scholars’ contributions to the study of conflict are undeniable. Richardson, for example, was the first to analyse arms races mathematically (Richardson, 1949).

The apparent paradox between these men’s professional lives, and their personal convictions, is simple to explain. It is only by understanding conflict and violence that we can prevent it. As Rapoport said in *The Origins of Violence* (1994, pp. 586),

The predominance given to the Cold War and to the concomitant chronic confrontation between the superpowers reflects the overwhelming importance I ascribe to this situation... The theory of games and other branches of formal decision theory can define fruitful directions for peace research.

My thesis examines human conflict in three different eras and although the historical events analyzed in each case are different, the unifying phenomenon is the relationship I show between economic shocks and the initiation and persistence of conflict.

The implications for policymakers aiming for peace, however, are different in each case. In my paper on the Scottish witchcraft trials for example, since it is the elites of Scottish society that are financing the witchcraft trials, then sanctions that target elite income might quell persecution. On the other hand, if African Americans are being lynched because of labour market competition during harsh economic times, then policies like work insurance may be

more suitable. Indeed, in his study of India's Naxalite insurgency, Fetzer (2014) finds that the otherwise strong link between low monsoon rains and greater insurgency was reduced when India implemented a rural work insurance program. The correct policy intervention relies on knowledge of context - but without empirical analysis of cause and effect, policy makers will not have the tools with which to make effective interventions for peace.

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Chapter 2

Lynchings, Labour, and Cotton in the US South

2.1 Introduction

A growing literature examines persecution of minorities, and how its effects persist through time (Waldinger, 2010; Voigtlander and Voth, 2012; Jha, 2013). Racial discrimination continues to affect the United States (Fryer et al., 2013). This is particularly salient today concerning issues like police discrimination and overrepresentation of African Americans in prisons (Alexander, 2010). Understanding the causes and consequences of racial discrimination and violence is thus relevant, and important in preventing it.

In this paper, I examine lynchings of African Americans in the US South from 1882 to 1930, and find evidence that lynchings prevented black workers from participating in the labour market. When whites lynch blacks, this scares other blacks into outmigrating, thus reducing labour supply and increasing wages. Using the fact that world cotton prices are exogenous from a single county's perspective, I find that cotton price shocks strongly predict lynchings. More precisely, one standard deviation *decrease* in the world cotton price results in a 0.095 to 0.16 standard deviation *increase* in lynchings within a cotton-producing county.

These effects are more pronounced in counties that had railroads in 1890, suggesting that links to world markets and greater local labour demand had an impact on lynchings. I show in my data that lynchings predict greater black out-migration, and higher state-level agricultural wages. A one standard deviation *increase* in lynchings within a county leads to 6.5 to 8 % *more* black out-migration, and a 1.2 % *increase* in state-level wages. To understand these results, I analyse a simple model in which white workers can lynch blacks during low cotton price shocks, reducing labour supply and increasing wages. All this is indicative that greater numbers of lynchings served, at least in part, as a way of controlling black labourers.

I then turn to the long-term effects of lynchings, starting with the Civil Rights era. Although lynchings became very rare in the 1930s, discrimination against blacks continued. I focus on the 1964 Mississippi Summer project, a campaign to register African Americans to vote - the campaign's organisers encountered violence and discrimination throughout the summer. I show that Mississippi counties with more 1964 violence also had more lynchings in the past. Using data from the 2008-2012 American Community Survey, I also show that lynchings in the past predict white-black wage and income gaps today. This is robust to the inclusion of various controls and state fixed effects. Furthermore, I test the sensitivity of the coefficient estimates to control variables using Altonji, Elder, and Taber (2005) statistics. My results are shown to be robust to these tests, strongly suggesting that labour market discrimination has persisted from lynchings to the present day.

My paper is the first to show a direct labour market mechanism through which persecution operates. Previous economic studies have emphasised cultural and institutional factors as causes of persecution. Voigtlander and Voth (2012) use county-level data from medieval pogroms to show the persistence of anti-Semitic culture in Germany up to the 1920s and 1930s. Anderson et al. (2013) find that Jewish expulsions from European cities, between 1100 and 1800, are more common during colder-temperature periods, when rulers expelled Jews to satiate disgruntled citizens. As a final example, Yanagizawa-Drott (2014) shows that

the Rwandan genocide was encouraged by anti-Tutsi propaganda broadcast through radio. My study, by contrast, shows that lynchings of African Americans was related to Southern whites' labour market concerns.

I also find that historical labour-motivated violence can persist in present-day labour market outcomes. An increasing body of work finds historical persistence of past events and institutions: colonial settlement (Acemoglu et al., 2001; Angeles, 2007), slavery and coercive labour institutions (Nunn, 2008; Dell, 2010; Acharya et al., 2014), and violence (Voigtlander and Voth, 2012; Jha, 2013; Dincecco et al., 2014). I contribute to this literature by showing that lynchings from 1882 to 1930 strongly predict higher white-black earnings gaps today, and I suggest some channels through which this occurs.

Furthermore, I contribute to research on the economics of conflict. A number of studies (Dube and Vargas, 2013; Fetzer, 2014) detail mechanisms through which economic shocks cause conflict. Dube and Vargas (2013) show that in Colombia, positive oil shocks and negative coffee price shocks predict conflict. They also provide mechanisms through which this happens: with oil, there is a 'rapacity effect,' such that when the returns to gaining control of oil go up, oil-related conflict increases. With coffee, there is an 'opportunity cost effect,' so that when coffee prices are low, the returns to becoming a violent fighter increase. Similarly, I demonstrate a mechanism through which cotton price shocks affect lynchings in the US South, through a potential labour market channel.

Finally, I add to the growing economics literature on the postbellum US South (Naidu, 2010; Vollrath, 2013; Naidu and Hornbeck, 2014). Naidu (2010), for example, shows that postbellum restrictions on black agricultural workers' ability to change employment decreased black wages and lowered job turnovers. Hornbeck and Naidu (2014) use the Great Mississippi Flood of 1927 to show that counties with more flooding had greater African American out-migration, which incentivised landowners to mechanise agricultural production.

In Section 2, I briefly survey the relevant literature and historical context on lynchings,

and provide a theoretical framework. In Section 3, I describe the data and empirical strategy for 1882-1930 lynchings, before reporting my estimates in Section 4. I also analyse a mechanism through which lynchings might regulate southern labour markets. In Section 5, I examine how the effects of lynchings persist through time by examining violence during a 1964 Civil Rights campaign, and contemporary black-white wage and income gaps. Section 6 concludes.

2.2 Historical Context

In 1863, the Emancipation Proclamation freed 4 million African Americans from slavery. When the Civil War ended, the Union Army occupied the South, and the federal government established the Bureau of Refugees, Freedmen, and Abandoned Lands to help former slaves integrate into society. Until 1877, blacks did relatively well, and many were even elected to important Republican office posts (Foner, 1996), despite Ku Klux Klan intimidation. Historians call this period Reconstruction.

When Reconstruction ended, so did the protections African Americans enjoyed in the South. The Klan and similar groups waned in influence, as informal mob violence replaced the KKK's organised resistance to blacks. The South was a cotton-intensive region, and many blacks worked as wage labourers or sharecroppers in white-owned cotton plantations, often competing with white workers. It was during this time that lynchings became common.

Lynchings must be set against the postbellum South's broader political and social context. Racist violence could not have existed without racial tension to begin with, which was exacerbated by discriminatory laws such as poll taxes and grandfather clauses that prevented African Americans from voting. Attempts to better the plight of blacks were heavily resisted by white planter elites, who gained from paternalistic relations with their African American tenants (Alston and Ferrie, 1999), to whom they offered protection from violence. Amidst this background, lynchings occurred due to economic factors.

The suggestion that lynchings were tied to the cotton economy comes from Tolnay and Beck (1995), whose study was the first to systematically collect lynchings data from county archives, newspapers, and National Association for the Advancement of Colored People (NAACP) records. According to their study, lynchings were due to competition between black and white farm workers. They claim that lynchings had little to do with popular justice or politics, and more to do with cotton. My work develops Tolnay and Beck’s general thesis by subjecting it to more rigorous tests and analysis that were not available to the authors. I also provide empirical evidence for a mechanism through which black-white competition led to lynchings through lower labour supply, and therefore less wages. Furthermore, I empirically test the long-term consequences of lynchings, and how they might have had an effect on labour markets today.

Figure 2.1, at the end of this chapter, shows the world cotton price (dotted line), and the difference (solid line) between mean lynchings in cotton and non-cotton producing counties. Mean lynchings in cotton counties were greater than those in non-cotton counties, giving credence to historians’ general observations. Lynchings broadly follow cotton prices, and years of low cotton prices involve more lynchings. This is the variation that drives my results.

Work subsequent to Tolnay and Beck’s study has attempted to establish a link between lynchings and contemporary outcomes: homicides (Messner et al., 2005), hate crimes (King et al., 2009), and capital punishment (Zimring, 2003). These studies, however, all make links between lynchings and present-day crime. Yet, as this paper shows, lynchings were in fact a means of enforcing labour market outcomes, and no work to my knowledge has examined the link between lynchings and current black-white wage and income gaps.

2.2.1 A Simple Model of Lynchings

I use a simple model to conceptualise my theory of lynchings. In a number of cases lynchings had economic motives. Starting in the early 1890s, poor white farmers engaged in the practice of “whitecapping” to intimidate African-American tenant farmers and labourers into leaving

a community (Holmes, 1969). Although this and many cases highlight economic motives behind lynchings, my focus is on the *effects* lynchings had on labour markets.

In this stylised setup, there is only one player, white labourers (hencefort ‘whites’). Wages are denoted w , and utility is as follows:

$$U = \begin{cases} w, & \text{if } w \geq \bar{w} \\ 0, & \text{if } w < \bar{w} \end{cases}$$

Here, $\bar{w}$ is the subsistence wage, since the US South had much poverty during this period. This is also consistent with a story in which when prices (and therefore wages) fall, employers prefer to hire cheaper black labourers. This displaces white workers, causing unemployment among whites, and hence a utility of zero. This is congruent with qualitative evidence (see Tolnay and Beck, 1995; Wood, 2011), though is difficult to test here, since there is no county-level dataset of unemployment from this period.

Wages are a function of cotton prices, p , and labour supply, L , both of which are determined before whites make any decision. Assume that $w = w(p, L)$, where $w_p > 0$ and $w_L < 0$. This is to capture the idea that wages increase when prices increase, and decrease when labour increases.

Whites can lynch blacks to reduce L and increase w . As I show, this happens when lynchings scare African Americans into migrating, lowering labour supply and increasing wages. However, lynchings cost whites $c\tilde{L}$, where $\tilde{L}$ is the amount of labour reduced by a lynching. Suppose that $0 < c < 1$.

Also assume that $|\frac{\partial w}{\partial L}| > c$; if this were not the case, then lynchings would never occur, because the marginal benefit from lynchings would be less than the marginal cost of lynchings.

First, prices are determined exogenously by world markets, in turn affecting wages. Whites observe this, and if $w < \bar{w}$, then they derive zero utility, and can increase utility by lynching. Their optimisation problem can thus be written:

$$\max_{\tilde{L}} \{w(p, \hat{L}) - \tilde{L}c\}$$

where $\tilde{L}$ is the amount that labour must decline so that whites can gain positive utility (i.e. so that $U = w$). Concretely, $\tilde{L}$ denotes the number of blacks that need to migrate away from a county, leading to a new labour supply of $L - \tilde{L}$, i.e. the original labour supply minus the blacks who left.

Taking partial differentials,

$$\frac{\partial w}{\partial p} - \frac{\partial w}{\partial \hat{L}} \frac{\partial \tilde{L}}{\partial p} - \frac{\partial \tilde{L}}{\partial p} c = 0$$

And rearranging to isolate the effect of price on lynchings,

$$\frac{\partial \tilde{L}}{\partial p} = \frac{\frac{\partial w}{\partial p}}{\frac{\partial w}{\partial \hat{L}} + c} < 0$$

The above expression is negative, since $\frac{\partial w}{\partial \hat{L}} < 0$ and $\frac{\partial w}{\partial p} > 0$. Furthermore, $|\frac{\partial w}{\partial \hat{L}}| > c$, by assumption. Thus, as prices decline, lynchings increase.

In other words, as prices decline, wages decline to below subsistence levels, compelling white workers to lynch blacks. This causes other blacks to outmigrate in fear, decreasing labour supply and increasing wages.

2.3 Data and Empirical Strategy

In this section, I introduce my sources of data and my empirical strategy. I give summary statistics in Table 2.1.

2.3.1 Data

Lynchings. Lynchings data are from Tolnay and Beck (1995), who identified lynchings in

ten southern states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, and Tennessee. The authors began with data from the National Association for the Advancement of Colored People (NAACP), Chicago *Tribune* and Tuskegee Institute’s lynchings databases. They then attempted to confirm a lynching by locating a contemporary newspaper account of it. The authors’ definition of lynching, and therefore my own, requires that: (1) there must be evidence that the person was killed; (2) the person met death illegally; (3) a group of three or more persons participated in the killing; (4) the killers acted under the pretext of service to judgment or tradition. Number (3) is important in order for lynchings to be classified as mob activity. There are 2,805 lynchings in their dataset, and data is collected at the county level, from 1882 to 1930. Figure 2.2 shows the geographic spread of lynchings.

Other states, like Texas and Virginia, also had a significant number of lynchings, but Tolnay and Beck were unable to find reliable newspaper sources on these events. The data span from 1882 to 1930 for two reasons: (1) there are no reliable lynchings data prior to 1882; and (2) Lynchings declined significantly after 1930.

As a placebo check for present day income-gap outcomes, I use California lynchings data from Gonzales-Day (2006). He collects data on lynchings from 1850 to 1935, at the county level, from various sources, including the Tuskegee Institute and NAACP records. There are 352 lynch victims in his dataset, only 6 of whom are black. A large number of California lynch victims were Native American, Chinese, or Hispanic.

Cotton. World cotton price data are from Blattman, Hwang, and Williamson (2007), and are time series data. Cross-sectional data on which counties produced cotton are from the dicennial US Censuses from 1880 to 1930. I acquire data for county-level cotton-growing from the dicennial US Censuses from 1880 to 1930, inclusive.

Controls and Compliers. I include a dummy for counties along the Mississippi river, interacted with the lagged cotton price index. The Mississippi River control is included because counties along the river have more fertile soil, ideal for growing cotton. I also

include the 1880 black proportion and 1880 black proportion squared, each interacted with the lagged cotton price index. This is to account for the fact that the African American proportion of the population will account for more lynchings. Data on legal executions are from Espy and Smykla (2004).

For compliers, I acquire additional data. Data on farm labourers by race are from the 1880, 1900, 1910, 1920 and 1930 microcensus, from IPUMS (see Ruggles, 2010); specifically, I use the data on occupations to create a ratio of black to white farm workers - this helps to test the labour market competition theory. Slavery data are from the 1860 US Census, which shows how many slaves exist per county. Data on which counties had railroads in 1890 are from Donaldson and Hornbeck (2013).

To account for county boundary changes during this period, I use the Atlas of Historical County Boundaries Project, and then merge counties to their smallest consistent unit, forming multi-county units in many cases. This reduces my number of counties from 876 to 275.

Migration. I acquire micro-level migration data from Boustan et al. (2012), which describes whether an African American migrated from their county of origin between 1920 and 1930. The authors acquired these data from US Census microdata for this period. As controls, I include data on Rosenwald schools from Aaronson and Mazumder (2011).

State-level Agricultural Wages. Data on state-level agricultural wages are from Naidu (2010), who acquires his data from the US Department of Agriculture’s “Crops and Markets” reports from 1866 to 1930.

2.3.2 Empirical Strategy

I first test whether adverse cotton price shocks are related to lynchings. My main specification is:

$$(1) \text{Lynching}_{i,t} = \beta \text{Shock}_{i,t} + \delta_i + \eta_t + \epsilon_{i,t}$$

Here, $Lynching_{i,t}$ is the number of lynchings of African Americans in county i in year t .

$Shock_{i,t}$ is a lagged value of $Cotton_i^j \times \ln Price_t^j$.¹ Here, $Cotton_i^j$ is an indicator for whether the county produces cotton from 1882-1930 - it is not time varying. $\ln Price_t^j$ is the world commodity price index for cotton, and varies over time.

I employ linear regression to control for both county and time fixed effects. County fixed effects are denoted δ_i , and control for time-invariant county characteristics such as soil quality, elevation, or ruggedness. Time fixed effects are η_t , which I use to control for omitted heterogeneity at the level of years. I show that this is robust to the inclusion of county-specific trends. For my identification strategy, I exploit the fact that world commodity prices are exogenous from a single county's perspective. My unit of observation is a county-year.

What I am interested in, then, is whether a cotton price shock affects lynchings *within* a cotton-producing county. I also include, as controls, the lagged cotton price interacted with: a dummy for a Mississippi River county, the 1882 black fraction of the population, and 1882 black proportion of the population squared. Black population is important, since African Americans were largely employed in cotton plantations in the US South. I also show that this is robust to interacting the cross-sectional control variables with a time trend, instead of the price.

2.4 Results

2.4.1 Main Results and Robustness

I claim that lynchings occurred during poor economic times, when whites lynched blacks to terrify the black labour force into migrating, thus reducing labour and increasing wages. It is therefore important that cotton shocks predict lynchings.

¹The cotton production cycle started in winter, when land was broken for planting. Cotton grew during the summer, and handpicking would begin in September. This ended in November, when cotton bolls were cleared (Tolnay and Beck, 1995). These seasonal patterns justify my use of a lagged cotton shock, since production and hiring decisions the following year depend crucially on current prices.

Results for (1) are shown in Table 2.2. In the first column, I conduct a simple regression of lynchings on the cotton shock. In column 2, I include controls, which are the lagged cotton price multiplied by a dummy for Mississippi river county, 1880 black proportion, and 1880 black proportion squared. I replicate these in columns 3 and 4 with county-specific time trends. I show that the results are robust to double clustering (Cameron et al., 2011) by both year and county in columns 5 and 6. Clearly, lynchings reacted strongly to world cotton price shocks. A standard deviation decrease in the cotton shock leads to a 9.5 to 16 percent standard deviation increase in lynchings.

In Table 2.3, I show that these results are robust to the use of a lynchings dummy, to the use of different controls, and to using post 1892 data. Post 1892 data is used for robustness because Thurston (2011) raises the concern that data before then suffers from reporting quality. In each case, results are robustly significant and the effect of the cotton shock is large.

To get a more grounded sense of how large this effect is, consider columns (1)-(2) in Table 2.3. These suggest that a standard deviation cotton price shock increase causes a 3.9 to 4.8 percent increase in lynchings. This is comparable to Hsiang et al.'s (2013) systemic review; the authors find that a standard deviation change in temperature leads to a 4% increase in interpersonal violence across studies. Thus, my result compares favourably to existing literature.

However, these outcomes could be merely due to frustration rather than labour market concerns; whites who feel economic hardship might take to violence of any kind. I therefore report additional tests in Table 2.4. Instead of white-on-black lynchings as a dependent variable, I use legal executions of African Americans. Southern executions, even towards the turn of the century, were public affairs. According to Wood (2011),

Even when southern judges and sheriffs... did attempt to hold executions in private,... citizens often actively resisted by bribing sheriffs, climbing rooftops, and breaking down enclosures. Not incidentally, at the same time, lynchings were

becoming more public, more ritualized, and more spectacular. Just as they resisted when local and state authorities prevented them from attending executions, white southerners resorted to lynching to guarantee their active involvement in and witnessing of criminal punishment.

I also include white-on-white lynchings to show that lynchings were driven by labour market concerns for southern whites. White-on-white lynchings are from Tolnay and Beck (1995). Although lynchings of whites were rare, the ones that occurred mainly victimised immigrants, who threatened the status of southern white workers in a similar manner as African Americans. Therefore, the white-on-white lynchings should still react to cotton shocks.

As the table shows, cotton shocks do not predict executions with much significance, and predict white-on-white lynchings with significance. The coefficient on cotton shocks is also smaller than that for lynchings of blacks. This suggests that white-on-black lynchings were not merely motivated by general feeling of frustration. It further indicates that such lynchings were not uniquely racial phenomena, and used as a signal to force ‘outsiders’ to migrate away from a county, reducing their labour supply.

2.4.2 Compliers

Table 2.5 reports results for some compliers. The first column reports compliers for the presence of railroads in a county in 1890 (from Donaldson and Hornbeck, 2013). Railroads were important in the American South even up to the 1920s, when the automobile replaced more traditional methods of transport. Early automobiles and trucks were not as efficient at transporting agricultural produce as railroads. Railroads proxy connection to world markets, as well as local demand for labour. It is therefore unsurprising that railroads have a strong effect on lynchings’ responsiveness to cotton price shocks; if a county grows cotton and has a railroad, then a standard deviation decrease in the cotton price implies a 0.20 standard deviation increase in lynchings.

Column 2 reports the impact of slavery in 1860. The interaction is 10,000 slaves. Slavery exacerbates the shock's effect, implying the persistence of slavery as an institution over time. More specifically, areas which had more slaves in 1860 are more likely to have black cotton workers after Reconstruction. This threatens white farm workers. It could also be that slave-intense regions retain their racism after slavery is abolished, and this exacerbates the effects of cotton price shocks, a finding in line with Acharya et al. (2014).

Column 3 reports the ratio of black farm workers to white farm workers in 1880. When this ratio is higher, there is a potentially greater threat to white workers. As the result shows, when the greater the ratio, the stronger the shock is felt. This again supports the labour market story, where lynchings scare blacks into migrating and reducing their labour supply.

Finally, columns 4 and 5 report results for poll taxes and literacy tests, which were intended to disenfranchise African Americans. These do not appear to have significant effects, and the point estimates are small. This suggests that areas that repressed blacks through formal laws did not need to use informal practices such as lynchings.

2.4.3 Migration and Wages

If lynchings were indeed an effective tool of labour control, then we should observe blacks migrating away from areas with more lynching. As Wood (2011) says of one event in Statesboro, Georgia, when a number of lynchings occurred, “violence subsided only when county farmers began to notice that many black laborers were fleeing the county just before the cotton harvest.” Table 2.6 reports results for regressions of migration on black lynchings. The dependent variable is an indicator for whether an African American migrated away from a county from 1920 to 1930, and the main independent variable is lynchings from 1920 to 1930. Controls are 1920 US Census values for urban population and African American population. I also include the number of Rosenwald schools a county had, since Aaronson and Mazumder (2011) find that Rosenwald schools caused more black out-migration. Rosenwald schools,

founded by Northern philanthropist Julian Rosenwald to educate black children, increased African Americans' human capital and pushed them to seek employment opportunities in Northern cities.

As the results show, a lynching predicts more county out-migration. In Column 1, a standard deviation increase in lynchings predicts an 0.08 standard deviation increase in out-migration. In Column 2, it predicts a 0.065 standard deviation increase in out-migration. These results are significant and support the idea that lynchings were an effective tool in controlling black labour. Furthermore, I include 1910 to 1919 lynchings as a placebo, and find that the point estimate is small and insignificant, further supporting my hypothesis.

To provide evidence that lynchings affected wages, I regress log state-level agricultural wages (Naidu, 2010) on past lynchings, using various specifications. State-level agricultural wages are from 1866 to 1930. Results are reported in Table 7. According to columns (1) and (2), a standard deviation increase in lynchings within a county predicts a 0.013 standard deviation increase in state-level agricultural wages. In columns (3) and (4), a standard deviation increase in the *change* in lynchings predicts a 0.002 standard deviation increase in wages. These results are not large, as I would expect; a lynching within a county will have limited state-level effects.

In sum, lynchings predict out-migration, and out-migration appears to predict wage increases. Furthermore, lynchings predict an increase in agricultural wages. This is all consistent with a labour market mechanism in which whites lynch blacks, which has the effect of raising wages.

2.4.4 Further robustness: instrumental variables and cotton acreage

As a final robustness check, I use the fact that Great Britain had the world's largest cotton textile industry (Davis et al., 2009), and imported most US cotton. Indeed, the British textile industry was an important part of the island's economy, and played a key role in the Industrial Revolution.

I therefore instrument my main explanatory variable, the cotton shock, with the following: $Cotton_i^j \times GBgpc_t$, where $Cotton_i^j$ is an indicator equal to 1 if a US county produces cotton, and $GBgpc_t$ is Great Britain's GDP per capita in 1990 US dollars. The data on British income is from Angus Maddison (2006).

This arguably captures British demand for cotton, which in turn affects cotton prices. It is unlikely that British demand for cotton had a direct impact on lynchings of African Americans, except through its effect on the price shock. The exclusion restriction is therefore likely to be satisfied.

In Table 2.8, I report results from the Instrumental Variables regressions. In each case, the first-stage F-statistic is above 10, mitigating weak instrument concerns (Staiger and Stock, 1997). Furthermore, the cotton price shock continues to strongly and significantly predict lynchings. A standard deviation decrease in the cotton price within a cotton-producing county leads to a 0.31 to 0.32 standard deviation increase in lynchings.

In the same table, I also show that my shock, $Cotton_i^j \times \ln Price_t^j$, is robust to replacing $Cotton_i^j$ with acres (thousands) planted in cotton in 1879, rather than a dummy for whether or not a county grew cotton during this period. The results are robust to this change, and indeed, larger: a standard deviation decrease in the cotton price causes a 0.204 standard deviation increase in lynchings.

2.5 Civil Rights and Contemporary Outcomes

Lynchings subsided after the 1930s. In 1933, the US Congress passed the Agricultural Adjustment Act, which shifted production inputs from labour to capital. The increased mechanisation reduced economic competition between whites and blacks, and reduced lynchings (Markovitz, 2004).

However, it is possible that lynchings continue to have an effect up until the present by affecting attitudes through time. Although violence against African Americans declined,

there are other methods of labour market discrimination, such as hiring practices (Bertrand and Mullainathan, 2004) and bank lending (Blanchflower et al., 2003) that may have replaced mob violence. This section explores this question.

2.5.1 Civil Rights Era Violence

Using primary source data from McAdam (1990), I briefly examine the Civil Rights Era, because of its economic implications for African Americans. From the beginning, its campaigners had economic motives and goals in mind, primarily for the U.S. South. For example, the 1950s bus boycotts and 1960s sit-ins involved demands for equal black employment (Wright, 2013). Many Southern whites actively opposed the movement, partly because it threatened to provide African Americans with better labour market opportunities. White resistance is likely to manifest in regions with historical labour coercion - like those counties with more lynchings. To test this, I consider a particular Civil Rights campaign: the Mississippi Summer Project.

In 1964, the Civil Rights group the Council of Federated Organizations (COFO) sent over 1,000 volunteers - mostly white students from the Northeast, Midwest, and California - to Mississippi to register African Americans to vote. This was known as the Mississippi Summer Project, under the leadership of the Student Nonviolent Coordinating Committee (SNCC), a COFO affiliate. Its volunteers encountered violence and harassment throughout the summer and autumn.

SNCC recorded such incidents in its internal documents, which I read from McAdam's (1990) appendix. For example, the report for June 21 in McComb County says "Homes of two civil rights workers planning to house summer volunteers bombed." I hand collected data from the SNCC documents on these incidents, a subset of which were violent, and manually input them into my computer. I then regressed these incidents on past lynchings. The results are in Table 2.9. Columns 1 and 2 describe an incident of any kind, while columns 3 and 4 are violent incidents.

According to the table, counties with a past lynching are more likely to have a racist incident. More precisely, a standard deviation increase in lynchings predicts 2 to 3 racist incidents. These effects are large, and support the path dependency of lynchings.

2.5.2 Contemporary Outcomes

Data

Most of my contemporary data come from the American Community Survey's 2008-2012 estimates. Rural-Urban Continuum Codes, a discrete, 9-point code which classifies counties' rural status (lower values mean less urbanised areas), are from the US Department of Agriculture. The racial income gap is calculated as the difference between median white and black incomes. These figures are annual dollar amounts. Table 2.10 contains Summary Statistics for these contemporary data.

The American Community Survey is an ongoing project that samples a small proportion of the population each year, surveying 250,000 addresses per month. By using the five-year data, I have the widest coverage.

It is possible that lynchings affect present-day outcomes because of geography, or historical conditions that precede lynchings. To account for this, I include controls for cotton suitability and proportion slave. The proportion slave measure is taken from O'Connell (2012), who bases her data on the 1860 US Census. Cotton suitability data is from the Food and Agriculture Organization.

Results and Robustness

Table 2.11 reports OLS regression results for the black-white income gap on past lynchings. I use three types of income gaps: that between households, between families, and between workers. This provides a good range of measures. Columns 1 and 2 suggest that a past lynching in a county implies a black-white household income gap of \$188 to \$296. Columns 2 and 3 report that a past lynching predicts a black-white family income gap of \$347 to \$450.

Finally, columns 4 and 5 report that a lynching predicts a black-white worker gap of \$87 to \$102. These effects are large; a standard deviation rise in lynching increases the black-white income gap by 0.06 to 0.16 standard deviations.

To account for arbitrary spatial correlation, I use Conley (1999) standard errors. If, for example, lynchings in one county had effects that spill over into others, then Conley standard errors can account for this. As Table 2.11 shows, my results are robust to using this strategy. At worst, column (6)'s result for the worker wage gap shows a 10 % level of significance.

Finally, to control for state-level heterogeneity, I use state fixed effects in all regressions. This is particularly important since US states have jurisdiction over labour and race discrimination law, which may affect results.

The only result that is not significant is in column (6), the worker wage gap. This could be because past lynchings affect the extensive margin (for example, whether or not an African American gets a job, and hence their family or household income), but not the intensive margin (i.e. antidiscrimination policy makes it difficult to pay blacks less on the job). To test whether this is plausible, I regress the black-white unemployment ratio (black-unemployment/white-unemployment) on past lynchings. Results, reported in Table 2.12, show that lynchings predict a higher black-white unemployment ratio in columns (1) and (2). A standard deviation increase in lynchings predicts a 0.08 to 0.10 standard deviation increase in the unemployment ratio. When I add historical controls in column (3), the significance disappears, yielding support to my extensive margin explanation.

These results support a story in which lynchings have persistence, since their effect can be felt today in black-white income gaps. However, they are only associations. To subject them to stricter tests, and to argue for causality, I employ three strategies: the use of California as a falsification test, placebo checks using white-on-white lynchings, and reliance on Altonji, Elder, and Taber statistics (2005). I find that the results are robust to these additional tests.

California works as a falsification in this context because lynchings in the state were not targeted against African Americans. Only 8 of the 352 lynchings in my California lynchings

data were against blacks. Mexicans and Native Americans were primary targets. California lynchings should therefore not show up in the contemporary black-white wage gap in that state.

I show California results in Table 2.13. None of the results are statistically significant, and in the case of worker earnings are negative, suggesting that lynchings had no effect on the black-white wage gap in California. Furthermore, the coefficients are much smaller than those for the US South.

Next, I use white-on-white lynchings in the US South from 1882 to 1930 as a placebo check. If these lynchings predict a higher black-white income gap today, then this would call into question my main results. However, as I show in Table 2.14, white-on-white lynchings do not predict present income gaps with significance.

Next, I consider AET statistics. These are based on the observation that selection on observables can be used to assess potential bias from unobservables. This measure is calculated using the coefficient from two regressions: one without controls ($\hat{\beta}^r$), and one with controls ($\hat{\beta}^f$). The coefficient on lynchings is saved in each case, and used to calculate the statistics according to the following formula: $\frac{\hat{\beta}^f}{\hat{\beta}^r - \hat{\beta}^f}$. The intuition behind this is that the smaller the difference between $\hat{\beta}^f$ and $\hat{\beta}^r$, the less selection on observables affects the estimate, and the stronger selection on unobservables needs to be to explain away the entire result.

Table 2.11 reports AET statistics. Of the three ratios reported, none are less than one. The ratios range from 1.77 to 2.80. In the worst case, selection on unobservables would have to be 1.77 times the selection on observables. The median ratio is 2.12. This makes it less likely that the estimated effects of lynchings on contemporary income gaps is fully driven by unobservables.

As a final robustness check, I instrument for past lynchings using cotton acreage planted in 1879, from the 1880 US Agricultural Census. Such an instrument is valid if 1879 cotton acreage affects black-white income gaps only through its effect on lynchings. This is plausible, since cotton acreage determined the extent of black-white labour market competition from

1882 to 1930, and therefore could affect income gaps today. Table 2.15 reports these results. My number of observations drops in each case because a number of counties in 1880 had not yet been created. In each case, the first-stage F-statistic is above 9, mitigating weak instrument concerns (Staiger and Stock, 1997). Furthermore, lynchings significantly predict current income gaps: indeed, the effect size is larger.

2.5.3 Discussion

The modern-day and Mississippi Summer results suggest that the effects of lynchings persist up until the present day. This is consistent with a mechanism in which discrimination continues to affect African Americans. Such prejudice starts with lynchings of African Americans, and subsequently manifests in violence when Civil Rights community organisers went to Mississippi in 1964. It continues to affect contemporary black incomes, relative to their white neighbours.

In particular, these results imply that African Americans fear of retaliation is at the heart of this persistence. If they try to improve their condition economically, as was the case during the Civil Rights movement, then African Americans can expect retaliation, often violent, from antagonistic Southern whites.

2.6 Conclusions

This article adds to a growing literature on the economics of persecution, and the way that past events and institutions persist through time. The empirical literature has so far not demonstrated that labour market concerns can motivate persecution. In this study, I argue that lynchings of African Americans in the US South were a response by whites to suppress blacks' participation in the labour market.

I have shown that lynchings were a response to cotton price shocks, and that these effects were stronger in counties with more black farm workers and railroads. The shock's effect

was also stronger in counties with slavery in 1860. Tests using white-on-white lynchings and legal executions of African Americans suggest that this was not merely due to general feelings of frustration. I provide an explanation for these results: during times of bad cotton prices, whites lynch blacks, and this has the effect of reducing labour supply and increasing wages. Indeed, blacks appear to migrate after a lynching takes place, as my results show.

I then turn to the long-term effects of lynchings, first showing that Mississippi counties with greater violence in 1964 also had more lynchings in the past. I also demonstrate that lynchings contribute to the explanation of black-white income gaps in the US South today.

These results suggest that violent events, like lynchings, can be motivated by labour market concerns and their effects can persist through time. They further indicate that economic shocks can lead to racist violence. Finally, the evidence seems to support Miguel et al.'s (2004) policy suggestion, that income insurance during hard times may reduce violence. The government can invest in public works programs or provide its citizens with better access to savings to prevent violent conflict. Furthermore, contemporary black-white income gap results suggest the need for affirmative action policies to redress historical imbalances that have persisted up until the present.

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2.7 Figures and Tables

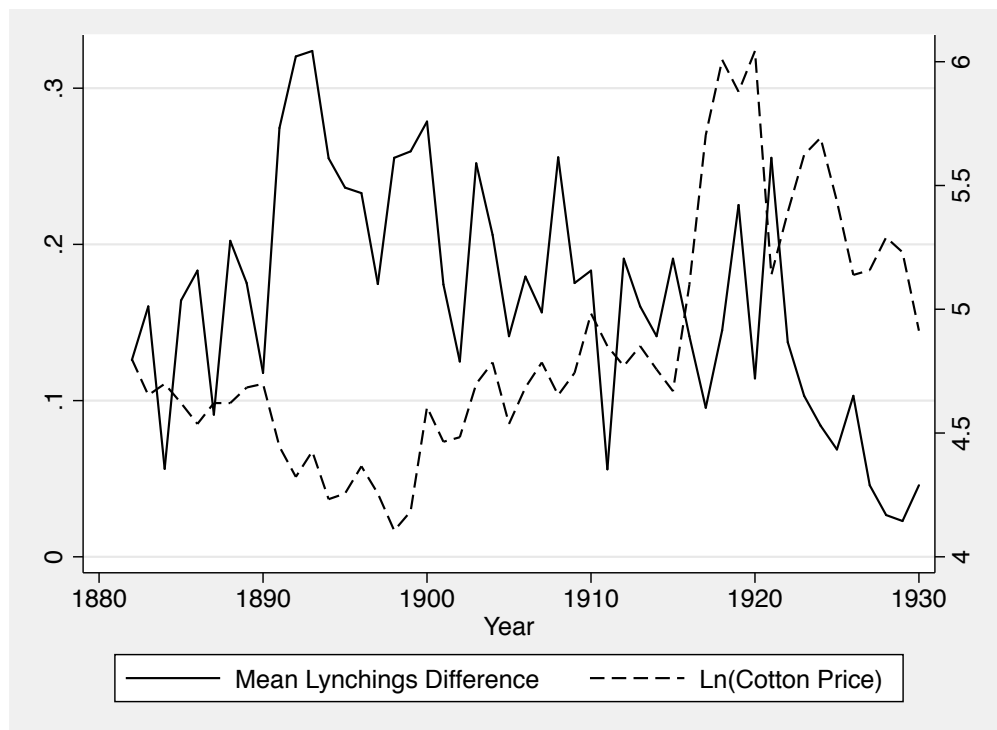


Figure 2.1: Cotton Price and Lynchings

The solid line depicts the difference between (mean) lynchings in cotton counties and (mean) lynchings in non-cotton counties. It is associated with the left y-axis. The dotted line is the logged cotton price, and is associated with the right y-axis. [Source] Cotton prices are from Blattman, Hwang and Williamson (2007), and data on cotton counties are from the dicennial US Census. Lynchings data are from Tolnay and Beck (1995).

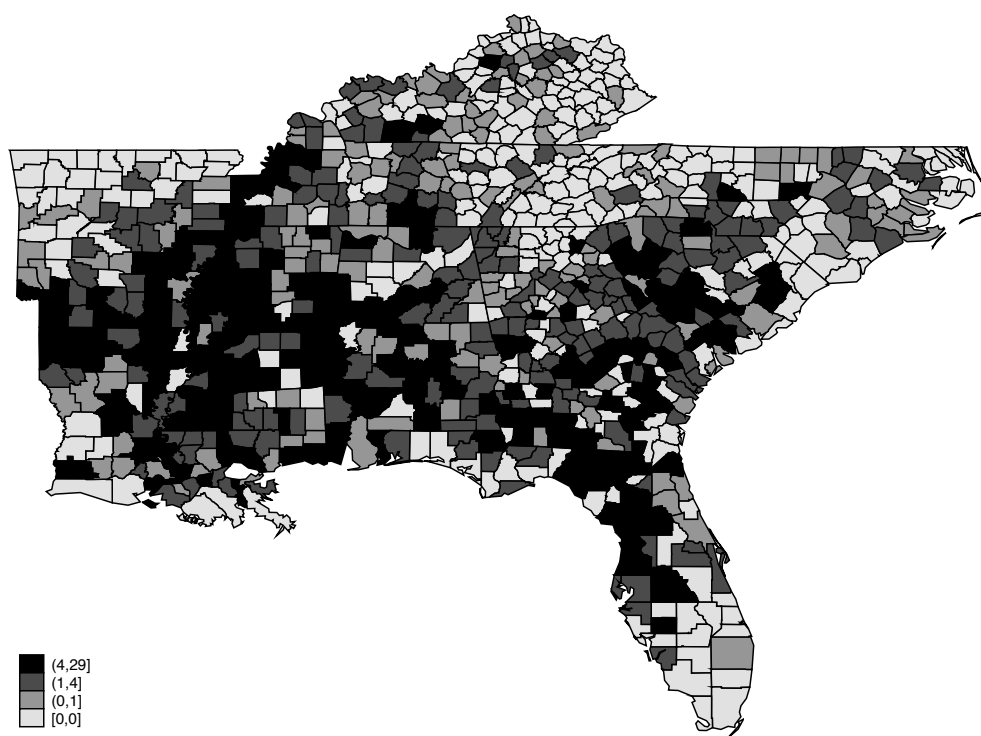


Figure 2.2: Lynchings in Ten US Southern States, 1882-1930

Table 2.1: Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max
Lynchings	13,475	0.18	0.86	0	24
Cotton $\times$ ln(Price)	13,475	4.20	1.71	0	6.04
Mississippi River $\times$ ln(price)	13,475	0.42	1.38	0	6.04
1880 Black Proportion $\times$ ln(price)	13,475	1.84	1.10	0.03	5.51
White lynchings	13,475	0.02	0.23	0	11
Executions	13,475	0.12	0.59	0	16
California lynchings	4,032	0.08	0.55	0	15

Table 2.2: OLS Results for the Effect of Cotton Shocks on Lynchings

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
Lynchings						
Cotton shock	-0.082*** (-2.92)	-0.071*** (-3.12)	-0.048 (-1.62)	-0.057*** (-2.79)	-0.082** (-1.96)	-0.071*** (-2.63)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
County time trends	No	No	Yes	Yes	No	No
Double clustering	No	No	No	No	Yes	Yes
Observations	13,475	13,475	13,475	13,475	13,475	13,475
Counties	275	275	275	275	275	275

Standard errors are clustered by county, and t statistics are in parentheses. Controls are the lagged cotton price interacted with: 1880 black proportion of the population, 1880 black proportion squared, and Mississippi River county dummy. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: OLS Results for Cotton Shock Robustness Checks

Dependent Variable:	(1) Dummy	(2) Dummy	(3) Count	(4) Count	(5) Lynchings after 1892	(6) Lynchings after 1892
Cotton shock	-0.028*** (-3.38)	-0.023*** (-2.63)	-0.082*** (2.92)	-0.066*** (-2.62)	-0.083*** (-2.90)	-0.072*** (-3.17)
Controls with time trend	No	No	No	Yes	No	No
Main Controls without time trend	No	Yes	No	No	No	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,475	13,475	13,475	13,475	10,450	10,450
Counties	275	275	275	275	275	275

Standard errors are clustered by county, and t statistics are in parentheses. ‘Controls with time trend’ means that 1880 black proportion, 1880 black proportion squared, and Mississippi River county dummies are interacted with the time trend instead of the lagged world cotton price.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: OLS Results for Executions and White-on-white Lynchings

	(1)	(2)	(3)	(4)
	Executions	Executions	White lynchings	White lynchings
Cotton shock	-0.027* (-1.80)	-0.002 (-0.16)	-0.013* (-1.93)	-0.127** (-2.22)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	13,475	13,475	13,475	13,475
Counties	275	275	275	275

Standard errors are clustered by county, and t statistics are in parentheses. Controls are the lagged cotton price interacted with: 1880 black proportion of the population, 1880 black proportion squared, and Mississippi River county dummy.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: OLS Results for Complifiers

	(1)	(2)	(3)	(4)	(5)
Shock	-0.009 (-0.37)	0.033* (1.66)	-0.027 (-1.34)	-0.082*** (-2.83)	-0.095*** (-2.77)
Shock $\times$ Interaction	-0.082** (-2.14)	-0.002*** (-5.27)	-0.025** (-2.56)	-0.0003 (-0.03)	0.005 (0.61)
Interaction	1890 railroad	1860 Slavery	Black-white ratio	Poll tax	Literacy test
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
No. of observations	13,475	13,475	13,475	13,475	13,475

Standard errors are clustered by county, and t statistics are in parentheses. Controls are the lagged cotton price interacted with: 1880 black proportion of the population, 1880 black proportion squared, and Mississippi River county dummy.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: OLS Results for the Effect of Lynchings on County Out-Migration

Dependent Variable:	(1)	(2)	(3)	(4)
1920-1930 County Out-Migration				
1920-1930 Lynchings	0.03** (2.23)	0.02* (1.66)		
1910-1919 Lynchings			0.006 (1.04)	-0.004 (-0.63)
Controls	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Constant	0.69*** (28.6)	0.68*** (23.3)	0.69*** (27.3)	0.68*** (22.5)
<i>N</i>	864	601	864	580

Standard errors are clustered by county, and t statistics are in parentheses. Controls include the African American population, urban population, and number of Rosenwald Schools. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: State-Level Agricultural Wages on Lynchings

Dependent variable:	(1)	(2)	(3)	(4)
State-level wages				
Lynchings _{<i>t</i>-1}	0.006* (1.97)	0.006*** (5.56)		
Δ Lynchings _{<i>t</i>-1}			0.0009*** (3.47)	0.0009** (2.52)
Cluster	State	County	State	County
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
No. of observations	13,200	13,200	12,925	12,925

t statistics are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for the lagged world cotton price (time varying) multiplied by a dummy for whether the county produces cotton (cross-sectional).

Table 2.8: Further Robustness: IV and specification

Dependent variable:	(1)	(2)	(3)	(4)
Lynchings				
Cotton shock	-0.161*** (-3.48)	-0.158*** (-3.40)		
Cotton shock (acreage)			-0.002* (0.001)	-0.005** (0.002)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
F-stat	35.84	3743.54	-	-
Observations	13,475	13,475	13,475	13,475
Counties	275	275	275	275

z statistics in columns (1) and (2) are in parentheses, and standard errors are clustered by county.

The F-stat is from the first-stage regressions. t statistics in columns (3) and (4) are in parentheses. Controls are the lagged cotton price interacted with: 1880 black proportion of the population, 1880 black proportion squared, and Mississippi River county dummy.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: OLS Results for Freedom Summer Incidents

	(1)	(2)	(3)	(4)
Dependent Variable:	Incident	Incident	Violent	Violent
Lynchings	0.733** (2.26)	0.532* (1.74)	0.821** (2.77)	0.642** (2.34)
White proportion 1960		-1.149*** (-2.80)		-1.238*** (-3.07)
White proportion squared		0.007** (1.97)		0.008** (2.15)
Poverty rate 1960		-0.551*** (-3.15)		-0.499*** (-2.58)
Constant	3.72 (1.58)	75.3*** (3.71)	4.87* (2.02)	75.8*** (3.74)
R^2	0.209	0.365	0.203	0.410
Observations	35	35	28	28

Standard errors clustered by county, and t statistics in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.10: Summary Statistics for Contemporary Data

Variable	N	Mean	Std. Dev.	Min	Max
Household Gap	697	18,442	7,570.4	-25,341	51,195
Family Gap	697	23,826	10,142.1	-21,820	79,720
Worker Gap	699	8,504	4,318.4	-6,635	22,006
Unemployment Rate	699	0.11	0.03	0.03	0.27
Finance, Insurance, and Real Estate	699	0.04	0.02	0	0.12
Agriculture	699	0.04	0.04	0	0.29
Manufacturing	699	0.12	0.06	0.003	0.33
Service	699	0.16	0.03	0.07	0.34
High School Education	699	0.54	0.15	0.18	1
Single Mothers	699	0.08	0.03	0.02	0.19
Rural-Urban Code	699	4.30	2.45	1	9

Table 2.11: OLS Results for the Effect of Lynchings on Black-White Income Gaps

Black-White Gap:	(1) Household	(2) Household	(3) Family	(4) Family	(5) Worker	(6) Worker
Lynchings	295.6*** (4.11) [3.19]	188.95** (2.24) [2.66]	448.3*** (4.78) [3.67]	330.5*** (3.23) [4.27]	101.9** (2.57) [1.81]	69.3 (1.43) [1.68]
Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
AET Stats		1.77		2.80		2.13
Constant	19043.5*** (25.65) [19.53]	15710.1*** (4.33) [4.79]	23267.8*** (19.66) [14.54]	13678.4*** (2.96) [3.30]	8471.2*** (14.77) [11.86]	4165.6** (2.05) [3.98]
<i>N</i>	697	634	697	634	699	633

Round parentheses show t statistics using standard errors clustered by county. Square brackets show t-statistics using Conley standard errors with 5-decimal-degree cutoffs. Controls include the unemployment rate; proportion of the population working in the FIRE (financial, insurance, and real estate), agriculture, manufacturing, and service industries; proportion with a high school education, proportion of single mothers, a rural-urban continuum code, proportion slave in 1860, and cotton suitability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.12: OLS Results for the Effect of Lynchings on Black-White Unemployment Ratio

Dependent Variable:	(1)	(2)	(3)
Black-White Unemployment Ratio			
Lynchings	0.04** (2.38)	0.03* (1.93)	0.02 (1.18)
Modern Controls	No	Yes	Yes
Historical Controls	No	No	Yes
State FE	Yes	Yes	Yes
Constant	2.11*** (15.37)	1.71*** (3.22)	1.85*** (3.21)
<i>N</i>	848	848	784

t statistics are in parentheses, and standard errors are clustered by county. Modern controls include the unemployment rate; proportion of the population working in the FIRE (financial, insurance, and real estate), agriculture, manufacturing, and service industries; proportion with a high school education, proportion of single mothers, and a rural-urban continuum code. Historical controls are proportion slave in 1860, and cotton suitability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.13: OLS Results for Black-White Wage Gap in California

Black-White	(1)	(2)	(3)	(4)	(5)	(6)
Gap:	Household	Household	Family	Family	Worker	Worker
Lynchings	166.9 (0.97)	99.8 (0.67)	34.6 (0.17)	33.9 (0.22)	-126.4 (-1.50)	-126.0 (-1.64)
Controls	No	Yes	No	Yes	No	Yes
Constant	12155.0*** (3.70)	82862.9* (2.01)	18335.0*** (4.71)	92224.0* (1.86)	5179.9 *** (3.59)	3069.0 (0.15)
<i>N</i>	40	40	39	39	41	41

t statistics are in parentheses, and standard errors are clustered by county. Controls include the unemployment rate; proportion of the population working in the FIRE (financial, insurance, and real estate), agriculture, manufacturing, and service industries; proportion with a high school education, proportion of single mothers; and a rural-urban continuum code. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.14: Placebo Results for the Effect of White-on-white Lynchings on Black-White Income Gaps

Black-White Gap:	(1) Household	(2) Household	(3) Family	(4) Family	(5) Worker	(6) Worker
Lynchings	353.6 (1.38)	408.5 (1.23)	824.9 (1.54)	466.7 (0.91)	163.9 (1.25)	262.4 (1.45)
Controls	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	20107.0*** (28.59)	29430.1 (1.53)	24772.6*** (21.27)	48728.3* (1.83)	8822.1*** (16.25)	9509.3 (0.88)
<i>N</i>	697	634	697	634	699	633

t statistics are in parentheses, and standard errors are clustered by county. Controls include the unemployment rate; proportion of the population working in the FIRE (financial, insurance, and real estate), agriculture, manufacturing, and service industries; proportion with a high school education, proportion of single mothers, a rural-urban continuum code, proportion slave in 1860, and cotton suitability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.15: IV Results for the Effect of Lynchings on Black-White Income Gaps

Black-White	(1)	(2)	(3)	(4)
Gap:	Household	Household	Household	Family
Lynchings	1797.11*** (4.88)	2110.16*** (3.49)	2539.65* (1.88)	2482.67*** (4.82)
Modern Controls	No	Yes	Yes	No
Historical Controls	No	No	Yes	No
State FE	Yes	Yes	Yes	Yes
F-stat	24.55	15.15	9.44	24.55
Constant	12828.44*** (6.60)	14731.2*** (3.50)	13686.8*** (2.78)	14828.02*** (5.95)
<i>N</i>	612	612	555	612
(5)	(6)	(7)	(8)	(9)
Family	Family	Worker	Worker	Worker
2470.03*** (3.57)	2939.08** (1.99)	625.47*** (3.46)	671.21*** (2.65)	966.21* (1.70)
Yes	Yes	No	Yes	Yes
No	Yes	No	No	Yes
Yes	Yes	Yes	Yes	Yes
15.15	9.44	24.56	15.24	9.41
12597.69** (2.51)	10380.9* (1.78)	6287.3*** (6.71)	4305.68** (2.00)	3814.03 (1.51)
612	555	612	612	

z statistics are in parentheses, and standard errors are clustered by county. The F-stat is from the first-stage regression. Modern controls include the unemployment rate; proportion of the population working in the FIRE (financial, insurance, and real estate), agriculture, manufacturing, and service industries; proportion with a high school education, proportion of single mothers; and a rural-urban continuum code. Historical controls are proportion slave in 1860, and cotton suitability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Economic shocks and unrest in French West Africa

3.1 Introduction

Unrest and the threat of unrest have historically been important sources of economic and institutional change (Acemoglu and Robinson, 2000; Ponticelli and Voth, 2011). The opportunity for citizens to contest power during transitory negative shocks can produce democratic transitions (Acemoglu and Robinson, 2001; Bruckner and Ciccone, 2011). Unrest has also changed history within former colonies, where violent resistance shapes institutional legacies and current attitudes (Acemoglu et al, 2002; Wantchekon and Garcia-Ponce, 2013). Unrest disrupts economic activity (Kilian, 2008). Where unrest turns to civil conflict, it may discourage investment (Abadie and Gardeazabal, 2003), disrupt life, labor, and human capital formation (Blattman and Miguel, 2010), interrupt trade (Rohner et al., 2013) and limit state capacity (Besley and Persson, 2010). It is important, then, to understand what causes unrest.

In this paper, we show that unrest in colonial French West Africa was more likely in years of adverse rainfall, higher temperatures, and, with exceptions, low export prices. We

take data on anti-colonial incidents in French West Africa from Huillery (2011) in order to construct a district-level panel covering the years 1906-1956. We merge these data with temperature and rainfall series from Matsuura and Wilmott (2009), a map of commodity production from Hopkins (1973), and multiple sources of commodity price data.

We find that higher levels of rainfall and lower temperatures predict less unrest, as do more favorable prices of cocoa, cotton, and bananas in the districts that produce these. By contrast, higher prices of groundnuts predict greater levels of unrest in regions where these are grown. We demonstrate the robustness of our results to the inclusion of district-specific time trends and to whether we use contemporaneous shocks or moving averages of prices, rainfall and temperature.

To account for our empirical findings, we construct a simple model with two players: a profit-maximizing state, and a group of farmers. The state sets a head tax on farmers, and can choose to invest in suppression (for example, building a military base). Farmers then observe the realization of their income, and can decide whether or not to resist taxation. We find that there are conditions under which such unrest is an equilibrium response to bad shocks. Colonial suppression of such unrest may or may not exist in equilibrium. Consistent with our model, we find that the response of unrest to adverse shocks is strongest where the lack of a pre-colonial kingdom made it more difficult for the state to collect payment from farmers who resisted taxation, in remote areas that lacked diverse income sources, and in areas where the reduced frequency of rainfall shocks encouraged levels of taxation that prompted unrest in bad years.

The former colonies of French West Africa provide an opportunity to evaluate the link between economic shocks and unrest in the same location, but under different institutional contexts. Merging our districts with geo-coded riots and protests since 1997 recorded in the Armed Conflict Location and Event Data Project's (ACLED) database, we show that the pattern of unrest has changed. We find that greater levels of rainfall continue to reduce unrest in the present, but that the link with temperature is no longer apparent. Similarly,

we no longer find significant correlations between commodity prices and present-day riots and protests.

The pattern of response to these shocks has also changed. Riots and protests are more responsive to rainfall in districts where French military intervention lasted longest. The effect of rainfall on unrest is no longer mediated by the presence of pre-colonial states. There is no correlation between past and present levels of unrest, and districts of French West Africa in which unrest was more common under colonial rule are not more responsive to rainfall shocks in the present. We show, then, that the response of unrest to economic shocks depends crucially on institutions. In our example, the political economy of taxation directed the link between economic shocks and unrest.

3.1.1 Related Literature

Our contribution is to offer evidence on the effects of both environmental and commodity price shocks in a single location, under both colonial and post-colonial regimes. Our research is related to an established literature that demonstrates effects of economic shocks on conflict, unrest, and political transitions (Bignon et al., 2014; Bruckner and Ciccone, 2010; Burke and Leiga, 2010; Miguel et al., 2004). This is part of a broader literature linking economic conditions to unrest and conflict (Collier and Hoeffler, 2004; DiPasquale and Glaeser, 1998; Field et al., 2008). Existing literature on the role of these shocks in development has shown that their importance depends on both time and context (Bazzi and Blattman, 2014; Ciccone, 2011). By studying resistance to French colonialism within French West Africa, we reduce potential confounds and offer a specific mechanism through which shocks can precipitate political disturbance.

Our paper is closest to Dube and Vargas (2013), who find that in Colombia negative coffee price shocks cause violence due to an opportunity cost effect, while positive oil price shocks cause conflict due to a rapacity effect. In colonial French West Africa, adverse economic shocks did reduce the opportunity cost of resisting colonial taxation. While rapacity explains

present-day responses of civil conflict to resource prices (e.g. Berman and Couttenier, 2014; Sanchez de la Sierra, 2014), it has no analogue in the colonial period. Rather, we explain the positive link between civil unrest and the prices of palm produce and groundnuts by appealing to the local consumption of these goods, the coincidence of price increases with higher food prices, and political agitation by elites within the class of cultivators. Like Dube and Vargas (2014), we also focus on a single location, but investigate a greater number of shocks, and uncover variables that moderate the responsiveness of conflict both historically and in the present.

Our paper is, then, a contribution to the emerging literature on the heterogeneous effects of economic shocks. Recent work has demonstrated that the response of violence to economic shocks is mediated by several variables, including the cohesiveness of economic institutions (Besley and Persson, 2011), culture (Kung and Ma, 2014), production technologies (Jia, 2014), and the state of the labor market (Fetzer, 2014). Closely related to our work is that of Papaioannou (2014), who finds that extreme weather events in colonial Nigeria increased the incidence of homicide, imprisonment, and court cases. Our contribution is twofold. First, we use the heterogeneity of responses to colonial shocks in order to validate a model that explains the political economy of economic shocks and unrest in a specific context. Second, we demonstrate that the response of unrest to these shocks changes with the political environment within a single location.

Finally, we add to the literature on historical persistence (e.g. Dell, 2010; Nunn, 2008) since we explore the reactions of both historical and modern unrest to shocks and the persistence of these reactions over time. Much of this literature has focused on the long-run effects of historical institutions on economic growth and living standards (Banerjee and Iyer, 2005; Bruhn and Gallego, 2012; Iyer, 2010). A portion of this literature has uncovered deep roots of violence in the present and recent past (Jha, 2013; Voigtlander and Voth, 2012), including conflict in Africa (Depetris-Chauvin, 2013; Heldring, 2014). Recently, Nunn (2014) has suggested that geography can shape long-run development by mattering at crucial periods

in history. Although the environmental shocks that we study have this potential, we find no evidence of such long-run effects. Similarly, the trend in much of this literature is to demonstrate the presence of historical persistence. Our contribution, by contrast, is to show an example in which this persistence is absent. The political economy of unrest in French West Africa has changed since the colonial period.

We proceed as follows. In Section 3.2 we present both historical background on French West Africa and analyze a model of civil unrest. The model’s predictions guide our empirical work. In Section 3.3 we describe the data and empirical strategy, before reporting estimates in Section 3.4. Section 3.5 concludes.

3.2 Model

3.2.1 Historical Background

France occupied trading posts along the West African coast such as Gorée throughout the period of the trans-Atlantic slave trade. From the 1850s to the 1880s, French military units attacked coastal regions along Senegal, Mauritania, Guinea, and Dahomey, but it was not until the 1880s that the French expanded inland, advancing as far as current Mali. In 1895, French West Africa (*Afrique Occidentale Française*) was officially created.

French rule in this region depended on taxable production of commodities such as coffee, cocoa, and cotton. In return for their taxes, Africans received little; military conquest and pacification accounted for 80% of the cost of French West Africa to France, and the salaries of French civil servants were a disproportionate share of local expenditure Huillery (2012). At the same time, France devoted few of its own resources to African development: from 1844 to 1957, only 0.29 percent of French public expenditures were spent in French West Africa. Head taxes unrelated to income accounted for 39% of total revenue over the 1907-1957 period Huillery (2012). The collection of these taxes was delegated to the canton and village chiefs,

although farmers owned the land they cultivated (Suret-Canale, 1971, p. 346).¹

Although we focus on head taxes in our model, there were several forms of extraction that would have provoked African resistance during depressed periods. Economic rents were extracted through artificially low export prices, high import prices, compulsory cultivation of certain crops, and coercive labor institutions (Berg, 1960; Tadei, 2013a, b). Compulsory labor was used for the construction and maintenance of roads and other public works, in portage, for public works, and in private concessionary companies (Conkin, 1998). Labor compulsion was greatest where the production of export crops was possible (Tadei, 2013c). The burdens of direct taxation and forced labor were more extensive in French West Africa than in Britain's West African colonies (Asiwaju, 1976, p. 583). A parallel system of law and courts known as the *indigénat* gave local administrators legal cover for this coercion. As Mann (2009, p. 334-335) puts it:

In 1932, when an African auxiliary (*garde-cercle*) stuffed recalcitrant taxpayers into a small dwelling in which ten of them would suffocate, it was the *indigénat* that originally provided legal justification for his actions. And in the same year, when a man died of a cerebral hemorrhage after a week in detention, the only charge against him – the cause of his imprisonment – was a ‘bad attitude towards paying his taxes’, an offense punishable under the *indigénat*.

Importantly, colonial taxes did not adjust smoothly in response to conditions of production. As these were often decided at the level of the colony (there were eight colonies) and subject to the authority of Dakar, they were not easily tailored to local conditions (Manning, 1998).² Huillery (2011) does not report a complete series of tax rates, but her incomplete data do allow for some illustrative examples. Across 87 districts for which she reports head tax rates in both 1928 and 1936, 65 saw their tax rate increase over the course of the Great Depression. The average percentage change was an increase of 21%. The colonial state

¹Those who could not grow cash crops were encouraged to migrate to wage-earning areas.

²The Governor General in Dakar also had control over the military.

continued to recruit labor and collect taxes during a severe 1913-14 famine (Clark, 1995). Similarly, officials recognized that tax obligations were prompting migration from the frontier of the Ivory Coast to the neighboring Gold Coast (Ghana) in 1935, but did not act on proposals to reduce the tax burden (Asiwaju, 1976, p. 585). In Koudougou, French policies worsened a 1931 famine by hindering food-crop production in favor of cotton (Cordell and Gregory, 1982, p. 212). As commodity prices fell during the Great Depression, poll taxes remained unchanged (Suret-Canale, 1971, p. 280). It was under this often unsympathetic and authoritarian regime that resistance to colonial rule became common.

Resistance could take many forms, ranging from quiet out-migration to urban protests to armed insurrection (Asiwaju, 1976; Saul and Royer, 2001; Wantchekon and Garcia-Ponce, 2013). Suppression could also take many forms. Dogon resistance to taxation in 1908 was met with a ‘police tour’ that destroyed local crops (Suret-Canale, 1971, p. 106). The colonial government countered the independence movement in the Ivory Coast by “falsifying election results, creating opposition parties, arresting [political] leaders, persecuting supporters, shutting down newspapers, and shooting demonstrators” (Wantchekon and Garcia-Ponce, 2013). The economic literature on French West African violence is small. In an important contribution, Huillery (2011) instruments European settlement in French West Africa using cross-sectional variation in early colonial resistance. She finds that colonial settlement improves current economic outcomes, an argument in line with other studies of historical persistence (e.g. Acemoglu et al., 2001). What caused this resistance has not been shown empirically, and this is a question we turn to in this paper.

3.2.2 A Simple Model of Unrest in French West Africa

Setup

We model civil unrest as a game between two risk-neutral players: a profit-maximizing colonial state, and a group of farmers. This is a stylized representation of the specific case we consider, since French West Africa’s economy was largely agricultural. In our model, the state

moves first, setting a head tax on farmers, and also deciding whether to invest in suppression. Practically, suppression could mean building a fort or stationing military equipment in a district. Farmers then realize their income, and can choose to resist. Resistance in this case includes actions such as riots or protests that signal farmers' refusal to pay taxes. The first purpose of this model is to show that it is possible for resistance to be an equilibrium response to adverse income shocks. The second purpose is to derive conditions that make such equilibria more likely.

The game's timing is as follows. In Period 1, the state sets a head tax, $\tau \geq 0$. The state also chooses whether or not to invest in suppression. This is a binary decision; investing in suppression costs s , while not investing costs nothing. The importance of suppression is that it allows the state to recover a portion of the farmers' income in the event of resistance. Investment in suppression, then, will discourage resistance. We assume for simplicity that a state that is indifferent between investing in suppression and not will choose no suppression. Similarly, a state that is indifferent between two tax rates will choose the lower rate.

In Period 2, Nature moves, and farmers' income is realized as either low, y_L , or high y_H . Clearly, $y_H > y_L$. Low income occurs with probability α and high income occurs with probability $1 - \alpha$.

In Period 3, farmers observe both whether the state has invested in suppression and the tax τ . We assume limited liability. If the tax exceeds income, so that $y < \tau$, then farmers pay all their income as tax in the event that they do not resist taxation. This captures farmers' credit constraints. We show later that when $y < \tau$, unrest always occurs.

Farmers choose to resist or not in this period. No resistance means that farmers simply pay the tax, and carry on as normal. This leads to a payoff of $y - \tau$. If the farmers choose resistance, then outcomes depend on whether or not the state chose suppression in Period 1. If the state chose suppression, then unrest succeeds with probability p . If the state chose no suppression, then unrest always succeeds. Unrest is destructive, damaging proportion $(1 - \mu)$ of income, where $\mu \in (0, 1)$. If resistance succeeds, farmers do not pay their tax,

and so they receive a payoff of μy . If unrest is unsuccessful, then farmers must pay the state a proportion γ of their remaining income, where $\gamma \in (0, 1)$. They receive a payoff of $\mu(1 - \gamma)y$. We assume for simplicity that a farmer indifferent between resisting taxation and not resisting will choose not to resist.

Finally, in Period 4, payoffs are realized for both the state and farmers. We solve this game using backward induction.

Period 4

Final payoffs are denoted $V^i(a_i|a_j)$, where $i \in \{f, s\}$ denotes either farmers or the state, and a_i denotes i 's action, given a tax rate τ . The actions available to the state are either S , suppression, or NS , no suppression. The farmers' actions are either R , resistance, or NR , no resistance. We use y_k to denote income, where $k \in \{L, H\}$.

Farmers' final payoffs are, then:

$$\begin{aligned} V^f(R|S) &= p\mu y_k + (1 - p)(1 - \gamma)\mu y_k \\ V^f(R|NS) &= \mu y_k \\ V^f(NR|S) &= V^f(NR|NS) = \max\{y_k - \tau, 0\} \end{aligned}$$

Payoffs for the state are similarly given by:

$$\begin{aligned} V^s(S|R) &= (1 - p)\gamma\mu y_k - s \\ V^s(NS|R) &= 0 \\ V^s(NS|NR) &= \min\{\tau, y_k\} \\ V^s(S|NR) &= \min\{\tau - s, y_k - s\} \end{aligned}$$

Period 3

Farmers observe whether the state invested in suppression in Period 1, and also observe the tax rate. The tax rate, τ , may be less than or greater than income, y_k . This gives us four cases to consider: suppression and $\tau \leq y_k$; suppression and $\tau > y_k$; no suppression and $\tau \leq y_k$; and, finally; no suppression and $\tau > y_k$. We consider these cases in turn.

In the first case, when the state chooses to suppress resistance and y_k exceeds τ , then farmers resist if and only if $V^f(R|S) > V^f(NR|S)$. That is, farmers resist taxation if resistance offers a better payoff than none. Comparing payoffs under the two scenarios suggests that farmers resist if taxes are too high. Under high income, they will resist if:

$$\tau > \{1 - \mu[1 - \gamma(1 - p)]\}y_H \equiv \tau_{SH}.$$

Similarly, under low income, farmers will resist taxation if:

$$\tau > \{1 - \mu[1 - \gamma(1 - p)]\}y_L \equiv \tau_{SL}.$$

Here, we have defined τ_{SH} and τ_{SL} as the tax rates above which further taxation will induce resistance when the state has invested in suppression under high and low incomes. Because the opportunity cost of resistance is increasing in income, this tax threshold is lower in the low income state. These cutoff rates are both increasing in γ , the proportion of output recovered by the state. Both are decreasing in p , the probability of a successful revolt. Both p and γ can be thought of as measures of state capacity. Conditional on the tax rate, a weaker state will be more likely to face resistance. This could explain, for example, why we find below that unrest manifests more in response to adverse shocks in stateless societies. In these districts, there is no state capacity that the colonial authority can build upon to easily suppress dissidents. In terms of the model, γ is low and p is high.

In the second case, when the state chooses suppression and $y_k < \tau$, taxes exceed income, and unrest always occurs. This is because it is always better to resist than to receive zero income. More precisely, when farmers' outside option is zero, then unrest offers higher expected payoffs.

In the third case, when the state does not invest in suppression and $\tau \leq y_k$, resistance

occurs when $V^f(R|NS) > V^f(NR|NS)$. Again, comparing payoffs under the two scenarios suggests that farmers resist if taxes are too high. Under high income, they will resist under high income if:

$$\tau > (1 - \mu)y_H \equiv \tau_{NSH}.$$

Under low income, this condition is

$$\tau > (1 - \mu)y_L \equiv \tau_{NSL}.$$

Following similar notation as above, we have defined τ_{NSH} and τ_{NSL} as the tax rates above which further taxes will induce resistance when the state has not invested in suppression under high and low incomes. Intuitively, a low cost of rebellion $(1 - \mu)$ implies a higher likelihood of unrest, conditional on the tax rate. It is also apparent from the above that $\tau_{SL} > \tau_{NSL}$ and $\tau_{SH} > \tau_{NSH}$; investment in suppression enables the state to collect more tax revenue without facing resistance.

Finally, if $\tau > y_k$, then the state simply taxes all the farmers' income. As under the case when the state has invested in suppression, resistance always occurs, because it is better to resist than to be left with nothing.

Period 2

Nature draws y_k from set $\{y_L, y_H\}$ with probabilities α and $1 - \alpha$, respectively.

Period 1

In period 1, the state decides on its preferred tax rate and whether or not to invest in suppression. In equilibrium, there are only four tax rates that the state might choose: τ_{SL} , τ_{SH} , τ_{NSH} , and τ_{NSL} . These correspond to the maximum tax rates that prevent resistance in the high income state, or in both states, conditional on the decision of whether or not to invest in suppression. τ_{SL} ensures that there will be no resistance in the case that the state has chosen to invest in suppression. τ_{SH} , by contrast, guarantees that a state that has invested in suppression will face no resistance when income is high but will face resistance if income is

low. A state that has invested in suppression will not choose any other tax rate. If $\tau < \tau_{SL}$, the state can increase taxes without facing any increased risk of resistance. Similarly, if $\tau_{SL} < \tau < \tau_{SH}$, the state can again increase taxes without changing farmers' decisions to resist taxation. We show in Appendix A.1.1, which is at the end of this thesis, that the payoff to setting $\tau > \tau_{SH}$ is lower than choosing $\tau = \tau_{SH}$ after investing in suppression, because unrest is destructive. A similar logic rules out $\tau > \tau_{NSH}$ when the state does not choose suppression.

This implies that there are four possible outcomes:

1. *Case SH*: The state invests in suppression and chooses $\tau = \tau_{SH}$. Farmers will resist taxation if incomes are low, and so unrest with suppression occurs with probability α .
2. *Case SL*: The state invests in suppression and chooses $\tau = \tau_{SL}$. Farmers will not resist taxation in any state, and so unrest and suppression do not occur.
3. *Case NSH*: The state invests in suppression and chooses $\tau = \tau_{NSH}$. Farmers will resist taxation if incomes are low, and so unrest without suppression occurs with probability α .
4. *Case NSL*: The state invests in suppression and chooses $\tau = \tau_{NSL}$. Farmers will not resist taxation in any state, and so unrest and suppression do not occur.

It remains to consider the conditions under which each of these possible equilibria may exist. We show in Appendix A.1.2 that the expected payoff to the state in each of the four cases is given by:

$$EV_{SH}^s = \alpha(1-p)\gamma\mu y_L + (1-\alpha)\{1-\mu[1-\gamma(1-p)]\}y_H - s$$

$$EV_{SL}^s = \{1-\mu[1-\gamma(1-p)]\}y_L - s$$

$$EV_{NSH}^s = (1-\alpha)(1-\mu)y_H$$

$$EV_{NSL}^s = (1-\mu)y_L$$

The state will chose taxes and suppression depending on the values of y_H , y_L , α , γ , μ , p and s . The outcome achieved in equilibrium will depend on which is greater: EV_{SL}^s , EV_{SH}^s , EV_{NSH}^s , or EV_{NSL}^s .

Equilibrium

Here, we describe the levels of suppression and taxation that exist in equilibrium, as well as the incidence of resistance. Begin by defining:

$$\begin{aligned}\Phi_{SL}^{SH}(y_L) &\equiv \frac{1-\mu[1-(1-\alpha)\gamma(1-p)]}{(1-\alpha)\{1-\mu[1-\gamma(1-p)]\}}y_L \\ &\equiv \theta_{SL}^{SH}y_L\end{aligned}$$

$$\begin{aligned}\Phi_{NSH}^{SH}(y_L) &\equiv \frac{s}{(1-\alpha)\mu\gamma(1-p)} - \frac{\alpha}{1-\alpha}y_L \\ &\equiv \lambda_{NSH}^{SH} - \theta_{NSH}^{SH}y_L\end{aligned}$$

$$\begin{aligned}\Phi_{NSL}^{SH}(y_L) &\equiv \frac{s}{(1-\alpha)\{1-\mu[1-\gamma(1-p)]\}} + \frac{1-\mu[1+\alpha\gamma(1-p)]}{(1-\alpha)\{1-\mu[1-\gamma(1-p)]\}}y_L \\ &\equiv \lambda_{NSL}^{SH} + \theta_{NSL}^{SH}y_L\end{aligned}$$

$$\begin{aligned}\Phi_{NSL}^{SL} &\equiv \frac{s}{\mu\gamma(1-p)} \\ &\equiv \theta_{NSL}^{SL}\end{aligned}$$

$$\Phi_{SL}^{NSH}(y_L) \equiv -\frac{s}{(1-\alpha)(1-\mu)} + \frac{1-\mu[1-\gamma(1-p)]}{(1-\alpha)(1-\mu)}y_L$$

$$\begin{aligned}
&\equiv -\lambda_{SL}^{NSH} + \theta_{SL}^{NSH} y_L \\
\Phi_{NSL}^{NSH}(y_L) &\equiv \frac{1}{1-\alpha} y_L \\
&\equiv \theta_{NSL}^{NSH} y_L
\end{aligned}$$

These functions of y_L allow the (y_H, y_L) space to be partitioned in order to describe the conditions under which the state prefers one outcome to another:

$$\begin{aligned}
\mathcal{S}_{SH} &= \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \geq \Phi_{SL}^{SH}(y_L), y_H \geq \Phi_{NSL}^{SH}(y_L), y_H \geq \Phi_{NSH}^{SH}(y_L)\}, \\
\mathcal{S}_{SL} &= \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \leq \Phi_{SL}^{SH}(y_L), y_L \geq \Phi_{NSL}^{SL}\}, \\
\mathcal{S}_{NSH} &= \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \leq \Phi_{NSH}^{SH}(y_L), y_H \geq \Phi_{NSL}^{NSH}(y_L)\}, \\
\mathcal{S}_{NSL} &= \{(y_H, y_L) \in \mathbb{R}_+^2 : (y_H, y_L) \notin \mathcal{S}_{SH} \cup \mathcal{S}_{NSH} \cup \mathcal{S}_{NSL}\}.
\end{aligned}$$

At this point, we can derive the main result of our model.

Proposition. *The payoffs to the state lead to the following equilibrium outcomes:*

1. *The state (weakly) prefers suppression with $\tau = \tau_{SH}$ if $(y_H, y_L) \in \mathcal{S}_{SH}$.*
2. *The state (weakly) prefers suppression with $\tau = \tau_{SL}$ if $(y_H, y_L) \in \mathcal{S}_{SL}$.*
3. *The state (weakly) prefers no suppression with $\tau = \tau_{NSH}$ if $(y_H, y_L) \in \mathcal{S}_{NSH}$.*
4. *The state (strongly) prefers no suppression with $\tau = \tau_{NSL}$ if $(y_H, y_L) \in \mathcal{S}_{NSL}$.*

If $(y_H, y_L) \in \mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$, resistance occurs with probability α , i.e. when farmers face adverse income shocks.

Proof. See Appendix A.1.3, at the end of this thesis. □

These regions are shown in Figure 3.1.

Comparative statics

As we show in appendix A.1.4, at this thesis's end, the regions $\mathcal{S}_{SH}$, $\mathcal{S}_{SL}$, $\mathcal{S}_{NSH}$, and $\mathcal{S}_{NSL}$ shown in Figure 3.1 are straightforward to depict once $\Phi_{SL}^{SH}(y_L)$, $\Phi_{NSL}^{SH}(y_L)$, $\Phi_{NSH}^{SH}(y_L)$, $\Phi_{SL}^{NSH}(y_L)$, $\Phi_{NSL}^{NSH}(y_L)$ and Φ_{NSL}^{SL} are defined.

This proposition yields the intuitive empirical prediction that unrest, if it occurs, will happen during periods of low income. In our data, this will correspond to bad harvests or periods of depressed commodity prices. It is also clear from Figure 3.1 that this outcome will be most likely when y_H is large relative to y_L . This too is intuitive; the high tax rates that prompt resistance are only worth imposing if the expected gain to the state when incomes are high outweighs the loss when incomes are low.

It is also clear from the proposition and Figure 3.1 that suppression will be observed during periods of low income if y_L is large relative to s (i.e. y_L is to the right of $\Phi_{NSH}^{SH}(y_L)$). This is again intuitive: the value of output that can be recovered in the low state must be large relative to the cost of recovery in order to justify investment in suppression.

There are five parameters apart from y_L and y_H that determine whether or not unrest will be an equilibrium response to low incomes: α , γ , μ , p and s . Here we discuss the effect of these parameters on the size of the region in which unrest is an equilibrium response to low incomes, i.e. the size of $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$. This region is bounded by $\Phi_{SL}^{SH}(y_L)$, $\Phi_{NSL}^{SH}(y_L)$, and $\Phi_{NSL}^{NSH}(y_L)$. We derive comparative statics in Appendix A.1.5, and summarize these here.

In response to an increase in α , $\Phi_{SL}^{SH}(y_L)$ becomes steeper, $\Phi_{NSL}^{SH}(y_L)$ shifts upward, and $\Phi_{NSL}^{NSH}(y_L)$ becomes steeper. These changes work to shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$. An increase in α , then, works against the existence of unrest in equilibrium. This is intuitive. Unrest exists if the state has set high taxes that increase its payoff in the high income state at the expense of its payoff in the low income state. If the low income state is more likely, this becomes a less profitable strategy.

In response to an increase in γ , $\Phi_{SL}^{SH}(y_L)$ rotates downwards, $\Phi_{NSL}^{SH}(y_L)$ shifts downward, and $\Phi_{NSL}^{NSH}(y_L)$ does not move. These changes work to expand the region $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$.

Although farmers are less likely to resist a given tax burden if state capacity is greater, as measured by γ , a greater ability to recover payment from farmers in the case of unrest makes the state more willing to set taxes that will be resisted in the low income state.

In response to an increase in μ , $\Phi_{SL}^{SH}(y_L)$ may rotate in either direction, $\Phi_{NSL}^{SH}(y_L)$ shifts upward, and $\Phi_{NSL}^{NSH}(y_L)$ does not move. These changes have an ambiguous effect on the region $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$.

In response to an increase in p , i.e. a reduction in $(1 - p)$, $\Phi_{SL}^{SH}(y_L)$ rotates upwards, $\Phi_{NSL}^{SH}(y_L)$ shifts upwards, and $\Phi_{NSL}^{NSH}(y_L)$ does not move. These changes work to shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$. This is again intuitive; a greater probability of successful unrest reduces the state's incentive to set tax rates that will prompt resistance under low incomes.

In response to an increase in s , $\Phi_{SL}^{SH}(y_L)$ does not move, $\Phi_{NSL}^{SH}(y_L)$ shifts upward, and $\Phi_{NSL}^{NSH}(y_L)$ does not move. These changes work to shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{NSH}$. Intuitively, if it is more costly for the state to recover revenue in a state of unrest, this will discourage the state from setting levels of taxation that prompt unrest.

The conditions under which the state will invest in suppression are not central to our results, but we present the relevant comparative statics in thesis appendix A.1.6.

3.3 Data and Empirical Strategy

3.3.1 Empirical Strategy

To test the effects of temperature, rainfall, and price shocks on unrest in both the colonial and modern periods, we estimate the following specification:

$$Unrest_{i,t} = \beta Shock_{i,t} + \delta_i + \eta_t + \epsilon_{i,t}. \quad (3.1)$$

Here, $Unrest_{i,t}$ is a dummy for whether unrest occurred in district i in year t . In practice, this is one of two variables. For the colonial period, we use $Incident_{i,t}$, an indicator for

whether the annual report for district i in year t fails to report that nothing happened in the district. For the modern period, we use $Riots/Protests_{i,t}$. This is an indicator for whether a riot or protest occurred.

$Shock_{i,t}$ is a measure of the climate or price shock experienced by district i in year t . For environmental shocks, we use the three year moving average of either $\ln(Rain)_{i,t}$ or $\ln(Temperature)_{i,t}$. The definition of this shock is the same in both the colonial and modern periods. For colonial price shocks, we interact the three year moving average of international commodity prices with dummies that indicate whether a district produced a specific commodity: $Exporter_i^j \times \ln(Price^j)_{i,t}$. For example, we may interact the moving average of the log world cocoa price with an indicator for whether a district produces cocoa.

δ_i and η_t are district and year fixed effects, respectively.³ We use these to control for omitted heterogeneity at the level of districts and time periods. The equation is estimated using OLS. Standard errors are clustered by district. For our identification strategy, we exploit the fact that rainfall, temperature, and world commodity prices are exogenous from the perspective of a single district. A negative coefficient on β implies that the shock negatively predicts unrest, while a positive coefficient β means that the shock positively predicts unrest.

3.3.2 Data

We acquire colonial unrest data from Huillery (2011). She collected these from Annual Political Reports housed in the Archives Nationales in Paris. These reports were written to the federal governor of French West Africa by the local governors of each colony. Intended to inform both the federal governor and the French administration about political developments in each colony, these were compiled from quarterly district reports. Her sample covers years ending in 3, 6, and 9, between 1906 and 1956, inclusive. For additional documentation, see Huillery (2011).

Events described in the original reports cover a wide range of categories, including the

³15.6% of the variance in colonial incidents is explained by these fixed effects.

disposition of the population, riots, opposition by local chiefs, problems in tax collection, and resistance to labor recruitment. The anti-colonial resistance expressed by these events includes the initial resistance to military conquest, refusal to obey, pay taxes, provide coerced labor, and enroll in the police, as well as nationalist activities (Huillery, 2011). Our main dependent variable is a summary indicator, taking a value of 0 if the report notes that nothing happened in the district in a given year, and 1 otherwise.

We collect data on modern riots and protests from the *ACLED* (Armed Conflict Location and Event Data) database. These cover the years 1997 to 2011. The data contain both dates and geographic coordinates for all reported instances of political violence in the countries that they cover. Geographic coordinates allow us to assign these events to the former colonial districts of French West Africa, so that our sample of districts remains unchanged across periods. Information on these events are compiled from multiple sources, including media and country reports, publications from humanitarian agencies, and published research. For the closest similarity between the colonial and modern data, we focus on riots and protests. Other events reported by the *ACLED* are not analogous to the events in the Huillery (2011) data. These include military battles and the establishment of bases. *ACLED* codes protests as non-violent meetings of three or more persons assembled for a for a political purpose, whether planned or spontaneous. Previously recognized groups, including political parties, may participate in protests. If these turn violent, they are then counted as “riots.”

Rainfall and temperature data are from the standard Matsuura and Wilmott (2009) series.⁴ The raw data are reported on a $0.5^\circ \times 0.5^\circ$ grid. To compute the annual temperature for an individual district, we average over the monthly data for all the points within a district’s boundaries. For districts too small to contain a single point in the Matsuura and Wilmott (2009) data, we use temperatures at the nearest grid point.

We take commodity prices and price indices from several sources: Allen (2003); Allen and Unger (2003); Bazzi and Blattman (2014); Blattman et al. (2007); the *Historical Statistics of*

⁴See <http://climate.geog.udel.edu/~climate/>.

the *United States*;⁵ Lord and Boye (1991) and *Historical Statistics for Mineral and Material Commodities in the United States*.⁶ The specific series used for each commodity are specified in Table A1 in the Thesis Appendix. Commodity prices for the modern period are all taken from Bazzi and Blattman (2014).

Commodity prices that were reported in pounds were converted to real values using the price index from Allen and Unger (2003), taking 1930 as a base year. Similarly, prices reported in US dollars were converted to real values using the price index from the *Historical Statistics of the United States*, taking 1930 as a base year for the historical period. We convert commodity UK price series to US dollars using nominal exchange rates from Measuring Worth, and then deflate by the US price index (only coffee and palm oil are affected by this). Modern commodity prices were converted into real prices using the Bureau of Labor Statistics' Consumer Price Index with 2000 as the base year. Historical prices for cocoa, coffee and palm produce required merging the series from Blattman et al. (2007) and Bazzi and Blattman (2014). We normalize the Bazzi and Blattman (2014) series so that they are equal to the Blattman et al. (2007) series for the year 1950, which is the one point at which they overlap. The same procedure was used to join the cotton series from New York (1900-1941), ten markets (1941-1956) and the Bureau of Labor Statistics (1956-1960) reported in the *Historical Statistics for Mineral and Material Commodities in the United States*. We graph our commodity price series of interest in Figure 3.2.

To assign commodity price shocks to individual districts, we require information on which districts in French West Africa produce which products. These data are taken from the Hopkins (1973) map of export crop production in West Africa. We treat a district as a producer of a given commodity if the production zone mapped by Hopkins (1973) intersects with any part of this district. As an example, we provide a map of districts coded as cocoa-producing according to the Hopkins (1973) map in Figure 3.3. Classifications in Hopkins (1973) are consistent with the less detailed map that Austin (2009) gives for 1914, and the

⁵See <http://hsus.cambridge.org>.

⁶See <http://minerals.usgs.gov/minerals/pubs/historical-statistics/>.

list of major exports by country in the late 1950s provided by Thompson and Aldoff (1975).

The commodities we consider are bananas, cocoa, coffee, cotton, groundnuts, and palm produce (kernels and oil). This exhausts the list of products that intersect French West Africa in the Hopkins (1973) map. Though this is not a complete list of the products exported by the colonies of French West Africa, it accounts for the bulk of export earnings. Indeed, groundnuts, coffee and cocoa together accounted for 70% of French West Africa's export earnings during the 1950s (Berg, 1960). Each of the products recorded in the Hopkins (1973) map was exported, though some had domestic markets as well.

The inclusion of district fixed effects in our baseline specification eliminates the need to control for district characteristics. We do, however, use additional variables from Huillery (2009, 2011) in order to test for heterogeneous responses to conflict. In particular, we use an indicator for access to the sea, an indicator for a navigable river, a dummy variable for a kingdom at the end of the nineteenth century, an indicator for a trading post before the colonial conquest, an indicator for an acephalous society before the colonial conquest, an indicator for above-median distance from the coast, an indicator for above-median distance from a port, an indicator for above-median area, an indicator for above-median years of resistance to French colonial conquest, an indicator for above-median date of the start of colonial conquest, and an indicator for above-median date of the final military intervention before a district submitted to French rule. We compute the coefficient of variation of rainfall using the Matsuura and Wilmott (2009) data.

Summary statistics for our principal dependent variables, our shocks of interest, and district characteristics are shown in Table 1. We map the mean prevalence of colonial incidents and modern riots/protests in Figures 3.4 and 3.5, respectively.

3.4 Results

3.4.1 Colonial period

Main results

Table 2 reports our main results for the 1906 to 1956 period. In addition to clustering by district, we report Conley (2009) standard errors adjusted for spatial dependence at distances of both 5 and 10 decimal degrees. These standard errors are smaller than when we cluster by district. Columns (1) and (2) show the results using rainfall and temperature shocks. Both effects are similar in magnitude. A standard deviation decrease in rainfall causes a 0.43 standard deviation increase in unrest, or 14.6 percentage points. A standard deviation increase in temperature causes a 0.45 standard deviation increase in unrest, or 15.2 percentage points. This is as we would expect; a decrease in rainfall or an increase in temperature means less income for farmers, and so the opportunity cost of violence declines.

A similar logic holds for results in columns (3) to (6). A decrease in world commodity prices increases unrest. For example, a 50% decrease in the price of cocoa leads to a 6 percentage point increase in the probability of an incident, or 0.18 standard deviations.⁷ These are all labor-intensive crops, so the results are, again, intuitive.

Columns (7) and (8) present a puzzle. Positive price shocks to groundnuts and palm increase unrest. There are several possible reasons for this.⁸ First, groundnut producers may also be groundnut consumers, so that a positive price shock increases living costs. Groundnuts have historically provided a high-protein substitute to meat for many poor people in colonial West Africa (Moradi et al., 2013).

Further, we can discuss the history of specific episodes of quiet and unrest in pro-

⁷ $-0.124 \times 0.5 \approx 0.062$, $-0.124 \times 0.5/0.34 \approx -0.182$. Because of zeroes for non-producing districts, a one standard deviation change in the shock measure is not meaningful. Figure 3.2 shows that a 0.5 unit change in the log price of any of the commodities of interest is well-within the sample variation.

⁸Although groundnut prices are not available for 1906 or 1909, this does not explain the difference. Baseline results for our other shocks of interest are identical in significance and sign and similar in magnitude if these years are dropped from the sample (not reported).

ducing regions. Our groundnut result is driven largely by two episodes. First, many groundnut-producing districts were unusually quiet during the low prices of the 1930s: Kayes and Bafoulabe in 1933, Dedougou and Gaoua in 1936, and Tivaouane in 1939. Second, groundnut-producing districts were universally unquiet in 1943, despite the high prices paid for groundnuts during the Second World War.

Both of these episodes are explainable. During the 1930s, the colonial state used agricultural cooperatives to stock and distribute seeds, and to strengthen the power of the Muslim religious leaders that dominated groundnut production (Boone, 2006, p. 43). The French government temporarily provided producers with price support (Thompson and Adloff, 1958, p. 312). These efforts would have quelled unrest. By contrast, during the Second World War, imports were scarce due to rationing and coercion of African labor increased under the Free French (e.g. Lawler, 1990). This offset greater export prices, and led to protest.

In addition to these temporary pressures, several factors made groundnut production unusual in French West Africa, connecting its expansion with processes of upheaval and social change. Even in the nineteenth century, groundnuts were an expansive crop cultivated largely by migrants (Brooks, 1975). Many of these early cultivators left behind a troubled past; they included ruined peasants, liberated slaves, and redundant laborers (Lachenmann, 1993). The spread of groundnut production was largely bound up with Islamic religious movements. Cultivators worked the land of these leaders, and migrated with them to expand production. French authorities upheld these men as local leaders through whom to exercise colonial rule. These leaders, in turn, were critical in the recruitment of forced laborers and soldiers for the colonial state (Robinson, 1991). Further, the expatriate Lebanese merchants who were active in the groundnut trade largely sided with Africans during the push for independence (Boumedouha, 1990).

Turning to the positive but insignificant correlation of palm prices with unrest in our baseline results, we show below that the significance of this correlation is sensitive to the inclusion of additional controls and district trends. Like groundnuts, palm oil was often

grown by smallholders whose communities consumed the produce (Kiple et al., 2000). The oil palm was used as a source of cooking oil, fermented drinks, medicine, and kerosine substitutes (Thompson and Adloff, 1958, p. 315). Similarly, unrest concerning palm prices in Senegal in 1952 was met with minor government efforts to improve the situation of producers (Thompson and Adloff, 1958, p. 466).

Further, the positive correlation between palm oil prices and unrest in palm-producing districts is an artefact of the Second World War. It is entirely explained by the unusual quiet in coastal Conakry, Ouidah, Allada, and Porto-Novo in 1943: if this year is removed from the analysis, the correlation becomes negative and insignificant.

Compliers

Table 3 reports compliers for our colonial results. This exercise is similar to that in Dell (2012). We report heterogeneous compliance with rainfall and temperature shocks. In both cases, results are strongest in regions furthest from ports or from the coast. For both shocks, having had a pre-colonial state attenuates the effect of shocks, though the interaction is only significant for temperature. Areas where French conquest was resisted for longer respond more to temperature shocks. Both types of shock matter less where the coefficient of variation of rainfall is high, i.e. where shocks are more common. Neither type of shock is significantly attenuated by the mean frequency of colonial incidents or modern riots and protests.

Some of these responses can be accounted for by appealing to the comparative static results of the model. An increase in the coefficient of variation of rainfall is analogous to an increase in α , the probability of an adverse income shock. As in the model, this reduces the incentive of the state to set tax rates that will prompt resistance in the low-income state.

Similarly, the presence of an existing pre-colonial state gave colonial authorities greater tools with which to recover taxes despite resistance from farmers. This pattern was repeated across much of Africa (Acemoglu et al., 2014; Michalopoulos and Papaioannou, 2013). This reduced the returns to unrest. After falling commodity prices made it difficult for Daillonke

producers in Sangalan to pay their taxes in 1931-32, French officials leaned on the Fulbe canton chiefs who were the successors of the former slave-owning class in order to find a way to extract payment (Klein and Roberts, 1987, p. 27). In regions without pre-colonial states, the French found it difficult to fill the difficult role of village chief; the true traditional leader often hid behind a former slave or another man of low rank (Suret-Canale, 1971, p. 82). In parts of Côte d'Ivoire, for example, locals resented the colonial chiefs for their role in tax collection and labor recruitment, as well as the inconsistency between how they were appointed and customary rules (Asiwaju, 1976). While the model predicts that greater state capacity would prompt higher tax rates that leave the incidence of unrest unchanged, French authorities may have been unable to fully exploit this advantage or may have been overconfident in the ability of appointed chiefs to collect payment from uncooperative farmers.

The greater responsiveness of incidents to shocks in more isolated districts is a reflection of the scarcity of alternative income opportunities from trade. In these regions, the gap between y_H and y_L is widened because of the lack of economic diversification. Districts that were more distant from a port or from the coast are less likely to have had a trading post before colonization or to be producers of cocoa, coffee, cotton, or palm produce in the Hopkins (1973) map, and they are no more likely to produce groundnuts. These areas were also poorer. They received less colonial investment as measured by teachers, public health staff or public work spending, rates of head taxes were lower, and less tax revenue was collected per capita.⁹

Robustness

We perform several robustness exercises that we report in the appendix, at the end of this thesis. In Table A2, we show that the results are largely unchanged if district-specific trends are added. In Table A3, we show that replacing three-year moving averages of our shocks

⁹For each of these variables except groundnuts, the district-level cross-sectional correlations with coast and port distance are negative and significant at the 5% level (not reported). The correlation with groundnuts is insignificant.

of interest with one-year lags of these same shocks gives nearly identical results, though the effect of rainfall is now marginally insignificant ($t = -1.58$).

In Table A4, we present marginal effects estimates of (4.1) using a logit estimator. These are largely similar to our baseline, except that the effect of cotton price shocks is now insignificant ($t = -1.38$). In Table A5, we include all shocks in the same specification. Again, the results resemble our baseline, except that the effect of banana price shocks is now insignificant ($t = -1.62$), while palm oil prices now enter positively and significantly. In Table A6, we control for the value of the dependent variable in the most recent year in the data. This is the closest our data permit to adding a true lagged dependent variable. The lag does not enter significantly in any specification. Results are largely similar to the baseline, except that palm prices now enter significantly and cotton is no longer significant ($t = -0.21$). Finally, we show that the results are very similar if we discard any districts that either reported no incidents in the colonial period, or an incident at every observation in the colonial period. These results are reported in Table A6.

We perform additional robustness checks and tests for additional compliers that we do not report. If we weight commodity price shocks by the share of a district that Hopkins (1973) depicts as producing that commodity, all coefficient signs remain the same as in our baseline. The effects of cotton and groundnuts become insignificant, however ($t = -1.21$ and $t = 1.13$, respectively). We interact the French colonial investments studied by Huillery (2009) with rainfall and temperature shocks.¹⁰ Although the signs on these interactions suggest a mitigating effect, they are not significant.

There are additional results we do not report. We find no evidence that the effects of rainfall or temperature are greater in the second half of the colonial period than during the first half. The effects of colonial rainfall and temperature shocks are greater in Islamic regions. Finally, we interact our rainfall and temperature shocks with the ecological regions

¹⁰In particular, these are indicators for whether the number of teachers per capita averaged over the period 1910-1928 was above the median, whether the number of public health staff per capita averaged over the 1910-1928 period was above the median, and whether public works expenditure per capita over the period 1910-1928 was above the median.

mapped by White (1983).¹¹ In all regions where the effects of rainfall are significant, they are negative. These are the azonal, edaphic, semidesert, and woodland zones. Similarly, temperature enters significantly only in the desert and woodland zones, where its sign is positive. Our main results survive discarding areas that are majority desert, as well as restricting our sample to districts in which the Murdock (1967) *Ethnographic Atlas* reports that local ethnic groups derived most of their subsistence from agriculture.

We have not yet distinguished the types of incidents that responded to economic shocks. For 79% of “incidents” in the data, Huillery (2011) provides details on the reason for the disturbance, classifying these as land, taxes, recruitment, borders, or “other.” We estimate (4.1), replacing the dependent variable with an indicator for an incident within each of these categories. We plot 95% confidence intervals for the response of these variables to the three year moving average of log rainfall in Figure 3.6. None of these are significant at the 5% level, and so the data do not allow us to distinguish which motives for unrest were the most responsive to economic shocks. Suggestively, the coefficients on all categories except “other” are negative, and the largest estimated effect is for incidents related to taxes.

3.4.2 Modern period

Main results

Table 4 reports results for modern riots in the former colonies. Column (1) shows that rainfall predicts riots with some degree of accuracy; a standard deviation decrease in rainfall leads to a 0.49 standard deviation increase in riots, or roughly a 13 percentage points.¹² Modern unrest, then, still reacts in a negative way to rainfall shocks. Temperature and commodity price shocks, by contrast, no longer follow their historical patterns. The effect of temperature is insignificant, and the point estimate has the opposite sign than what we would expect. All estimated effects of commodity prices are now insignificant, though their

¹¹The land areas within French West Africa are classified by White (1983) as azonal, bushland, desert, edaphic, forest, forest transitions, semi-desert, and woodland.

¹² $-0.165 \times 0.89/0.30 \approx 0.490$, $-0.165 \times 0.89 \approx 0.147$.

signs remain the same as in the colonial period.

Compliers

In Table 5, we show that heterogeneity in how districts respond to rainfall and temperature shocks has also changed between the colonial and modern periods. It remains the case that areas that are more remote respond more to rainfall shocks; this is significant in columns (2) and (3), and marginally insignificant in column (1) ($t = 1.61$). Pre-colonial kingdoms no longer attenuate rainfall shocks. Although the effect remains smaller in areas that are more prone to rainfall variability (column (6)), this difference is no longer significant. Now that the main effect of temperature has a different sign and is no longer statistically significant, it is no surprise that the pattern of compliers has completely changed.

Robustness

We use Tables A7 and A8 to show that the link between shocks and conflict in the present is less robust than it was in the past. In Table A7, we show that the correlation between rainfall and modern riots disappears after controlling for district-specific trends.¹³ Similarly, the effect of rainfall does not survive the use of lagged values rather than moving averages.¹⁴ Results remain similar if we use the count of riots and protests, rather than an indicator; this is reported in Table A9. In Table A10, we show that our results are not sensitive to exclusion of capital cities from the data.

¹³Although our result differs from the more robust relationship between drought and civil war found by Miguel et al. (2004), this should not be surprising given the different outcome, later time period, and alternative econometric specification that we consider.

¹⁴We also find no response of riots and protests to commodity prices if we use the FAO Agro-MAPS used by ?) data to code districts as commodity producers (not reported). We treat a district as a “producer” if it intersects an administrative unit that produces the commodity in the FAO Agro-MAPS data. For each commodity, we use the Agro-MAP from the available year closest to 1997, the start of the ACLED data. In separate exercises, we use production data from first-level and second-level administrative units. There are two major shortcomings of the Agro-MAPS data that prevent us from using these data in our main analysis. First, there are substantial gaps in the data; many products are only available for a handful of African countries in any given year. Second, the administrative units in the Agro-MAPS are very different from the colonial districts that are our units of observation. This introduces substantial error into our coding of districts as “producers.”

Do modern riots and protests have deep roots?

In Tables 6 and 7 we show that, not only do colonial and modern unrest respond differently to economic shocks, their cross-sectional correlates differ. In Table 6, we regress the district-level prevalence of incidents on the geographic, political, and economic variables in our data. We use a tobit estimator to account for districts with no incidents or with no incident-free years. In Table 7 we do the same for modern riots and protests.

For the colonial period, we find that resistance was greater in areas of higher mean rainfall. We also find higher levels of unrest in groundnut-producing and cocoa-producing districts, though only in some specifications. All of these are regions in which higher incomes during good periods would have raised the return on taxation to the colonial state. Lastly, it is clear that areas with a pre-colonial kingdom and those that were acephalous experienced more unrest relative to the chiefdoms and city-states that form the excluded category. This latter result mirrors the findings from Huillery (2011) on anti-colonial hostility over the 1906-1920 period. None of these three patterns are evident in Table 6.

We report in Table 6 that there is almost no correlation between the incidence of colonial unrest and modern day riots and protests. This too mirrors results from Huillery (2011), who found that hostility over the 1906-1920 period was uncorrelated with hostility in the later colonial era once a dummy variable for the Casamance region was included. In her view, this was because anti-colonial hostility had a substantial “accidental” component. Indeed, it is striking how little variation is explained by our controls. Once country fixed effects are added, only the pre-colonial political variables and rainfall are significant in Table 6. In Table 7, there are no correlates of riots and protests that are robust across all specifications.

Huillery (2011) identifies two sources of random variation in hostility that explain this lack of persistence. The first is the individual personalities of specific administrators; some were more diplomatic, while others were more brutal. Second, the individual personalities of African leaders mattered. Some, like Samori Ture or Koulibaly of Kaarta, were “personally inclined to resistance towards colonial authority.”

3.4.3 Discussion

It is clear from the results in section 3.4 that the model in section 3.2 helps explain the patterns of unrest we observe in colonial French West Africa, but not those experienced in the present. This is consistent with the existing secondary literature.

Much of the work on forms of resistance in colonial French West Africa describes it as a response to obligations such as taxation and forced labor. Asiwaju (1976) claims these impositions prompted migration into British territories, while Cordell and Gregory (1982, p. 220) make similar claims about internal migration. Parents in Koudougou protested against recruitment of their children as contract labor in 1932 (Cordell and Gregory, 1982, p.220). Other forms of unrest that existed in response to economic shocks similarly have no parallel in the modern period. Clark (1994), for example, links the mass departures of slaves from their masters in the period from 1905 to 1911 to a series of droughts in the region.

This existing literature also recognizes that unrest was episodic. Resistance to conscription, for example, was less intense during peace time than it was during the First World War (Echenberg, 1975, p. 181). This recruitment followed on the famine of 1913-14. In the Upper Senegal Valley, this was the most severe famine in recent memory, and had devastated agriculture, pastoralism and commerce. The government had persisted in levying head and herd taxes at their usual rates (Clark, 1995). The literature similarly acknowledges that colonial suppression was often in response to failures to pay tax or meet forced labor obligations; half of all recorded punishments under the *indigénat* during the 1930s were for offences of this type (Mann, 2009, p. 342). Collective punishments for these infractions were also common under the *indigénat*; one official in 1921, for example, inflicted identical punishments on 15 individuals from a single village (Mann, 2009, p. 343).

The Volta-Bani revolt of 1915-16 is a useful example. Like other wartime episodes of resistance, it followed closely on the drought and famine of 1913-14. This had affected much of the Sahel, and was possibly the most severe famine in the region in the twentieth century (Van Beuskom, 1999). In our data, the three-year moving average of rainfall was visibly

below its mean in 1913 and 1916 in Burkina Faso, Niger, Mali, and Senegal. The same drought had pushed Saharan groups such as the Tuareg south, leading to conflicts between migrants and locals (Suret-Canale, 1971, p. 82). Though recruitment into the army had at first appeared to be an honorable way to escape the effects of the famine, this illusion was destroyed once news of the conditions of war spread (Suret-Canale, 1971, p. 139). The Volta-Bani rising was a movement to resist taxation, forced recruitment and French rule. Organizers mobilized some 20,000 soldiers to fight against the French in an area stretching from Mali to Burkina Faso, and were only suppressed by a force of 5,000 French troops. This took place in a region of considerable ethnic diversity that lacked major pre-colonial states. The core of the rebellion was formed by alliances among villages and segmentary societies (Saul and Royer, 2001).

By contrast, modern riots and protests in the former colonies of French West Africa have frequently focused around issues such as prices, upward mobility, and service delivery. These events have had unions, students, youth, and members of political parties as participants (e.g. Mueller, 2013; Wise, 1998). Only 3 of 982 events in the data are described as relating to taxes; all concern protests over a motor vehicle tax in Burkina Faso in 2010. Some of the events that have followed on periods of low rainfall and that are statistically influential in our modern results have included: a 2005 strike by the National Confederation of Guinean Workers the former Kindia district of Guinea, accompanied by a youth riot focused on high prices; events in 2004 and 2005 in the former Pita district of Guinea during which rioters protested electricity prices and students protested the poor conditions of their schools, and; Senegalese soldiers in the former Thies district protesting pay and benefits in 1999.

A series of marches and riots by students in the former colonial Bassam, Lahou, and Sassandra districts of Côte d'Ivoire in 1999 also followed a period of depressed rainfall. These were part of a larger set of student protests led by the Federation of Ivorian Students that had included meetings, sit-ins and demonstrations, concerning issues ranging from political suppression to medical care, examination requirements for higher education, student fees

and loans, and university residences (Farmer, 1999). The politicization of education and student groups has a long history in Côte d’Ivoire. The Federation of Ivorian Students was established in 1990 to protest repression under the Houphouët-Boigny government, and student groups were mobilized for violence during the First Ivorian Civil War (Sany, 2010). The correlation between economic shocks and unrest in the present day, then, follows a different logic and often involves a different set of actors than during the colonial period.

3.5 Conclusion

In this paper, we collect data on both colonial unrest and modern riots in French West Africa. We show that from 1906 to 1956, unrest reacted positively to higher temperatures, negatively to greater rainfall, and, with some exceptions, negatively to greater commodity prices. These patterns are mitigated by institutions, geography, and history. Specifically, colonial unrest’s reaction to shocks is strongest in remote areas with less history of states and less vulnerability to environmental shocks. These patterns can be understood as the equilibrium response of a tax-paying peasantry to an extractive colonial state.

To the best of our knowledge, our study is the first to provide evidence on unrest’s response to both environmental and commodity price shocks within a single setting. We find differences in the responsiveness to shocks in the colonial and modern settings. The negative correlation between rainfall and unrest remains in the present, but it is smaller, less robust, and no longer mitigated by pre-colonial institutions or the general frequency of rainfall shocks. Temperature is no longer a significant predictor of unrest, nor are commodity price shocks.

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3.6 Figures and Tables

Figure 3.1: Equilibria in the model

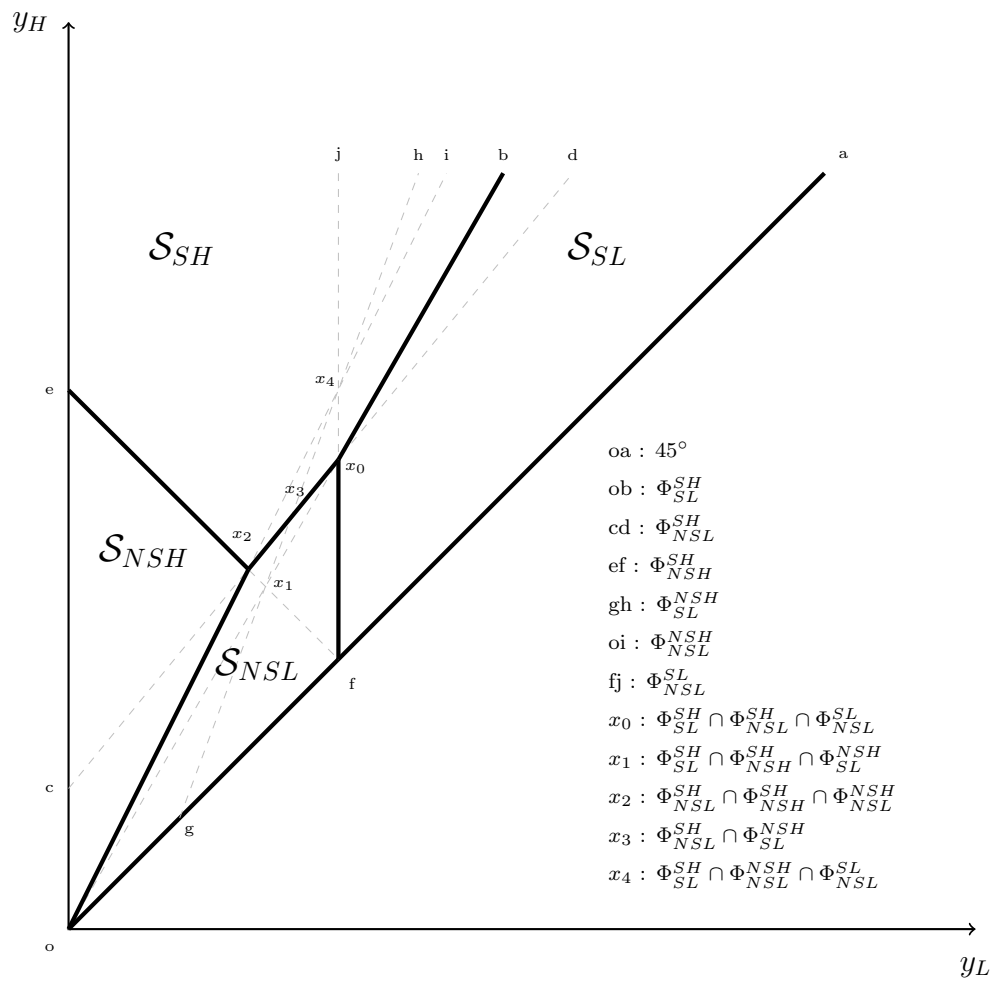


Figure 3.2: 3 Year Moving Average Log Prices in Survey Years, Relative to 1931

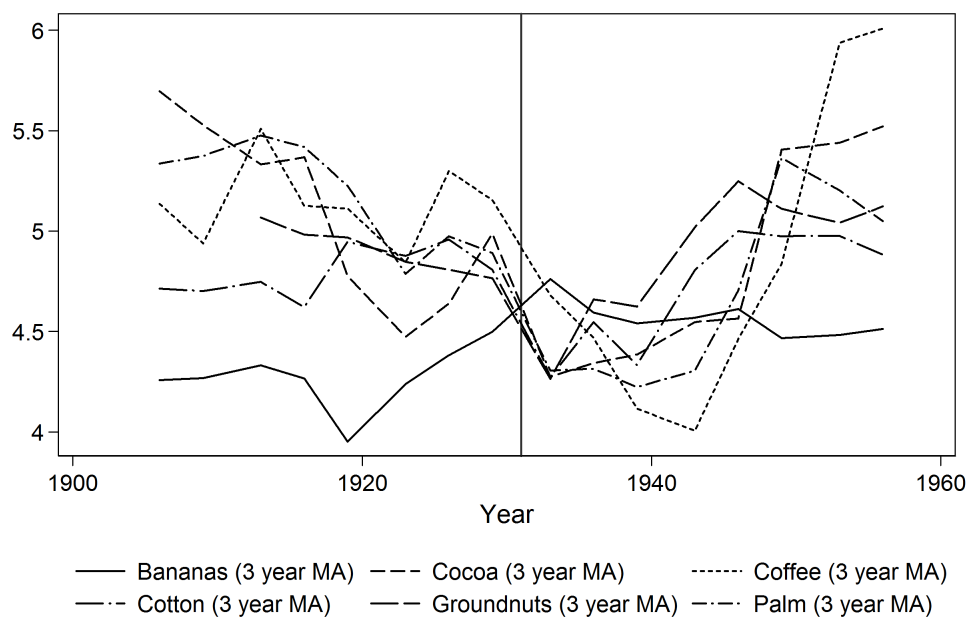


Figure 3.3: Cocoa production in Hopkins (1973)

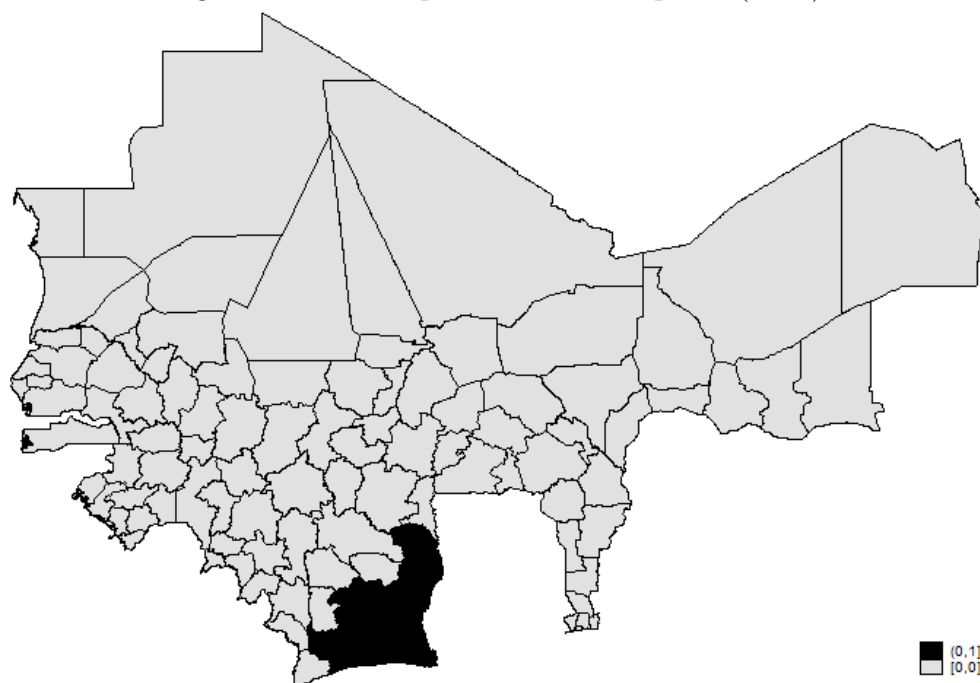


Figure 3.4: Colonial Incidents, 1906-1956

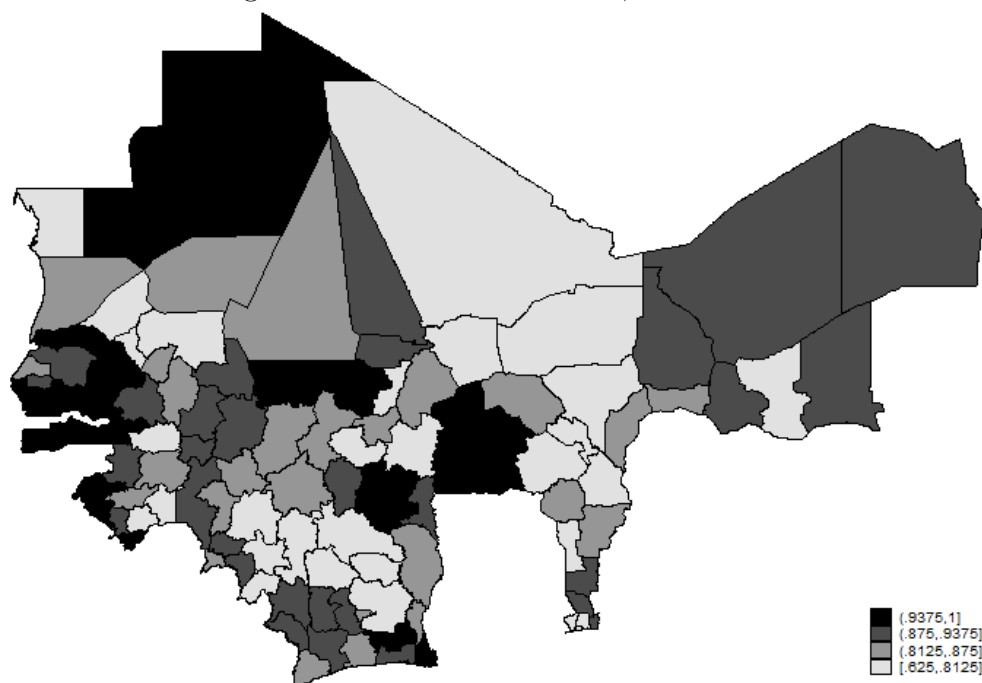


Figure 3.5: Riots and protests, 1997-2011

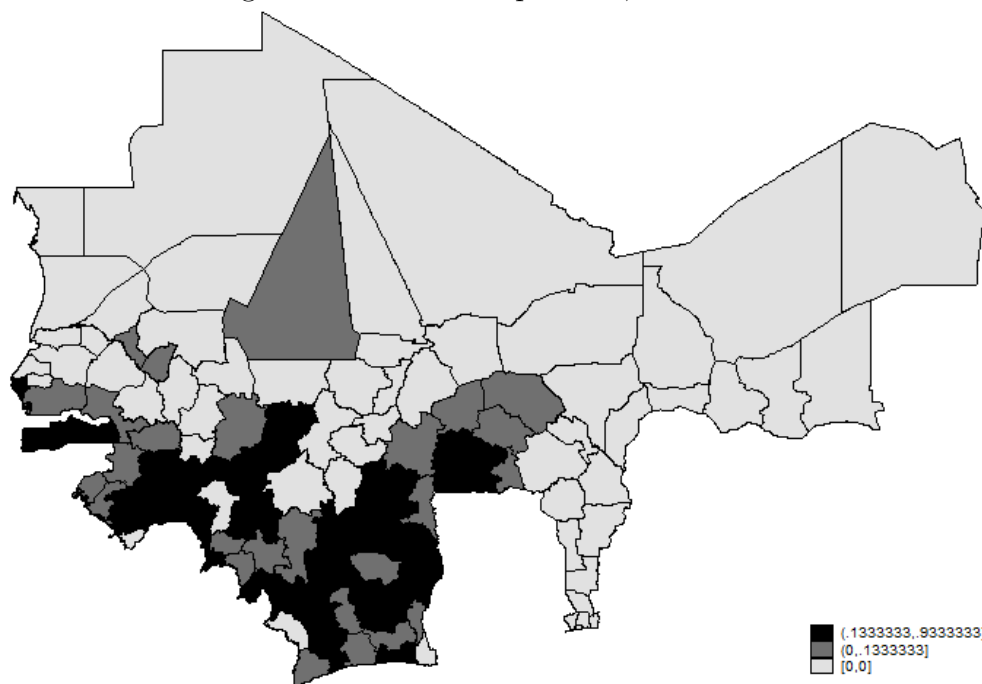


Figure 3.6: Types of incident: Coefficients and 95% Confidence Intervals

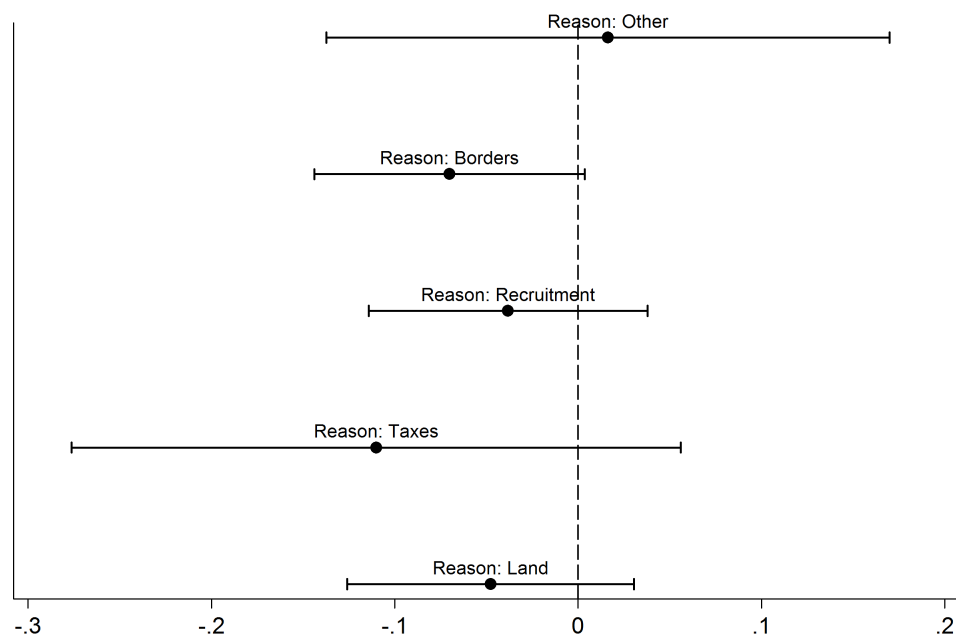


Table 1. Summary statistics

	(1) Mean	(2) s.d.	(3) Min	(4) Max	(5) N
<i>Colonial data</i>					
Incident	0.86	0.34	0	1	1,431
Log rainfall (3 year MA)	4.22	0.89	-0.059	5.79	1,760
Log temperature (3 year MA)	3.30	0.052	3.14	3.40	1,760
Exporter X Log Price: Bananas (3 year MA)	0.44	1.33	0	4.76	1,760
Exporter X Log Price: Cocoa (3 year MA)	0.50	1.49	0	5.70	1,760
Exporter X Log Price: Coffee (3 year MA)	0.81	1.86	0	6.01	1,760
Exporter X Log Price: Cotton (3 year MA)	0.26	1.08	0	5.00	1,760
Exporter X Log Price: Groundnuts (3 year MA)	0.89	1.89	0	5.25	1,540
Exporter X Log Price: Palm (3 year MA)	0.72	1.75	0	5.48	1,760
<i>Modern Data</i>					
Riots/Protests	0.10	0.30	0	1	1,680
Log rainfall (3 year MA)	4.09	0.88	0.17	5.59	1,554
Log temperature (3 year MA)	3.32	0.056	3.16	3.42	1,554
Exporter X Log Price: Banana (3 year MA)	0.72	1.99	0	6.46	1,456
Exporter X Log Price: Cocoa (3 year MA)	0.85	2.34	0	7.59	1,456
Exporter X Log Price: Coffee (3 year MA)	0.78	1.67	0	4.94	1,456
Exporter X Log Price: Cotton (3 year MA)	0.29	1.05	0	4.54	1,456
Exporter X Log Price: Groundnut (3 year MA)	1.32	2.66	0	6.90	1,456
Exporter X Log Price: Palm (3 year MA)	0.97	2.22	0	6.38	1,456
<i>District Characteristics</i>					
Indicator of access to the sea	0.21	0.41	0	1	112
Indicator of a kingdom at the end of the nineteenth century	0.48	0.50	0	1	112
Distance of the main city from the coast in km: Above median	0.49	0.50	0	1	112
Distance of the main city from the nearest port in km: Above median	0.49	0.50	0	1	112
Number of years of resistance to French colonial conquest: Above median	0.47	0.50	0	1	112
Mean of incidents: Above median	0.48	0.50	0	1	112
Mean of riots: Above median	0.48	0.50	0	1	112
Coefficient of variation of rain above median	0.50	0.50	0	1	112

Table 2. Main results: Colonial incidents

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
Shock	-0.164**	2.931***	-0.265**	-0.124**
<i>s.e. Clustered by district</i>	(0.066)	(1.094)	(0.114)	(0.063)
<i>Conley s.e. (5 degree cutoff)</i>	(0.065)	(0.906)	(0.112)	(0.050)
<i>Conley s.e. (10 degree cutoff)</i>	(0.063)	(0.810)	(0.097)	(0.039)
Observations	1,431	1,431	1,431	1,431
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.120*	-0.279**	0.230**	0.028
<i>s.e. Clustered by district</i>	(0.065)	(0.131)	(0.101)	(0.04)
<i>Conley s.e. (5 degree cutoff)</i>	(0.043)	(0.166)	(0.113)	(0.041)
<i>Conley s.e. (10 degree cutoff)</i>	(0.040)	(0.157)	(0.103)	(0.038)
Observations	1,431	1,431	1,212	1,431
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses unless otherwise indicated. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table 3. Compliance with colonial shocks.

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
<i>Panel A. Rainfall Shocks</i>				
Shock	-0.166** (0.075)	0.014 (0.090)	-0.021 (0.101)	-0.223** (0.085)
Interaction	0.010 (0.117)	-0.281** (0.124)	-0.210* (0.124)	0.134 (0.111)
<i>Panel B. Temperature Shocks</i>				
Shock	3.890*** (1.170)	0.512 (1.778)	0.270 (1.804)	5.314*** (1.626)
Interaction	-4.823* (2.847)	3.997* (2.146)	4.412** (2.182)	-4.313** (2.055)
Interacted Variable	<i>Access to the sea</i>	<i>Distance of the main city from the coast in km: Above median</i>	<i>Distance of main city from nearest port: Above median</i>	<i>Kingdom at the end of the nineteenth century</i>
Observations	1,431	1,431	1,431	1,431
	(5)	(6)	(7)	(8)
	<i>Dependent variable: Incident</i>			
<i>Panel A. Rainfall Shocks</i>				
Shock	-0.203** (0.091)	-0.552*** (0.191)	-0.212* (0.117)	-0.163** (0.065)
Interaction	0.094 (0.114)	0.438** (0.190)	0.081 (0.120)	-0.009 (0.165)
<i>Panel B. Temperature Shocks</i>				
Shock	1.346 (1.156)	6.050*** (2.010)	3.073* (1.800)	3.055** (1.250)
Interaction	4.539** (2.171)	-4.505* (2.406)	-0.268 (2.032)	-0.447 (2.532)
Interacted Variable	<i>Years of resistance to conquest: Above median</i>	<i>CV of rainfall: Above median</i>	<i>Mean of incidents: Above median</i>	<i>Mean of riots/protests: Above median</i>
Observations	1,431	1,431	1,431	1431

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table 4. Modern riots and protests

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
Shock	-0.138*	0.054	-0.246	-0.049
	(0.075)	(1.135)	(0.170)	(0.126)
Observations	1,554	1,554	1,456	1,456
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Riots/protests</i>			
	(5)	(6)	(7)	(8)
Shock	-0.009	-0.130	0.194	0.039
	(0.053)	(0.165)	(0.144)	(0.059)
Observations	1,456	1,456	1,456	1,456
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for riots/protests.

Table 5. Compliance with modern shocks.

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
<i>Panel A. Rainfall Shocks</i>				
Shock	-0.165*	-0.098	-0.088	-0.060
	(0.090)	(0.106)	(0.113)	(0.082)
Interaction	0.094	-0.106	-0.119	-0.165
	(0.137)	(0.113)	(0.120)	(0.133)
<i>Panel B. Temperature Shocks</i>				
Shock	-0.067	3.558**	2.449	0.542
	(1.131)	(1.649)	(1.663)	(1.469)
Interaction	1.211	-5.699***	-4.057**	-0.925
	(3.114)	(1.691)	(1.655)	(1.524)
Interacted Variable	<i>Access to the sea</i>	<i>Distance of the main city from the coast in km: Above median</i>	<i>Distance of main city from nearest port: Above median</i>	<i>Kingdom at the end of the nineteenth century</i>
Observations	1,554	1,554	1,554	1,554
	(5)	(6)	(7)	(8)
	<i>Dependent variable: Riots/protests</i>			
<i>Panel A. Rainfall Shocks</i>				
Shock	-0.031	-0.284*	-0.244*	-0.183***
	(0.089)	(0.145)	(0.128)	(0.045)
Interaction	-0.222*	0.225	0.192	0.120
	(0.132)	(0.152)	(0.133)	(0.172)
<i>Panel B. Temperature Shocks</i>				
Shock	0.503	0.502	-0.666	-0.824
	(1.299)	(1.715)	(1.362)	(1.021)
Interaction	-1.111	-0.805	1.395	2.753
	(1.727)	(1.552)	(1.513)	(1.971)
Interacted Variable	<i>Years of resistance to conquest: Above median</i>	<i>CV of rainfall: Above median</i>	<i>Mean of incidents: Above median</i>	<i>Mean of riots/protests: Above median</i>
Observations	1,554	1,554	1,554	1,554

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for riots/protests.

Table 6. Deep roots of colonial unrest?

	(1)	(2)	(3)	(4)	(5)	(6)
	ρ	<i>Dependent variable: Mean incident X 100</i>				
Mean of riots	0.04					
Mean Rainfall	0.04	0.070** (0.032)			0.113*** (0.033)	0.111** (0.043)
Mean Temperature	0.13	1.054 (1.684)			1.883 (1.398)	1.651 (1.326)
Coefficient of variation of rain	0.01	-11.392 (13.451)			4.218 (12.818)	7.415 (12.092)
Altitude of district's main city in meters	-0.19	-0.005* (0.003)			-0.004 (0.003)	-0.001 (0.003)
Latitude of district's main city	0.08	0.582 (0.688)			0.671 (0.685)	0.673 (0.904)
Longitude of district's main city	-0.11	-0.075 (0.232)			0.065 (0.267)	-0.814 (0.589)
Indicator of access to the sea	0.09	1.044 (3.526)			0.369 (3.841)	-2.065 (3.909)
Indicator of a navigable river	-0.01	-0.451 (2.148)			0.066 (1.758)	-0.029 (1.772)
Distance of the main city from the coast in km	0.01	-0.010 (0.012)			0.017 (0.012)	-0.001 (0.013)
Distance of the main city from the nearest port in km	0.03	0.016 (0.011)			-0.011 (0.012)	0.008 (0.014)
Area in km2	0.06	0.000 (0.000)			0.000 (0.000)	0.000 (0.000)
Indicator of a kingdom at the end of the nineteenth century	0.16		5.184** (2.123)		7.421*** (1.909)	5.733*** (1.845)
Indicator of an acephalous society before the colonial conquest	0.13		8.651*** (2.605)		14.834*** (3.476)	11.914*** (3.549)
Number of years of resistance to French colonial conquest	-0.05		-0.144 (0.089)		-0.162** (0.073)	-0.127** (0.062)
Year of the last military intervention before final submission	-0.17		-0.213*** (0.081)		-0.184 (0.151)	-0.229 (0.153)
Bananas	0.17			4.592 (3.880)	-1.104 (3.711)	0.071 (3.997)
Cocoa	0.08			4.153 (3.206)	6.365** (3.142)	12.028*** (4.345)
Coffee	0			-3.479 (3.428)	-0.512 (3.120)	0.036 (3.537)
Cotton	-0.21			-7.379* (3.854)	-2.284 (4.449)	-0.074 (4.873)
Groundnuts	0.29			8.707*** (2.550)	8.825*** (2.283)	2.555 (2.355)
Palm	0.12			3.815 (3.844)	3.056 (3.560)	3.443 (4.077)
Indicator of a trading post before the colonial conquest	0.04			-0.360 (4.110)	-3.000 (4.458)	-0.295 (4.477)
Country Fixed Effects		No	No	No	No	Yes
Observations		110	110	110	110	110

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Robust standard errors in parentheses. The estimator is tobit. The dependent variable the fraction of years during which an incident occurred. Column (1) contains correlation coefficients with mean incidents.

Table 7. Deep roots of modern riots and protests?

	(1)	(2)	(3)	(4)	(5)	(6)
	ρ	<i>Dependent variable: Mean incident X 100</i>				
Mean Rainfall	0.36	-0.153 (0.125)			0.001 (0.119)	-0.150 (0.150)
Mean Temperature	-0.3	-1.164 (3.460)			1.310 (3.353)	3.055 (3.867)
Coefficient of variation of rain	-0.24	-211.961* (106.850)			-278.834** (110.503)	-312.621*** (102.890)
Altitude of district's main city in meters	0.04	0.007 (0.007)			0.009 (0.007)	0.003 (0.007)
Latitude of district's main city	-0.3	-5.796** (2.234)			-0.779 (2.261)	-2.957 (4.707)
Longitude of district's main city	-0.17	-3.756*** (0.933)			-2.773*** (0.935)	-1.919 (2.566)
Indicator of access to the sea	0.13	19.753* (10.903)			6.765 (11.336)	15.645 (12.867)
Indicator of a navigable river	0.25	16.765** (6.681)			14.514** (6.594)	6.673 (6.456)
Distance of the main city from the coast in km	-0.23	0.085* (0.044)			0.000 (0.040)	0.067 (0.062)
Distance of the main city from the nearest port in km	-0.23	-0.073* (0.039)			0.005 (0.037)	-0.056 (0.048)
Area in km2	-0.13	0.000* (0.000)			0.000* (0.000)	0.000*** (0.000)
Indicator of a kingdom at the end of the nineteenth century	-0.04		-0.864 (7.333)		5.545 (6.186)	5.799 (7.462)
Indicator of an acephalous society before the colonial conquest	0.09		18.126** (8.342)		-13.376 (13.296)	-5.044 (13.601)
Number of years of resistance to French colonial conquest	-0.07		-0.382 (0.253)		0.139 (0.314)	0.218 (0.285)
Year of the beginning of the colonial conquest	0.06		0.045 (0.281)		0.472 (0.491)	0.495 (0.509)
Bananas	0.29			23.012* (13.402)	19.867* (10.567)	16.957 (10.376)
Cocoa	0.29			7.264 (11.286)	16.940* (9.084)	4.734 (12.314)
Coffee	0.26			16.064** (7.853)	5.085 (11.220)	8.631 (11.115)
Cotton	0.22			16.595 (12.478)	27.474** (11.727)	34.343** (13.544)
Groundnuts	0.12			12.598 (9.023)	18.026* (9.446)	29.250** (12.769)
Palm	0.16			-10.157 (9.728)	-0.811 (9.317)	0.623 (11.949)
Indicator of a trading post before the colonial conquest	0.15			-1.022 (18.219)	17.279 (17.109)	19.791 (19.387)
Country Fixed Effects	No	No	No	No	No	Yes
Observations		111	112	112	111	111

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Robust standard errors in parentheses. The estimator is tobit. The dependent variable the fraction of years during which a riot or protest occurred. Column (1) contains correlation coefficients with mean riots and protests.

Chapter 4

Elites, Weather Shocks, and Witchcraft Trials in Scotland

Fair is foul, and foul is fair.

- William Shakespeare, Macbeth

4.1 Introduction

Persecution has deletrious consequences. Acemoglu et al. (2011) provide evidence that the Holocaust in Russia resulted in long-term harmful economic and political outcomes, while Voigtlander and Voth (2012) show that persecution is persistent, affecting persecuted minorities long after initial oppressions. This is especially significant in developing countries, where violence can impede economic growth (Blattman and Miguel, 2010; Abadie and Gardeazabal, 2003) and limit state capacity (Besley and Persson, 2010).

Understanding what causes persecution is therefore important. In particular, persecution often requires resources and organisation. Examining the Rwandan Genocide, Yanagizawa-Drott (2014) finds that access to Hutu-financed radio propaganda explains more killings, while Rogall (2014) discovers that military elites incited Hutu civilians to massacre Tutsis.

Heldring (2013) in turn finds that greater state capacity in Rwanda facilitated more genocide atrocities in 1994. Planning and resources are crucial to carrying out persecutions.¹

In this paper, I use data from Early Modern Scotland to examine a particular type of persecution: witch hunts, which were motivated by a genuine belief that witches are evil. Scottish law, which made witchcraft a secular crime in 1563, de facto required local elites to commit resources to trying witches. When these resources were more ample, witchcraft prosecutions increased. Consistent with this, and given that Scotland was an agricultural economy, I find that favourable (warmer) temperatures predict more trials. This is robust to county-specific time trends, different specifications, and placebo tests using forward lags.

Turning to mechanisms, I document secondary historical evidence that Scottish elites derived income from export-heavy, taxable commodities such as herring and wool. Using this as a guide, I find that positive price shocks to herring and wool predict more trials. As a falsification, I find that shocks to Scotland's main subsistence commodity, oats, do not predict trials, since this crop was not a major source of elite income. I provide historical evidence that these patterns were not due to greed, since witchcraft suspects had little valuable property, and trials were costly.

I ground these observations in a simple model of witchcraft trials, where a local laird (Scottish lord) must decide whether or not to pursue a prosecution. The laird receives an income shock, and must then weigh the choices of funding witch trials, or non-trial activities. The laird gains benefit from both, but trials cost money. I find in my model that more income means more trials. This effect is amplified by a greater marginal benefit of trials, and is attenuated by a higher marginal cost - results that I test for my empirical section.

My paper proceeds as follows. In Section 2, I review related literature, and demonstrate my study's contribution. In Section 3, I provide historical background. I derive a model with testable implications in Section 4. In Section 5, I describe my empirical strategy and data in order to identify the causal impact of shocks on witch trials. Section 6 provides my

¹The recently concluded Sri Lankan Civil War, planned and executed by Sinhalese leaders, led to the systematic murders of thousands of Tamil civilians (Weiss, 2012).

results, along with robustness checks and mechanisms. In Section 7, I conclude.

4.2 Related Literature

My study investigates the idea that economic shocks cause violence, and thus follows an important literature in this vein (Hsiang et al., 2013; Collier and Hoeffler, 2004; Miguel et al., 2004). In particular, Bazzi and Blattman (2014) have stressed the need for case study evidence in order to disentangle underlying mechanisms. By examining witchcraft trials in Early Modern Scotland, I focus on a single case study, and am therefore able to uncover mechanisms that drive the link between temperature shocks and witch trials, while limiting confounding factors. This is an important contribution, since economic channels that drive conflict and violence are still ill understood, though recent research has made progress in this direction (Dell, forthcoming; Fetzer, 2014; Dube and Vargas, 2013).

The debate over these channels is long-lasting. Dube and Vargas (2013) use data on Colombian conflict in the 1990s and early 2000s to identify two widely-discussed mechanisms: the opportunity cost effect, and the rapacity effect. The former effect states that when the *value* of being violent rises, relative to other activities, then violence itself increases (Besley and Persson, 2011; Dal Bo and Dal Bo, 2011; Angrist and Kugler, 2008). For example, if agricultural wages decline, then farm workers are more likely to join guerrilla squads. The rapacity effect, and its analogue the state prize effect, claims an opposite impact: higher income means that there is more to fight over (Hirshleifer, 1991; Grossman, 1999). If, say, oil prices rise, then paramilitaries are more likely to attack oil fields and kidnap oil executives. Although I find that positive environmental shocks predict more witch trials, this is not due to a rapacity effect. Rather, it is due to the ability of local elites to finance a trial.

The papers closest to mine in subject matter are Oster (2004), Miguel (2005), and Johnson and Koyoma (2014). Oster uses time series data to document a negative relationship between air temperatures and witch trials in Early Modern Europe, and argues that poor eco-

conomic conditions prompt such trials. Unlike Oster, I focus on a single case study, Scotland, which had more centralised institutions than the rest of Europe for prosecuting witches. I uncover a completely different result: positive economic shocks predict *more* witchcraft trials. I also provide empirical evidence for an explicit mechanism driving this result.

Miguel's (2005) study of Tanzania suggests a similar story as Oster's: when rainfall is low, people kill unproductive members of society by blaming them for witchcraft. Tanzania, however, does not have the legal institutions that Scotland had for dealing with witchcraft accusations. Witch killings in Tanzania are much more decentralised, and are often carried out by family or community members without judicial restraint. In Scotland, sizable costs were incurred to ensure that legal procedure was carried out before a witch was executed.

Johnson and Koyama (2014) investigate witchcraft trials in France between 1550 and 1700, and argue that increases in fiscal capacity strengthened rule of law, reducing the number of witchcraft trials. Scottish witchcraft trials were different from France's in this regard, since Scottish trials were more centralised, and Scotland's laws themselves provided a strict judicial framework for the trials. At any rate, I am interested in an entirely different question: whether economic shocks caused witchcraft trials.

Because most Scottish witch suspects were women, I make a small contribution to the literature on violence against women.² Estimates find that intimate partner violence, and sexual violence against women, cost as much as \$4.49 trillion per year, or 5.3% of world GDP (Fearon and Hoeffler, 2014). Causes of domestic violence, in particular, have been well-studied and include women's household bargaining power (Doss, 2013; Bloch and Rao, 2002) and emotional cues that prompt male spouses to act aggressively (Card and Dahl, 2011). In Early Modern Europe, women were viewed as inferior and corruptible beings, more naturally prone to witchcraft than men, which caused them to be hunted as witches (Rowlands, 2013). I find that weather shocks cause more witchcraft trials.

I contribute to the literature on historical witchcraft trials by using a panel dataset to

²Roughly 85% of Scottish witch suspects were women.

study the impact of temperature on Scottish trials. Historians have postulated a number of factors that contributed to witch trials, including changing religious values (Levack, 2006), state expansion (Larner, 1981), and patriarchy (Apps and Gow, 2003). I offer another explanation: the costs of financing a trial must be sufficient.

Finally, a growing literature in economics has examined persecutions (Jha, 2013; Voigtlander and Voth, 2012; Waldinger, 2010). In addition to the Rwandan studies I mention in the introduction, a paper by Anderson et al. (2013) finds that bad weather shocks predict more Jewish expulsions from European cities between 1100 and 1800. The authors claim that these expulsions are driven by political economy concerns, as local rulers scapegoat Jews for economic woes. My story is also a political economy one, driven by the ability of local elites to finance a trial.

4.3 Historical Background

The Protestant Reformation consolidated itself in Scotland in 1560, when Edinburgh's Parliament formally rejected papal authority. Prior to the Reformation, witches were, at worst, seen as beings to be appeased, not persecuted. The Reformation changed this with its insistence on deliberate eradication of evil, thus making witchcraft a secular crime in 1563 with the Scottish Witchcraft Act (Cowan, 2008). The cultural reasons behind this are beyond this study's scope, though other authors have covered this in detail (Goodare, 2013; Roper, 1994; Smout, 1973).

Historical evidence suggests that central authorities, especially the Privy Council in Edinburgh, were able to exert some influence over proceedings, so that no local trials occurred without first going through Edinburgh.³ Local authorities were indeed content to have their trials sanctioned by central bureaucracy. According to a number of scholars, central control over witchcraft prosecutions was an important part of state building in Early Modern Scotland, a country that was difficult to govern (Dillinger, 2013; Larner, 1981).

³Treason was the only other crime to enjoy such a level of central oversight (Larner, 1981).

There were five stages of witch hunts (Goodare, 2002):

1. A witch is identified locally.
2. Evidence is gathered through local kirks (churches) and elites.
3. The Privy Council or parliament in Edinburgh reviews the evidence, and grants permission to local elites and witch hunters to set up a ‘commission of judiciary’ to try the accused.
4. The commission tries the witch.
5. The convicted witch is executed.

The bulk of expenses for the trial was incurred locally. The Privy Council was very interested in ensuring that proper judicial procedure was carried out, in order to prevent witch trials from descending into decentralised and unregulated killings.

The first stage, the identification of a witch, happened locally.⁴ A triggering event, such as a death in the family, would incite Scottish people to accuse neighbours of witchcraft. It usually took years before a neighbour’s accusation of witchcraft would result in a formal complaint, a pattern consistent with the rest of Europe (Briggs, 1998). Another way to identify a witch was for already accused witches to name their co-conspirators, although such evidence was given less weight.

In the second stage, a confession was sought, and this stage often involved torture. Sleep deprivation was commonly used, and was very effective in obtaining confessions, since it led to hallucinations (Dudley and Goodare, 2013). This stage might involve local kirk (church) officials, who otherwise had a very limited role in witchcraft prosecutions. Local officials and elites had to ensure that the evidence collected during this stage was suitable for Edinburgh’s vetting.

⁴An important exception, the 1590 North Berwick trials, started when King James accused witches of trying to sink his ship. However, even these trials relied on local identification of suspected witches.

The third stage involved acquiring Privy Council or parliament's permission to set up a commission of judiciary, composed of local elites (lairds, burgesses, justices, etc.) to try a witch. As Levack (2008) says,

Most cases... were adjudicated by local authorities who petitioned the privy council or parliament for permission... These local commissioners then assembled an assize (jury) to determine innocence or guilt, which in most cases turned out to be the latter. (p. 4)

It was at this stage that the central government in Edinburgh got involved in local affairs. However, besides granting permission, the Privy Council and parliament usually did not intervene directly in local trials.

The trial itself, the fourth stage, relied on four types of evidence: confessions, neighbours' testimony, other witches' testimony, and The Devil's Mark. This last proof, either a visible blemish or insensitive spot on the body, was discovered by court examiners or professional witch-prickers. It was a sign of a witch having made a Satanic pact.⁵

To attain a more concrete analysis, I cite Paterson (2013), who uses case study evidence from Scottish witchcraft prosecutions to examine trial expenses. In 1596, when a day-labourer's wages were 40 pence per day (Gibson and Smout, 1994), it cost £20 to imprison a witch in Aberdeen.⁶ This paid for the accused's sustenance, as well as the costs of extracting a confession. Elites paid the amount; as Paterson documents, the bill was laid upon a laird's estate, a burgh council composed of merchant elites, or a town council comprised of local elites and magistrates. Although a witch's property was sometimes seized to pay part of the trial costs, most witches had little valuable property, and the bulk of expenses were paid by local elites.

The last Scottish witchcraft trial was held in 1727, and witchcraft was removed from secular criminal offenses in 1736. Historians investigating the decline and end of Scottish

⁵The pact was sexual in nature, culminating in a witches' orgy.

⁶There were 240 pence in one Scottish pound.

witch hunting have concluded that lawyers became less convinced about the validity of evidence in witchcraft cases: confessions under tortures were seen as questionable, and witch-pricking for the Devil's Mark was exposed as fraudulent (Wasser, 2008; Levack, 2008).

4.4 A Model of Witch Trials

I model witchcraft prosecutions as a game between Nature and a local laird. This stylised representation puts emphasis on local elites' role in carrying out witch prosecutions. Because Early Modern Scotland was largely agricultural, a laird's income is based on profits from export-heavy, taxable agriculture. Food that Scottish peasants grow for their own subsistence, like oats, has a negligible impact on elite income, and therefore on the elite's resources to carry out witch trials.

I assume that there is local demand for witch trials from the peasant population. This assumption is tenable, since case study evidence suggests that most witch accusations came from peasants and non-elites (Wasser, 2008; Larner, 1981). Accusations from nobles and elites were rare.

Nature moves first, determining y , the laird's income, at random. From this endowment, the laird can either spend y_w on witchcraft trials, giving him utility $u(y_w)$, or on non-trial consumption, c , to gain utility $v(c)$. For simplicity and exposition, I assume that this non-trial consumption is wholly spent on private goods. These functions have the following properties: $u', v' > 0$, while $v'', u'' \leq 0$. I justify this assumption based on the historical observation that even elites, like lairds, feared witches in their midsts, and therefore spending on trials was important to ease their minds (Cowan, 2008; Wasser, 2008).

Witch trials cost Ry_w . This includes the cost of travelling to Edinburgh to seek Privy Council or parliamentary permission to try a witch. The laird also has to spend his own resources gathering evidence and prosecuting the witchcraft suspect. The cost of private goods is pc .

The laird's overall optimisation problem, then, is:

$$\begin{aligned} & u(y_w) + v(c) \\ & \quad s.t. \\ & pc + Ry_w = y \end{aligned}$$

To make this into an unconstrained optimisation problem is straightforward:

$$u(y_w) + v\left(\frac{y}{p} - \frac{R}{p}y_w\right)$$

The optimal spending on trials, y_w^* , is given by the First Order Condition:

$$\phi = \frac{\partial u}{\partial y_w^*} - \frac{\partial v}{\partial c} \frac{R}{p} = 0$$

Because R and p are constants, the above expression means that in equilibrium, the marginal utility and marginal disutility of witch trials must be equal, taking R and p into account. This result is rather standard. Similarly, the marginal rate of substitution between trials and non-trial consumptions is given by ratio between R and p .

The Second Order Condition is simply given as $\frac{\partial^2 u}{\partial y_w^2} + \frac{\partial^2 v}{\partial c^2} \left[\frac{R}{p}\right]^2$. Because $\frac{\partial^2 u}{\partial y_w^2} < 0$, and $\frac{\partial^2 v}{\partial c^2} < 0$, by assumption, this entire expression is negative, meaning that the solution is a maximum.

On a related note, spending on trials will increase if the marginal utility of trials, $u'(y_w^*)$, increases. To see this, note that if u' goes up, then v' must fall, which only happens if y_w rises. The marginal benefit of trials can increase if, for example, a national political crisis threatens the laird, making it more beneficial for him to invest in witch trials to quell discord.

Furthermore, as y increases, so does y_w^* . To show this, I differentiate the First Order

Condition with respect to y to yield the following expression:

$$\frac{\partial^2 u}{\partial y_w^{*2}} \frac{\partial y_w^*}{\partial y} - \frac{\partial^2 v}{\partial c^2} \frac{R}{p^2} = 0$$

Rearranging the above expression,

$$\frac{\partial y_w^*}{\partial y} = \frac{\frac{\partial^2 v}{\partial c^2} \frac{R}{p^2}}{\frac{\partial^2 u}{\partial y_w^{*2}}} > 0$$

The fact that this is positive follows from $\frac{\partial^2 u}{\partial y_w^2} < 0$, and $\frac{\partial^2 v}{\partial c^2} < 0$, by assumption.

Thus, as the laird earns more income, he can afford to spend more on witch prosecutions. This is amplified by u' , the marginal benefit of trials, and attenuated by R , the marginal cost of trials. I test this crucial prediction in my paper.

Using similar reasoning as above, I can show that:

$$\frac{\partial y_w^*}{\partial R} = \frac{\frac{\partial v}{\partial c} \frac{1}{p} - \frac{\partial^2 v}{\partial c^2} \frac{R y_w}{p^2}}{\frac{\partial^2 u}{\partial y_w^{*2}}} < 0$$

In other words, as R , the marginal cost of trials increases, then trials themselves will decrease.

I summarise these findings in the following proposition.

Proposition. *Witchcraft trials respond positively to economic shocks. The higher a laird's income, y , the more he can afford to spend on trials, y_w .*

These results are intuitive, and I shall test for evidence consistent with them in the rest of the paper.

4.5 Data and Empirical Strategy

4.5.1 Empirical Strategy

To test for the effects of temperature on witchcraft trials, I estimate the following specification:

$$Witch\ Trial_{i,t} = \beta Shock_{i,t} + \delta_i + \eta_t + \epsilon_{i,t}. \quad (4.1)$$

Here, $Witch\ Trial_{i,t}$ is either a dummy for whether or not a trial occurred, or a count for the number of witchcraft trials in county i in year t .

$Shock_{i,t}$ is simply a temperature shock, which is either a three-year, five-year, or ten-year moving average for temperature. The reason I use moving averages is because historians have documented that it took a long buildup for witchcraft suspicions to become full-blown accusations - sometimes as long as twenty years (Larner, 1981). To examine mechanisms related to agricultural commodities, I use $Suitability_i \times Price_t$ for the shock, where $Suitability$ is county i 's suitability for the particular commodity, while $Price_t$ is its price on world markets in year t .

δ_i and η_t are county and year fixed effects, respectively. I use these to control for omitted heterogeneity at the level of counties and time periods. I also report county-specific trends for robustness. The equation is estimated using OLS, and I cluster standard errors by county.

My identification strategy is based on the fact that temperature and world commodity prices are exogenous from a single county's point of view. A negative coefficient on β implies that the shock negatively predicts unrest, while a positive coefficient β means that the shock positively predicts unrest. In northern Europe, unlike tropical zones such as Africa, higher temperature are better for agriculture.

4.5.2 Data

I acquire witchcraft trial data from the Survey of Scottish Witchcraft, which is available through the University of Edinburgh. The database was derived from previously existing printed data, and was enhanced through extensive archival research. Not all of those who were tried were executed: of the 305 cases we know the outcome for, 205 were executed, 52 were acquitted, and the rest were banished. However, while this sample gives an execution rate of 67%, the Survey's researchers believe that the actual execution rate was much higher, since this sample mostly comprises trials at Edinburgh's justiciary court, which followed judicial procedure more rigidly than the vast majority of courts. The Survey offers wide coverage of the year and county of witchcraft trials, for 3,098 witch suspects. For additional documentation, please see the Survey's website.⁷

Weather data for this period are scant, and the only panel data available for Europe are from Guiot and Corona (2010). These authors collect data from proxy sources, including ninety-five tree ring series, sixteen indexed climate series based on archives, ice-core series, and pollen series to construct grids of reconstructed growing-season (April to September) temperature for Europe from 900 AD to today. I use geospatial software to match counties with their nearest grid points. The measured temperatures are based on deviations from the 1961-1990 average.

There is substantial evidence that warmer temperatures in northern Europe are better for agriculture (Olesen and Bindi, 2002). Studies of Early Modern northern Europe have shown that warmer temperatures predict lower wheat prices (Waldinger, 2014) and greater grain yield (Holopainen and Helama, 2009). Parry (1975), in particular, examined cereal cultivation in south-east Scotland from the late Middle Ages to the eighteenth century, and found that colder temperatures substantially reduce yields. It is therefore a sound assumption, for my analysis, that warmer temperatures improve agricultural conditions.

Scottish counties during this period look very different than they do today. Because no

⁷<http://www.shca.ed.ac.uk/Research/witches/>

digital map of Early Modern Scottish counties is easily available, I constructed a map based on *The Atlas of Scottish History to 1707*. This gives me borders of counties that existed from 1563 to 1727, the years of my analysis.

In my specifications, I always control for population density, since this could impact trials. These data are from the History Database of the Global Environment (HYDE), and are available for the years 1500, 1600, 1700, 1710, 1720, and 1730.⁸ HYDE data are based on historical sources of population numbers. Because there are gaps in my data, I linearly interpolate between years, within counties, to construct a balanced panel.

Price data, in real amounts, are from the Allen-Unger database. These data were collected by Robert C. Allen and Richard W. Unger based on various sources, and contains the price of commodities in grams of silver per litre. I examine three commodities in my analysis: wool, herring, and oats. None of the price data come from Scotland, but rather from important trading posts and cities from around the world, like Massachusetts, London, and Paris. Based on the *Atlas of Scottish History to 1707*, I located and mapped Scotland's eight major trading ports: Leith, Glasgow, Bo'ness, Dundee, Ayr, Aberdeen, Burntisland, and Inverness. I then located the Allen-Unger location that is closest to Scotland in that particular time period, and used the commodity price from that port.

For wool, there is only one port for this period, "England." Herring price data are from Antwerp, "England," Frankfurt, Linkoping, and London. There are gaps in the herring price data, which I used linear interpolation to fill.

For oats, I use the price for London from 1550 to 1565, "Southern England" from 1566 to 1702, and Coutances for 1702 onwards.⁹ I also use oats price data with Gibson and Smout's (1994) price series for Scotland, which contains oats prices for Fife. Although oats prices from other regions are also available, Fife has the widest coverage. The correlation between the Allen-Unger oats price, and that from Gibson-Smout, is 0.2872, with corresponding p-value 0.0058. I can thus say that the two price series for oats are well-correlated with each

⁸<http://themasites.pbl.nl/tridion/en/themasites/hyde/>

⁹There is a gap in this data for the year 1582, for which I use the oats price from Antwerp

other.

Suitability for growing oats is from the *Food and Agriculture Organization's* Global Agro-Ecological Zones (GAEZ) database. I use the values for rainfed, low input oats suitability, and merge this with my map of Scotland. Because oats suitability may not identify where oats were actually grown historically, I also use cropland usage data from the History Database of the Global Environment for the year 1500. For pasture land, I acquire data from Ramankutty and Foley (1999), which helps me to identify the suitability for herding sheep. Finally, for herring, I use information from Rorke's (2005) article on the Scottish herring trade from 1470 to 1600; instead of a continuous suitability measure, I use an indicator, since Rorke describes whether or not a region caught herring for export.

County-level fixed effects mean that I do not need to control for time-invariant county characteristics. However, I interact variables, such as distance from Edinburgh and justices of the peace, with my shock to test for heterogeneous responses of trials to weather shocks. These are above-median indicators. For example, if distance from Edinburgh is greater than median, I code this as a 1, and as a 0 otherwise. I acquire justices of the peace data from the *Atlas of Scottish History to 1707*; this is the average number of justices over the period 1587 to 1663, when data is available.

Summary statistics for my dependent variables, shocks of interest, and county characteristics are shown in Table 4.1. I also provide a map of the total number of witchcraft trials over this period in Figure 4.1.¹⁰ As can be seen, Edinburgh and East Lothian (Haddington) had the most intense witch-hunting. In Figure 4.2, I plot time series data for witch trials and temperature in Edinburgh county.

¹⁰The colour palette of my map was selected to reflect the particular chromatic characteristics of the external physiology of witches.

4.6 Results

4.6.1 Main Results

In Table 4.2, I report my main results for witch trials from 1563 to 1727, using three different moving averages for 3 years, 5 years, and 10 years. As can be seen, positive shocks to temperature predict more trials. For example, in column (2), a standard deviation increase in temperature causes a 0.06 standard deviation increase in trials. In column (5), a standard deviation increase in temperature causes a 0.14 standard deviation increase in the probability of a trial, or 4.06%. All regressions control for county-level population density, which might also predict trials.

These effects can be favourably compared to Hsiang et al's (2013) meta-analysis of the literature on climate and conflict, which finds that a standard deviation increase in temperature causes a 4% median increase in interpersonal violence, and a 14% increase in intergroup violence, across studies. Hsiang and his coauthors classify Tanzanian witch killings as "Personal Violence and Crime," and claim that these median figures are substantial, lending support to my results on Scottish witch trials.

4.6.2 Robustness

It is possible that I am not accounting for time-varying, county-specific factors that might affect witchcraft prosecutions. I thus repeat the analysis using county-specific time trends in Table 4.3. The results are still positive, large, and generally significant: for example, in column (6), a standard deviation increase in temperature (10 year MA) causes a 0.19 standard deviation increase in the probability of a trial, or 5.63%.

In Table 4.4, I conduct additional robustness checks by changing the empirical specification. In Columns (1)-(3), I run a logistic regression analysis, and in columns (4)-(6), I use $\ln(trials + 1)$ as my dependent variable. Results are still large and significant. Overall, warmer Scottish temperatures predict more trials.

As an additional check, I exclude trials prior to 1610. This is for two reasons. First, King James set off a national witch panic in 1590 when he accused witches of trying to kill him by sinking his ship. Thus, witch trials in 1590 were not due to local factors. Furthermore, Goodare (2002) claims that autonomous local trials, without central approval, occurred prior to 1610. These trials were conducted in regality courts, private courts that landlords held to settle disputes, and so were not as costly as establishing a commission of justiciary. Results are shown in Table 4.5. The results are significant, and larger than before, as I would expect. For example, in column (1) a standard deviation increase in temperature (3 year MA) causes a 0.11 standard deviation increase in trials. Favourable weather continues to cause more witchcraft trials.

My crucial identifying assumption is that weather is unrelated to unobservables that could bias my estimates. To determine if this is in fact the case, I perform a placebo test in Table 4.6, replacing current moving-average weather shocks with future moving-average weather shocks (one year forward). If my identification is sound, then there is no reason that future weather should predict current witch trials. As my results show, there is no significant relationship between witch trials and future weather, and the coefficient estimates are smaller than those for my main results. For example, in column (2), a standard deviation increase in the five-year moving average predicts only a 0.01 standard deviation increase in trials. Compare this to column (2) in Table 2, with a 0.06 standard deviation increase in trials predicted. This supports my identification strategy.

An alternate explanation for these patterns is greed: namely, witchcraft suspects had possessions that neighbours and the Crown wished to seize, and these possessions increased in value during beneficial years. There were a handfull of cases like this - notably, there were seven high-status women who were accused by heavily indebted men with property disputes (Yeoman, 2002). However, most such prosecutions failed, and the vast majority of witchcraft suspects were low-status women with no valuable property. Of the 316 witch suspects in my dataset whose socioeconomic status is known, only 9 are classified as either “Lairds/Baron”

or “Nobility/Chiefs”. Given the time and resources it took to try witches, historians do not believe that witchcraft trials were driven by greed (Goodare, 2010). Early Modern Scots, including educated elites, believed strongly in witchcraft, and were willing to take concrete measures to extirpate evil.

In Table 4.7, I briefly consider simple lagged temperature shocks, since my hypothesis depends crucially on the buildup of elite resources prior to a trial. In other words, elites need to accumulate resources over time in order for trials to occur, while there is unsatisfied demand. Although only column (4) in Table 4.7 is significant, all specifications yield positive and large coefficients. For example, column (2) shows that a standard deviation increase in temperature causes a 0.08 standard deviation increase in trials.

My hypothesis of a supply-side constraint is further demonstrated through a political incident: the 1660 end of Republican occupation of Scotland. After the English republicans left Scotland to its own devices, there was an outbreak of witchcraft trials. According to my data, only 2 trials were held in 1660. From 1661-1662, there were 612 witches prosecuted. Although this was a political event, it nonetheless supports my resource constraint theory of Scottish witch trials.

I have documented a robust relationship between beneficial temperatures and witchcraft prosecutions. This fits well with the observations that local elites required resources to conduct witchcraft prosecutions. It also supports my theoretical prediction that years of higher income should experience more trials.

4.6.3 Compliers

In Table 4.8, I interact the 5 year moving average weather shock with various compliers. First, I examine whether distance from Edinburgh had an impact. Those wishing to try a witch had to seek the Privy Council or parliament’s permission in Edinburgh; in my model, this proxies for a higher R , the marginal cost of conducting a trial. Indeed, I find that a greater distance from Edinburgh (higher than median) attenuates the impact of the shock

on trials. This lends support to my theoretical framework.

I then determine whether having more justices of the peace has an impact. Data on justices of the peace is available for every county except Cromarty, and therefore my sample size is slightly smaller. More justices of the peace implies a lower R , since there is some legal state capacity available to try a witch. Indeed, this amplifies the impact of the shock, as my model would predict.

To get a sense of u' , the marginal benefit of prosecuting a witch, I use the intuition that during times of national political crisis, u' increases - it is dangerous to let witches roam during these times. I use Levack (2008) to identify five periods of political instability: In 1590-91, James I was fending off a series of rebellions; In 1597, there was a large-scale quarrel between church and the state; In 1643-44, a radical group of presbyterians, the covenanters, consolidated political power in Scotland; In 1649-50, some covenanters fought English military efforts to occupy Scotland; Finally, in 1661-1662, the covenanters were displaced by royal power. I code each of these years as a '1', and a zero otherwise, and then interact this with the weather shock. Clearly, times of crisis exacerbate the impact of this shock on trials. This corresponds to an increase in u' leading to more witch trials.

Finally, I examine distance from the nearest trade port. Counties further from trading ports have less access to alternate sources of income. If this is greater than median, I indicate this as a '1', with a zero otherwise. This is intended to capture y , the value of income. According to my model, the smaller this income is, the less likely a trial is to occur. Indeed, a higher distance from a port attenuates the impact of the shock on witch trials, lending support to my hypothesis.

4.6.4 Mechanisms

Because local elites, such as lairds and burgesses, were responsible for the time and money involved in carrying out witch trials, I expect that positive shocks to their income would lead to more witchcraft trials. Specifically, shocks to export-heavy, taxable commodities like

herring and wool should have an impact on trials, while shocks to Scotland's main subsistence commodity, oats, should have no effect.

Historical evidence states that local elites derived income not only from peasant rents, but also from exports. Customs duties were levelled on goods exiting Scotland, helping to fill the coffers of burgesses and lairds through customs farming. Evidence from Gowrie and Aberdeen also suggests that tenants paid landlords rents in the form of surplus agricultural produce, which landlords then sold to merchants to buttress their income (Young, 2007; Whyte, 1986). Furthermore, many elites (burgesses) of royal burghs and burghs of barony were themselves merchants, who relied on export income to exert local political and economic power (Brown, 1992; Smout, 1973). Based on data from the *Atlas of Scottish History to 1707*, I was able to identify two of the most common exports: wool, and herring. Based on a 1614 survey of exports from the *Atlas*, wools and wool products composed about 15% of total exports, while herring composed 13% of total exports. Fells (timber) is the only commodity that comprises a larger percentage of exports (21%), but based on customs receipts from 1595 to 1599, wool and herring were taxed at a higher rate than fells. I therefore focus on wool and herring as examples of export-heavy, taxable commodities.

Scotland's main subsistence commodity, oats, was not exported much at all. Oats comprise 0.4% of total exports, based on the 1614 survey. Oats' short growing season combined with their nutritious content made them a staple diet among Scots (Smout, 1973). Therefore, although it was an important crop, oats do not contribute significantly to elite income, and therefore should not impact witch trials.

My reduced form relationship between weather and witch trials requires some discussion here. Warm temperatures in Scotland were important for growing oats, but also for producing wool and catching herring. Veterinary studies show that sheep shear better when temperatures are warm, and they can die or fall ill under inclement conditions (Glass and Jacob, 1991; Torell et al., 1969). With regards to herring, a common method of storage at the time was salting (Rorke, 2005), which works better under warmer temperatures.

In Table 4.9, I show the regressions for wool/herring price shocks and witch trials. Total witch trials is the outcome variable. Clearly, higher prices of both commodities lead to more trials. For example, in column (1), a standard deviation increase in the wool price shock (3 year MA) increases trials by 0.15 standard deviations. In column (4), a standard deviation increase in the herring price shock (3 year MA) increases trials by 0.10 standard deviations.

In Table 4.10, I regress witch trials on oat price shocks. In columns (1)-(3), I interact the world oats price with the FAO data for oats suitability. There is no significant effect, although the coefficient estimates' sizes are comparable to those for herring and wool. In column (3), a standard deviation increase in the oat price shock reduces trials by 0.13 standard deviations. The coefficient estimates are negative, which could suggest the following story: peasants who grow and sell oats blame witches when oat prices are low. Although this is plausible, there is no historical evidence to support it, and at any rate, the coefficients are insignificant: even if peasants complain about witches, elites need the resources to prosecute. In columns (4)-(6), I interact the world oats price with HYDE data for cropland usage, and again find an insignificant impact, although it is now positive, further indicating that the oats price does not robustly predict trials. The effect is still sizable - for instance, in column (4), a standard deviation increase in the oats shock predicts a 0.09 standard deviation increase in witchcraft trials - however, given the overall insignificance, and the fact that this insignificance is robust across specifications, I can conclude that oats shocks do not predict witch trials. In columns (7)-(9), I use Gibson and Smout's (1994) oats price data for Fife, and again find an overall insignificant effect. However, in column (9), the 10-year-moving-average is slightly significant, and the coefficient estimate is large: a standard deviation increase in the shock reduces trials by 0.18 standard deviations. Nonetheless, the overall impact of oats prices on trials appears insignificant.

There is, additionally, anecdotal evidence that spending on other public goods increased following favourable weather. The town of Aberdeen, for example, was unique in its retention of a large Justice of the Peace Court, which tried crimes like fornication and adultery.

Running the court was fairly expensive and, according to DesBrisay (1986),

The forces of nature could clearly influence the court's work: in 1697, when... poor weather and serious food shortages led to disease and high mortality, the justice court sat only thirteen times... it seems likely that backlogs of cases occasionally built up. (p. 81)

It therefore makes sense that witch trials would increase following favourable temperature.

4.7 Conclusion

Persecution of populations is not always a disorganised, unruly affair. Events like the Khmer Rouge killings, East Timorese massacres, and Rwandan genocide were all planned and organised by elites. Even the frequent persecution of political dissidents by Saudi Arabia and China requires resources. Similarly, Early Modern Scottish witchcraft trials required local elites' time and material resources.

In this paper, I have shown that positive weather shocks caused more witchcraft trials in Early Modern Scotland. During such good times, local elites had more resources to devote to witch prosecutions. Consistent with this, I find that positive price shocks to export-heavy, taxable commodities, herring and wool, caused more trials, while shocks to oats, Scotland's main subsistence commodity, did not.

A further question raised by this paper is that of policy: namely, how can we prevent persecution, when elites finance it? Based on my findings, the answer might be to target the export of goods that elites derive wealth and power from.¹¹ Indeed, such sanctions are used against states like North Korea and Iran ostensibly for this purpose (Elliot, 1998; Marinov, 2005). An Early Modern 'omnipotent economic planner' wishing to limit witchcraft trials would therefore sanction the export of wool and herring from Scotland. Although such a

¹¹This abstracts from general equilibrium concerns. Sanctions can, after all, harm even persecuted populations by denying them of food, medicine, and income.

thought exercise risks overgeneralising, especially since actors may react unexpectedly to sanctions, the policy implications are worthy of future research.

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4.8 Figures and Tables

Figure 4.1: Total Number of Witchcraft Trials, 1563-1727

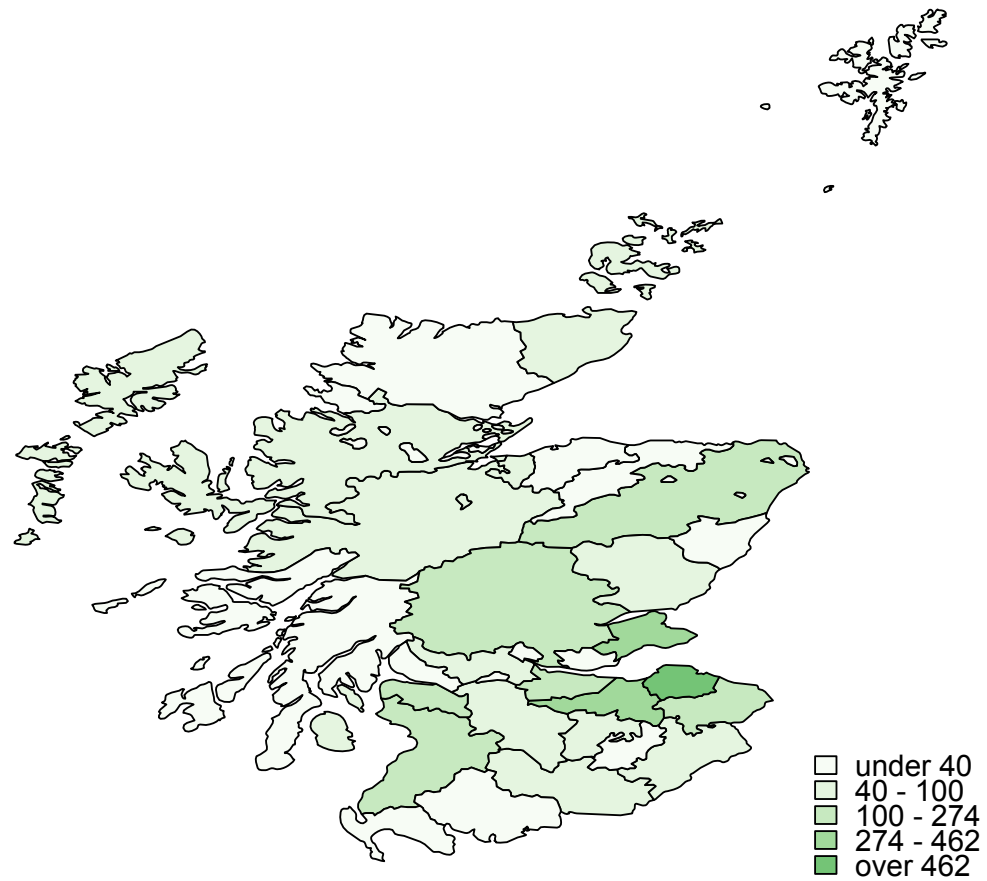


Figure 4.2: Witch Trials and Temperature (5-year MA) in Edinburgh, 1563-1727

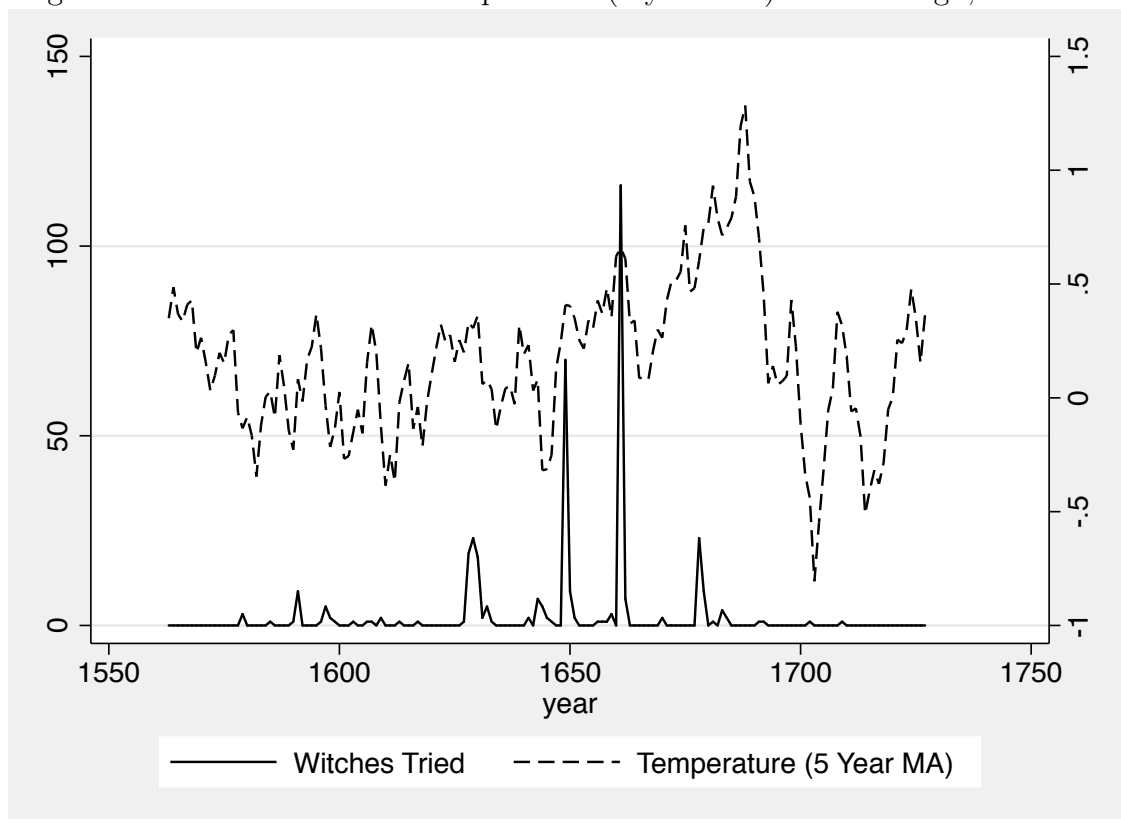


Table 4.1: Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max
<i>Main Results</i>					
Witch Trials Count	5,610	0.55	4.16	0	116
Witch Trials Dummy	5,610	0.10	0.30	0	1
Temperature (3 year MA)	5,610	0.16	0.42	-1.11	1.83
Temperature (5 year MA)	5,610	0.16	0.35	-0.81	1.73
Temperature (10 year MA)	5,610	0.16	0.29	-0.55	1.30
Population Density	5,610	4,008	11,028	18.36	93,331
<i>Commodity Price Shocks</i>					
Suitability $\times$ Price Wool (3 year MA)	5,610	8.78	3.25	0.78	16.74
Suitability $\times$ Price Wool (5 year MA)	5,610	8.78	3.17	0.81	15.98
Suitability $\times$ Price Wool (10 year MA)	5,610	8.77	3.05	0.88	15.26
Suitability $\times$ Price Herring (3 year MA)	5,610	12.45	22.63	0	99.10
Suitability $\times$ Price Herring (5 year MA)	5,610	12.37	22.42	0	90.06
Suitability $\times$ Price Herring (10 year MA)	5,610	12.13	22.02	0	83.35
Suitability $\times$ Price Oats (3 year MA)	5,610	130.20	51.02	24.16	328.10
Suitability $\times$ Price Oats (5 year MA)	5,610	129.84	49.89	34.95	301.38
Suitability $\times$ Price Oats (10 year MA)	5,610	130.04	47.92	39.15	269.41
<i>Compliers</i>					
Distance from Edinburgh: above median	34	0.5	0.5	0	1
Times of crisis: indicator	5,610	0.05	0.23	0	1
Justices of the Peace: above median	33	0.48	0.5	0	1
Distance from a Port: above median	34	0.5	0.5	0	1

Table 4.2: Effect of Temperature Shocks on Witchcraft Trials

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Count	Count	Count	Dummy	Dummy	Dummy
Temperature (3 year MA)	.548 (.504)			.104** (.046)		
Temperature (5 year MA)		.711** (.341)			.116*** (.040)	
Temperature (10 year MA)			.805 (.499)			.161** (.071)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01 , ** < 0.05 , and * < 0.1 .

All regressions control for population density.

Table 4.3: Main Results with County-Specific Trends

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Count	Count	Count	Dummy	Dummy	Dummy
Temperature (3 year MA)	.619 (.571)			.110** (.049)		
Temperature (5 year MA)	.	.850** (.374)			.126*** (.036)	
Temperature (10 year MA)	.		.962 (.601)			.194** (.084)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01 , ** < 0.05 , and * < 0.1 .

All regressions control for population density.

Table 4.4: Main Results: Different Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Dummy	Dummy	Dummy	<i>ln</i>	<i>ln</i>	<i>ln</i>
Temperature (3 year MA)	.012* (.008)			.149* (.076)		
Temperature (5 year MA)	.	.024*** (.009)			.176*** (.061)	
Temperature (10 year MA)	.		.004 (.011)			.220** (.101)
Robustness Check:	Logit	Logit	Logit	Dep Var	Dep Var	Dep Var
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Table 4.5: Excluding Years Prior to 1610

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Count	Count	Count	Dummy	Dummy	Dummy
Temperature (3 year MA)	1.12** (.545)			.196*** (.059)		
Temperature (5 year MA)	.	1.30*** (.413)			.228*** (.059)	
Temperature (10 year MA)	.		1.50*** (.526)			.290*** (.095)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	4,012	4,012	4,012	4,012	4,012	4,012

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Table 4.6: Future Weather Shocks (One Year Forward) and Current Witch Trials

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Count	Count	Count	Dummy	Dummy	Dummy
Temperature (3 year MA)	-.277 (.297)			.048 (.068)		
Temperature (5 year MA)	.	.158 (.415)			.085 (.061)	
Temperature (10 year MA)	.		.417 (.404)			.120 (.071)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01 , ** < 0.05 , and * < 0.1 .

All regressions control for population density.

Table 4.7: Effect of Temperature Shocks on Witchcraft Trials: One Year Lags

	(1)	(2)	(3)	(4)
Dependent variable: Count		Count	Dummy	Dummy
Temperature	.093 (.372)	.524 (.367)	.067 (.055)	.123** (.060)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Counties	34	34	34	34
No. of observations	5,610	4,012	5,610	4,012

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Table 4.8: Compliers

Dependent variable: witch trials	(1)	(2)	(3)	(4)
Temperature (5 year MA)	.194*** (.052)	.198 (.539)	.270 (.216)	2.045** (.804)
Temperature $\times$ interaction	-.060*** (.021)	.813* (.472)	10.09* (5.42)	-1.043** (.482)
Interacted variable:	<i>Distance from Edinburgh</i>	<i>Justices of the Peace</i>	<i>Political Crises</i>	<i>Distance from a Port</i>
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County-Specific Trends	Yes	Yes	Yes	Yes
Counties	34	33	34	34
No. of observations	5,610	5,445	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Table 4.9: Effect of Wool and Herring Shocks on Witch Trials

	(1)	(2)	(3)	(4)	(5)	(6)
Price Shock:	Wool	Wool	Wool	Herring	Herring	Herring
3 year MA	.198** (.092)			.019** (.009)		
5 year MA	.	.186** (.087)			.018** (.008)	
10 year MA	.		.236** (.103)			.015** (.007)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Table 4.10: Effect of Oats Shocks on Witch Trials

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
3 year MA shock	-.013 (.011)			.011 (.013)			-.0006 (.0006)		
5 year MA shock	.	-.012 (.010)			.008 (.011)			-.0004 (0.0004)	
10 year MA shock	.		-.011 (.009)			.008 (.011)			-.001* (.0006)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Counties	34	34	34	34	34	34	34	34	34
No. of observations	5,610	5,610	5,610	5,610	5,610	5,610	2,890	2,822	2,652

Standard errors, clustered at the county level, are reported in parentheses.

Significance levels are *** < 0.01, ** < 0.05, and * < 0.1.

All regressions control for population density.

Appendix A

Appendix

A.1 Derivations and proofs

A.1.1 Demonstration that $\tau > y_H$ will not be chosen.

If the state invests in suppression and chooses $\tau > y_H$, unrest always occurs and is always suppressed. The state's payoff if income is low will be $(1 - p)\gamma\mu y_L - s$, while it will be $(1 - p)\gamma\mu y_H - s$ if high income is realized. The expected value of the state's payoff, then, will be:

$$EV_{S,\tau>y_H}^s = \alpha(1 - p)\gamma\mu y_L + (1 - \alpha)(1 - p)\gamma\mu y_H - s.$$

Contrast this with the case where the state chooses $\tau = \tau_{SH} = \{1 - \mu[1 - \gamma(1 - p)]\}y_H$. If income is high, no unrest occurs, and the state receives $\tau_{SH} - s$, or $\{1 - \mu[1 - \gamma(1 - p)]\}y_H - s$. If income is low, then resistance occurs, and the state receives $(1 - p)\gamma\mu y_L - s$.

The expected value of the state's payoff, then, will be:

$$EV_{SH}^s = \alpha(1 - p)\gamma\mu y_L + (1 - \alpha)\{1 - \mu[1 - \gamma(1 - p)]\}y_H - s.$$

If it were the case that $EV_{S,\tau>y_H}^s$ were preferred to EV_{SH}^s , it would have to be the case that:

$$\alpha(1-p)\gamma\mu y_L + (1-\alpha)(1-p)\gamma\mu y_H - s >$$

$$\alpha(1-p)\gamma\mu y_L + (1-\alpha)\{1 - \mu[1 - \gamma(1-p)]\}y_H - s$$

This simplifies to $\mu > 1$, which is false by assumption. Similar logic shows that EV_{NSH}^s is always preferred to $EV_{NS,\tau>y_H}^s$.

A.1.2 Derivation of expected payoffs for the state

There are four cases to consider: 1) suppression with $\tau = \tau_{SH}$, 2) suppression with $\tau = \tau_{SL}$, 3) no suppression with $\tau = \tau_{NSH}$, and 4) no suppression with $\tau = \tau_{NSH}$.

First, consider suppression with $\tau = \tau_{SH}$. If high income is realized, there will be no resistance, and the state receives $\tau_{SH} - s$, or $\{1 - \mu[1 - \gamma(1-p)]\}y_H - s$. If low income is realized, there is resistance, and the state receives $(1-p)\gamma\mu y_L - s$. The expected payoff of this is:

$$EV_{SH}^s = \alpha(1-p)\gamma\mu y_L + (1-\alpha)\{1 - \mu[1 - \gamma(1-p)]\}y_H - s.$$

Second, consider suppression with $\tau = \tau_{SL}$. Regardless of the realization of income, the state will receive $\tau_{SL} - s$. The state's expected payoff, then, is:

$$EV_{SL}^s = \{1 - \mu[1 - \gamma(1-p)]\}y_L - s.$$

Third, consider no suppression with $\tau = \tau_{NSH}$. If high income is realized, there will be no resistance, and the state receives τ_{NSH} , or $(1-\mu)y_H$. If low income is realized, there is resistance, and the state receives nothing. The expected payoff of this is:

$$EV_{NSH}^s = (1 - \alpha)(1 - \mu)y_H.$$

Finally, consider no suppression with $\tau = \tau_{NSL}$. Regardless of the realization of income, the state will receive τ_{NSL} .

The expected payoff of this is:

$$EV_{NSH}^s = (1 - \mu)y_L.$$

A.1.3 Proof of Main Proposition

The state (weakly) prefers suppression with $\tau = \tau_{SH}$ if $EV_{SH}^s \geq EV_{SL}^s$, $EV_{SH}^s \geq EV_{NSH}^s$, and $EV_{SH}^s \geq EV_{NSL}^s$.

$EV_{SH}^s \geq EV_{SL}^s$ if:

$$\alpha(1 - p)\gamma\mu y_L + (1 - \alpha)\{1 - \mu[1 - \gamma(1 - p)]\}y_H - s \geq \{1 - \mu[1 - \gamma(1 - p)]\}y_L - s.$$

This simplifies to:

$$y_H \geq \frac{1 - \mu[1 - (1 - \alpha)\gamma(1 - p)]}{(1 - \alpha)\{1 - \mu[1 - \gamma(1 - p)]\}}y_L \equiv \Phi_{SL}^{SH}(y_L).$$

$EV_{SH}^s \geq EV_{NSH}^s$ if:

$$\alpha(1 - p)\gamma\mu y_L + (1 - \alpha)\{1 - \mu[1 - \gamma(1 - p)]\}y_H - s \geq (1 - \alpha)(1 - \mu)y_H.$$

This simplifies to:

$$y_H \geq \frac{s}{(1-\alpha)\mu\gamma(1-p)} - \frac{\alpha}{1-\alpha}y_L \equiv \Phi_{NSH}^{SH}(y_L).$$

$EV_{SH}^s \geq EV_{NSL}^s$ if:

$$\alpha(1-p)\gamma\mu y_L + (1-\alpha)\{1 - \mu[1 - \gamma(1-p)]\}y_H - s \geq \frac{1}{1-\alpha}y_L.$$

This simplifies to:

$$y_H \geq \frac{s}{(1-\alpha)\{1 - \mu[1 - \gamma(1-p)]\}} + \frac{1 - \mu[1 + \alpha\gamma(1-p)]}{(1-\alpha)\{1 - \mu[1 - \gamma(1-p)]\}}y_L \equiv \Phi_{NSL}^{SH}(y_L).$$

Together, these three conditions define the region $\mathcal{S}_{SH}$:

$$\mathcal{S}_{SH} = \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \geq \Phi_{SL}^{SH}(y_L), y_H \geq \Phi_{NSL}^{SH}(y_L), y_H \geq \Phi_{NSH}^{SH}(y_L)\}.$$

The state (weakly) prefers suppression with $\tau = \tau_{SL}$ if $EV_{SL}^s \geq EV_{SH}^s$, $EV_{SL}^s \geq EV_{NSH}^s$, and $EV_{SL}^s \geq EV_{NSL}^s$.

The condition that $EV_{SL}^s \geq EV_{SH}^s$ is simply the reverse of the above condition that $EV_{SH}^s \geq EV_{SL}^s$, and so simplifies to $y_H \leq \Phi_{SL}^{SH}(y_L)$.

$EV_{SL}^s \geq EV_{NSH}^s$ if:

$$\{1 - \mu[1 - \gamma(1-p)]\}y_L - s \geq (1-\alpha)(1-\mu)y_H.$$

This simplifies to:

$$y_H \leq -\frac{s}{(1-\alpha)(1-\mu)} + \frac{1-\mu[1-\gamma(1-p)]}{(1-\alpha)(1-\mu)}y_L \equiv \Phi_{SL}^{NSH}(y_L)$$

$EV_{SL}^s \geq EV_{NSL}^s$ if:

$$\{1 - \mu[1 - \gamma(1 - p)]\}y_L - s \geq (1 - \mu)y_L.$$

This simplifies to:

$$y_L \geq \frac{s}{\mu\gamma(1-p)} \equiv \Phi_{NSL}^{SL}.$$

Note that Φ_{SL}^{NSH} is redundant. For Φ_{SL}^{NSH} to matter in the definition of $\mathcal{S}_{SL}$, it would need to intersect Φ_{SL}^{SH} to the right of the intersection between Φ_{SL}^{SH} and Φ_{NSL}^{SL} . However, because Φ_{SL}^{NSH} and Φ_{SL}^{SH} intersect when $y_L = \frac{s(1-\mu+\mu\gamma(1-p))}{\mu\gamma(1-p)[(1-\mu+\mu\gamma(1-p))+\alpha(1-\mu)]}$, while Φ_{SL}^{SH} and Φ_{NSL}^{SL} intersect when $y_L = \frac{s}{\mu\gamma(1-p)}$, this cannot occur.

Together, the two remaining conditions define the region $\mathcal{S}_{SL}$:

$$\mathcal{S}_{SL} = \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \leq \Phi_{SL}^{SH}(y_L), y_L \geq \Phi_{NSL}^{SL}\}.$$

The state (weakly) prefers no suppression with $\tau = \tau_{NSH}$ if $EV_{NSH}^s \geq EV_{SH}^s$, $EV_{NSH}^s \geq EV_{SL}^s$, and $EV_{NSH}^s \geq EV_{NSL}^s$.

The condition that $EV_{NSH}^s \geq EV_{SH}^s$ simply reverses the earlier condition that $EV_{SH}^s \geq EV_{NSH}^s$, and so simplifies to $y_H \leq \Phi_{NSH}^{SH}(y_L)$. The condition that $EV_{NSH}^s \geq EV_{SL}^s$, likewise, reverses the condition that $EV_{SL}^s \geq EV_{NSH}^s$. It simplifies to $y_H \geq \Phi_{SL}^{NSH}(y_L)$. Note that Φ_{SL}^{NSH} is again redundant. For Φ_{SL}^{NSH} to matter in the definition of $\mathcal{S}_{NSH}$, it would need to intersect Φ_{NSL}^{NSH} to the left of the intersection between Φ_{NSL}^{NSH} and Φ_{NSH}^{SH} . However, because Φ_{SL}^{NSH}

intersects Φ_{NSL}^{NSH} when $y_L = \frac{s}{\mu\gamma(1-p)}$, while Φ_{NSL}^{NSH} intersects Φ_{NSH}^{SH} where $y_L = \frac{s}{\mu\gamma(1+\alpha)(1-p)}$, this cannot occur.

$EV_{NSH}^s \geq EV_{NSL}^s$ if:

$$(1 - \alpha)(1 - \mu)y_H \geq (1 - \mu)y_L.$$

This simplifies to:

$$y_H \geq \frac{1}{1 - \alpha}y_L \equiv \Phi_{NSL}^{NSH}.$$

Together, the two binding conditions define the region $\mathcal{S}_{NSH}$:

$$\mathcal{S}_{NSH} = \{(y_H, y_L) \in \mathbb{R}_+^2 : y_H \leq \Phi_{NSH}^{SH}(y_L), y_H \geq \Phi_{NSL}^{NSH}(y_L)\}.$$

Finally, the state (strongly) prefers no suppression with $\tau = \tau_{NSL}$ if $EV_{NSL}^s > EV_{SH}^s$, $EV_{NSL}^s > EV_{SL}^s$, and $EV_{NSL}^s > EV_{NSH}^s$. Each of these simply reverse conditions stated above, and so reduce to $y_H < \Phi_{NSL}^{SH}(y_L)$, and $y_L < \Phi_S^{NSL}L$, and $y_H < \Phi_{NSL}^{NSH}(y_L)$ respectively. Together, these define the region $\mathcal{S}_{NSL}$:

$$\mathcal{S}_{NSL} = \{(y_H, y_L) \in \mathbb{R}_+^2 : (y_H, y_L) \notin \mathcal{S}_{SH} \cup \mathcal{S}_{NSH} \cup \mathcal{S}_{NSL}\}.$$

A.1.4 Notes on Figure 3.1

Figure 3.1 is drawn to be consistent with several characteristics of $\mathcal{S}_{SH}$, $\mathcal{S}_{NSH}$, $\mathcal{S}_{SL}$, and $\mathcal{S}_{NSL}$. Note that λ_{NSL}^{SH} , λ_{NSH}^{SH} , θ_{SL}^{SH} , λ_{SL}^{NSH} , θ_{NSL}^{SH} , θ_{SL}^{NSH} , θ_{NSL}^{NSH} , and θ_{NSL}^{SL} are all positive scalars, while θ_{NSL}^{SH} is ambiguous in sign. The equations $\Phi_{SL}^{SH}(y_L)$, $\Phi_{NSL}^{SH}(y_L)$, $\Phi_{SL}^{NSH}(y_L)$

and $\Phi_{NSL}^{NSH}(y_L)$ are all upward-sloping in y_L . Because $\theta_{SL}^{SH} > \theta_{NSL}^{SH}$, $\Phi_{SL}^{SH}(y_L)$ is steeper than $\Phi_{NSL}^{SH}(y_L)$. Since $\theta_{SL}^{SH} < \theta_{SL}^{NSH}$, $\Phi_{SL}^{SH}(y_L)$ is flatter than $\Phi_{SL}^{NSH}(y_L)$. Because $\theta_{SL}^{SH} > \theta_{NSL}^{NSH}$, $\Phi_{SL}^{SH}(y_L)$ is steeper than $\Phi_{NSL}^{NSH}(y_L)$.

Because $\theta_{NSL}^{SH} < \theta_{NSL}^{NSH}$, $\Phi_{NSL}^{SH}(y_L)$ is flatter than $\Phi_{NSL}^{NSH}(y_L)$. Similarly, $\theta_{NSL}^{SH} < \theta_{NSL}^{NSH}$, and so $\Phi_{NSL}^{SH}(y_L)$ is flatter than $\Phi_{NSL}^{NSH}(y_L)$. Finally, because $\theta_{SL}^{NSH} > \theta_{NSL}^{NSH}(y_L)$, $\Phi_{SL}^{NSH}(y_L)$ is steeper than $\Phi_{NSL}^{NSH}(y_L)$.

Also note that $\Phi_{NSL}^{SH}(y_L)$ and $\Phi_{NSH}^{SH}(y_L)$ have strictly positive intercepts. Because $\lambda_{NSL}^{SH} < \lambda_{NSH}^{SH}$, $\Phi_{NSL}^{SH}(y_L)$ has a lower intercept than $\Phi_{NSH}^{SH}(y_L)$.

Define $\beta \equiv 1 - \mu + \mu\gamma(1 - p)$. The points at which $\Phi_{SL}^{SH}(y_L)$, $\Phi_{NSL}^{SH}(y_L)$, $\Phi_{NSH}^{SH}(y_L)$, $\Phi_{SL}^{NSH}(y_L)$, $\Phi_{NSL}^{NSH}(y_L)$ and Φ_{NSL}^{SL} intersect are given by:

$$\begin{aligned}
\Phi_{SL}^{SH} \cap \Phi_{NSL}^{SH} & \text{ at } \left\{ \frac{s}{\mu\gamma(1-p)}, \theta_{SL}^{SH} \frac{s}{\mu\gamma(1-p)} \right\} & \equiv x_0 \\
\cap \Phi_{NSH}^{SH} & \text{ at } \left\{ \frac{s\beta}{\mu\gamma(1-p)[\beta + \alpha(1-\mu)]}, \theta_{SL}^{SH} \frac{s\beta}{\mu\gamma(1-p)[\beta + \alpha(1-\mu)]} \right\} & \equiv x_1 \\
\cap \Phi_{SL}^{NSH} & \text{ at } \left\{ \frac{s\beta}{\mu\gamma(1-p)[\beta + \alpha(1-\mu)]}, \theta_{SL}^{SH} \frac{s\beta}{\mu\gamma(1-p)[\beta + \alpha(1-\mu)]} \right\} & \equiv x_1 \\
\cap \Phi_{NSL}^{NSH} & \text{ at } \{0, 0\} & \equiv o \\
\cap \Phi_{NSL}^{SL} & \text{ at } \left\{ \frac{s}{\mu\gamma(1-p)}, \theta_{SL}^{SH} \frac{s}{\mu\gamma(1-p)} \right\} & \equiv x_0
\end{aligned}$$

$$\begin{aligned}
\Phi_{NSL}^{SH} \cap \Phi_{NSH}^{SH} & \text{ at } \left\{ \frac{s}{\mu\gamma(1+\alpha)(1-p)}, \frac{s}{(1-\alpha)\mu\gamma(1+\alpha)(1-p)} \right\} & \equiv x_2 \\
\cap \Phi_{SL}^{NSH} & \text{ at } \{y_L(x_3), y_H(x_3)\} & \equiv x_3 \\
\cap \Phi_{NSL}^{NSH} & \text{ at } \left\{ \frac{s}{\mu\gamma(1+\alpha)(1-p)}, \frac{s}{(1-\alpha)\mu\gamma(1+\alpha)(1-p)} \right\} & \equiv x_2 \\
\cap \Phi_{NSL}^{SL} & \text{ at } \left\{ \frac{s}{\mu\gamma(1-p)}, \theta_{SL}^{SH} \frac{s}{\mu\gamma(1-p)} \right\} & \equiv x_0
\end{aligned}$$

Here,

$$y_L(x_3) = \frac{(\beta + 1 - \mu)s}{\beta^2 - (1 - \mu)[\beta - (1 + \alpha)\mu\gamma(1 - p)]},$$

$$y_H(x_3) = -\frac{s}{(1 - \alpha)(1 - \mu)} + \frac{\beta}{(1 - \alpha)(1 - \mu)} \times \frac{(\beta + 1 - \mu)s}{\beta^2 - (1 - \mu)[\beta - (1 + \alpha)\mu\gamma(1 - p)]}$$

$$\begin{aligned} \Phi_{NSH}^{SH} \cap \Phi_{SL}^{NSH} \text{ at } \left\{ \frac{s\beta}{\mu\gamma(1 - p)[\beta + \alpha(1 - \mu)]}, \theta_{SL}^{SH} \frac{s\beta}{\mu\gamma(1 - p)[\beta + \alpha(1 - \mu)]} \right\} &\equiv x_1 \\ \cap \Phi_{NSL}^{NSH} \text{ at } \left\{ \frac{s}{\mu\gamma(1 + \alpha)(1 - p)}, \frac{s}{(1 - \alpha)\mu\gamma(1 + \alpha)(1 - p)} \right\} &\equiv x_2 \\ \cap \Phi_{NSL}^{SL} \text{ at } \left\{ \frac{s}{\mu\gamma(1 - p)}, \frac{s}{\mu\gamma(1 - p)} \right\} &\equiv f \end{aligned}$$

$$\begin{aligned} \Phi_{SL}^{SH} \cap \Phi_{NSL}^{NSH} \text{ at } \left\{ \frac{s}{\mu\gamma(1 - p)}, \frac{s}{(1 - \alpha)\mu\gamma(1 - p)} \right\} &\equiv x_4 \\ \cap \Phi_{NSL}^{SL} \text{ at } \left\{ \frac{s}{\mu\gamma(1 - p)}, \frac{s}{(1 - \alpha)\mu\gamma(1 - p)} \right\} &\equiv x_4 \end{aligned}$$

$$\Phi_{NSL}^{NSH} \cap \Phi_{NSL}^{SL} \text{ at } \left\{ \frac{s}{\mu\gamma(1 - p)}, \frac{s}{(1 - \alpha)\mu\gamma(1 - p)} \right\} \equiv x_4$$

A.1.5 Comparative statics: Unrest

First, consider a change in α , the probability of low income. It is the case that:

$$\begin{aligned}
\frac{\partial \theta_{SL}^{SH}}{\partial \alpha} &= \frac{\gamma \mu (1-p)(N(\theta_{SL}^{SH}) - D(\theta_{SL}^{SH})) + (1-\mu)N(\theta_{SL}^{SH})}{D(\theta_{SL}^{SH})^2} > 0 \\
\frac{\partial \lambda_{NSL}^{SH}}{\partial \alpha} &= \frac{\lambda_{NSL}^{SH}}{(1-\alpha)} > 0 \\
\frac{\partial \theta_{NSL}^{SH}}{\partial \alpha} &= \frac{\gamma \mu (1-p)(N(\theta_{NSL}^{SH}) - D(\theta_{NSL}^{SH})) + (1-\mu)N(\theta_{NSL}^{SH})}{D(\theta_{NSL}^{SH})^2} \leq 0 \\
\frac{\partial \theta_{NSL}^{NSH}}{\partial \alpha} &= \frac{\alpha}{(1-\alpha)^2} > 0
\end{aligned}$$

We use $N(X)$ and $D(X)$ to denote the numerator and denominator of X , respectively. These results rely on the observations that $N(\theta_{SL}^{SH}) > D(\theta_{SL}^{SH})$, and $N(\theta_{NSL}^{SH}) \leq D(\theta_{NSL}^{SH})$.

Second, consider a change in γ .

$$\begin{aligned}
\frac{\partial \theta_{SL}^{SH}}{\partial \gamma} &= -\frac{(N(\theta_{SL}^{SH}) - D(\theta_{SL}^{SH}))\mu(1-\alpha)\gamma(1-p)}{D(\theta_{SL}^{SH})^2} < 0 \\
\frac{\partial \lambda_{NSL}^{SH}}{\partial \gamma} &= -\frac{s(1-\alpha)\mu(1-p)}{D(\lambda_{NSL}^{SH})^2} < 0 \\
\frac{\partial \theta_{NSL}^{SH}}{\partial \gamma} &= \frac{(D(\theta_{NSL}^{SH}) - N(\theta_{NSL}^{SH}))(1-\alpha)\mu(1-p) - D(\theta_{NSL}^{SH})\mu(1-p)}{D(\theta_{NSL}^{SH})^2} \leq 0 \\
\frac{\partial \theta_{NSL}^{NSH}}{\partial \gamma} &= 0
\end{aligned}$$

We again rely on the fact that $N(\theta_{SL}^{SH}) > D(\theta_{SL}^{SH})$.

Third, consider a change in μ .

$$\begin{aligned}
\frac{\partial \theta_{SL}^{SH}}{\partial \mu} &= \frac{(N(\theta_{SL}^{SH}) - D(\theta_{SL}^{SH}))(1 - (1-\alpha)\gamma(1-p)) - \alpha N(\theta_{SL}^{SH})}{D(\theta_{SL}^{SH})^2} \leq 0 \\
\frac{\partial \lambda_{NSL}^{SH}}{\partial \mu} &= \frac{s(1-\alpha)[1 - \gamma(1-p)]}{D(\lambda_{NSL}^{SH})^2} > 0 \\
\frac{\partial \theta_{NSL}^{SH}}{\partial \mu} &= \frac{(N(\theta_{NSL}^{SH}) - D(\theta_{NSL}^{SH}))\alpha\gamma(1-p) + N(1-\alpha-\gamma(1-p))}{D(\theta_{NSL}^{SH})^2} \leq 0 \\
\frac{\partial \theta_{NSL}^{NSH}}{\partial \mu} &= 0
\end{aligned}$$

Fourth, consider a change in p .

$$\begin{aligned}
\frac{\partial \theta_{SL}^{SH}}{\partial(1-p)} &= -\frac{(N(\theta_{SL}^{SH}) - D(\theta_{SL}^{SH}))(1-\alpha)\gamma\mu}{D(\theta_{SL}^{SH})^2} < 0 \\
\frac{\partial \lambda_{NSL}^{SH}}{\partial(1-p)} &= -\frac{s(1-\alpha)}{D(\lambda_{NSL}^{SH})^2} \\
\frac{\partial \theta_{NSL}^{SH}}{\partial(1-p)} &= \frac{(N(\theta_{NSL}^{SH}) - D(\theta_{NSL}^{SH}))\alpha\gamma\mu - \mu\gamma N(\theta_{NSL}^{SH})}{D(\theta_{NSL}^{SH})^2} \\
\frac{\partial \theta_{NSL}^{NSH}}{\partial(1-p)} &= 0
\end{aligned}$$

We again use the fact that $N(\theta_{SL}^{SH}) > D(\theta_{SL}^{SH})$.

Finally, consider a change in s .

$$\begin{aligned}
\frac{\partial \theta_{SL}^{SH}}{\partial s} &= 0 \\
\frac{\partial \lambda_{NSL}^{SH}}{\partial s} &= \frac{1}{D(\lambda_{NSL}^{SH})} > 0 \\
\frac{\partial \theta_{NSL}^{SH}}{\partial s} &= 0 \\
\frac{\partial \theta_{NSL}^{NSH}}{\partial s} &= 0
\end{aligned}$$

A.1.6 Comparative statics: Suppression

Here, we discuss the effect of α , γ , μ , p and s on the size of the region in which the state invests in suppression, i.e. the size of $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$. This region is bounded by $\Phi_{NSH}^{SH}(y_L)$, $\Phi_{NSL}^{SH}(y_L)$, and $\Phi_{NSL}^{SL}(y_L)$.

First, consider an increase in α . $\Phi_{NSH}^{SH}(y_L)$ shifts upwards and becomes steeper. $\Phi_{NSL}^{SH}(y_L)$ shifts upwards and rotates ambiguously. $\Phi_{NSL}^{SL}(y_L)$ does not move. Together, these shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$, making suppression less likely.

Second, consider an increase in γ . $\Phi_{NSH}^{SH}(y_L)$ shifts downwards. $\Phi_{NSL}^{SH}(y_L)$ shifts down-

wards and rotates ambiguously. $\Phi_{NSL}^{SL}(y_L)$ shifts left. Together, these enlarge the region $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$, making suppression more likely.

Third, consider an increase in μ . $\Phi_{NSH}^{SH}(y_L)$ shifts downwards. $\Phi_{NSL}^{SH}(y_L)$ shifts downwards and rotates ambiguously. $\Phi_{NSL}^{SL}(y_L)$ shifts left. Together, these enlarge the region $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$, making suppression more likely.

Fourth, consider an increase in p . $\Phi_{NSH}^{SH}(y_L)$ shifts upwards. $\Phi_{NSL}^{SH}(y_L)$ shifts upwards and rotates ambiguously. $\Phi_{NSL}^{SL}(y_L)$ shifts right. Together, these shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$, making suppression less likely.

Finally, consider an increase in s . $\Phi_{NSH}^{SH}(y_L)$ shifts upwards. $\Phi_{NSL}^{SH}(y_L)$ shifts upwards. $\Phi_{NSL}^{SL}(y_L)$ shifts right. Together, these shrink the region $\mathcal{S}_{SH} \cup \mathcal{S}_{SL}$, making suppression less likely.

Table A1. Sources of Price Data

Product	Source	Currency	Years
Bananas	Lord and Boye (1991)	USD	1900-1988
Cocoa	Blattman, Hwang, and Williamson (2007), Bazzi and Blattman	GBP (BHW), USD (BB)	1840-1950 (BHW), 1950-1956 (BB)
Coffee	Blattman, Hwang, and Williamson (2007), Bazzi and Blattman	GBP (BHW), USD (BB)	1846-1950 (BHW), 1950-2009 (BB)
Cotton	Historical Statistics of the United States	USD	1784-1998
Groundnuts	Historical Statistics of the United States	USD	1909-1999
Palm-kernels/Palm Oil	Blattman, Hwang, and Williamson (2007), Bazzi and Blattman	GBP (BHW), USD (BB)	1846-1950 (BHW), 1950-2009 (BB)
US Wholesale Price Index	Historical Statistics of the United States	N/A	1890-1951
UK Wholesale Price Index	Allen (2003)	N/A	1750-2002

Table A2. Main results with district trends

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
Shock	-0.165** (0.075)	2.554** (1.144)	-0.228** (0.103)	-0.190* (0.112)
Observations	1,431	1,431	1,431	1,431
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.077 (0.067)	-0.237* (0.123)	0.196* (0.113)	0.090* (0.047)
Observations	1,431	1,431	1,212	1,431
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year and with district-specific trends. The dependent variable is an indicator for an incident.

Table A3. Main results with one-year lags as shocks

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
Shock	-0.081 (0.051)	1.723* (0.913)	-0.250** (0.118)	-0.142** (0.063)
Observations	1,431	1,431	1,431	1,431
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.104* (0.053)	-0.185** (0.090)	0.175** (0.084)	0.075 (0.046)
Observations	1,431	1,431	1,212	1,431
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table A4. Main results estimated using logit

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
Shock	-0.160** (0.070)	3.773*** (1.221)	-0.340** (0.146)	-0.160** (0.066)
Observations	1,145	1,145	1,145	1,145
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.138 (0.089)	-0.119 (0.086)	0.411** (0.191)	0.089 (0.059)
Observations	1,145	1,145	968	1,145
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Marginal effects reported. Standard errors clustered by district in parentheses. The estimator is logit, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table A5. All shocks at once

	(1)
	<i>Dependent variable: Incident</i>
Log rainfall (3 year MA)	-0.247*** (0.087)
Log temperature (3 year MA)	3.728*** (1.238)
Exporter X Log Price: Bananas (3 year MA)	-0.217 (0.134)
Exporter X Log Price: Cocoa (3 year MA)	-0.258* (0.140)
Exporter X Log Price: Coffee (3 year MA)	-0.050 (0.078)
Exporter X Log Price: Cotton (3 year MA)	-0.258** (0.110)
Exporter X Log Price: Groundnuts (3 year MA)	0.202** (0.098)
Exporter X Log Price: Palm (3 year MA)	0.152** (0.059)
Observations	1,212

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table A6. Further robustness

	(1)	(2)	(3)	(4)
<i>Panel A: Main results with lag incident</i>				
	<i>Dependent variable: Incident</i>			
Shock	-0.219*** (0.075)	3.566** (1.364)	-0.421** (0.162)	-0.250** (0.113)
Lag Incident	-0.050 (0.034)	-0.050 (0.034)	-0.055 (0.033)	-0.053 (0.034)
Observations	1,183	1,183	1,183	1,183
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.158 (0.124)	-0.034 (0.159)	0.264** (0.133)	0.117** (0.049)
Lag Incident	-0.058* (0.033)	-0.054 (0.034)	-0.046 (0.038)	-0.052 (0.034)
Observations	1,183	1,183	1,074	1,183
Shock	Coffee	Cotton	Groundnuts	Palm
<i>Panel B: Main results discarding districts with no variation in the outcome</i>				
	(1)	(2)	(3)	(4)
	<i>Dependent variable: Incident</i>			
Shock	-0.199** (0.076)	3.742*** (1.274)	-0.335** (0.147)	-0.151** (0.073)
Observations	1,166	1,166	1,166	1,166
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Incident</i>			
	(5)	(6)	(7)	(8)
Shock	-0.119* (0.069)	-0.234* (0.131)	0.282* (0.158)	0.085 (0.057)
Observations	1,166	1,166	989	1,166
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for an incident.

Table A7. Modern results with district trends

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
Shock	-0.073 (0.081)	-0.251 (1.480)	-0.188 (0.159)	-0.003 (0.143)
Observations	1,554	1,554	1,456	1,456
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Riots/protests</i>			
	(5)	(6)	(7)	(8)
Shock	-0.027 (0.060)	-0.778** (0.392)	0.209 (0.147)	0.032 (0.062)
Observations	1,456	1,456	1,456	1,456
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year and with district-specific trends. The dependent variable is an indicator for riots/protests.

Table A8. Modern results with one-year lags as shocks

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
Shock	-0.051 (0.039)	0.053 (0.752)	-0.188 (0.116)	0.057 (0.094)
Observations	1,665	1,665	1,456	1,456
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Riots/protests</i>			
	(5)	(6)	(7)	(8)
Shock	0.002 (0.047)	-0.266 (0.181)	0.101 (0.097)	0.049 (0.055)
Observations	1,456	1,456	1,456	1,456
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for riots/protests.

Table A9. Modern results with incident counts as the dependent variable.

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
Shock	-1.034 (0.815)	19.353 (12.805)	-4.148 (5.468)	-1.239 (1.256)
Observations	1,554	1,554	1,456	1,456
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Riots/protests</i>			
	(5)	(6)	(7)	(8)
Shock	-0.117 (0.156)	-0.283 (0.532)	1.156** (0.579)	0.128 (0.198)
Observations	1,456	1,456	1,456	1,456
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is the number of riots/protests in a year.

Table A10. Modern results discarding capital cities.

	(1)	(2)	(3)	(4)
	<i>Dependent variable: Riots/protests</i>			
Shock	-0.134*	0.175	-0.240	-0.058
	(0.074)	(1.182)	(0.195)	(0.128)
Observations	1,442	1,442	1,352	1,352
Shock	Rain	Temp.	Bananas	Cocoa
	<i>Dependent variable: Riots/protests</i>			
	(5)	(6)	(7)	(8)
Shock	0.003	-0.135	0.282**	0.042
	(0.057)	(0.188)	(0.126)	(0.063)
Observations	1,352	1,352	1,352	1,352
Shock	Coffee	Cotton	Groundnuts	Palm

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by district in parentheses. The estimator is OLS, with fixed effects for district and year. The dependent variable is an indicator for riots/protests.