

Neighbours and Enemies: Inter-Ethnic Exposure and Mobilisation in Mandate Palestine*

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Abstract

How does local exposure between two ethnic groups in conflict affect contentious activity at the local level? Existing work on intergroup contact has focused on attitudinal outcomes, while the literature on ethnic violence has examined only armed conflict, leaving the relationship between local interethnic exposure and the full repertoire of contention unexplored. This paper addresses this gap by leveraging a novel village-level event dataset of contentious activity during the 1936 general strike in Mandatory Palestine. To identify a causal effect, I employ a shift-share instrument exploiting exogenous variation in Jewish immigration driven by rising persecution in Europe. I find that Palestinian villages with greater predicted exposure to Jewish settlements experienced significantly higher rates of contentious activity, and that this effect is principally driven by a mechanism of resentment—rather than opportunity cost or appropriation. This suggests that Palestinian exposure to Jewish communities may have intensified the destructive and punitive dimensions of Palestinian mobilisation.

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Introduction

In settings of ethnic division, some localities experience sustained contentious activity—strikes, boycotts, demonstrations, sabotage, armed attacks—while neighbouring ones remain quiet. This local variation is pervasive, yet poorly understood. What role does local exposure between two ethnic groups play in driving contention at the local level?

A large literature has examined the effects of interethnic exposure, but with sharply divergent conclusions. Proponents of the positive effects of intergroup contact, drawing on Allport’s (1979) original formulation of contact theory, argue that proximity to an outgroup can reduce prejudice (Paluck et al. 2019; Pettigrew and Tropp 2006). At the same time, others argue the opposite under the rubric of conflict theory—that exposure to an outgroup may heighten perceived competition and hostility (Forbes 1997; Hardin 1995; Olzak 1992). Decades of research have produced extensive evidence on both sides (Alrababa’h et al. 2021; Kaufmann and Harris 2015; Paluck et al. 2019; Sturgis et al. 2014), yet overwhelmingly in relation to attitudes and preferences rather than political behaviour. Whether exposure to an outgroup may drive mobilisation, rather than simply updating attitudes and beliefs about an outgroup, remains largely untested.

Work on ethnic violence has moved closer toward connecting local exposure to political action. Scholars have linked economic inequality between groups, competition over resources, and local economic shocks to armed conflict (Cederman et al. 2011; Collier and Hoeffler 2004; Dube and Vargas 2013). Explanations have typically centred on mechanisms of resentment, rapacity, and opportunity cost (McGuirk and Burke 2020; Panza and Swee 2023). While this literature has helped to link local exposure to political action, it has overwhelmingly focused on violence—the most extreme manifestation of political action. It remains difficult, therefore, to assess whether interethnic exposure may have similar effects on non-violent contention, and therefore drive mobilisation overall, or whether it only pushes individuals toward violence.

To overcome these limitations, I draw on insights from the literature on contentious politics, which centres repertoires of collective action and opportunity structures as drivers of contention (Ketchley 2017; McAdam et al. 2001; Tilly 1978; Tilly and Tarrow 2015). This framework provides the tools through which to study the full spectrum of political action—both violent and non-violent. I argue that local interethnic exposure will increase both non-violent and violent forms of contentious activity, and that disaggregating contention by repertoire provides a means through which to test which mechanism drives this effect. Resentment predicts more destructive and punitive repertoires designed to inflict maximal harm and destruction on an outgroup; rapacity predicts appropriation-oriented repertoires such as theft and seizure of property; and opportunity cost predicts contention in areas where the economic costs of mobilising are lowest. These predictions are distinguishable only when contention is observed at the local level—where exposure varies—and across a full range of repertoire types.

I examine the case of Palestine under the British Mandate, focusing on the period of the 1936-1939 Revolt. Palestine under the British Mandate provides an ideal setting under which to examine the role of interethnic exposure in driving contentious activity, forming a most-likely or extreme case study of these effects. The fall of the Ottoman Empire, and Palestine’s subsequent transformation into a mandatory state under the supervision of the British, brought with it a colonial power sympathetic to the emergent Zionist cause.¹ Under the control of the British, Jewish immigration, land purchasing, and settlement construction all accelerated, creating grievances among local Palestinians. This trend in immigration and settlement was exacerbated by Europe’s growing sentiment of anti-semitism during the early-1930s, leading to record levels of Jewish immigration and land purchasing by 1935. By 1936, these factors had generated widespread Palestinian opposition to the Mandatory regime and to Jewish settlement, and the country erupted into revolt. Historians have demarcated the Revolt into two episodes: one encompassing the six-month-long national strike, and the other the two-year-long open armed revolt (Hughes 2019, 1–4). To avoid potential confounders, I focus my analysis on the first period of the Revolt from the 15th of April 1936 to the 11th of October 1936.

To test my argument, I construct a novel village-level event dataset by digitising and coding about 1,000 issues from the Arabic-language daily newspaper *Filastin*. I code for variables such

¹Only a few years prior in 1917, the British government had famously via the Balfour Declaration expressed its support for the “establishment in Palestine of a national home for the Jewish people”.

as location, actor, size, violence, and repertoire.² To my knowledge, this event dataset represents the first systematic record of Palestinian contentious activity at the local level under the British Mandate, allowing me to explore the micro-level effects of interethnic exposure. I combine this event dataset with historical census and settlement data to map out Palestinian village-level exposure to Jewish populations. To isolate plausibly exogenous variation in Palestinian exposure to Jews, I employ a shift-share design, creating an instrument that interacts pre-existing Jewish demographic settlement composition with aggregate immigration inflows to Palestine driven by the rise of Nazism in Europe in the early 1930s. This creates a measure of the predicted variation in local Jewish presence that is plausibly exogenous to Palestinian mobilisation.

I find that Palestinian villages with greater predicted exposure to Jewish settlements experience significantly higher rates of contentious activity during the revolt period. This result is robust to alternative functional form assumptions and survives a range of sensitivity checks. Disaggregating by repertoire, exposure predicts both violent and non-violent contention, though the effect on the latter is imprecisely estimated. The proportional association is additionally stronger for destructive repertoires—arson, assault, bombings—which is consistent with a resentment driven mechanism. Tests for alternative mechanisms of rapacity and opportunity cost yield null results.

This paper makes several contributions to the study of the effects of interethnic exposure and contention. First, it introduces interethnic exposure into the study of contentious politics, connecting a literature on what exposure does to attitudes with a literature on what drives political action. Second, it moves beyond the ethnic violence literature’s focus on armed conflict to examine the full repertoire of contention, showing that disaggregation by repertoire enables more direct mechanism tests than studies of violence alone can conduct. Third, it provides the first systematic village-level event dataset of Palestinian contentious activity under the British Mandate. Finally, the paper also contributes to a growing body of quantitative political science research on Palestine, which remains understudied relative to its prominence in adjacent disciplines (notable exceptions include [El Kurd 2020](#); and [Haran Diman and Miodownik 2024](#)).

Theories of Interethnic Exposure and Contention

Studies of the effects of intergroup exposure across the social sciences have tended to focus on its potential attitudinal effects, with quite divergent and contradictory findings. On the one hand, Allport’s seminal work on intergroup contact argued that contact between different groups can reduce prejudice when groups interact under conditions of equal status, shared goals, and institutional support ([Allport 1979](#); [Sassenberg and Vliek 2019](#)). While these conditions are generally seen as hard to meet in the real world, scholarship has shown that Allport’s original hypothesis appears to be robust to violations of some of these conditions ([Paluck et al. 2019](#); [Pettigrew and Tropp 2006](#); [Vezzali et al. 2014](#); [Zhou et al. 2019](#)). For proponents of this view, intergroup contact and exposure is seen as a mechanism through which to reduce intergroup hostility, thereby implying a reduction in intergroup contention and violence. A small number of studies have connected interethnic contact to political actions directly. Varshney (2002) in his study of Hindu-Muslim conflict in India finds that interethnic associational ties can help to control ethnic violence. Likewise, Jha (2013) shows interethnic complementarity in trade networks and profits can reduce the incidence of ethnic violence.

Despite this, significant debate remains over whether inter-ethnic proximity and exposure does actually lead to a reduction in intergroup hostility. While some studies have found that interethnic contact reduces intergroup hostility ([Alrababa’h et al. 2021](#); [Sturgis et al. 2014](#); [Weber 2019](#)), other scholars have found the opposite ([Hangartner et al. 2019](#); [Kaufmann and Harris 2015](#); [Putnam 2007](#)). Detractors of the positive effects of intergroup contact argue, under the broad label of conflict theory, that the presence of an outgroup in sufficient numbers may lead to them becoming more threatening to the ingroup. Three primary mechanisms have been identified in the literature through which this effect may occur: an outgroup may threaten the distinctiveness of an in group; it may threaten the balance of power and political control that an ingroup enjoys; and, most importantly, it may trigger competition over scarce resources, such as jobs, housing, and welfare ([Forbes 1997](#); [Hardin 1995](#); [McDoom 2013](#); [Olzak 2022](#)). Olzak, in her study of ethnic competition in the US,

²For a full list of the variables coded for, as well as my coding practices, please see Section A.4 in the Appendix.

finds suggestive evidence that economic contraction combined with labour market competition may increase interethnic violence (Olzak 1992).

Scholarship on conflict more broadly has examined the role of economic factors in driving violence between different groups. Initial work emphasised the economic feasibility of conflict, arguing that low income and lootable resources—operating through mechanisms of opportunity cost and rapacity—better predicted civil war than intergroup grievances (Collier and Hoeffler 2004; Fearon and Laitin 2003; Grossman 1991). Later empirical work has sought to sharpen these mechanisms into testable predictions. Dube and Vargas (2013) in their study of Colombia show that commodity price shocks can have variable effects on the onset of conflict depending on the nature of the economic goods. For labour intensive goods, an increase in income reduces the likelihood of conflict by reducing the opportunity cost of participation in violence. For capital intensive goods, increases in income are associated with greater conflict, as individuals and groups seek to maximise gains from appropriation. Likewise, McGuirk and Burke (2020), focusing on the impacts of food price shocks on producers on consumers, show that both opportunity cost and rapacity can drive conflict.

Subsequent scholarship has sought to re-introduce and rehabilitate the important role of interethnic grievances stemming from ethnic inequality. Building on the seminal work of Gurr (1970) and Horowitz (1985), scholars have posited that ethnic inequality can make interethnic grievances and divisions more salient—a mechanism more broadly termed as resentment (Esteban and Ray 2011a, 2011b). Cederman et al. (2011), for example, show that horizontal inequalities between ethnic groups can drive interethnic violence. Panza and Swee (2023), in a study of Mandatory Palestine, test all three proposed mechanisms and find empirical evidence that Jewish-Arab income inequality intensified interethnic violence between both groups, with this violence most likely being driven by a mechanism of Arab resentment toward Jews rather than by opportunity cost or rapacity.

While these works provide valuable insight into the potential effects of interethnic exposure and the potential drivers of ethnic violence, their scope remains somewhat limited. The literature on intergroup contact has for the most part focused on attitudinal outcomes. Where studies have attempted to explore the connection between intergroup contact and behavioural outcomes (Jha 2013; Varshney 2002), they have either lacked the ability to causally identify effects or have focused exclusively on interethnic violence rather than contention more broadly. While scholarship on the role of income and interethnic inequality has helped to establish empirical proof for the importance of various mechanisms, work in this vein has also only focused on establishing the causes of ethnic violence. Examining what role exposure between two ethnic groups might play in driving contention—both violent and non-violent—therefore, remains an open empirical question.

To address this gap, I draw on insights from the literature on contentious politics, which examines a broad array of contentious collective action (Ketchley 2017; McAdam et al. 2001; Tilly 1978; Tilly and Tarrow 2015). Importantly, scholarship in this tradition has highlighted how different repertoires of contention may possess different logics and may be carried out by different actors (Kadivar and Ketchley 2018; Tilly 2003). Recent work by Barrie et al. (2024) has applied these insights to the study of ethnic violence, disaggregating it into its constituent forms and repertoires. By distinguishing between the target and level of violence, Barrie et al. (2024) are able to derive differing observable implications depending on the repertoire of violence used. I extend this theoretical insight to the study of all forms of interethnic contention, thereby allowing me to compare the effects of interethnic exposure on violent forms of contention—assault, arson, bombing, riots, shootings—as well as non-violent forms of contention—demonstrations, rallies, strikes. Overall, I expect that local interethnic exposure will be associated with increased levels of both non-violent and violent forms of contentious activity. I therefore derive the following hypothesis:

H1: Non-violent and violent forms of contentious activity will be higher in places where interethnic exposure is greater.

Having identified three mechanisms through which economic conditions may drive interethnic violence—resentment, rapacity, and opportunity cost—I now derive predictions about the forms of contention each should produce. To help distinguish between these three mechanisms and derive differing observable implications, I disaggregate ethnic contention into its various repertoires and forms. By doing so, I am able to more clearly test the importance of mechanisms of rapacity and

resentment. Prior empirical work has tested these mechanisms indirectly. Dube and Vargas (2013), for example, infer rapacity from the differential effect of commodity price shocks across sectors. Disaggregating contention by repertoire allows for a more direct test: if rapacity drives contention, we should observe it in the specific repertoires used.

Should exposure be driving contentious activity through the mechanism of rapacity, then we would expect to see more contentious events characterised by repertoires which seek to appropriate the material goods of an ethnic outgroup. These might include actions such as looting, theft, as well as attempts to occupy and possess an ethnic outgroup’s land or material property. I therefore propose the following hypothesis:

H2: Exposure should increase events characterised by appropriative repertoires (looting, theft, occupation)

By contrast, should exposure be driving contentious activity through the mechanism of resentment, we would expect to see more contentious events characterised by punitive and destructive repertoires. For individuals affected in such a way, punitive actions form a means of redressing grievances and inflicting costs on an outgroup perceived as responsible for them. Thus, rather than seeking to appropriate or enrich themselves, individuals should want to exact maximal damages and inflict maximal suffering on an outgroup. I therefore derive the following hypothesis:

H3: Exposure should disproportionately increase destructive/punitive events (arson, assault, destruction of property, bombing, stoning)

Unlike resentment and rapacity, which predict the form contention takes, opportunity cost predicts the conditions under which it occurs. Where the economic costs of participating in contention are high, individuals face stronger incentives to abstain regardless of their grievances. Contention, therefore, should be greater in places where mobilisation costs are lower. I therefore propose the following hypothesis:

H4: The effect of interethnic exposure on contention will be greater where the economic costs of mobilising are lower.

Mandatory Palestine during the period of the 1936–1939 Revolt provides a particularly well-suited setting in which to test these hypotheses. The case offers a sharp exogenous shock to interethnic exposure driven by European anti-semitism and a period of sustained contention encompassing both violent and non-violent repertoires. In what follows, I describe the historical context of the Revolt and the conditions that make this case amenable to causal identification.

Case Study: Mandatory Palestine

During the late-Ottoman period, Palestine’s historical Jewish community only numbered around 10,000, with the vast majority being Sephardic Jews and almost all residing in urban areas of Palestine (Wallach 2017, 5).³ The final decades of Ottoman rule marked the beginning of Zionism-inspired Jewish migration to Palestine. The first *aliyah* (1881-1903) and second *aliyah* (1904-1914) brought with them tens of thousands of Jewish migrants, predominantly from Eastern Europe, who began to establish Jewish agricultural settlements across Ottoman Palestine (Lesch 1979; Panza and Zylberberg 2024).⁴ By 1914, Palestine’s Jewish population had reached approximately 65,000 (McCarthy 1990).

The conclusion of the First World War brought with it an end to about four centuries of Ottoman rule over the Middle East. In its stead, under the Sykes-Picot agreement of 1916, the Middle East would be divided into newly drawn nation states to be placed under the colonial supervision of

³Wallach notes that in around 1850 two-thirds of Palestine’s Jewish population were Sephardic Jews, with the remainder being Ashkenazi Jews. Palestine’s Jewish population, he describes, resided primarily in Jerusalem, with the remainder living in the cities of Safad, Tiberias, and Hebron (Wallach 2017, 5).

⁴*Aliyah* is a term that refers to the return of the Jewish diaspora to Israel.

Britain and France (see [Barr 2012](#) for more information). For Palestine, the end result was the creation of a formally unified Palestinian polity under the colonial administration of the British.⁵

Yet, British colonial administration intensified the emerging threat of Zionism to Palestine's indigenous Arab population. Historical literature has pushed back against the idea that British colonial policy during this period actively sought to create a Zionist state, arguing instead that Britain's ultimate goal was the creation of a significant Jewish minority in Palestine which, while dependent on Britain for its survival, would act as a counterweight to Palestine's Arab population ([Black 2017](#); [Fieldhouse 2008](#)). Nevertheless, British policy remained significantly more sympathetic toward the Zionist cause. For example, Britain encouraged the formation of state-like institutions within the Zionist movement, such as supporting the Jewish Agency. Conversely, and in contrast to British policy in Jordan and Iraq during this period, Arab attempts to create democratic institutions of self-rule were largely denied by the British. As Fieldhouse argues, when Arab elites sought to get British recognition for an Arab Executive Committee (AE), the High Commissioner Sir Herbert Samuel refused, ostensibly on grounds of representativeness but in reality because the committee rejected the Balfour Declaration ([2008, 154](#)).

At the same time, Jewish immigration to Palestine increased sharply in the mid-1930s. The fifth *aliyah* (1930-1939) would bring with it almost 250,000 Jewish migrants, most of whom were German and Eastern European Jews. Importantly, this wave of Jewish immigration was largely the result of strong, exogenous factors at origin, rather than the particular pull or attraction of Palestine ([Bugge et al. 2023](#)). Immigrants during this period, primarily from Eastern and Central Europe (see [Figure 1](#)), were fleeing their countries of origin due to the rising wave of anti-semitism across Europe and resultant Jewish persecution. The exogenous nature of this immigration shock forms the basis of my identification strategy.

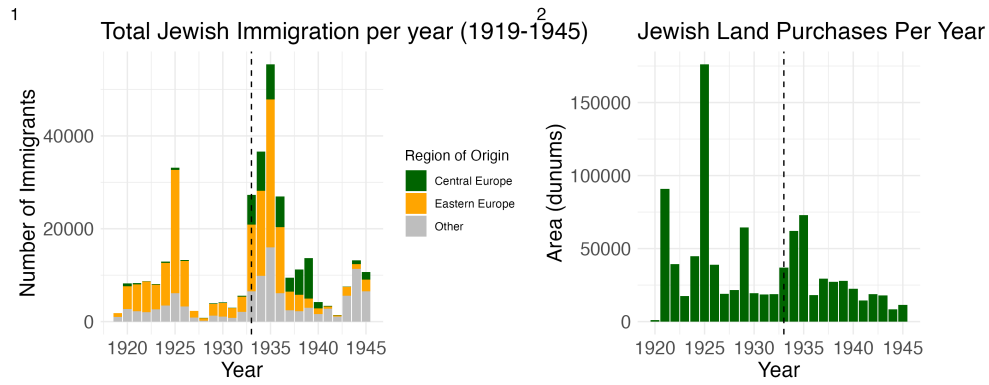
As a result of these push factors, the Jewish population in Palestine would grow from 175,000 to 400,000 between 1931 and 1935, representing an increase from 17% to 31% of the population ([Swedenburg 1988, 184](#); [Shepherd 2000, 98](#); [Khalidi 2007, 24](#); [Black 2017, 80](#)). This influx of European Jewry brought with it a significant amount of capital as well. Under the Ha'avara agreement, signed between the World Zionist Organisation and the Nazis, Jews fleeing Germany were allowed to leave with their capital, leading to a large influx of capital into Palestine ([Swedenburg 1988, 184](#)). This led to a rapid increase in Zionist land acquisition. [Figure 1](#) shows the total number of Jewish immigrants to Palestine per year, broken down by their region of origin, as well as the total number of land purchases conducted by Jewish individuals and organisations per year. As can be seen, each measurement witnesses a significant spike in the early 1930s.

Growing Jewish immigration and land purchases, in turn, coincided with worsening conditions for many of Palestine's Arab population. By 1930, roughly 30 percent of Palestinian villages were entirely landless, and as many as three-quarters held insufficient land for subsistence ([Swedenburg 1988, 182](#)). By 1936, the average Palestinian family's annual debt equalled or exceeded their annual income of approximately £P27 ([Swedenburg 1988, 185](#)).

Despite worsening economic conditions and growing political tensions, relations between Jews and Arabs at the individual level were not uniformly hostile. For example, issues from the Arabic-language daily newspaper *Filastin* from 29 and 30 April 1936 recount the fact that many Arab residents of al-Manshiyya, an Arab neighbourhood in Jaffa bordering Tel Aviv, hid Jewish passerbys in their houses to prevent them from being attacked by groups of Arabs in the streets of the city. *Filastin* also reports Palestinian youth from Jaffa attempting to visit their Jewish lovers in Tel Aviv after the onset of the general strike. Likewise, Haran Diman, in a study of the later 1948 Arab-Israeli War, finds that neighbouring Jewish and Palestinian villages sometimes engaged in peace- and alliance-building ([Haran Diman 2023, 2](#)).

Yet, these interpersonal ties could not overcome the structural incompatibility of the two competing national projects—the creation of an inclusive Palestinian state with a Jewish minority on the one hand, and on the other the creation of an ethnocratic Jewish state underlined by the forced expulsion and ethnic cleansing of the Palestinian majority ([Fieldhouse 2008, 130](#); [Segev 2000, 404](#)). While contention between Jews and Arabs had broken out in the 1920s—the 1921 Jaffa riots and the 1929 Wailing Wall riots being the primary examples—the intensifying political and

⁵Under the Ottoman Empire, Palestinian had been administratively sub-divided into several different governorates. The northern parts of the country—including cities such as Acre, Haifa, and Nablus—fell under the district (*vilayet*) of Beirut, while much of the rest of Palestine had been under the supervision of the district of Damascus.



Note: Plot 1 shows the total number of Jewish migrants to Palestine per year between 1919 and 1945, broken down by the region of origin. Central Europe captures Jews migrating from Austria, Czechia, and Germany. Eastern Europe captures Jews migrating from Poland, Lithuania, and the USSR. Plot 2 shows the total amount of land, measured in dunams, purchased by Jews and Jewish organisations between 1920 and 1945. The dashed line in both plots indicates the Nazi rise to power in 1933. Data is sourced from the Statistical Handbook of Jewish Palestine compiled by the Jewish Agency in 1947, and British Census data

Figure 1

socio-economic situation in the mid-1930s would lead to the outbreak of the nationwide 1936-1939 Revolt ([Anderson 2018](#)).

Historians have generally demarcated the Revolt into two episodes: one encompassing a six-month-long national strike, and the other a two-year-long open armed revolt ([Hughes 2019, 1-4](#)). I focus on the first period—15th of April 1936 to 11th of October 1936—to hold the institutional and repressive context constant. For example, qualitative historical work has shown that British repression intensified during the second period of the revolt ([Segev 2000](#)).

Research Design

Data

To examine the effects of interethnic exposure on contention at the local level, I construct an event dataset at the village-day level that covers contentious events undertaken by Jews and Arabs during the 1936-1939 Revolt (see Section A.4 for a full description of my data collection strategy). Event data was collected from the Arabic-language national-level daily newspaper *Filastin*, which was in publication between 1911 and 1967.⁶ Several features of *Filastin* help to mitigate potential sources of selection bias mentioned in the literature (see Section A.4.1 for more information on current practices in creating event datasets in contentious politics). First, the newspaper tends to routinely publish the High Commissioner’s daily “Official Communication”, which summarises events that occurred that day and the previous day (see Figure A3 for an example). Likewise, the newspaper also routinely reports on what Hebrew-language newspapers, such as *HaBoker* and *Haaretz*, were reporting, often summarising the attacks they report on Jewish settlements. Both practices allow for the events reported in *Filastin* to be, at least partially, cross-referenced. The latter practice also helps to mitigate potential selection bias between Arabic- and Hebrew-language sources in event coverage.

For each event, I code for the date when it occurred, the location where it occurred (at the district and town/village level), the actor committing the event (Palestinian, British, or Jew), the number of actors involved, the target of the event (Palestinian, British, or Jew), the repertoires used, whether the event was violent, whether the event was repressed, and whether individuals were injured or killed (see Section A.4.3 for a full breakdown of coding conventions). Events were then

⁶Future versions of this paper will hopefully incorporate data from two additional national-level Palestinian newspapers, as well as records from the Palestine Police Force.

Table 1: Descriptive Statistics of Contentious Events, April-October 1936

Actor	Total	Violent	Non-violent
All Events	7090	1961	5129
British	42	41	1
Jews	200	164	36
Palestinians	6607	1526	5081
Unknown	241	230	11

georeferenced using coordinates from the Palestine Remembered Project’s digitisation of Palestinian Village Statistics and from Jewish settlement records held in the National Archives.

In total, the final dataset captures 7090 events, of which 1961 (27.7%) were violent and 5129 (72.3%) were non-violent. Table 1 provides a brief overview of the breakdown of these events by actor. The vast majority of events recorded were committed by Palestinians representing 93.2% of all events. The size and granularity of this event data allows me to disaggregate by repertoire and examine within-district variation, neither of which is possible with existing data. The most comparable existing dataset for this setting, compiled by Panza and Swee (2023), reports 430 conflict events across 18 districts over most of the Mandate period (1926-1948). By contrast, my dataset records over 1900 violent events during the six-month strike period alone, reflecting both the granularity of the Arabic-language source material and the intensity of contention during the revolt. Figure 2 shows the distribution of events per day over the strike period.

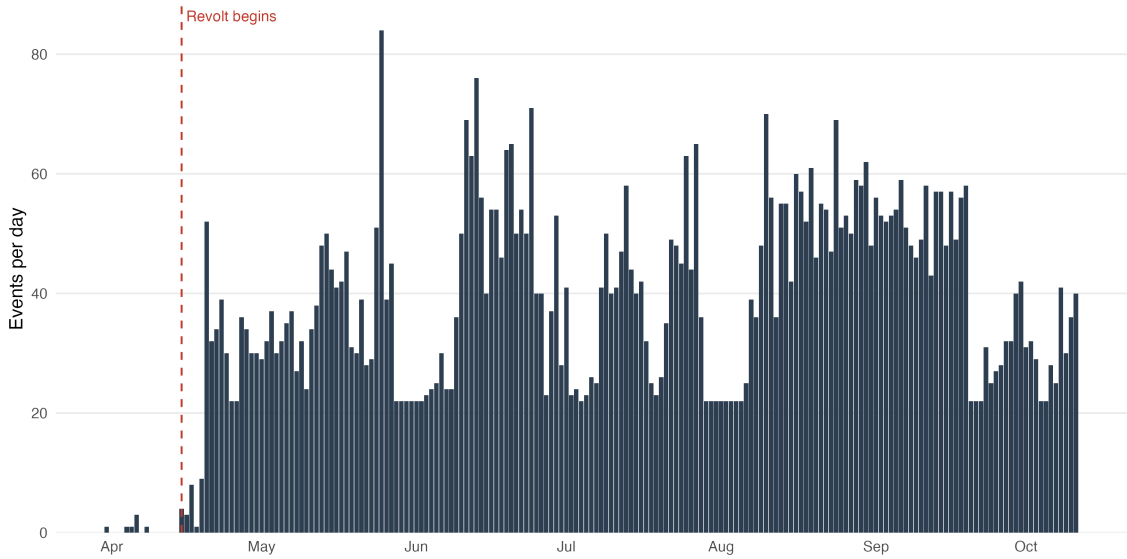


Figure 2: Number of contentious events per day across Palestine between 01/04/1936 and 11/10/1936

Dependent Variable

My dependent variable is a count of contentious events with a Palestinian actor and a Jewish target, aggregated to the village level over the six-month strike period. Subsetting the full dataset to these events yields 4011 observations—728 violent and 3283 non-violent—spanning 1089 Arab towns and villages. Events that occurred at or near Arab villages are attributed directly. Events at Jewish settlements, however, were likely carried out by Palestinians from surrounding communities, and attributing them requires an additional step. I distribute these events ($n=406$) to nearby Arab villages using an exponential-decay function, whereby each village receives a share of the event that decreases exponentially with its distance from the settlement (the same functional form used to construct the exposure measure described below). Events in mixed towns, such as Haifa, are

attributed directly, since perpetrators in these cases are likely residents of the town itself. In addition to the overall event count, I disaggregate the dependent variable into violent and non-violent events to test whether interethnic exposure has differential effects across these two broad categories of contention. I test robustness to excluding attributed events in later specifications.

Independent Variable

My main explanatory variable is an Arab village’s exposure to nearby Jewish populations, be it a settlement or a town. Exposure, in this instance, is a function of how many Jews live nearby and how close they are to an Arab village. To calculate this, I use spatial data on the location of Jewish settlements, derived from Jewish settlement records in the National Archives, and data on the population of Jewish settlements kindly provided by Panza and Zylberberg (2024). I then calculate a measure of exposure using the following formula:

$$Exposure_i = \sum_j P_j \cdot e^{-\lambda d_{ij}}$$

Where P_j is the Jewish population at settlement j , d_{ij} is the distance in kilometres between Arab village i and Jewish settlement j , and λ is the decay parameter. In my main specification this is set to 0.2, which results in exposure halving every 3.5km. In robustness checks, I vary this measure of λ and utilise other functions of distance, such as inverse and inverse-squared functions.

Utilising Jewish population figures, however, may produce biased estimates. Jewish settlement locations were not randomly distributed across Palestine (see Figure A2). Instead, they tended to be clustered in the coastal plain and the more fertile northern parts of the country (Kamel 2014, 240). Indeed, a strategic logic underpinned the location and expansion of Jewish settlement activity. Panza and Zylberberg (2024) describe how considerations such as the suitability of the land and the strengthening of areas with a pre-existing Jewish presence shaped the logic of Jewish settlement expansion. If these same factors also correlate with Palestinian mobilisation potential then naive estimates of the effect of exposure on contention will be biased. For example, fertile land may have attracted both Jewish settlements and a denser, more economically precarious Arab population. Similarly, Jewish population growth after initial settlement may itself reflect local conditions, if settlers were attracted to areas with less Arab hostility.

To address these concerns, I construct a predicted measure of Jewish settlement exposure using a shift-share design. The instrument combines two components. The “shares” capture the origin region composition of each Jewish settlement’s foreign-born population as of 1929, drawn from settlement-level data provided by Panza and Zylberberg (2024).⁷ Specifically, for each settlement j and origin region r , I calculate the shares s_{jr} as settlement j ’s proportion of all foreign-born Jews from region r residing in Palestine. The “shifts” are annual aggregate immigration flows to Palestine by origin country between 1930 and 1935, which I aggregate to origin region to match the settlement-level data from Panza and Zylberberg (2024). The predicted increase in Jewish population at each settlement is then:

$$P_j = \sum_r s_{jr} \cdot g_r$$

Where g_r is the total immigration from origin region r over the period. This predicted population is then substituted into the exposure formula defined above, giving each Arab village a measure of predicted exposure to Jewish settlements that is driven by the interaction of pre-existing settlement composition and aggregate immigration inflows.

Identification in shift-share designs requires either that shares are quasi-randomly assigned or that the shifts are exogenous and sufficiently numerous (Adão et al. 2019; Borusyak et al. 2022, 2025; Goldsmith-Pinkham et al. 2020). The non-random distribution of Jewish settlements precludes reliance on the former. I therefore rely on the exogeneity of shifts. Annual Jewish immigration inflows to Palestine during this period were driven overwhelmingly by push factors at origin—such as rising anti-semitism and persecution across Europe, most dramatically following the Nazi rise to power in 1933—rather than by conditions in specific Palestinian localities (Bugge

⁷The regions are Asia, Africa, the Balkans, Eastern Europe and Central/Western Europe.

et al. 2023). To ensure that the instrument captures variation in the *composition* of nearby Jewish populations rather than their scale, I control for the distance-weighted total foreign-born Jewish population.

To assess whether predicted exposure is correlated with pre-treatment village characteristics, I regress the instrument on 1931 population, elevation, cultivable land area, and cereal land share. The results, reported Table A1, show no significant relationship between predicted exposure and these characteristics, with the exception of a marginally significant correlation for elevation ($p = 0.058$). This is consistent with the geographic concentration of Jewish settlements in lower-lying areas. I present specifications with and without elevation controls in my main results and the coefficient remains substantively the same in both.

Since the shift-share generates predicted Jewish populations rather than instrumenting for an independently observed endogenous variable, constructing a synthetic 1936 Jewish population for use in a two-stage least squares framework would make the endogenous regressor mechanically a function of the instrument. I therefore estimate the reduced form directly, regressing contention outcomes on predicted exposure to capture the intent-to-treat effect. I therefore estimate the following regression:

$$Y_i = \alpha + \beta Z_i + \delta_d + \epsilon_i$$

Where Y_i captures the number of contentious events in Arab town or village i , Z_i is the distance-weighted predicted Jewish exposure (the shift-share instrument), and δ_d are sub-district fixed effects. I cluster standard errors at the sub-district level.

Results

I now present my results. Table 2 reports the results of the effects of predicted exposure on all forms of contention. Column 1 reports the baseline OLS specification with district fixed effects and standard errors clustered at the district level. I find that predicted exposure to Jewish settlements is positively and significantly associated with Palestinian contentious activity ($p < 0.05$). The point estimate of 0.0025 implies moving from the 25th to the 75th percentile of predicted exposure (an increase of 706 units) is associated with approximately 1.77 additional contentious events per village, representing a 48% increase relative to the sample mean of 3.68 events. Column 2 adds elevation as a control; the coefficient remains positive but precisions weakens ($p < 0.1$). As discussed above, Jewish settlements were concentrated in lower-lying areas, so controlling for elevation likely absorbs part of the identifying variation in exposure rather than addressing any confounding. Column 3 restricts the sample to directly attributed events, excluding those distributed from Jewish settlements via the distance-decay function. The results remain significant ($p < 0.05$), indicating that the finding is not driven by the attribution procedure.

As a further check on functional form, I estimate a negative binomial specification to account for the substantial overdispersion in event counts. The results, reported in Table A2, are consistent with the OLS findings. Finally, I check whether the results are robust to alternative functional forms for the distance-decay parameter (see Table A3). The results are robust to a slower decay parameter ($\lambda = 0.1$), inverse distance, and inverse distance squared. Under a faster decay ($\lambda = 0.3$), where only the nearest settlements contribute meaningfully to exposure, the result remains positive but marginally significant ($p = 0.06$), suggesting some sensitivity to how rapidly exposure is assumed to diminish with distance.

Table 2: Effect of Predicted Exposure on Contentious Activity

Model:	Main Specification (1)	With Controls (2)	Direct Events (3)
<i>Variables</i>			
Exposure	0.0025** (0.0009)	0.0018* (0.0010)	0.0022** (0.0010)
Average Elevation		0.0101** (0.0040)	
<i>Fixed-effects</i>			
District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	1,089	768	1,089
R ²	0.03177	0.02385	0.02562
Within R ²	0.01414	0.01486	0.01115

Clustered (District) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

I next disaggregate the dependent variable into violent and non-violent events to test whether exposure affects both forms of contention. Table 3 presents the results. Exposure significantly predicts violent contention ($p < 0.01$), with a point estimate of 0.0008. This implies that moving from the 25th to the 75th percentile of exposure is associated with 0.53 additional violent events per village, representing a 79% increase relative to the sample mean. The effect on non-violent contention is positive and larger in absolute terms (point estimate of 0.0018), representing a smaller proportional increase of 41% relative to its mean. However, falls short of conventional significance ($p = 0.06$), likely reflecting greater variance in non-violent event counts across villages. Both coefficients are positive, providing support for H1.

Table 3: Effect of Predicted Exposure on Violent and Non-Violent Events

Model:	Violent Events (1)	Non-Violent Events (2)
<i>Variables</i>		
Exposure	0.0008*** (0.0001)	0.0018* (0.0009)
<i>Fixed-effects</i>		
District	Yes	Yes
<i>Fit statistics</i>		
Observations	1,089	1,089
R ²	0.16160	0.02140
Within R ²	0.06199	0.00821

Clustered (District) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

I now turn to the mechanism predictions derived in the theory section. H2 predicts that if rapacity drives contention, exposure should increase appropriation-oriented repertoires such as looting, theft, and seizure of property. This prediction cannot be meaningfully tested as the dataset only contains 8 appropriation-oriented events across the entire strike period. The near-absence of such repertoires effectively rules out rapacity as a significant driver of Palestinian contention in this

setting.

H3 predicts that if resentment drives contention, exposure should disproportionately increase destructive and punitive repertoires. I test this by disaggregating events into destructive repertoires (arson, assault, attack, bombing, shooting, stoning, destruction of property) and non-destructive repertoires (boycott, demonstration, rally, sabotage, strike, roadblock). ⁸ Table 4 presents the results. Exposure significantly predicts destructive events ($p < 0.01$), with an IQR-scaled effect implying a 62% increase relative to the sample mean. The effect on non-destructive events is positive but marginal ($p = 0.08$), representing a 51% proportional increase. The proportional effect on destructive effects is larger, consistent with H3, though the difference is modest and the formal interaction test does not reach significance. Together with the violent/non-violent disaggregation, where the proportional gap is wider at 79% versus 41%, the pattern is consistent with a resentment-driven mechanism, though the evidence remain suggestive rather than conclusive.

This finding converges with Panza and Swee (2023), who, using rainfall-instrumented income inequality at the district level, independently find that resentment is the dominant mechanism linking interethnic economic conditions to violence in Mandatory Palestine.

Table 4: Effect of Predicted Exposure on Resentment-driven Contention

Model:	Destructive (1)	Non-Destructive (2)	Interaction (3)
<i>Variables</i>			
Exposure	0.0007*** (0.0001)	0.0012* (0.0006)	0.0010*** (0.0003)
Non-Destructive			2.001*** (0.4323)
Interaction Term			-9.53×10^{-5} (0.0002)
<i>Fixed-effects</i>			
District	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	1,089	1,089	2,178
R ²	0.17365	0.01755	0.02038
Within R ²	0.05082	0.00397	0.00953

Clustered (District) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

H4 predicts that the effect of exposure on contention should be greater where the economic costs of mobilising are lower. As noted in the historical background, Palestinian peasants during this period faced widespread poverty and landlessness. By 1936, the average family's annual debt equalled or exceeded their income (Swedenburg 1988). For communities dependent on subsistence cereal agriculture, the opportunity cost of participating in contention would have been directly related to their agricultural labour. This cost varied across villages with those more reliant on subsistence cereal cultivation having lower and more precarious incomes, meaning that the marginal cost of abandoning work to mobilise was smaller. I therefore test H4 by interacting predicted exposure with the share of village land devoted to cereal cultivation, drawn from the Village Statistics. If opportunity cost moderates the effect of exposure, we should observe a stronger association between exposure and contention in villages with greater cereal dependence. Table 5 presents the result. Column 1 reports the baseline OLS specification, while Column 2 adds the total Cultivable Land as a control. Neither interaction term is statistically significant, providing no support for the opportunity cost mechanism.

Combined with the null result for rapacity, these findings suggest that resentment is the most

⁸Sabotage was included in the latter as it overwhelmingly was utilised in a strategic manner.

plausible mechanism through which interethnic exposure drives contention in this setting — though the evidence, as noted, is suggestive rather than definitive.

Table 5: Effect of Opportunity-Cost on Contention

Model:	OLS (1)	With Controls (2)
<i>Variables</i>		
Exposure	0.0023** (0.0008)	0.0023** (0.0008)
Cereal Agriculture	0.0005* (0.0003)	-0.0020 (0.0012)
Interaction Term	5.37×10^{-8} (2×10^{-7})	3.17×10^{-8} (1.66×10^{-7})
Cultivable Land		0.0022** (0.0010)
<i>Fixed-effects</i>		
District	Yes	Yes
<i>Fit statistics</i>		
Observations	782	782
R ²	0.06428	0.10548
Within R ²	0.04263	0.08479

Clustered (District) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Conclusion

This paper asked what role local interethnic exposure plays in driving contentious activity and in shaping the forms it takes. Drawing on the contentious politics tradition’s emphasis on repertoires of collective action, I argued that disaggregating contention beyond the violent/non-violent binary enables mechanism tests that studies of ethnic violence alone cannot conduct. I tested this argument using a novel village-level event dataset constructed from the Arabic-language daily newspaper *Filastin*, combined with a shift-share instrument that exploits exogenous variation in Jewish immigration to Palestine driven by Nazi persecution in Europe.

The results demonstrate that Palestinian villages with greater predicted exposure to Jewish settlement experienced significantly higher levels of contentious activity during the 1936 general strike. This effect is substantively large: moving from the 25th to the 75th percentile of predicted exposure is associated with a 48% increase in events relative to the sample mean. Disaggregating by violence type, the proportional effect on violent contention (79%) is nearly twice that of non-violent contention (41%). Further disaggregating by repertoire, destructive repertoires associated with a resentment mechanism show a larger proportional response to exposure than non-destructive repertoires, though the formal difference does not reach statistical significance. Tests for alternative mechanisms yield null results: appropriation-oriented repertoires are virtually absent from the data, ruling out rapacity, and interactions with cereal dependence provide no support for opportunity cost. The overall pattern, suggestive of resentment but not conclusive on any single test, is bolstered by the convergence with Panza and Swee (2023), who arrive at the same mechanism through an entirely independent identification strategy at a different spatial resolution.

Several limitations warrant acknowledgement. First, while the shift-share instrument provides plausibly exogenous variation in exposure, the effective number of independent shifts is limited by the number of origin regions, and a marginally significant correlation between the instrument and village elevation suggests that the identifying variation is not fully independent of geographic determinants of settlement location. Second, the event dataset, while substantially larger and

more granular than existing alternatives, is drawn from a single newspaper source, raising the possibility of selection bias in event coverage—a concern I partially mitigate through *Filastin*’s practice of republishing British official communications and Hebrew-language press reports. Third, the reduced-form design estimates the intent-to-treat effect of predicted exposure rather than the structural effect of actual Jewish population, limiting the ability to quantify the precise elasticity of contention with respect to exposure. Finally, the mechanism evidence is suggestive rather than definitive: the proportional differences between destructive and non-destructive repertoires are consistent with resentment but the formal interaction tests do not reach significance.

The paper contributes to three literatures. First, it introduces interethnic exposure as an explanatory variable into the study of contentious politics, demonstrating that local proximity to an outgroup affects not just attitudes — as the contact and conflict theory literatures have extensively documented — but political action. Second, it moves beyond the ethnic violence literature’s exclusive focus on armed conflict to examine the full repertoire of contention, showing that exposure drives mobilisation more broadly rather than only its most extreme manifestation. The disaggregation of contention by repertoire type also provides a more direct test of the mechanisms linking exposure to political action than has previously been possible. Third, it contributes to a growing body of quantitative political science research on Palestine, a setting that remains understudied relative to its prominence in adjacent disciplines.

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A Appendix

A.1 Tables

A.1.1 Balance Tests

Table A1: Balance Tests: Covariate Balance on Predicted Exposure

Variable	Mean	SD	Coefficient	SE	P-value	N
Cultivable Land (dunams)	7263.435	9899.836	-1.86e-01	2.53e-01	0.463	794
Cereal Land (dunams)	5806.063	8751.286	-1.55e-01	1.05e-01	0.140	782
Citrus Land (dunams)	842.958	1335.939	4.45e-02	9.38e-02	0.636	166
Elevation (m)	331.526	264.438	-2.42e-02	1.28e-02	0.058	768
Distance to District Centre (km)	15.951	8.851	-5.89e-04	6.37e-04	0.355	839
Arab Population (1931)	1418.664	4355.221	5.60e-01	6.37e-01	0.380	238

A.1.2 Negative Binomial Count Model

Table A2: Negative Binomial Count Model

	Direct Events (NB)
Predicted Exposure	0.00074*** (0.00016)
Num.Obs.	1089
Log.Lik.	-667.611

* p < 0.1, ** p < 0.05, *** p < 0.01
District fixed effects included.

A.1.3 Alternative Distance-Decay Functions

Table A3: Alternative Distance-Decay Functions

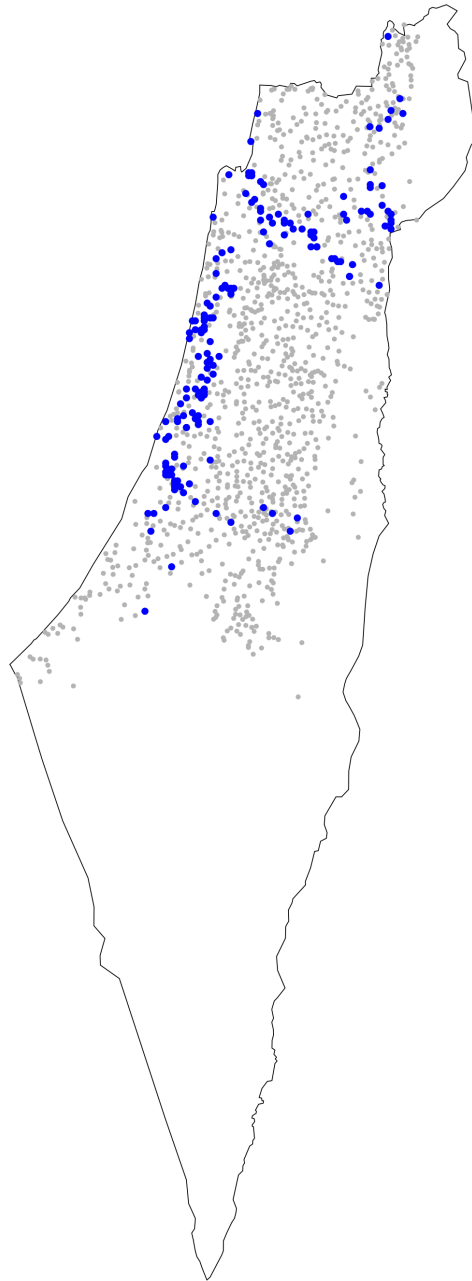
Model:	Lambda = 0.1 (1)	Lambda = 0.3 (2)	Inverse Distance (3)	Inverse Distance Squared (4)
<i>Variables</i>				
Exposure Slow	0.0009** (0.0004)			
Exposure Fast		0.0041* (0.0020)		
Inverse Distance			0.0058** (0.0025)	
Inverse Distance Squared				0.0413*** (0.0126)
<i>Fixed-effects</i>				
District	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	1,089	1,089	1,089	1,089
R ²	0.02402	0.03544	0.03717	0.13145
Within R ²	0.00625	0.01788	0.01965	0.11564

Clustered (District) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A.2 Maps



Figure A1: Map of Mandatory Palestine showing its districts



• Arab villages • Jewish settlements

Figure A2: Distribution of Arab Villages and Jewish Settlements

A.3 Newspapers

البلاغ الرسمي رقم ١٠٢ - ٣٦

القوم الجنوبي : وجد مسا : أيس أن النار شبت في قوامع سكة الحديد الحشبية في الخط الفرعي بالقرب من بيت نبالا ، وقد أطفئت النار في الحال ، وأقيمت قبلة في الليل على حدود بانكا — تل ابيب وأخرى على منير يهودي للمضخات بموارد تل ابيب ولم يحدث ضرر

وأطفئت النار صباح هذا اليوم على باص يهودي في قرية يازور ولم يصب احد باذى . وقد طعن صباح هذا اليوم كوستايل برطاني غير مسلح في ظهره بالقرب من قطرة بوليس المجدي والمرح ليس خطرا وحصل في صباح اليوم الباكر حريق منير في منطقة النشبة في بانكا ، وانفجرت قتال في غزة وبئر السبع وقد انزلت القنابل التي انفجرت في بئر السبع غرة من غرف بيت التزلا .

وفي صباح هذا اليوم اطلق جهور من اليهود في محنا يهودا مرة ثانية شحنة من الحصار اشترها شخص يهودي من احد العرب وقد فرقوا بالعمى ولم يرد نيا عن وقوع اصابات

القوم الشمالي : اطلقت في اقبل بعض الميارات النارية على مسكرات الجنود في نابلس ، جرى كثير من اطلاق النار في جوار عين حارود وهاهوكيم وأطلقت النار على سيارة تابعة لكفر حزقيل بالقرب من محطة عين حارود وجرح احد الركاب . وأطلقت الميارات النارية على دورية عسكرية على خط نيسان وقد اشتبك الجنود والبوليس مع المتمردين الذين تفرقوا بالنهاية وانسحبوا الى جبال جنين بعد ان اوقعت بهم كما هو معتقد عدة اصابات وقد قتل أسلاك التلغون في نفس المنطقة

لواء القدس : أطلقت في السا وفي اقبل من بيت صفاقا ومارالياس ميادات نارية على مستمرة كزاشيون موزا وفي ناحية موكورحاييم ولم يرف شي عن وقوع اصابات

وحصلت مساء أمس في الخليل مظاهرة أثناء تشييم جنازة احد ضحايا القنبلة التي انفجرت في باب الخليل في القدس وقد رشق الجهور البوليس بالحجارة . الامر الذي اضطره الى استعمال الدمي في تفرقه وقد اصاب خمسة اشخاص اعدم اصابته خطيرة

وطعن احد عمال العرب . احد سكان المستعمرات الالمان والتي اقبض على المتهدي . والتي اقبض في حيفا على عربي وجد لديه قنبلتان وانفجرت قنبلة في معبد بيت ضررا بسيطا لامرأة . واتلف عدد وافر من اشجار العنب في كرم برشتاين في زمارين وحصل حريق منير في حيفا

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Figure A3

A.4 Codebook for the Arab Revolt 1936-1939 Event Dataset

The purpose of this dataset is to compile a list of the contentious events which occurred during the 1936-1939 Arab Revolt in Palestine between 1 April 1936 and 31 August 1939. It draws currently on one Palestinian Arabic-language newspaper source from the period, that being the publication *Filastin*, although it aims to increase this to three to four newspaper sources. This event dataset was constructed by Luqman Abu El Foul. Please contact me at l.abu-el-foul@lse.ac.uk with any questions or criticism. The remainder of this document explains the coding procedures of the event dataset as well as the data collection strategy. This is still a work-in-progress and may change as and when new problems or obstacles are identified.

A.4.1 Current Practices within Protest Event Analysis

While newspaper data has been the mainstay of Protest Event Analysis (PEA) within the field of Contentious Politics (Tilly 1978; McAdam 1982; Kriesi et al. 1995), scholarship has highlighted the potential weaknesses associated with using newspaper data, due to the potential for selection bias (Earl et al. 2004, 69). In response, several strategies have been developed through which to reduce potential selection bias. Firstly, regarding the type of newspaper to use, scholarship has highlighted that national-level newspapers are subject to less selection bias in comparison to local level newspapers (Barranco and Wisler 1999). Moreover, the triangulation of multiple media sources is preferred as it “is more likely to capture more events (addressing selection bias) and to provide multiple accounts of each event (addressing description bias)” (Earl et al. 2004, 74). This is additionally important given that some newspapers were partially censored or shut down during periods of the revolt (Najjar 1975). In line with standard practice today, this paper will utilise multiple sources so that problems of underreporting, fact-checking, and cross-referencing associated with the use of newspapers in event catalogues are minimised (Ketchley 2017, 13).

Regarding potential selection bias between Arabic and Hebrew media, *Falastin* as a newspaper source does well to overcome this for several reasons. Firstly, the paper regularly reports on claims made in Hebrew language newspapers, including *HaBoker* and *Haaretz* among others. These often include reports of contentious events, such as attacks on settlements and Jewish individuals. Secondly, *Falastin* often prints the daily statements provided by the office of the High Commissioner summarising the events of the day across Palestine. During the period of the Revolt, these tend to be summaries of contentious events. The inclusion of both sources in *Falastin*’s reporting helps to address and reduce the selection and description bias summarised above.

A.4.2 Data Collection Strategy

Data for the first six months of the Revolt - corresponding to the period of the strike - was collected manually by the author. For the remaining period, i.e. from 11/10/1936-31/08/1939, I used a machine learning model to OCR each issue of the newspaper, breaking each issue down into specific columns. A dictionary filter of contentious terms, collated based on the repertoires I had identified during the manual collection phase, was then used as a first pass to discard irrelevant columns within each newspaper issue.

After this, I used Claude API (Claude Haiku 4-5-20251001) to classify this filtered set of newspaper columns to further weed out irrelevant columns. I validated a sample of 100 columns classified as “YES” (i.e. containing relevant information) to test for the rate of false positives following LLM classification. This returned a precision rate of 96% (i.e. 96 out of 100 columns classified as “YES” contained information regarding contentious events).

Following the LLM classification step, I employ Arabic sentence embeddings to recover potentially missed events (false negatives) from the full corpus. This addresses a fundamental limitation of the dictionary + LLM pipeline: columns describing contentious activity using unexpected vocabulary or indirect language may have been filtered out at earlier stages. When testing this via sampling, I find that the rate of false negatives is about 8% in my classified columns.

The process works as follows. First, I generate vector representations (embeddings) for each of the ~13,000 LLM-confirmed positive columns using a pre-trained Arabic language model (CAMELBERT or AraBERT). These embeddings capture semantic meaning rather than exact word matches. Second, I embed the entire 120,000-column corpus. Third, for each column not already in the

positive set, I compute cosine similarity to the confirmed positives. Columns with high similarity scores—indicating semantic resemblance to known event reports—are flagged as candidate false negatives. Finally, I manually validate a sample of these recovered columns to assess the quality of the similarity-based retrieval before adding them to the final event dataset.

This approach exploits the observation that news reports describing similar types of events (e.g., shootings, arson, strikes) cluster together in embedding space regardless of exact terminology, allowing me to recover relevant content that the keyword dictionary missed.

The resultant positive cases were then manually transcribed by myself into my event dataset.

A.4.3 Coding Conventions

All reported contentious events were coded, no matter how small. In line with the European Protest and Coercion Dataset (Francisco, n.d.), events which continue across more than one day, such as strikes and occupations, are coded each day as a separate event. This allows me to capture variable state responses to these events across time. Similarly, events that grow out of a parent event are coded separately from the parent event. For example, where a demonstration contains instances of assault against passers-by, the instances of assault are coded separately. In instances where an event contains more than one repertoire, such as a demonstration which generally descends into a riot, I have coded this as one event and labelled the first repertoire as “demonstration” and the second repertoire as “riot”. Where newspapers mentioned future plans for protests or strikes, I have ignored these.

Two additional points must be made in regard to the above. Firstly, there are several instances wherein which the British engage either in pre-emptive repression, such as the mobilisation of troops to other parts of the country and the occupation of various buildings, or repression in response to a more general set of events. For example, following the weekend of violence in Tel Aviv and Jaffa, which began on the 17th April 1936 and continued until 20th April 1936, the British police set up roadblocks along major roads in order to search passers by, shut down certain roads, and instituted perimeters around specific neighbourhoods near to the Tel Aviv-Jaffa border to prevent Arabs and Jews from crossing over. These repressive events occurred in response to the larger backdrop of interethnic violence, rather than in response to a specific incident. Such repression is, therefore, difficult for me to code in my event dataset using the variables I am coding for. Likewise, pre-emptive repression, by its nature, occurs prior to an event rather than in response to an event. For these repressive events, I have coded a separate “British Repression” dataset wherein I replicate the majority of the variables I code for in my original dataset. This standardisation will allow me to later merge both datasets together, creating a district/sub-district day panel dataset whereby I observe a district every day and then merge in the count of protestors in that district on that day, and merge in a dummy for these broader forms of British repression, such as whether the British had closed roads in that district on that day, etc.

Secondly, for events which continue across more than one day, such as strikes, wherein I code each day as a separate event, I have made several choices that should be outlined and justified. Firstly, for long strike periods, it is often the case that newspapers will not report on the strike for each day it occurs, and certainly not for each city or region wherein it occurs. In the case of the general strike, which occurred in virtually all towns, and some larger villages, and spanned a period of six months, this is especially true. Given that the newspapers report the varying start date of the strike in different towns, and that the strike was uniformly called off by the Arab Higher Committee in October, I have chosen to interpolate values based on the last recorded day for that event in the given locale within which it occurred. I.e. if my sources do not report on day 53 of the strike in Nablus because the events were not newsworthy, I would interpolate values for day 53 of the strike in Nablus based on the last recorded mention of the strike in Nablus. The second choice I have made is to additionally code dummy variables capturing when individual events belong to a larger collective of events. For instance, all strike events undertaken as part of the general strike are captured by a “general_strike” dummy variable. Likewise, instances of interethnic violence in Tel Aviv and Jaffa in April 1936 are captured by a “telaviv_jaffa_april_riots” dummy variable. This allows me to later distinguish between isolated events and ones which belong to a larger series.

A.4.4 Description of Variables

The following represent the variables recorded within the event dataset:

(*date_start*): This corresponds to the beginning date of a contentious event. Events for which a specific date was not mentioned are coded as having occurred one day before the publication of that specific Filastin issue (which published daily during this period).

(*date_end*): This corresponds to when a contentious event ended if it lasted for more than one day. It has been left blank where an event occurred only over one-day or where there was no clear information.

(*dist*): this represents the administrative district within which the event occurred. This event dataset uses the district boundaries proposed by the British when they formed the Mandate of Palestine (see Figure A1).

(*sub_dist*): this represents the sub-district where an event occurred. Again, I rely on the British classification of sub-districts. Where an event has been recorded as occurring “near” a sub-district, I have labelled it as occurring within that sub-district.

(*loc_info*): any extra information on location provided in the article. This has been provided in Arabic so as to reduce the potential for translation errors when collecting the data.

(*size*): this refers to the number of participants in a contentious event. Where the number of participants of a contentious event has been qualitatively described by a newspaper, for example “dozens”, “hundreds”, “thousands”, I have generally followed the inferential coding convention of the European Protest and Coercion Dataset (Francisco, n.d.), with some exceptions. Thus, I have coded “tens” as “30”; “hundreds” as “300”; “thousands” as “3000”; and so forth. To differentiate between inferences and reported numbers, I have recorded inferences with a final 1, e.g. 31, 301, 3001. “Several” has been coded as “3.1” (to indicate it is an inference); “dozens” or a “group/party” has been coded as “31”; “a crowd” or “many” as “301”; a “large crowd” or “the people” as “1001”. Where newspapers indicate an estimate such as “about 10,000 protestors”, I have coded this as “10,001”. Likewise, where they indicate an estimate of “almost 10,000 protestors”, I have coded this as “9,999”.

(*actor*): this refers to the group or individuals who initiated the contentious event. “1” refers to Palestinians and “2” to Jews”

(*target*): this refers to the target of the contentious event, be it the British, the Jews, or other Palestinians.

(*repertoire*): this refers to the repertoire employed by the individuals in the contentious event, such as whether it is a march, a demonstration, a strike, a violent attack etc. I have drawn on the repertoires put forward in the “Codebook for European Protest and Coercion Data, 1980 through 1995”, as well as their definitions. Two such repertoire variables have been included so as to be able to capture events wherein which more than one repertoire is observed, e.g. demonstrations which turn into riots.

(*violent*): this is a dummy variable indicating whether the contentious event was violent or not.

(*organised*): this is a dummy variable indicating whether a contentious event was organised top-down by a group or not.

(*organising_actor*): this states the actor or group which organised the event, e.g. the Irgun, the Black Hand, a specific union etc.

(*repression*): this is a dummy variable indicating whether the contentious event was repressed or not.

(*rep_actor*): this variable indicates who undertook the repression, be it the British Palestine Police (1), the British Army (2), Orde Wingate’s Special Night Squad Units (3), or the Nashashibi led “peace bands” (4).

(*rep_type*): this variable indicates the type of repression employed, including the use of live ammunition (1); destruction of private property (2); fines (3); corporal punishment (4); arrests (5); dispersal of crowds in a non-violent way (6); dispersal of crowds in a violent way (7); occupation of buildings (often government ones) (8); roadblocks (9); collective punishment (10); plane bombing and shooting (11)

(*no_killed*): this indicates the number of casualties associated within a contentious event. This variable has been split into three so as to distinguish between Arab casualties, Jewish casualties, and casualties from state forces such as the police or the military.

(*no_injured*): this indicates the number of injured within a contentious event. This variable has been split into three so as to distinguish between Arab injuries, Jewish injuries, and injuries from state forces such as the police or the military.

(*no_arrested*): this indicates the total number of arrested individuals if there were arrests. This variable has been split into two to distinguish between Arabs and Jews who were arrested.

(*georef_lat*): this represents the georeferenced latitude of where the event occurred

(*georef_long*): this represents the georeferenced longitude of where an event occurred. Where additional information of location is given in the newspaper, I have tried to triangulate that with the high quality maps I have. Otherwise, I have defaulted to the geolocation of the subdistrict.

(*other_info*): this represents a brief summary of the event as described in the newspaper article.

(*newspaper*): this represents the newspaper from where the event has been coded. “F” represents Filastin. Where an event has been mentioned by more than one newspaper, the letters of the newspapers have been combined, e.g. “FL” represents that the event was mentioned in both Filastin and al-Liwaa.

(*issue*): this represents the date of issue of the newspaper from where the event was mentioned. Events mentioned across more than one newspaper contain more than date.

A.4.5 Additional Notes

Where village/settlement/town locations have been given that I cannot match to my dataset of Palestinian villages and Jewish settlements, but more information has been given about nearby villages, I have used the nearest recognisable sub-district name.

In instances where the village/settlement/town name cannot be matched – to my dataset on Palestinian villages, my dataset on Jewish settlements, and Google does not provide any clarification – and no further information is given in the article, I have excluded these events from my event catalogue. For example, in an article from the 16th of June 1936, it is mentioned that the settlement of Kadima () was attacked by a group of Arabs. This settlement does not show up in either of my datasets, nor does searching the name of the town online provide any further clarification. The event, thus, is not recorded in my event catalogue.