The Emergency

British detention camps and the origins of distrust in Kenya*

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Abstract

This study examines the long-run effects of British detention camps in colonial Kenya on contemporary economic well-being and trust. During the dawn of colonial rule in Kenya, the British Empire was confronted with a violent uprising to which it responded with far-reaching measures, in effect suspecting anyone sharing the ethnicity of the so-called Mau Mau tribes, and incarcerating a significant share of the native population between 1954 and 1959. Exploiting geographic and individual characteristics to identify the affected individuals and households, we show that individuals exposed to detention camps have worse development outcomes today. We use rich contemporary survey data to document that affected individuals tend to be less trusting, accumulate less wealth, and are less literate, even though their ethnic kin belong to the ruling class of contemporary Kenya.

Keywords: detention camps, conflict, discrimination, trust, development, colonialism
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1 Introduction

Towards the end of British colonial rule in East Africa, a violent land dispute erupted in Kenya. From their arrival, British settlers had claimed some of the colony's most fertile land, while natives where confined to reserves with limited space and restrictions on the crops they could produce, or squatting as laborers on white farms (Mosley, 1982, Moradi, 2009, Fazan, 2014). When capacity cuts after WW II and technological progress led to large numbers of squatters being evicted (Anderson, 2005), disgruntled farmers, former soldiers and radical politicians started attacking Africans and white settlers who supported the colonial government. This nationalist independence movement, which was later coined the "Mau Mau,"[1] became a major challenge to the fading empire. A small but influential settler population—vastly outnumbered by Africans—pressured the colonial state to respond forcefully and proclaim a state of emergency.[2] In the following years, Britain set up a large number of detention camps in Kenya and interned anyone they believed to be associated with the uprising. As a consequence, the vast majority of three specific Kenyan tribes (the Kikuyu, Embu and Meru) were interrogated and many of them subsequently sent to detention camps between 1952 and 1959 (see e.g. Majdalany, 1963, Odhiambo and Lonsdale, 2003, Elkins, 2005).

Estimates suggest that somewhere between 50,000 and (at most) 300,000 people died while being held in a camp or shortly thereafter, while survivors suffer from physical and psychological abuse to this day (Elkins, 2005, Blacker, 2007). Testimonials and court cases reveal that detention often involved hard labor, beatings, torture, castration and rape (Elkins, 2005, Anderson, 2011). Interviews with survivors further suggest that when a household member was incarcerated, their children were dropping out of school, infant mortality spiked, and families were expropriated of their possessions (Elkins, 2005). A clinical study among 180 former Mau Mau detainees shows that they experience PTSD at higher intensities than American prisoners of war in Japan during WW I (Atwoli et al., 2006). Although much has been written about the Mau Mau movement and its role in Kenyan history, we are not aware of any comprehensive empirical study of this event. Systematic destruction of the archival records and inmate registries has turned any quantitative assessment of the effects of detention into a challenge (Anderson, 2011).

This study uses microeconometric methods and a rich body of original data to analyze the short and long run effects these internment camps had on the Kenyan people. The historical record on the Mau Mau uprising suggests that *i)* the British screened for alleged insurgents solely on an ethnic basis—anyone from the Kikuyu, Embu, or Meru tribes[3]

[1] The origin of the term *Mau Mau* is vague. Kariuki (1964) suggests that the it is an anagram of "uma, uma" (to ache) in Swahili, as anagrams and repetitions are often shouted by Kikuyu children. It was then used by a Christian leader and early opponent of the movement as a derogatory term.

[2] The state of emergency was declared in October 1952 and remained in place until December 1959.

[3] Odhiambo and Lonsdale (2003) and many others describe this process in detail. Non-Kikuyus (and related tribes) were interviewed and then allowed to return home, while "Kikuyu, Embu and Meru

was automatically considered a suspect, *ii)* a significant share of the 1.5 million Kikuyu, Embu, and Meru were in one of the camps during the period of operation from 1952 to 1959, although no precise estimate is available, and *iii)* Africans from other ethnic groups and Kikuyu loyalists[4] played a large role in fighting the Mau Mau, identifying suspects, and overseeing the camps (Anderson, 2017). In the absence of direct information identifying individuals or households affected by detention, we construct an intention-to-treat proxy along three dimensions: proximity to the location of a former camp, belonging to the Kikuyu, Embu or Meru Tribe, and being born not later than 1959—the end of the Emergency. Preliminary results suggest negative effects on contemporary socioeconomic outcomes. We find that exposure to British camps led to large decreases in literacy, wealth and trust which persist until today. We also find that the negative effects of internment are passed on to children but dissipate over approximately two generations.

This study contributes to our understanding of the effects of British detention camps in Kenya by empirically studying the direct and indirect effects of the Emergency. We leverage a combination of geospatial data on the location of camp sites, geocoded historical census data from 1948 until 1989 and contemporary DHS surveys to study the immediate and longer term effects of the detention camps on contemporary living standards. Our identification strategy exploits the indiscriminate nature of the British counterinsurgency campaign to identify the effects of labor camps on the affected population via a triple differences-in-differences strategy.

Our work relates to a nascent literature on forced labor, re-education or resettlement camps and their impact on social cohesion (see Lupu and Peisakhin, 2017, Dippel, 2014, Lowes and Montero, 2017, Abel, 2016). The literature typically finds increased levels of trust among the affected population, lasting until long after the event in question. While the camp population could have certainly developed an in-group bias in the Kenyan case, the British deliberately attempted to break ethnic bonds by using Kikuyu loyalists and other Africans as informants, overseers and guards (Anderson, 2017). Using individual data from the Afrobarometer, we study this negative shock to social cohesion and its diffusion over space and cohorts.

Finally, the study contributes to the broader literature on state building and the legacy of European colonization. Kenya's rural uprising is an interesting case in this respect, since it was led by the country's largest ethnic group (the Kikuyu) who would come to dominate post-independence politics. Kenyan politics today are ethnically charged and the 2008 post-election violence has been linked by observers to persistent in-group

suspects were not so fortunate. At the screening camp in Langata, specially constructed for the purpose, hooded informants surveyed the ranks of thousands of suspects, identifying those they considered to be Mau Mau activists or sympathizers" (p. 161).

[4]The term loyalist refers to individuals who were part of the three Central Province tribes and supportive of the colonial government. It is also often used more specifically to refer to supporters who served in the so-called Home Guard militias or the colonial military, i.e. the King's African Rifles.

favoritism at the expense of others (Anderson and Lochery, 2008, Wrong, 2010). While Kenya's experience seems to *prima facie* confirm the conjecture that rural uprisings gave rise to a culture of exclusion and continued violence (Garcia-Ponce and Wantchekon, 2018), our study suggests that the specific response of the colonial state to the rural uprising has exacerbated ethnic tensions in Kenya.

The remainder is organized as follows. Section 2 provides an overview of the historical context. Section 3 discusses the empirical specification, and Section 4 tests whether the British internment camps impact long-run social and economic outcomes. Section 5 examines robustness of the presented results. Section 7 concludes.

2 A brief history of the Emergency

Contemporary historians trace the origins of the Mau Mau uprising to historic grievances over land and increasing population pressures experienced by the Kikuyu on the native reserves (e.g. Odhiambo and Lonsdale, 2003, Anderson, 2005, Elkins, 2005). Kenya was one of the few settler colonies in Sub-Saharan Africa and the white settler minority claimed large parts of the fertile land (the so-called 'white highlands', an area in the central province of Kenya). Since the settler community only numbered a few thousand, most labor was carried out by Africans who were cohabiting on the farm. The remaining native population was assigned land in designated reserves. Increasing mechanization after World War I meant that African labor squatting on the farm became redundant, so that the native reserves were becoming increasingly crowded. Former district commissioner S.H. Fazan estimates that an average family of five in Fort Hall district had access to as little as 9 acres of land around the late 1930s (Fazan, 2014). Similar conditions prevailed in many other parts of Kenya's Central Province.

The colonial government did little to address this problem. Amid heightened grievances, the Kikuyu and related tribes started to form political groups demanding change and opposing (parts or all of) the colonial state. In 1920 the Kikuyu Central Association (KCA) was formed, was banned in 1940, and then reemerged as the Kenya African Union (KAU) in 1944. Both groups challenged the colonial law via petitions and constitutional redresses. The 1932 established Kenya Land Commission made several recommendations (summarized in Carter, 1934) which were not very far reaching and not adopted by the government.

Growing resentment led to more violent means adopted by a group of several thousand Kikuyu, who were released from the sharecropping contracts after World War II.[5] This group re-activated old Kikuyu war-time traditions to organize resistance, e.g. oathing. A key part of oathing was to swear allegiance to the tribe in several official ceremonies.

[5]Britain reserved fertile parts of the country for the white settler population, hence the name White Highlands. Kikuyu and other tribes were squatting on white farm and cultivating the land.

Oath takers were considered to be bound by a moral contract to assist the fight against the colonial power. Ignoring the promise of loyalty promise came with the treat of being killed, either by the supernatural elements or their ethnic kin (Luongo, 2006).

While a large share of the Kikuyu population took part in the oathing in the late 1940s and early 1950s, only a small fraction engaged in violent activities. The first openly violent act took place on October 9, 1952, when a small group of Mau Mau fighters presumably shot Senior Chief Waruhiu in the backseat of his car (Wamagatta, 2016). Numerous attacks followed, often aimed at loyalist Kikuyus, but sometimes involving white settlers. The violence of these attacks combined with the mysterious oathing ceremonies stoked widespread fear among the settler community, which pressured the colonial government to react forcefully to the violence. Evelyn Baring—the governor general of Kenya colony—announced a state of emergency immediately after the Waruhiu killing. Jomo Kenyatta, at that time heading the KAU, was arrested together with around 150 other suspected Mau Mau leaders. When these attempts failed to stem the violence, several counter-insurgency laws were announced by the government between January and April 1953. These new laws permitted unhindered information collection about the native population, gave control over any native property to the state, and allowed for detention without trial (Anderson, 2005). The British government instructed their police and military to systematically investigate anyone suspected of loyalty to the Mau Mau and sentenced these suspects to incarceration. Lacking actionable intelligence about the Mau Mau, the officials started to engage in a large scale interrogation process termed 'screening.'

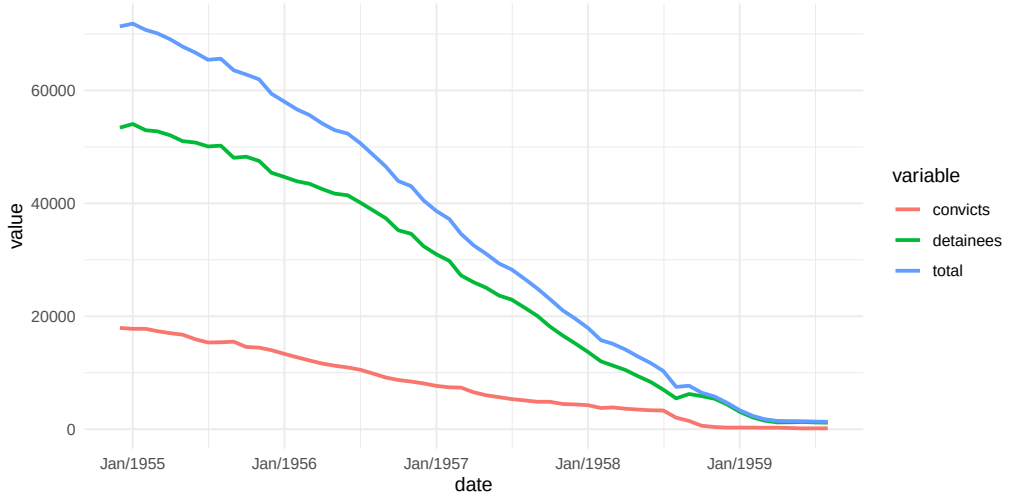
The main purpose of this screening was to identify those who were loyal to the Mau Mau, either by supporting them directly or by providing shelter and food (Elkins, 2001). British police and military relied heavily on loyal natives who helped to identify whether or not an individual could be attributed to the Kikuyu, Embu, or Meru tribe. Once a suspect was identified, the interrogators often resorted to torture and other brutal examination techniques to determine how loyal a suspect was and if the person was willing to squeal on other potential Mau Mau fighters. The rules restricting the British forces—the King's African Rifles and the Kikuyu Home Guard militia—in their interrogation techniques declined steadily over time. Anderson (2012), for example, discusses how violence was first considered a functional tool of interrogation, while after 1956 systematic torture became widespread. The resulting atrocities are still being litigated in British courts today.[6]

The internment camps were organized in a network called 'the Pipeline,' in which

[6] Anderson (2011) describes the allegations brought by Ndiku Mutwiwa Mutua and others. Suspected of giving Mau Mau fighters food, Mutua was dragged out of his hut one morning and violently beaten. After almost losing consciousness, he was driven to a prison where the beating continued. In the camp, Mutua was humiliated, beaten and castrated by European and African officers. Left in his cell to rot, he was accidentally rescued by one of the few Mau Mau attacks on a camp. Many of the other camp experiences were similar, often involving hard labor, beatings, torture, castration and rape (Elkins, 2001).

each inmate was to be assigned to a particular location (Elkins, 2001). Inmates were divided into white, grey or black according to the assessment following the interrogation. Those marked as "white" had confessed and were transferred to camps in their home district with the prospect of eventually being released after additional interrogations and education sessions. Those classified as "grey" were deported to a mid-level work camp for hard labor, re-education and counter-propaganda. Inmates in a grey camp were forced to work in stone pits or similar facilities, e.g. to build the foundation of what is now the Jomo Kenyatta International Airport in Embasaki. Inmates would only leave a grey camp once they were either considered redeemable or hard-core. The latter were designated "black" and deported into exile camps where they often remained until the end of the Emergency in 1959. The struggle was heavily imbalanced. The Mau Mau killed a total of 32 white settlers, fewer than the number of people killed in traffic accidents over the same period, while thousands of Kukuyu, EMbu and Meru were killed, detained, or both in return (Odhiambo and Lonsdale, 2003).

Figure 1 – Official estimates of the daily average detainee population



Notes: Based on Elkins (2001) who compiled these figures from Monthly Reports of the Ministry of Defence from January 1954 through September 1959.

The total number of casualties and scale of interment in the British camps is still subject to debate. Elkins (2005) offers an estimate of up to 300,000 Kikuyu, Embu and Meru who are unaccounted for during this period—much more than the 90,000 Mau Mau who were killed according to official numbers (Branch, 2007). Blacker (2007) instead suggests that there were at most 75,000 excess deaths during this period in total. Official sources suggest that about 70,000 people were held in the camps at the height of the Emergency in 1954 (see Elkins, 2001, and Figure 1). Many more will have spent at least a few months in the camps during their period of operation from 1952 to 1959. Some have been in over 14 different camps (Kariuki, 1964). We have no record of committals into detention camps under the Emergency Ordinances. However, the steady decline visible in

Figure 1 probably hides significant turnover. By comparison, the daily average population in Kenya's non-Emergency detention camps in 1954 was 3,591 but 25,970 people had been committed to such camps over the course of the year (Colony and Protectorate of Kenya, Prisons Department, 1954). While the average length of the Emergency sentences may have been longer, it seems save to assume that the entire affected population is several multiples of the daily average occupancy numbers. The entire Kikuyu, Embu and Meru population was only about 1.5 million people according to the 1948 census, about half of whom were children, so that a substantial proportion of the adult population faced a non-trivial probability of internment.

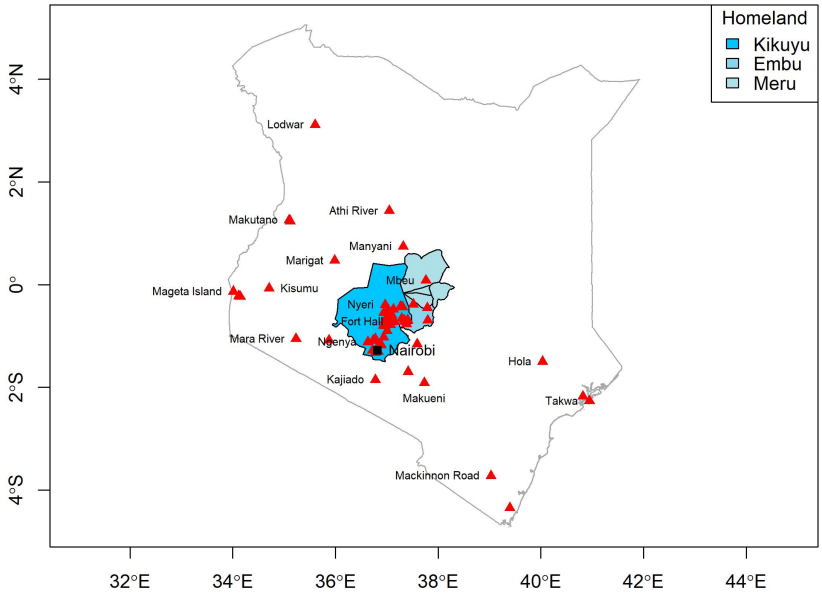
3 Data

Internment camps: We rely on three primary sources to identify the name, size, type and location of the Emergency detention camps. *i)* Annual reports from the Prisons Department and the Community Development Department of Kenya Colony and Protectorate, *ii)* fortnightly issues of the *Kenya Government Gazette*—the official government source for new legislation and official notices—and *iii)* parliamentary records from the United Kingdom (UK Hansard). We identify 58 Mau Mau detention camps and prisons in operation at some point between 1952 and 1959. This matches well with the number of camps reported at particular points in time, e.g. the Prisons Department refers to 49 special detention and works camp in 1955, after several camps have been closed. We then cross reference our findings with archival records and qualitative information about each camp compiled by Elkins (2001).

Most of these camps were named after the city or township where they were located. We define three precision codes during the manual geocoding process: exact location (accuracy below 1 km), city or township (accuracy below 5 km), and area or location (accuracy of 5–10 km). For 22 camps we are able to identify the exact location or building of the camp using newspaper articles and other information, for 34 sites we can identify the city or township, and for two sites we are only able to match the camp at a level corresponding approximately to a census location. The camps are plotted in Figure 2.

The camps cluster in Kenya's Central Province for obvious reasons. The historical homelands of the affected tribes of Kikuyu, Meru and Embu are located in this province. Camps were set up within or close to the home districts of the targeted population. Former inmates were released to their home location, under the auspices of loyalist chiefs and severe movement restrictions (enforced through a passbook system). Many camps are located close to Nairobi which was the site of some of the heaviest counter-insurgency crack downs by British and African forces. Away from the cluster, camps were established further away towards the East African coast or closer to the border region with Uganda and today's South Sudan. The selection of detainees into the different camps occurred

Figure 2 – Locations of detention camps in Kenya



Notes: Red triangles illustrate detention camp sites in Kenya. Homelands of the three Mau Mau related ethnic groups are added in shades of blue.

via the pipeline, so that inmates followed a progression from their capture until release. Large camps within Central Province, such as the Nairobi Dispersal Center or the Fort Hall Reception Center, served as holding camps in which prisoners were held for a limited amount of time for interrogations. Individuals who confessed were transferred into a camp or prison nearby, e.g. to Mbeu, Aguthi, or Kajiado. Political leaders and others who were deemed impossible to be redeemed and re-educated were deported into one of the farther away detention camps like Lokitaung, Lodwar, Mageta Island, Marsabit, Manyani or Mackinnon Road. According to former hard-core inmates (e.g. Kariuki, 1964), it was not uncommon to be transferred across a variety of camps, and repeatedly be moved up and down the pipeline, until one made it to the district work camp.

Dependent variables: Our main outcome variables—literacy, wealth, trust and civic engagement—are drawn from individual and household censuses and surveys. As a measure of basic education we create an indicator for literacy coded as one if an individual can easily read a whole sentence. For contemporary literacy and wealth, we use three survey rounds 4 (2003), 5 (2008/2009), and 7 (2014) from the Demographic and Health Surveys (DHS) to extract individual information of 62,584 individuals, including their geographical location.[7] In addition, we extract several control variables at the individual level (age, sex, years living at the current place) or at the household level (wealth,

[7]The DHS survey enumeration areas were geocoded on site, allowing us to locate a given individual or household within a range of less than 5 kilometers in Kenya (2 kilometers for urban households). Individuals are split among 399 clusters in the 2003 survey, 397 clusters in the 2008/09 survey, and 1585 clusters in the 2014 survey, where each cluster contains on average between five and ten households.

household size, urban vs. rural area, age and sex of the household head).

As a measure of contemporary household wealth, we rely on the wealth index provided by DHS. While the DHS surveys track income or expenditures directly, the surveys record several variables that can be linked to economic status. These are, among others, access to electricity, type of roof and floor, or whether the household owns a toilet, a TV, a bike, motorbike, or a car. Based on these indicators, the DHS computes a wealth index using principal component analysis and divides households into quintiles on this index (for details, see Rutstein et al., 2004). In other words, the DHS data allow us to distinguish households located in the poorest 20 percent in Kenya in a given survey year from those located in, say, the richest 20 percent. Cultural, geographic and other differences across countries can influence what kind of roof or floor can be attributed to wealthier as opposed to poorer households across different countries but these influences are less relevant in our context, as we are only comparing households *within* Kenya.

We supplement the contemporary data with historical census data from 1989. The 1989 census was one of the most comprehensive earlier censuses conducted in Kenya. It had wide geographic coverage, records literacy and housing conditions in a manner similar to the DHS surveys, and, perhaps most importantly, includes each individuals tribal affiliation.[8] Until the 2014 reform, Kenya was administratively divided into provinces, divisions, districts, locations and sublocations—the latter are comparable to census blocks in the United Sates and are only a few square kilometers in size in densely populated areas. Location and sub-location names are missing from the microdata provided by the Kenya National Bureau of Statistics (KNBS). We match the totals implied by the individual data with census reports and tabulations to recover the names of each geographic entity. The names can then be matched to a digitized map of census sub-locations provided by Kenya's International Livestock Research Institute (ILRI). Our final sample is a 5% sample of more than one million observations with the geographic centroid of each sublocation for 1989. Literacy is recorded in the individual census. For household wealth, we follow the DHS guidelines to construct an index that is strictly comparable to its wealth index quintiles (Rutstein et al., 2004). We base the wealth index only on housing condition indicators shared with the DHS surveys, i.e. type of roof, wall and floor, main source of drinking water, type of sewage disposal, cooking fuel, and type of lighting).

Contemporary outcomes related to social cohesion and civic engagement are from the Afrobarometer survey. All six rounds of the survey, conducted between 1999 and 2015, have been geocoded (BenYishay et al., 2017) and Kenya was part of rounds 2–6 which took place during the years 2003, 2005, 2008, 2011, and 2014. With 1,104 respondents in the smallest round, and 2,398 respondents in the largest one, this amounts to a total of 9,576 observations. Contrary to the DHS data, the geocoding of households in the

[8]While tribal affiliation is still surveyed by the enumerators, the KNBS stopped releasing this information at the individual level in the decennial censuses after 1989.

Afrobarometer was done ex post. This leads to considerable variation in geographic precision. The data contains a categorical precision code that assesses the quality of the provided coordinates, where 1 indicates that the coordinate pair corresponds to an exact location and 6 indicates that a location can only be attributed to an independent political entity. The exact location of a respondent is crucial for our identification strategy, which is why we restrict the sample to the two highest accuracy levels (1 = exact place and 2 = "near" or adjacent). The final sample includes 4,809 respondents.

We focus on two trust variables—trust in most people and trust in neighbors—to investigate how the British labor camps affected trust levels of citizens related to the Mau Mau uprising, compared to others.[9] The questions for trust are posed in the form: "How much do you trust each of the following..." The response options are categorical and are coded as integer values between 0 and 3, where 0 indicates "not at all," 1 indicates "just a little," 2 indicates "somewhat" and 3 indicates "a lot."[10] The exception is trust in most and in other people, which allows for binary responses only.

Geographic controls: Geographic factors directly and indirectly impacts historical outcomes, which affects economic development until today (e.g. Nunn and Puga, 2012, Sokoloff and Engerman, 2000). In the Kenyan case, the roots of the Mau Mau conflict can be traced to the alienation of some of the country's most attractive lands by the settlers. The area in the high highlands with its mild climatic conditions was particularly attractive to Europeans, much more so than the hot, humid and disease ridden areas near Lake Victoria or the coast around Mombasa. Nairobi lies on a plateau (the low and high highlands) with elevation of almost 1,800 meters, precipitation is regular, the temperature is moderate, and the disease vector is favorable (Whittlesey, 1953).

We account for this exceptional geography with a variety of controls derived from raster and vector data: elevation (Jarvis et al., 2008), slope (Jarvis et al., 2008), ruggedness (Nunn and Puga, 2012), wheat suitability (FAO/IIASA, 2011), the length of river and road networks (Natural Earth, 2017), prevalence of the tsetse fly (FAO/AGAH, 2007) and malaria suitability (World Health Organization, 2018), precipitation (Willmott and Matsuura, 2001), and temperature (Willmott and Matsuura, 2001). To extract the relevant information, we partition Kenya into grid cells at a $0.1° \times 0.1°$ resolution (approx. 11km $\times$ 11km). We then spatially join these grid cells with the geolocated survey and census data described earlier. In addition we control for great circle distances to Nairobi, the forests in which the Mau Mau fighters were hiding (the Aberdare Range and Mount Kenya) and the province capitals.

[9] We replicate results with two closely related trust variables (trust in other people, trust in relatives) and find similar results.

[10] We follow Nunn and Wantchekon (2011) and stick with the categorical values rather than re-coding all variables into binary indicators. Nunn and Wantchekon (2011) use the data to test the long term effects of the African slave trades on today's levels of trust in African society.

4 Empirical strategy

Our approach to approximating the (infeasible) experimental ideal of random assignment to camps is a triple differences-in-differences (DDD) startegy along three dimensions. First, we construct an indicator for either Kikuyu, Embu, or Meru to identify individuals who were likely to have been accused of Mau Mau activities at the time. Second, an incarcerated individual is likely to have lived close to but not necessarily in the immediate vicinity of a camp. We define our baseline measure of proximity as being within 30 km of the nearest former camp (similar to Isaksson and Kotsadam, 2018, and Abel, 2016). Third, we define an indicator for whether an individual was already alive during the time. This allows us to compare those who were born before 1959—the last year of the Emergency—to younger cohorts that were neither alive nor born in a camp. Similar DDD strategies have recently been used by Muralidharan and Prakash (2017), in their study of cohorts of Indian girls exposed to a cycling program, and Nilsson (2017), who studies the effects of increased alcohol availability for mothers on the long-term labour market outcomes of their children. Our context differs somewhat, in that we do not observe the actual treatment status of each individual but instead recover an intention-to-treat estimate using our exposure proxy.

All of our regressions are variants of the following specification

$$\begin{aligned} y_{il} = \; & \beta_1 M_{il} + \beta_2 P_{il} + \beta_3 C_{il} + \gamma_1 (M_{il} \times P_{il}) + \gamma_2 (M_{il} \times C_{il}) + \gamma_3 (P_{il} \times C_{il}) + \\ & \delta (M_{il} \times P_{il} \times C_{il}) + \mathbf{x}_{il}' \boldsymbol{\epsilon} + \mathbf{d}_l' \boldsymbol{\zeta} + \mathbf{z}_l' \boldsymbol{\eta} + \mathbf{FE}_{il} + u_{il} \end{aligned} \tag{1}$$

where y_{il} is an outcome for individual or household i in location l. M_{il} indicates whether the respondent identifies as a Mau Mau tribe (either Kikuyu, Embu, or Meru), P_{il} is a dummy variable equal to one if the individual is close to a former camp location (i.e. within 30km) and C_{il} is an indicator for individuals born before 1959. $\mathbf{x}_{il}$ is a vector of individual level controls (age, sex, household size), $\mathbf{d}_l$ is a vector of distances to economic or political centers and areas of shelter of the Mau Mau fighters.[11] $\mathbf{z}_l$ are geographic characteristics of the location or enumeration area (urban, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature). $\mathbf{FE}_{il}$ are different sets of fixed effects varying across specifications.

We present our main results by incrementally adding higher dimensions of fixed effects.

[11] The distances to Nairobi, Mount Kenya, and the Aberdare Range account for Mau Mau hot spots. Nairobi is located in the Kikuyu homeland and was the site of Operation Anvil. Mount Kenya and the Aberdare Range are the two forest areas where Mau Mau fighters were based and organized their attacks from. This was well known to British officials, who tried to deprive these areas of food supplies and carried out intense raids near the forest boundaries (Anderson, 2012). Finally, we also include the distance to each province capital, as these urban centers may host a camp site but differ on many other characteristics which could be correlated with our outcomes of interest.

Fixed effects are omitted in the most basic regressions. We then add age, tribe and province fixed effects to arrive at our preferred specification since it can be consistently estimated for all outcomes. For the strictest set of results, we include camp and distance interval fixed effects (running from 0–30, 30–60, 60–90, 90–120, and 120–150 km) and—if the variation in the data permits—interactions between the tribe, distance interval, and age fixed effects. This implies that we can only estimate all constituent terms of the triple interaction in the model without fixed effects. Age, tribe and province fixed effects remove C_{il} and M_{il}. Distance interval effects remove P_{il} and the high-dimensional interactions of the fixed effects absorb all constituent terms but the DDD interaction. Including these fixed effects generalizes the DDD estimator but does not fundamentally alter the interpretation. We only progressively account for systematic differences among non-Mau Mau tribes, age groups/ cohorts, province, distance-by-cohort specific factors, distance-by-tribe specific factors, differences in the age composition of ethnic groups, and make sure we do not compare respondents across different camp sites.

Our DDD parameter of interest, δ, captures the effect of being exposed to a detention camp or major prison site and can be decomposed as follows

$$\begin{aligned}\delta = \; & \mathbb{E}[Y|M_1, P_1, C_1] - \mathbb{E}[Y|M_1, P_0, C_1] - \{\mathbb{E}[Y|M_0, P_1, C_1] - \mathbb{E}[Y|M_0, P_0, C_1]\} - \\ & (\mathbb{E}[Y|M_1, P_1, C_0] - \mathbb{E}[Y|M_1, P_0, C_0] - \{\mathbb{E}[Y|M_0, P_1, C_0] - \mathbb{E}[Y|M_0, P_0, C_0]\})\end{aligned} \tag{2}$$

where $\mathbb{E}[Y|M_1, P_1, C_1]$ is a shorthand for the conditional expectation $\mathbb{E}[Y|M = 1, P = 1, C = 1, \mathbf{x}_{il}, \mathbf{d}_l, \mathbf{z}_l, \mathbf{FE}_{il}]$ and so on. The DDD is essentially the difference of two differences-in-differences (DDs). In the absence of covariates and other fixed effects, it coincides exactly with the differences in the means of eight different groups. With covariates and fixed effects, it becomes a generalized DDD. Specifically, the first DD eliminates region-specific confounders common to all ethnic groups of the cohort of interest, while the second DD eliminates region-specific differences for those that were born later. The resulting DDD then reflects the impact of camp exposure on individuals of Mau Mau related ethnicity that were already born at the time of the Emergency and live near former camp locations. The second difference over the non-affected cohort is essentially a placebo DD. If there are no spillovers, after accounting for observables and unobserved fixed factors, we would expect the treatment effect to be zero in the placebo DD and could proceed with a simple DD estimation. However, the DDD estimate allows us to weaken identification assumptions somewhat further. Instead of requiring the spatial equivalent of parallel trends, that is, spatial shocks have the same effect on Mau Mau and non-Mau Mau tribes, we can get away with varying selection into camps sites for different tribes, as long as it is independent of age.[12]

[12]Note that we could equivalently rearrange the DDD decomposition and interpret the cohort indicator as time (before and after the intervention). While this would bring the interpretation more into line with traditional DDD strategies and would lead us to talk about parallel trends, we do not think this avenue

Our baseline results limit the sample to households within 150 km distance to former camp sites. We use two types of standard errors throughout all tables. Errors clustered on the latitude-longitude pair identifying each location allows respondents in the same enumeration area or survey cluster to be arbitrarily correlated. Conley errors with a distance cutoff of 150 km allow for wide-ranging spatial correlation in the responses (Conley, 1999). Both account for the spatial clustering of households or individuals in the same enumeration area, but the latter also allows for spatial auto-correlation among different enumeration areas.

5 Results

For our main results we focus on household wealth, individual literacy, trust, and civic engagement. All tables follow the same structure. For illustration, columns (1) and (2) present results from the two separate DDs which form the basis of the DDD estimate. From column (3) onward we focus on the DDD parameter of interest after accounting for observable differences across individuals and geographic locations.[13] Column (4) adds fixed effects for age, tribe and provinces, while column (5) adds camp and distance interval effects and, for the census and DHS data, also includes higher order interactions of age, tribe and distance interval fixed effects. Note that the interaction fixed effects use up many degrees of freedom which is why we only include them in sufficiently large samples.

Wealth: Differences in wealth accumulation between those exposed to the camps and those who were not are of interest for at least three reasons. First, some will have spent up to six and a half years in custody if we take the total duration of the Emergency as the maximum upper bound. Hence, internment translates into a loss of valuable time which they could have spent in gaining experience, earning income, and obtaining more education (see Chin, 2005, who finds a large effect of Japanese interment on labor market earnings 25 years later). Second, anecdotal evidence suggests that detainees were often expropriated, in effect losing the assets they acquired up to the point of incarceration or being forced to divest (see Kariuki, 1964, who was forced to sell his business on internment). Third and most importantly, systematic abuse during the interrogations and widespread offenses by prisons guards are likely to have significantly affected the physical and mental health of detainees. Such negative effects on well-being will have

is attractive in our setting. C_{il} only vaguely resembles time, since we compare everyone born before 1959 to everyone born after. Similar applications of DDD in cross-sectional data (e.g. Muralidharan and Prakash, 2017) have used cohorts in lieu of time but then restricted themselves to adjacent cohorts, e.g. girls (boys) aged 14 and 15 versus girls (boys) aged 16 and 17 in two different states. In any case, the assumption of parallel trends and selection into camp sites that this the same for Mau Mau tribes and others are interchangeable.

[13] To reduce clutter, we do not report the DDD without controls and fixed effect. It can be easily calculated by subtracting the estimates provided in columns (1) and (2).

contributed to further income losses and closed off entire career paths.

Table 1 – DHS and Census: Wealth

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel A) DHS 2003-2014				
Exposure (P × M)	−0.413	−0.411	−0.059	0.002	
	(0.108)***	(0.092)***	(0.067)	(0.065)	
	[0.254]	[0.260]	[0.118]	[0.108]	
Exposure (P × M × C)			−0.196	−0.204	−0.188
			(0.085)**	(0.083)**	(0.086)**
			[0.090]**	[0.076]***	[0.073]**
	Panel B) 5% Sample of 1989 Census				
Exposure (P × M)	−0.346	−0.419	−0.016	0.049	
	(0.088)***	(0.077)***	(0.058)	(0.044)	
	[0.191]*	[0.178]**	[0.080]	[0.059]	
Exposure (P × M × C)			−0.111	−0.117	−0.076
			(0.044)**	(0.039)***	(0.035)**
			[0.049]**	[0.050]**	[0.056]
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (A)	6,631	30,811	37,442	37,442	37,442
Observations (B)	152,081	53,737	205,818	205,818	205,818

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a households head) controls for age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively.

Table 1 analyzes the impact of detention on household wealth. For the DHS surveys, we find that households headed by individuals who were alive during the Emergency and exposed to the camps are ranked about 1/5th lower in the quintiles of the wealth distribution today (columns (3) to (5)). This suggests a strong and lasting income effect which is still visible more than 50 years after detention. The results from the 1989 census confirm this general picture three decades after the end of the Emergency. The effect size falls to about 1/10 of a wealth quintile, which may be due to a variety of reasons. For instance, the census data could be more accurate or there is a negative survivor bias, in the sense that households still headed by camp survivors in the 2000s have been less able to adapt to the post-independence economy. DHS respondents were substantially

younger at Emergency than census respondents in 1989, which might indicate a stronger effect on younger children. In any case, the census estimates are usually within a standard error of the DHS results. Note that the Kikuyu and related tribes are on average better off than others which runs against this main finding.

Columns (1) and (2) reveal how biased simple DD estimates can be. We observe either approximately the same effect in both groups in the DHS data or even less of an effect on the relevant cohort in the census data. This effect on the non-Emergency cohort disappears immediately when we add individual and geographic controls in column (3) and remains close to zero in column (4). The estimate on the $P \times M$ interaction still measures the effect on the placebo cohort, while the coefficient on the $P \times M \times C$ interaction is the effect on the Emergency cohorts. In other words, the estimates in column (2) can be entirely explained by observable differences across households and locations, while the estimates in column (1) shrink but remain sizable in the DDD setting.

Literacy: The ability to read and write, our proxy for basic education, is usually acquired in early years during primary school. We focus on early education, as already literate individuals may have had more of chance to accumulate more human capital later on. Much of the anticipated negative effects will run through a lack of parental investments while they were interned, rather than rare instances of children being interned or being born inside detention camps (although both did occur). The microeconomic literature stresses the importance of "critical early windows" where shocks can have lifelong effects on cognitive skills (Cunha and Heckman, 2007). In line with this, we consider learning the ability to read and write a crucial indicator of early parental investments which likely has been negatively affected by the detention of the parents.

Table 2 shows the corresponding results. The simple DDs and the implied DDD estimates in columns (1) and (2) of both panels already suggest a strong negative effect on the exposed members of the Mau Mau cohort. As before, we observe that the effect on the post-1959 cohort vanishes once we add controls and our battery of fixed effects. Columns (3) to (5) contain the main results. For the DHS surveys, we find that individuals exposed to detention camps are 10 to 20 percentage points less likely to read and write. This effect is sizable and up to a fourth of the raw probability to be literate in our sample. Our estimates based on the 1989 census data point in the same direction but the effect size is considerably smaller—down to about 3.5 percentage points. The mean level of literacy of the census sample is somewhat lower with about 72% of the population indicating that they are literate. Clearly this only explains a small part of the drop in the DDD estimate. Most of the discrepancy is likely owed to differences in the age composition. Our exposure variable only affects people who were children during the Mau Mau revolt in the DHS surveys but are now in their 40s and 50s, while the relevant cohort in the 1989 census includes all adults older than 30, who will have often been literate before

Table 2 – DHS and Census: Literacy

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel A) DHS 2003-2014				
Exposure (P × M)	−0.192	−0.063	−0.024	0.005	
	(0.090)**	(0.017)***	(0.017)	(0.015)	
	[0.073]***	[0.036]*	[0.033]	[0.024]	
Exposure (P × M × C)			−0.119	−0.159	−0.201
			(0.087)	(0.079)**	(0.078)**
			[0.067]*	[0.066]**	[0.078]**
	Panel B) 5% Sample of 1989 Census				
Exposure (P × M)	−0.098	−0.061	−0.051	0.004	
	(0.026)***	(0.017)***	(0.014)***	(0.012)	
	[0.052]*	[0.040]	[0.030]*	[0.018]	
Exposure (P × M × C)			−0.035	−0.034	−0.034
			(0.011)***	(0.011)***	(0.011)***
			[0.013]***	[0.014]**	[0.017]**
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					✓
Observations (A)	1,001	59,135	60,136	60,136	60,136
Observations (B)	97,519	676,321	772,439	772,439	772,439

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual (or a households head) controls for age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects and three interaction fixed effects between tribe, distance to the closest camp, and age. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

the Emergency. A larger effect on those aged less than ten during the Emergency is also compatible with the earlier finding of lower household wealth in 1989. In as far as wealth proxies for the permanent income of parents, a lack of parental resources is a key impediment to investments in children (Carneiro and Heckman, 2003).

The estimate is likely to be underestimated in absolute value for another reason. Political leaders and educated rebels were often giving classes to non-literate inmates, especially in less violent camps and before torture became systematic (see e.g. Kariuki, 1964). Hence it seems safe to conclude that 3.5 percentage points is a lower bound of the effect of detention on literacy and that this effect is persistent until today.

Trust: We now turn to a test of the long-run effects of detention on social cohesion. The corresponding estimates are reported in Table 3. The first panel focuses on generalized trust. Recall that the variable is coded as binary indicator where one indicates that "Most people can be trusted" and zero corresponds to "You must be very careful." The second panel focuses on trust in neighbors which is based on four different categories ranging from "Not at all" to "A lot." In both instances, we ignore the binary or categorical nature of the dependent variable (as in Nunn and Wantchekon, 2011) and interpret the regressions as linear probability models in the case of the former, or like a continuous variable in the case of the latter.

Table 3 – Afrobarometer: Trust Most People and Neighbours

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel A) Trust in Most People				
Exposure (P × M)	−0.981	−0.046	−0.040	−0.001	0.041
	(0.062)***	(0.042)	(0.051)	(0.055)	(0.060)
	[0.054]***	[0.033]	[0.037]	[0.034]	[0.029]
Exposure (P × M × C)			−0.953	−0.802	−0.928
			(0.092)***	(0.101)***	(0.083)***
			[0.063]***	[0.077]***	[0.082]***
	Panel B) Trust in Neighbours				
Exposure (P × M)	−0.992	0.291	0.045	−0.077	−0.207
	(0.204)***	(0.163)*	(0.190)	(0.204)	(0.250)
	[0.150]***	[0.131]**	[0.136]	[0.116]	[0.113]*
Exposure (P × M × C)			−1.515	−2.240	−2.349
			(0.346)***	(0.353)***	(0.361)***
			[0.251]***	[0.205]***	[0.206]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (A)	220	1,291	1,362	1,362	1,362
Observations (B)	220	1,276	1,349	1,349	1,349

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

The raw DDs already summarize our main results. Column (1) shows that those who were exposed to detention are 98% less likely to trust others and score an entire category lower on the trust in neighbors index. The placebo DDs in column (2) cannot confirm

similar effects for the non-Mau Mau cohorts or even suggest the opposite. The effect remains large, no matter if we control for observable characteristics at the individual or location level, add age, tribe and province fixed effects, or include camp fixed effects together with distance interval effects in columns (3) to (5). In all instances, the probability of a respondent indicating generalized trust falls by 80% to 95%. Considering that Kenya is not a high trust society—only 9% of respondents indicate that others can be trusted—this effect size is remarkable. The results for trust in neighbors mirror this finding and increase in the strictness of each specification. Our estimates suggests that respondents who were exposed to a camp are substantially less likely to trust their neighbors—a decrease of almost 2.5 units (the average is 3.6, implying they tend to report "Not at all"). This implies that respondents whom we consider to have been most affected by the British repression are *both* less likely to have higher levels of generalized and in-group trust. As ethnic groups cluster in space, neighbors are likely to be of the same ethnicity, so that this variable has a strong in-group component.[14]

These results are unconventional, as much of the extant literature suggests that the experience of traumatic events increases *both* generalized and in-group trust (see, e.g., Abel, 2016, Bauer et al., 2016, Lowes and Montero, 2017). Instead, our results are more in line with Nunn and Wantchekon (2011) who focus on the African slave trade which was detrimental to overall and in-group trust. In the case of the Mau Mau uprising, the historical evidence sheds some light on potential explanations for this result. Breaking the cohesion of the Kikuyu, Embu and Meru was explicit British policy. Hooded loyalists informed on suspects during interrogation, "redeemed" Mau Mau rebels stayed in the camps to "turn" others, while the Kikuyu Home Guard and other tribes served as guards. Our findings imply that this policy was very successful and outweighed the countervailing effects of more cohesion in the camps (documented in Kariuki, 1964 and Elkins, 2005, among others). The effect of the camps also swamps the historical legacy of the transatlantic slave trade. Using Nunn and Wantchekon's (2011) preferred specification suggests that a doubling of slave exports per area decreases trust in neighbors by 0.271 units but relatively few Kikuyu were "exported" in the slave trades, so that this effect is only a fraction the impact of detention.

Civic engagement: To further delve into the question of how repression and the exposure to detention camps may have affected social cohesion, we now turn to outcomes of civic engagement. To this end we explore the effects on the participation in voluntary associations and participation in demonstrations.[15] Bauer et al. (2016) document a

[14]We repeat this exercise using trust in relatives as an alternative proxy for in-group trust and find comparable results (see Table A-1).

[15]The questions are "Have you, personally, have done any of these things during the past year. If not, would you do this if you had the chance: Attended a demonstration or protest march?" with answers based on five categories from "No" to "Yes, often" and "Are you an official leader, an active member,

general trend towards more pro-social behavior in post-conflict communities. We could still observe positive effects on these variables if the British policy of fostering distrust within and between groups in order to strengthen their allegiance to the colonial state had no spillover effects on these other components of pro-sociality.

Table 4 – Afrobarometer: Civic Engagement

	DD_1 $(C=1)$ (1)	DD_0 $(C=0)$ (2)	DDD (3)	DDD (4)	DDD (5)
	Panel A) Active Participation in Voluntary Associations				
Exposure (P × M)	0.462	0.144	0.119	0.263	0.301
	(0.328)	(0.099)	(0.139)	(0.159)*	(0.147)**
	[0.245]*	[0.092]	[0.145]	[0.142]*	[0.108]***
Exposure (P × M × C)			0.293	0.406	0.450
			(0.354)	(0.359)	(0.367)
			[0.278]	[0.253]	[0.245]*
	Panel B) Active Participation in Demonstrations				
Exposure (P × M)	−0.376	−0.053	−0.121	−0.014	0.076
	(0.408)	(0.084)	(0.101)	(0.120)	(0.118)
	[0.417]	[0.057]	[0.079]	[0.097]	[0.131]
Exposure (P × M × C)			−0.313	−0.038	−0.101
			(0.401)	(0.462)	(0.460)
			[0.404]	[0.444]	[0.437]
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Observations (A)	306	2,378	2,422	2,422	2,422
Observations (B)	414	2,812	2,919	2,919	2,919

Notes: The table reports weighted OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Our results weakly confirm this conjecture at best. The simple DDs and the DDDs consistently show a positive effect of camp exposure on participation in voluntary associations. However, the estimated coefficients are only marginally significant at the 10% level in some specifications. There is a tentative indication that these effects only manifest themselves in later generations, as we observe positive and significant differences

an inactive member, or not a member in some other voluntary association or community group?" with options to answer from "Not a member" to "Active member."

in participation for the post-1959 cohort in columns (4) and (5). Such a pattern would be in line with the historical narrative which suggests that the Mau Mau movement mobilized the Kikuyu people long after the Emergency, as they came to dominate post-independence politics. Kenya's first democratically elected president—Jomo Kenyatta—was a detainee himself and held in an exile camp for most of the Emergency. When we turn to participation in demonstrations, we cannot reject the null of no differences among the exposed and non-exposed, no matter the cohort. Taken together, these results imply that it is plausible but far from definitive that the trust-breaking intervention spared other dimensions of social cohesion.

6 Extensions and Robustness

Our results document lasting negative effects of camp exposure on individual and household outcomes today. We now explore the role of some of the characteristics that define our proxy for camp exposure. We base these extensions on our preferred specification in column (4) of Table 2. The corresponding tables are relegated to the Appendix. We only report errors clustered at the location level for the extensions.

Cohort: We first vary the definition of the affected cohort to trace the transmission of our effects over time. For the DHS survey, we alter the cohort definition to run from being born before 1959 in five-year intervals until 1979. Individuals born after the Emergency period were not directly affected by detention but could still be exposed to camps indirectly via other household members. In the absence of strong inter-generational transmission, we expect younger Kikuyu, Emu and Meru cohorts near former camp sites to have experienced smaller declines in literacy or none at all. The 1989 census provides us with the additional advantage to test how cohorts born prior to the Emergency were affected. We alter the cohort definition in five-year intervals in the other direction until 1939. We expect the negative shock in literacy to be most pronounced in those cohorts that were born between 1959 and 1949, that is, children up to age ten during the Emergency period.

The results are in line with our expectations. Table A-2 indicates that only the cohort directly linked to the detention period in the DHS surveys (individuals born before 1959) is negatively affected in terms of literacy. We do not find evidence for those born after the Emergency to have differential abilities to read and write today in relation to their proximity to camps and tribal affiliation. The census data adds that the effect indeed is strongest for those that were in a "critical early window" for literacy when the state of emergency was declared. The effect shows some signs of fading out for individuals born before 1944—those who were older than age ten at the height of the Emergency in 1954—both in terms of effect size and significance.

Proximity: Next we alter the proximity definition to test the effect on those living within 10 to 90 km of a camp site. We exclusively use the Census data for these perturbations, since the sample is large enough and contains a density of sublocations high enough to examine small variations in distance to a camp. In addition to the proximity adjustment, we also vary the cutoff until which the control group is included. This has the effect of comparing individuals which are increasingly close by and hence unlikely to vary on unobserved factors other than exposure to a camp site.

The results in Table A-3 show that it is really the close vicinity to a camp which drives our results. The first column limits the proximity indicator to those that live within 10 km and then varies the maximum spatial extent of the control group to 60 km, 100 km and 150 km (Panels A, B, and C, respectively). The DDD estimate is between -4.6 and -5.1 percentage points and highly significant at conventional levels in all variants. The other columns expand the proximity definition. We observe that the effect persists for those living up to 50 km near a former camp site but shrinks substantially in size and significance. The treatment effect vanishes from 70 km onward.

Camp types: Finally, we test the sensitivity of our results with respect to the type of camp that an individual was exposed to. Elkins (2001) provides us with a classification of camps which we supplement with estimates of their capacity. We distinguish between special, holding, exile, large, and work camps. Large camps have a capacity of above 5,000 detainees at any point in time. While there is some reason to believe that there may be some heterogeneity in the treatment effects across camp types, it is hard to establish a clear hierarchy. Some exile camps for hard-core detainees involved comparatively little hard work and abuse, while some work camps were particularly notorious. A change in camp command could alter the experience of inmates completely (Kariuki, 1964).

Table A-4 shows that our effect is driven by work camps which make up 50% of the camps in our sample (33 out of 56). We first exclude various type of camps that could be particularly notorious in nature (special camps were for hard-code Mau Mau supporters, holding camps were set up very hastily after mass arrests, and large camps were difficult to administer). Note that we drop all those who are close to any of the omitted camp types to not pollute the control group. Our main results hardly change when we exclude special and exile camps, although the size of the DDD estimate slightly increases. The effects decrease and standard errors increase moderately when large and holding camps are excluded from the sample. Finally, when restricting our sample to only work camps. We again find effects comparable to our base specification.

7 Concluding remarks

This study explores the long-run effect of the systematic repression of a nationalist uprising during Kenya's late colonial period in the 1950s. We show that the systematic detention of Kikuyu, Embu and Meru people affects their social and economic development until today. Our differences-in-differences estimates document that individuals who suffered from direct exposure of these measures are today poorer, less literate and have lower levels of general and in-group trust.

Affected individuals and households are 20 percentage points less likely to read and write and score up to 0.2 wealth categories lower than unaffected respondents. In addition, those who have been directly exposed to detention camps are 90% less likely to trust most people and score substantially lower on indicators of in-group trust. The results hold for an array of sensitivity and placebo tests.

More broadly, our results emphasize how Kenya's pathway to independence and its decolonization experience shaped the country's subsequent development. The Mau Mau uprising is known as the quintessential anti-colonial rural revolt on the African continent (Garcia-Ponce and Wantchekon, 2018). Our study suggests that the pervasive culture of distrust and ethnic politics in Kenya is partially rooted in the response of the colonial state to this rural uprising, rather than its rural and violent nature as such.

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A Appendix

A.1 Descriptive statistics

[Descriptive statistics table here. To be updated.]

A.2 Alternative trust variables

Table A-1 – Trust Other People and Relatives

	DD_1 $(C=1)$	DD_0 $(C=0)$	DDD	DDD	DDD
	(1)	(2)	(3)	(4)	(5)
	Panel A) Afrobarometer–Trust in Other People				
Exposure (P × M)	−1.090	0.060	−0.132	−0.033	−0.439
	(0.326)***	(0.151)	(0.203)	(0.218)	(0.188)**
	[0.270]***	[0.111]	[0.174]	[0.179]	[0.115]***
Exposure (P × M × C)			−1.105	−1.498	−1.579
			(0.337)***	(0.425)***	(0.422)***
			[0.276]***	[0.354]***	[0.356]***
	Panel B) Afrobarometer–Trust in Relatives				
Exposure (P × M)	−0.550	0.083	0.005	0.034	−0.247
	(0.185)***	(0.125)	(0.177)	(0.233)	(0.238)
	[0.129]***	[0.131]	[0.155]	[0.152]	[0.148]*
Exposure (P × M × C)			−0.541	−0.988	−1.151
			(0.220)**	(0.392)**	(0.437)***
			[0.154]***	[0.298]***	[0.359]***
Individual controls			✓	✓	✓
Geographic controls			✓	✓	✓
Age, Tribe & Province FEs				✓	✓
Camp & Distance Interval FEs					✓
Interactions of FEs					×
Observations (A)	156	1,139	1,161	1,161	1,161
Observations (B)	270	1,611	1,700	1,700	1,700

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Column (3) includes individual controls for age and sex as well as geographic controls which include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. Column (4) adds age, tribe, and province fixed effects, while column (5) in addition includes fixed camp and distance interval effects. We also include fixed effects for the survey year. Conley and clustered standard errors are reported below the coefficients in brackets and parentheses respectively. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

A.3 Extensions

Table A-2 – DHS and Census: Literacy—Altering the Definition of Cohorts

	Born before:				
	1959	1964	1969	1974	1979
	(1)	(2)	(3)	(4)	(5)
	Panel A) DHS 2003-2014				
Exposure (P × M)	0.005	0.006	0.006	0.004	0.013
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
Exposure (P × M × C)	−0.159	0.036	−0.027	0.008	−0.030
	(0.079)**	(0.056)	(0.041)	(0.036)	(0.034)
	Panel B) 5% Sample of 1989 Census				
	Born before:				
	1959	1954	1949	1944	1939
Exposure (P × M)	0.004	0.008	0.006	0.004	0.002
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Exposure (P × M × C)	-0.034	-0.036	-0.035	-0.028	-0.027
	(0.011)***	(0.013)***	(0.015)**	(0.015)*	(0.016)*
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (A)	60,136	58,325	55,894	51,312	44,958
Observations (B)	772,460	718,477	675,701	641,607	614,874

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a households head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A-3 – Census: Literacy—Altering the Definition of Proximity

	Proximity within:				
	10 km	30 km	50 km	70 km	90 km
	(1)	(2)	(3)	(4)	(5)
	Panel A) Cutoff = 60 km				
Exposure (P × M)	-0.006	0.017	0.017		
	(0.009)	(0.014)	(0.024)		
Exposure (P × M × C)	-0.046	-0.023	-0.018		
	(0.012)***	(0.016)	(0.025)		
	Panel B) Cutoff = 100 km				
Exposure (P × M)	-0.007	0.012	0.010	0.009	-0.066
	(0.008)	(0.011)	(0.012)	(0.008)	(0.064)
Exposure (P × M × C)	-0.051	-0.035	-0.029	-0.006	-0.077
	(0.011)***	(0.011)***	(0.011)**	(0.012)	(0.134)
	Panel C) Cutoff = 150 km				
Exposure (P × M)	-0.011	0.004	0.001	-0.001	-0.112
	(0.008)	(0.012)	(0.013)	(0.010)	(0.022)***
Exposure (P × M × C)	-0.051	-0.034	-0.029	-0.008	-0.019
	(0.011)***	(0.011)***	(0.011)**	(0.012)	(0.042)
Individual controls	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓
Observations (A)	579,058	579,058	579,058		
Observations (B)	757,986	757,986	757,986	757,986	757,986
Observations (C)	772,460	772,460	772,460	772,460	772,460

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a households head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A-4 – DHS and Census: Literacy—Exploring Camp Types

	Camp Types					
	All	No Special	No Holding	No Exile	No Large	Only Work
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A) DHS 2003-2014					
Exposure (P × M)	0.005	0.009	0.004	0.009	0.004	0.020
	(0.015)	(0.015)	(0.020)	(0.016)	(0.020)	(0.016)
Exposure (P × M × C)	-0.159	-0.161	-0.143	-0.188	-0.143	-0.178
	(0.079)**	(0.080)**	(0.087)	(0.082)**	(0.087)	(0.082)**
	Panel A) 5% Sample of 1989 Census					
Exposure (P × M)	0.004	0.002	0.011	0.017	0.011	0.014
	(0.012)	(0.012)	(0.014)	(0.012)	(0.014)	(0.012)
Exposure (P × M × C)	-0.034	-0.041	-0.030	-0.051	-0.030	-0.039
	(0.011)***	(0.011)***	(0.012)**	(0.012)***	(0.012)**	(0.012)***
Individual controls	✓	✓	✓	✓	✓	✓
Geographic controls	✓	✓	✓	✓	✓	✓
Age, Tribe & Province FEs	✓	✓	✓	✓	✓	✓
Observations (A)	60,136	57,414	53,824	60,136	53,824	55,123
Observations (B)	772,460	747,777	697,566	698,726	697,566	710,678

Notes: The table reports OLS estimates. The unit of analysis is the individual. All regressions control for the base levels of the interaction terms. Individual (or a households head) controls are age and sex as well as the household size. The geographic controls include an indicator for rural or urban regions, elevation, slope, ruggedness, wheat suitability, length of river and road networks, prevalence of the tsetse fly and malaria, precipitation, and temperature. In addition we control for the distances to Nairobi, the Aberdare Range, Mount Kenya and the province capital. We add age, tribe, and province fixed effects. Clustered standard errors are reported below the coefficients in parentheses. Significant at: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.