DISCUSSION PAPER SERIES

DP14366
(v. 5)

Fanning The Flames: Rainfall Shocks, Inter-Ethnic Income Inequality, and Conflict Intensification in Mandate Palestine

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ECONOMIC HISTORY

CEPR

ISSN 0265-8003

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Discussion Paper DP14366
First Published 30 January 2020
This Revision 08 May 2022

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www.cepr.org

This Discussion Paper is issued under the auspices of the Centre's research programmes:

- Economic History

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JEL Classification: N45, N35

Keywords: Inter-ethnic violence, Income inequality, Mandate Palestine

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[†]We thank Kim Bullimore for excellent research assistance, and Ayman Qwaider and Rami Osman for support with translating from Arabic. We are grateful to Victoria Baranov, Jeff Borland, Nisvan Erkal, Hilary Hoynes, Jordi Vidal-Robert, Sarah Walker, Jeff Williamson, Jun Xiao, Ekaterina Zhuravskaya, and Yanos Zylberberg for their helpful comments on earlier drafts. Participants at the Asian Meeting of the Econometric Society (CUHK), Australasian Cliometrics Workshop (Sydney), EEA Annual Congress (Lisbon), EHES Conference (Tübingen), Middle Eastern Economic History Workshop (ANU), Academia Sinica, Macquarie, Sant'Anna, SMU, Sydney, and UNSW, also provided useful feedback. We acknowledge financial support from the Faculty of Business and Economics, University of Melbourne and from the Australian Research Council. A previous version of this paper was titled "Inter-Ethnic Income Inequality and Conflict Intensification in Mandate Palestine".

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1 Introduction

Rising income inequality is one of the most complex and controversial topics of our time, and its implications for a wide-ranging set of socioeconomic and political outcomes, including inter-group conflict, are especially concerning. Historically, large swings in inter-group income inequality were commensurate with violent military, political, and economic conflicts (Piketty, 2014), and such patterns continue to shape our society, as demonstrated by recent conflict episodes that share a common theme of inequitable underpinnings, such as the Black Lives Matter movement, the Arab Spring, and the Colour Revolutions. Issues surrounding income inequality and conflict are therefore of significant relevance today as they were in the past.

The starting point in thinking about how an inequitable distribution of resources or income may lead to conflict is best expressed by Sen (1973), who observed that "a perceived sense of inequity is a common ingredient of rebellion in societies". This sort of *resentment* effect would materialise through relative income, and be distinct from *opportunity cost* or *appropriation* effects that would otherwise work through income levels per se. Though theoretically intuitive, the idea that income inequality can lead to conflict has found little empirical support in both the economics as well as the political science disciplines (Cramer, 2005; Blattman and Miguel, 2010).

In this paper, we look at a long-lasting conflict of historical and contemporaneous importance – the Jewish-Arab conflict – during the British Mandate to examine whether and how economic inequality drives inter-group violence. Figure 1 demonstrates a clear positive relationship between Jewish-Arab income inequality and conflict, which motivates our work. We construct a unique panel dataset at the district-year level, from a variety of archival, primary, and secondary sources, building from scratch a set of variables that portray both socioeconomic conditions and conflict intensity. The original conflict data that we use allow us to distinguish perpetrators from victims and thus uncover the distinct channels that induce violence – *appropriation, opportunity costs, and resentment* – because we can trace the conflict initiating actions of one group to changes in the income of that group (or of the opposing group). This ultimately enables us to verify directly the nature and direction of causality, which go a long way in reconciling the mixed evidence in the existing empirical work.

We study a particular time period before the creation of Israel, when Palestine witnessed an intensification in hostilities that eventually led to the first Arab-Israeli War. As such, it is a period that is crucial for understanding the dynamics of the inter-ethnic violence that still plagues modern-day Israel and the Occupied Palestinian Territories. Following the insights of Ray and Esteban (2017), we measure conflict using social unrest – demonstrations, strikes, riots, etc. – as it is the predominant form of inter-group hostilities at the time.

Like most inter-group conflicts around the world, Jewish-Arab hostilities can be identified by ethno-religious markers. While we do not deny that such primordial roots of violence are influential, our main interest lies in identifying the drivers of conflict intensification, not of conflict onset. That is, we take given the backdrop of Jewish-Arab conflict in Mandate Palestine at the time, and investigate the incidence of violent events that occur at the district-year level. Hence,

notwithstanding the role played by existing hatred and mistrust, we believe that economic factors play a significant role in exacerbating violence.

To isolate the effects of inter-ethnic income inequality on the intensity of conflict during the Mandate, we use an instrumental variable approach, exploiting time-variation in Palestine-wide rainfall shocks and district-level variation in pre-Mandate crop intensity. Our identification strategy relies on pre-existing (cross-sectional) rainfall reliance to capture the differential impact of exogenous rainfall shocks on crop harvest and, in turn, on agricultural income. This rainfall-induced effect is then reflected in Jewish-Arab income inequality since Arabs were mainly agrarian whereas Jews were not. Our findings point to a substantial effect of Jewish-Arab income inequality on conflict intensification. On average, a 10-percent increase in inequality is associated with a near doubling of conflict events and two times more casualties.[1] This effect is driven by Arab-initiated attacks which also resulted in more Arab casualties. We attribute these results to the fact that Arabs – being predominantly agrarian – were particularly primed for violence during periods of bad harvest, as impoverished Arab farmers faced strong incentives to take up arms.

To be clear, our results do not imply that the Jewish-Arab conflict in Mandate Palestine is inherently driven by Arabs; instead, we merely demonstrate that the part of income inequality that is moved by rainfall shocks affecting Arab agricultural income did intensify conflict. In addition, we uncover the distinct channels that motivate the Arab-initiated violence that we have identified. By making use of detail-rich event-level conflict data, we find that inequality-driven violence was most likely an expression of Arab *resentment* against Jews, and not because of Arabs wanting to appropriate Jewish assets or Arabs turning towards combat due to a reduction in agricultural income.

Finally, our empirical results also supports the idea that rising economic inequality can intensify conflict through grievance-based mechanisms, by making existing relations of hatred more salient or meaningful (Cramer, 2003; Cederman, Weidmann, and Gleditsch, 2011). In particular, we find that the effect of Jewish-Arab income inequality on conflict intensification are especially strong when considering institutional settings where the relationship between Arabs and Jews were particularly strained, such as periods of pro-Zionist British rule or high Jewish immigration.

Our work speaks to several strands of literature. Firstly, we contribute to recent developments in examining economic determinants of conflict, where several papers have successfully identified various channels through which income levels affect violence. Dal Bó and Dal Bó (2011) and Dube and Vargas (2013), for example, identify two potential sources behind the opposing effects of income on conflict, by distinguishing between economic shocks on labour intensive versus capital intensive goods (the former reducing conflict and the latter escalating conflict); McGuirk and Burke (2017) validates a similar "opportunity cost versus appropriation" idea based on a differential impacts of food price shocks for producers and consumers in Africa; Friedman (2013) uncovers an opportunity-cost channel that explains the relationship between unemployment and

[1]These results are evaluated at the mean inequality of 3.57, where a 10-percent increase involves increasing inequality by 0.357 (which approximates one standard deviation change in annualised inequality).

violence in Rwanda.

Secondly, as we make headway toward identifying the channels through which inter-group income inequality affects violence, our work adds to the relevant empirical literature wherein the evidence is mixed (Lichbach, 1989; Olzak and Shanahan, 1996; Collier and Hoeffler, 2004; Murshed and Gates, 2005; Østby, 2008; Mitra and Ray, 2014; Guariso and Rogall, 2017). In scenarios where the conflict is two-sided, prior literature do not have information that identify the identity of perpetrators, and so can only examine how inter-group inequality co-moves with aggregate violence, but not with group-specific violence. Mitra and Ray (2014), for instance, theorise that acts of violence may be motivated by a reduction in the opportunity costs (own income) of engaging in conflict, but in addition also by potential economic gains via appropriation (income of the other group); this creates some ambiguity over the net effect of inter-group inequality on conflict, which they then resolve using data on the Hindu-Muslim violence in India.[2] Our data, on the other hand, does not require us to make any assumptions on the identity of perpetrators and victims. This enables us to make breakthroughs in sorting out the channels that underlie the income inequality-conflict relationship.

More generally, because we define income inequality along ethnic lines (Arabs versus Jews, in this case), we relate to the expanding literature documenting ethnic inequality (Baldwin and Huber, 2010; Alesina, Michalopoulos, and Papaioannou, 2016), and to the subset of studies that explores the linkage between economic inequality, ethnic identity and violence. The seminal work of Gurr (1970) and Horowitz (1985) have long pointed to the role of ethnic inequality in facilitating conflict. Other key contributions in that literature include Esteban and Ray (1994), who axiomatise the ethnic polarisation index, Esteban and Ray (1999), who discuss the possible links between ethnic polarization and conflict, Montalvo and Reynal-Querol (2005), who empirically tests the relationship between ethnic polarisation and civil wars, Cederman, Weidmann, and Gleditsch (2011) who find that horizontal inequalities between ethnic groups can promote ethno-nationalist conflict, Amodio and Maio (2017), who show that ethnic markers can be used to mobilize groups for political violence, and Huber and Mayoral (2019), who reveal that within-group inequality is associated with between-group conflict. In addition, Esteban and Ray (2011) constructs a micro-founded behavioural model for thinking about how ethnic diversity affects conflict, while Esteban, Mayoral, and Ray (2011) test the model's predictions.

Thirdly, our paper is connected to the historical literature on conflict. Specifically, we provide the first empirical paper analysing the interaction of economics and violence during the first phase of the Israeli-Palestinian conflict, thus contributing to the body of work linking income and violence in Mandate Palestine (Swedenburg, 2003; Ben-Bassat, 2013a; Ben-Bassat, 2013b).[3] This represents a worthwhile contribution because quantitative studies of war, and especially those

[2]However, they could not directly estimate those channels separately since their data do not provide information on the initiator of each violent episode.

[3]In fact, we also add to the literature examining contemporary Israeli-Palestinian conflict [see for example, Jaeger and Paserman (2008) and Amodio and Maio (2017)].

that look at the economic roots of conflict onset and intensification, by economic historians are rather limited (Eloranta, 2016). Despite being one of the most intractable conflicts of our time, the available literature that examines the causes of violence in Palestine has not paid much attention to economic factors, let alone inter-ethnic income inequality.[4] Beyond Palestine, our paper joins a number of studies that connect economic and historical dimensions of violence (Findlay and O'Rourke, 2007; Iyigun, 2008; Glick and Taylor, 2010; Gennaioli and Voth, 2015; Dincecco and Onorato, 2016; Ticku, Shrivastava, and Iyer, 2018). In particular, we show that economic shocks can induce resentment-driven violence in the presence of existing segregation when opposing ethnic groups are economic substitutes (Jha, 2013; Grosfeld, Sakalli, and Zhuravskaya, 2020; Becker and Pascali, 2019).

The rest of the paper is structured as follows. We first describe the Mandate Palestine context, specifically, the origins of the ethnic conflict, in Section 2. Next, we describe our data set in Section 3 and the empirical strategy in Section 4. Finally, we present and discuss our results in Section 5, and conclude in Section 6.

2 Background

Tensions between the Zionist movement and the Arab Palestinians first emerged during Ottoman rule. The first *aliyah* (1882-1903) brought about a marked change in the character of the Jewish community from a majority of Sephardic Arabic-speaking Jews, to secular and politically-motivated European immigrants determined to reconstitute the Jewish nation on its ancient soil (Lesch, 1979, p.27).[5] Hostilities between the two groups then intensified as the second *aliyah* (1904-1914) immigrants antagonised their Arab neighbours by prohibiting neighbouring villagers and Bedouins from exercising pasture rights on their land (against local customary laws), and stood by a policy of only hiring Jews (Ro'i, 1968).

After World War I, Palestine emerged as a distinct entity in the new post-Ottoman Middle East.[6] As a result of the Sykes-Picot agreement, it fell under British control, whose rule as mandatory power was officially guaranteed in 1920 at the San Remo Conference.[7] The League of Nation's sanctioned Mandate repeated the pledges of the Balfour Declaration (1917) whereby the British administration committed to the "facilitation of Jewish immigration and to encourage close settlement by Jews on the land, including state lands and waste lands not required for public purposes" (art. 6, the Palestine Mandate). This provided the legal basis for Jewish migration and settlement,

[4]Existing studies predominantly examine the historical and institutional roots of violence (Pappe, 1999; Kayyali, 1978; Morris, 2008) while papers that look at the role of economic determinants are mainly of a descriptive nature (Kamen, 1991; Himadeh, 1939; Metzer, 1998; Nadan, 2006).

[5]The term *aliyah* refers to the return of Jewish diaspora to Israel.

[6]The borders of Palestine were drawn in 1922 when it separated from Transjordan.

[7]The Sykes-Picot agreement was a secret pact between Great Britain, France and Russia for the division of the Ottoman Empire into spheres of control.

which lay at the very heart of the struggle between Jews and Arabs in Palestine (Tyler, 2001).

Inter-ethnic inequality during the Mandate encompassed various dimensions: income, land acquisition and ownership, and a broad range of other social indicators such as schooling and health. An important determinant of inter-ethnic income inequality is differential human capital. Jewish immigrants were endowed with higher schooling attainment and therefore found employment in more skilled occupations, such as manufacturing, that provided higher remunerations; indeed, more than two-third of Jews were working in the non-agricultural sectors at the beginning of the Mandate period. More than 90 percent of Arabs, on the other hand, were engaged in agriculture and hence paid lower wages. Furthermore, Jews received higher wages even within the same occupation. It is unsurprising, therefore, that the Jewish-Arab income gap widened as the Jewish diaspora arrived in unprecedented numbers.[8] Overall, the Jewish population grew more than seven-fold during the Mandate, and its share of the total population rose from 12% in 1922 to 32% in 1947. Rising immigration also affected land inequality, since land purchases by the Zionists led to the dispossession of land by a large numbers of Arab peasants, generating a class of landless farmers.[9] This segment of the Arab society was especially prone to being primed for conflict, as impoverished farmers faced strong incentives to take up arms during periods of bad harvest.

Of course, the alteration in the ethnic and related human capital mix *per se* need not necessarily imply greater economic inequality, had Jews and Arabs coexisted within an integrated economy that allowed for free labour and capital mobility. However, the Mandate years saw the creation of dual institutions segregated along ethnic lines – a parallel Jewish economic system alongside the existing Arab economy (Himadeh, 1939, pp. 240-245).[10] Indeed, one of the key principles of mainstream Zionism was economic, social, and cultural separation from the Arab population as a means to consolidate a Jewish national home. This included the concept of *avodah ivrit* or Jewish labour, aimed at achieving Jewish-only employment in any type of economic activity (Smith, 2007). For example, land bought by the Jewish National Fund was held as inalienable property of the Jewish people;[11] hence, anyone subletting his holding to Arabs or hiring Arab labour would

[8]Mass migration to Palestine began in 1924, because of limitations on immigration to the US under the Johnson-Reed Act. It increased rapidly after the Great Depression, spurred on by the advance of anti-semitism in Poland, the consolidation to power of Nazi Germany, and the tightening of the US migration quota system in 1929 (Swedenburg, 1999). Between 1931 and 1935, Jewish immigration grew from 175,000 to 356,000, or from 18 to 29% of the total population of Palestine.

[9]When the land was sold, all resident tenant farmers had to leave, to be replaced by Jewish settlers. This practice broke the customary right in use during the Ottoman period of transmitting tenancy titles from father to son. While until the 1930 most land sales were made by absentee landlords residing outside Palestine as a result of the demise of the Ottoman Empire, after 1930 land sales by Arab landowners had eclipsed those by non-Arabs (Swedenburg, 1999).

[10]These dual institutions were, on the one hand, independent Jewish-only institutions (covering most aspects of the Jewish socioeconomic infrastructure, such as health, education, and military), and on the other, colonial institutions by which Arabs were managed. Britain's role in fostering ethnic fragmentation, via the management of Jewish immigration and land purchase, and in recognising the legitimacy of dual institutions, have been analysed by economic historians (Metzer, 1998).

[11]The Jewish National Fund was founded at the Fifth Zionist Congress to buy and develop land in Palestine.

automatically forfeit his lease (Lesch, 1979, p.45).

Worsening economic conditions contributed to the discontent among Arabs: by 1930, 30% of all Palestinian villagers were landless and 75-80% held insufficient land to meet their subsistence needs (Carmi and Rosenfeld, 1973). A large share of *fellaheen* (farmers) doubled up as casual labourers, while others were forced by indebtedness and land expropriation to emigrate to the shanty towns of the growing urban centres of Haifa, Jaffa, and Jerusalem. Rising inter-ethnic inequality stimulated the growth of Arab resistance against Jewish settlement and pushed Arab demands of independence, the establishment of a majority rule and democratic institutions (Tyler, 2001). After a period of relatively peaceful negotiations with the British authorities in the 1920s, the lack of progress in the creation of Palestinian institutions of self-rule – distinct from what the British granted to the Zionists – began to undermine the credibility of the Arab leaders.[12] Thus, the policy of political negotiation from the top in the 1920s was eventually replaced by mobilisation from below in the 1930s, via the proliferation of political parties and movements.

2.1 Conflict

The impossibility to reconcile the Balfour Declaration, promising the establishment of a Jewish state in Palestine, with article 22 of the Covenant of the League of Nations, stating that the former Ottoman provinces were to become independent except for "the rendering of administrative advice and assistance by a Mandatory" laid the foundations for antagonistic Arab and Zionist national movements. Political Zionism saw the establishment of an independent state for all Jews as essential for the existence of the Jewish people. A religious rationale was attached to it, based on the messianic belief that the Jews were promised the land of Palestine by God. The Arab national movement opposed Zionism, demanded independence from Britain and an interruption of Jewish immigration and land sales. Control over land became a key political issue for both communities.

The 1921 Jaffa riots were the first large scale episode of violence, which started as a fight between a Jewish Marxist group and the main socialist party Ahdut ha-Avodah, but developed into an attack by Arabs on Jews. They were followed, after a few years of relative calm, by the first nationwide riots in August 1929, which began at the Wailing Wall in Jerusalem, and then spread throughout the country. This consolidated the rise of extremist militant groups on both sides, such as the Arab guerrilla bands al-Kaff al-Aswad (the Black Hand), and Jewish right-wing military organisations Brit Habirionim and Etzel (Perliger and Weinberg, 2003). Illegal Arab demonstration were staged in 1933 and 1934, resulting in clashes with police. Such rallies were followed by prolonged periods of nationwide mobilisation, the six-months general strike in 1936, which escalated rapidly into violence, and the Arab Revolt (1936-1939), one of the bloodiest events during the Mandate. To contain the violence and fight the Arab guerrillas the British allied with the Jews by recruiting fighters from their paramilitary organisation, the Haganah.

In fact, the relationship between the British and the Jews was conflicted. While the Zionist

[12]The Arab national movement initially acted via a set of Muslim-Christian Associations, the Arab Executive and the Supreme Muslim Council led by Amin al-Husayni and from 1936 by the High Arab Committee (Swedenburg, 1999).

movement was recognised by the British rulers as the intermediary to advise and cooperate with the Palestine administration in economic, social, and political matters, and generally to assist the development of Palestine, by the late 1930s Zionist leaders started criticising British ability to control Arab violence and to grant unlimited immigration to the Jewish people. The foundation of the Revisionist party was the expression of such discontent and its offshoot the Irgun, engaged in terrorism both against the British and the Arabs. Also other Jewish militant groups turned to the active use of violence during the Arab Revolt: the Irgun and other paramilitary organisations directed violence towards both British and Arabs, raiding villages, sabotaging government buildings, placing bombs in public places.

Violence continued throughout the 1940s. These years saw the formation of the Stern Gang, a radical split from the already extremist Irgun, and an overall intensification of Zionist attacks.[13] The Arab nationalist movement also resorted to violence, which escalated after the adoption of Resolution 181 by the United Nations, recommending the partition of Palestine into a Jewish and and Arab state. The conflict further intensified from 1947, taking the form of a civil war from January 1948. It ended with the Nakba, the exodus of more than 700,000 Palestinian Arabs from their homes, the depopulation of hundreds of villages and the formation of the state of Israel on 15 May 1948 (Khalidi and Elmusa, 1992).

3 Data

An important contribution of this paper stems from the construction of a unique data set which covers the Jewish-Arab conflict and the socioeconomic conditions across 18 Palestinian districts in the 1926-1945 period. Specifically, we exploit a variety of archival, primary, and secondary sources, to build three sets of variables: the first relates to conflict events and their intensity, the second measures income and labour force, by occupation, while the third includes a variety of district characteristics. Summary statistics are produced in Tables 1 and 2.

3.1 Conflict

Our conflict data set covers the period 1926-1948. It reports the location and date of each conflict event, the ethnicity of the perpetrator, as well as the number of casualties – killed and injured – for each ethnic group. We then aggregate them up to the district-year level to form a panel series. These information are primarily compiled from Arab and Jewish sources – namely the *Zuaiter's Diaries* for the former, and the *Jewish Agency* and the *Jewish Telegraphic Agency* for the latter – which account for 14% and 35% of events in our sample respectively (210 out of 430 events).[14] They were then supplemented with official documents from the British Crown and the League of

[13] Among the bloodiest events are the blowing up of King David's hotel in Jerusalem by the Irgun in 1946; Stern Gang's bombing of Haifa's marketplace in 1940; the Haifa Oil Refinery massacre whereby Irgun militants explode two bombs into a crowd of Arab workers in 1947; the Balad al-Shaykh massacre at the hands of the Palmach, an arm of the Haganah, in 1947.

[14] The data from *Zuaiter's Diaries* cover the 1918-1948 period. The book traces the whole history of the Palestinian National Movement drawing on a rich set of primary documents such as letter exchanges, pamphlets and minutes from various organisations' meetings.

Nations, such as the *Peel Commission Report* and the *Reports to the League of Nations on Palestine and Transjordan, 1922-1939*, which amount to 35.5% of events in our sample (153 out of 430 events). The remaining events were identified through various secondary sources (see data appendix for details). To address possible reporting biases, which could stem from a misreporting (deliberate or not) of the identity of perpetrators or indeed of the casualties suffered by each ethnic group, we include controls for the share of Arab and Jewish data sources in our regressions. Moreover, to further alleviate concerns about the existence of systematic reporting biases, we regress violence outcomes on Arab and Jewish source dummies, at the event level: the results, reported in Table A1, do not detect any significant effect of neither variable.

To be able to establish the ethnicity of a conflict initiator, we rely on detailed accounts described in our sources. For example, a report from Porath (1977) says that "on 15 April 1936 a group of Palestinian Qassamites attacked a Zionist convoy along Tulkarem-Nablus road"; in this case, we consider the event as Arab-initiated. We remove all events where either the sole initiator was British or where hostilities were intra-ethnic rather than inter-ethnic.[15] Where the sources do not report clearly the identity of the perpetrator, or when they indicate that both parties were equally responsible in an episode of violence, we attribute responsibility equally to each group. In the end, our sample reflects 1.09 conflict events per year in a given district, most of which were Arab-initiated. Despite this, Arab casualties were more severe than Jewish casualties, which likely reflect differences in combatant training, organisation, and military technology (Table 1).[16]

Importantly, our assignment of initiator identity only reflects the fact that a particular ethnic group had started the violent event; it does not predict (or indeed preclude) casualties suffered by the initiator group, since both the initiator and victim group could in principle incur casualties. For example, in an Arab-initiated event on 13 December 1937, "six Jews and eight Arabs died after an Arab ambush of a Jewish bus near Haifa". In another Arab-initiated violence on 23-25 June 1938, "a series of incidents including bombing, shooting and stabbing resulted in 17 Arabs casualties (2 killed, 15 injured, including seven women) and nine Jewish casualties (2 killed, seven injured, including two women and a child)". Nonetheless, in what follows, we report the effects on total as well as group-specific casualties.

3.2 Inter-Ethnic Income Inequality

We assess Jewish-Arab income inequality by using labour force and wage earnings data. Labour force data are mainly taken from industrial and agricultural censuses and from the *Statistical Abstract of Palestine, 1936-1945* (henceforth *SAP*) and the *General Monthly Bulletin of Current Statistics*

[15]Specifically, 7.5 percent of conflict events were initiated by the British, while 9.9 percent of conflict events are identified as intra-group conflict since they generated casualties from only the initiator group.

[16]That the Arabs' weapons were less sophisticated than the Zionists' ones is confirmed by many British reports. For instance, a British Intelligence memorandum sent to Winston Churchill after the Jaffa riots of 1921 reports: "The state of the dead and wounded has proved that the Arabs were mainly armed with sticks, while the Jews had revolvers" (Huneidi, 2001).

of Palestine, 1937-1948 (henceforth *Monthly Bulletin*).[17] We then match these data, by occupation, to those on wage earnings, which are derived from the *Blue Books, 1926-1938*, the *Commercial Bulletin of the Department of Commerce and Industry, 1922-1936* (henceforth *Commercial Bulletin*), the *Monthly Bulletin*, and the *SAP*.[18]

As labour markets were segmented, Arabs and Jews always received different wages even within the same occupation. In addition, Jewish and Arab occupational structures were very different: Jewish workers had a higher skill intensity and a greater participation in the manufacturing sector, whereas the Arab economy was overwhelmingly agrarian. According to census data, 86% (42%) of the Arab (Jew) labour force, excluding the tertiary sector, was agrarian (Metzer, 1998, p.122), and the equivalent shares in our sample are similar, at 94% and 43% for Arabs and Jews respectively.

We deflate these wages using commodity price indices that reflect the consumption patterns of an average Palestinian household during the Mandate. Commodity markets as well as consumption baskets were ethnically-segmented, so we constructed two separate price indices for Arabs and Jews, using price data from the *Commercial Bulletin* and from the *Monthly Bulletin.*[19]

Our measure of Jewish-Arab income inequality is the ratio of Jewish to Arab per capita income as follows:

$$\text{Jewish-Arab income ratio}_{dt} = \frac{\left(\sum_o n^J_{odt} w^J_{odt}\right) / \sum_o n^J_{odt}}{\left(\sum_o n^A_o w^A_{odt}\right) / \sum_o n^A_{odt}} \tag{1}$$

where $\{n_{odt}, w_{odt}\}$ denote labour force and wage earnings for each occupation o in district d at year t, and the superscripts $\{J, A\}$ refer to the Jewish or Arab ethnic group.[20] Interpreting this ratio is straightforward: Jews are economically better off whenever the ratio exceeds one; the higher the ratio, the wealthier Jews are relative to Arabs. On average, Jewish per capita income was 3.57 times that of Arab per capita income, with larger gaps in districts where Jewish presence was more limited and its employment concentrated in manufacturing, such as Nablus, Jericho, Jenin, Hebron,

[17] Missing years (1926, 1928, 1932, 1938, 1940) were linearly interpolated as there were no major shifts in the labour force composition during those years. See data appendix for details.

[18] These sources report information on prevailing daily rates of wages by occupation, location and ethnicity.

[19] The goods included in the Arab basket are: cheese (7 oqia), coffee (2.5 oqia), eggs (40), flour (18 rotl), kerosene (2 tins), milk (5 rotl), mutton (2.5 rotl), olive oil (2 rotl), onions (1.5 rotl), potatoes (2 rotl), rice (3 rotl), soap (0.5 rotl) and sugar (2 rotl). The Jewish basket was made of: beef (2 rotl), bread (12 rotl), butter (5 oqia), cheese (5 oqia), eggs (105), fish (0.5 rotl), kerosene (1.5 rotl), milk (6 rotl), potatoes (3 rotl), rice (1 rotl), soap (1 rotl) and sugar (2 rotl). There are many examples of price segmentation: the price of olive oil in Jerusalem in 1938 was 180 mils per rotl in the Arab market versus 288 mils in the Jewish market; the price of bread in Haifa in 1938 was 45 mils per rotl in the Arab and 81 mils per rotl in the Jewish market. Furthermore, we later show that our results are robust to using a common deflator instead of ethnic-specific ones.

[20] The six occupations in the data are: agriculture, construction, printing and stationary, metal works, wood works, and tobacco. We refer to the non-agriculture occupations collectively as manufacturing.

and Bersheba (Table 2).[21]

3.3 Other

We use other district-level variables, such as demographic composition, per capita net revenues (municipal revenues minus expenditure), as well as Palestine-wide variables such as rainfall. These are taken from a series of British government reports and publication on the Mandate (see data appendix for details). Revenues were mainly derived from property taxes, the tithe, and the animal tax; expenditures mostly directed to the departments of Police and Prisons, Posts and Telegraph, and Public Works. Rainfall data (expressed in litres) are reported annually based on Palestine-wide station-level averages, and we use them to construct a rainfall shock variable which is the difference between current year rainfall and the Mandate-period average rainfall (1920-1948).

4 Empirical Model

We proceed with empirical tests of the effects of inter-ethnic income inequality on conflict. First, consider the baseline fixed effects (FE) specification:

$$\ln\left(Conflict_{dt}\right) = \beta Inequality_{dt} + \theta X_{dt} + \alpha_d + \gamma_t + \varepsilon_{dt} \tag{2}$$

where subscripts d and t denote district and year respectively. $Conflict_{dt}$ is defined as a moving average of conflict events in the next three years, and we follow Mitra and Ray (2014) in adding a very small number (0.01) to cases where the count is zero to avoid losing observations after taking logs. We take logs due to the fact that conflict events are relatively rare – only 21 percent of district-years experienced at least one conflict event.[22] $Inequality_{dt}$ denotes Jewish-Arab income ratio as specified in equation (1) at the district-year level. α_d and γ_t are district and year effects respectively. Clearly, district effects are important because they account for any unobserved (time-invariant) district-specific determinants of conflict, for example, if certain areas are more prone to conflict, over and above economic inequality. In addition, year effects help account for unobserved time factors that drive conflict intensity.

[21] While the income ratios may be large by conventional standards, recall that they are constructed from labour force data which contain plenty of variation thanks to rising Jewish immigration. Indeed, the ethnic wage differentials implied by our data are in line with those reported by the literature (Metzer, 1998; Himadeh, 1939): on average, we find that Jewish wages were 1.8 times that of Arab wages.

[22] To allay concerns about the large number of zero counts, we also estimate a linear probability model without taking logs, and probit regressions with a dichotomous dependent variable that indicates at least one conflict event in the next three years; the results, shown in Table A4, demonstrate that the linear probability model without logs [columns (1)-(2)] and the conflict onset specification [columns (3)-(8)] deliver qualitatively similar results to our earlier findings. While we would also like to employ count data techniques such as the IV Poisson, it is difficult to do so due to the incidental parameter issue that is associated with fixed-effects instrumental variables specifications when applied to a small-N-large-T panel structure such as ours. On a separate note, although the conflict data cover 1926-1948, our period of analysis will be limited to 1926-1945 because of the three-year leads that we use for the dependent variable. It is important that we consider future (not contemporaneous) events, although we are mindful not to select leads far into the future as eventually the effect will diminish. To this end, we conduct robustness tests regarding the choice of leads in Table 9, columns (7)-(10).

In subsequent analyses, we include a vector of time-varying district controls X_{dt} which comprise Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, and the share of Arab and Jewish data sources.[23] These controls help account for demographic and institutional (e.g. British favouritism towards particular ethnic group) determinants of conflict, as well as possible reporting biases.[24] We additionally control for Jew-Arab land ownership inequality since intergroup conflict may also be dictated by land ownership inequality, over and above income inequality.[25]

Despite the suite of controls and fixed effects above, it is difficult to identify the coefficient of interest β because the identification assumption required under the FE specification, that $cov(Inequality_{dt}, \varepsilon_{dt}) = 0$, is likely violated. Broadly speaking, there are two challenges to this assumption. First, there may be omitted variables that are correlated with conflict as well as with economic inequality. For instance, the extent of grievance, which may be driven by institutional factors, could be systematically different across districts and time. To some extent, our suite of time-varying district controls does allay concerns of this type, but still our controls cannot account for all possibilities. Second, there may be reverse causality, that is, conflict could in turn affect inter-ethnic income inequality. When one group attacks the other, the economic livelihoods of the victim group could be destroyed which might then affect inter-ethnic income inequality. Although we associate inter-ethnic income inequality with future (not contemporaneous) conflict events, these events may nevertheless be serially correlated.

4.1 Instrumenting Inter-Ethnic Income Inequality

To address the endogeneity issues above, we propose an instrument for inter-ethnic income inequality: the interaction of (Palestine-wide) annual rainfall shock and pre-Mandate district-level crop intensity. The first-stage regression in a two-stage least-squares framework is:

$$Inequality_{dt} = \delta(Crop_d \times Rain_t) + \theta X_{dt} + \alpha_d + \gamma_t + \varepsilon_{dt} \tag{3}$$

where $Crop_d$ is crop intensity and $Rain_t$ is rainfall shock. The idea is straightforward: in a context where the agricultural sector is overwhelmingly Arab, unanticipated levels of high rainfall should lead to better crop harvests which in turn raise Arab agricultural income, other things being equal,

[23] We do not control for the size of an "other" group – which includes European elites, missionaries etc. – since it represented only 1.1 percent of the population.

[24] We are mindful that the choice of time-varying controls may be a little arbitrary, but are nonetheless confident of our qualitative conclusions since the coefficient of Jewish-Arab income ratio in Tables 3 and 4 never varies by more than 27 percent even when we drop all controls.

[25] Due to the lack of district-year data on land ownership inequality, we constructed a variable that interacts annual Jew-Arab land ownership inequality with a district-level measure of crop intensity (which we explain later in Section 4.1). Historically, the Jewish diaspora made large land purchases which dispossessed many Arabs of their farmland, resulting in conflict, so this variable captures the combined variation in the annual land ownership gap as well as a district's agrarian tendencies.

especially in districts with higher crop intensity.[26] The exclusion restriction then requires that rainfall shocks that vary with district-level exposure affect conflict intensity only via changes in agricultural income (and hence in Jewish-Arab income inequality).

We face two challenges in constructing the instrument. Firstly, rainfall during our sample period was only observed at the aggregate (but not district) level; this necessitates district-level variation in crop intensity for us to be able to additionally net out year effects. While we are aware of alternative data sources that offer historical rainfall at a finer level – such as the European Centre for Medium-Range Weather Forecasts' (ECMWF) ERA-20C or the University of East Anglia's Climate Research Unit (CRU) TS – they unfortunately do not provide sufficient spatial variation for our analyses.[27] Secondly, since crop intensity depends not only on district-specific climate but also possibly on other factors that determine the sectoral composition of the economy, or possibly on the immigration of Jewish farmers and their ability to own cropland (which in turn could be correlated with conflict), the district-level crop intensity measure ought to reflect pre-Mandate (rather than Mandate-period) variation in order to satisfy the exclusion restriction. To this end, we rely on cereal output data from the annual report by the Department of Agriculture and Fisheries in 1920 – in particular, on wheat and barley, the two principal rain-reliant crops grown by farmers at the time – which we then normalise by land area, to construct a crop intensity measure.[28] Figure 2 depicts the spatial distribution of crop intensity (in kilotonnes of cereals per dunam)

[26] Rainfall was extremely important for agriculture during the Mandate period as only two percent of Palestine was irrigated. Arab agricultural production was typically centred around the dry farming of cereals and depended on the natural fertility of the soil. On the other hand, Jewish agriculture made larger use of irrigation and fertilisers, thus reducing reliance on the weather (Tyler, 2001, pp. 150-198). While the British recognised that Palestine needed investments in agriculture, especially in irrigation, spending on rural development was a low priority. Moreover the Mandatory government had little control over water resources, due to the lack of formal legislation to control water for irrigation purposes. Hence, local political economy was unlikely to be a mediating factor in deciding who accessed water (unlike, for example, the Indian case in Bhalotra, Clots-Figueras, Cassan, and Iyer (2014)).

[27] Specifically, the ERA-20C and the CRU TS data are represented at one and 0.5 degree resolution respectively, which would generate far fewer cells than the 18 districts in our sample. Moreover, these datasets rely on reanalysis algorithms that seem to be heavily weighted on national rainfall, so we do not expect them to add substantial variation to our data in any case; for example, we found that the cell-level reanalysis data from ERA-20C, when aggregated to the national level, are highly correlated with our rainfall data (correlation coefficient of 0.802).

[28] We are confident about the reliability of our crop intensity measure because it appears to line up well with Mandate-period agricultural data. For example, the annual reports by the Department of Agriculture and Fisheries (1922-1946), and data from Sawer (1922) on the agricultural situation in Palestine in 1920-1921 both confirm that district outputs of wheat and barley were highly correlated between the pre-Mandate and Mandate periods. We also investigated possible alternatives such as satellite data on cropland usage from the United States Department of Agriculture (USDA) or agro-climatic data on crop suitability from the Food and Agriculture Organization (FAO). But they are less than ideal for our purpose. USDA satellite imagery does not distinguish crop cultivation from other forms of grassland (such as pastures for grazing), so we found that it was not a good predictor of agricultural activity. FAO agro-climatic data are unavailable for the period of our historical study and, even if we are willing to assume that crop suitability is time-invariant, the data measure only absolute (but not comparative) advantage in agriculture. Indeed, we find that regions with high crop suitability as defined by FAO, e.g. coastal districts, do not necessarily exhibit higher levels of agricultural activity perhaps because their comparative advantage lies elsewhere. In addition, any potential use of the USDA or FAO data would have to be aggregated from grids to the district level and so would not offer much more variation since our conflict data is only observed at the district level.

across Palestinian districts, where darker shades represent higher crop intensity. By this measure, Bersheba, Bethlehem, Jericho, Ramallah, and Tiberias are districts with the highest levels of crop intensity.

Figure 3 best illustrates the intuition behind our interacted instrument. By categorising districts into high and low crop intensity – where the cutoff is the mean of 0.003 kilotonnes of cereals per dunam – we can see that rainfall shocks are positively correlated with Arab agricultural income – and thus negatively correlated with Jewish-Arab income ratio – in the 7 high crop intensity districts (left panel) whereas rainfall has a negligible effect on agricultural income and on Jewish-Arab income ratio in the 11 low crop intensity districts (right panel).[29] An examination of the first-stage coefficients on income components in the bottom panel of Table 3 further confirms this intuition. We find that our instrument is negatively correlated with Jewish-Arab income ratio: a one-standard deviation increase in annual rainfall shock (0.104 litres) is associated with a 6.42 percent decrease in Jewish-Arab income ratio, when evaluated at the mean income ratio of 3.57.

5 Results

5.1 Main Results

We present our main results in Tables 3 and 4, to examine the effects on conflict events and casualties, respectively. For each conflict dimension, we first run FE regressions without time-varying district controls, then with time-varying district controls, and finally IV regressions where we instrument Jewish-Arab income inequality with the interaction of annual rainfall shocks and district-level crop intensity, as explained in the earlier section. We present robust standard errors that are clustered at the district level, as well as wild cluster bootstrap p values (Cameron, Gelbach, and Miller, 2008) to address the issue of having a small number of clusters (18, in our case). Going forward, we draw conclusions based on the most conservative set of results.

First, we look at conflict events in Table 3. The empirical results yield strong and robust evidence of a large effect of Jewish-Arab income inequality on the number of conflict events [columns (1)-(3)]. From the IV estimate in column (3), we find that, a 10-percent increase in inequality is associated with a near doubling of conflict events, from 1.09 to 2.09 for a typical district-year. These results are evaluated at the mean Jewish-Arab income inequality of 3.57, where a 10-percent increase involves increasing inequality by 0.357 (which approximates one standard deviation change in annualised inequality). We then decompose conflict events by the identity of the perpetrator, and the results reveal that the effect of Jewish-Arab income inequality on conflict events is driven by Arab-initiated attacks [columns (4)-(6)] and not by Jewish-initiated attacks [columns (7)-(9)].[30]

[29] Although the slope coefficients in both cases are negative and statistically significant, in terms of magnitude, the high crop coefficient is in fact 2.5 times that of the low crop coefficient.

[30] It is somewhat peculiar that Jewish-initiated violence did not respond to changes in Arab income when one might have expected Jews to react strategically with violence. This, however, can be rationalised theoretically using a Tullock contest setup, where higher between-group income inequality causes elevated violence from only the group with an income reduction, under certain bounding conditions on the degree of inequality (theoretical results available upon request).

Notably, the first-stage partial F-statistic is quite large (31.85) and comfortably passes the Stock-Yogo weak instrument critical values. Also, our IV coefficients are substantially larger than the FE coefficients, implying that there is a sizeable downward bias in the FE regressions. If one believes that conflict decreases inequality, then reverse causality may be the culprit here. Alternatively, any omitted district-year variable that is correlated with Jewish-Arab income inequality and conflict in opposing direction could also be responsible for such a bias. Finally, classical measurement error in inequality may also cause an attenuation bias that is consistent with a smaller FE coefficient.

More importantly, our instrumental variables strategy generates a local average treatment effects (LATE) estimator, which in this case relies disproportionately on variation from rural districts that respond to rainfall shocks. It is very likely, therefore, that the large effects found here are reflecting local effects from such districts. For example, impoverished Arab farmers might have very strong incentives to take up arms during periods of bad harvest given the Palestine context of ethnic segregation; that is, since Arab farmers were not able to work for a Jewish landlord (agricultural) or for a Jewish-owned firm (manufacturing) due to segregated economic institutions, violence became a very likely activity to substitute into. In other words, the fact that opposing groups are economic substitutes accentuates the impact of transition into violence (Jha, 2013; Grosfeld, Sakalli, and Zhuravskaya, 2020; Becker and Pascali, 2019).[31]

Of course, one drawback of our identification strategy is that the LATE picks up variation from agrarian districts but not from urban areas – Haifa, Tel Aviv, and Jerusalem – where most of the violence occurred. Nonetheless, it is important to put things in perspective: while the urban areas did experience most of the conflict, rural areas were not spared either. In our sample period, the three main urban districts of Haifa, Jaffa/Tel-Aviv, and Jerusalem experienced 268 conflict events (62.3%) but the rest of Palestine still witnessed a non-negligible 162 conflict events (37.7%).[32]

We then go on to examine conflict casualties in Table 4. Generally speaking, our findings here are qualitatively similar to those reported earlier. Again, our FE estimates are smaller than the IV estimates, suggesting a downward bias. Specifically, the IV estimate in column (3) implies that a 10-percent increase in Jewish-Arab income inequality is associated with almost two times more casualties, from 10.81 to 31.27 for a typical district-year.[33]

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[31] To be clear, our conjecture is that Arab farmers who are hit by poor rainfall turn to violence locally. We do not detect any effect of income inequality in one district leading to conflict onto neighbouring ones (for example, of the kind that is documented by Novta (2016) for Bosnia). Specifically, we run regressions where the explanatory variable is the income inequality of neighboring districts (instead of own-district income inequality) which we instrument using the interaction of rainfall and the share of bordering districts that exhibit higher crop intensity. These results are available upon request.

[32] In fact, one key motivation of Arab-initiated violence was to limit the expansion of agricultural Jewish settlements in the rural areas, and a large number of Jewish settlements were attacked during the nationwide riots (1929) and the Arab Revolt (1936-1939) (Near, 1992, Ch. 9). In this regard, we argue that our LATE is still relevant for understanding the relationship between income inequality and conflict intensification.

[33] We also report the casualty results separately for killed and injured in Appendix Table A2, where we demonstrate that Jewish-Arab income inequality is associated with fatalities as well as injuries.

Results from columns (4)-(9) in Table 4 also suggest that increased Jewish-Arab income inequality leads to Arab (but not Jewish) casualties. As highlighted in our LATE interpretation, our instrument moves only the agricultural part of Arab income (first stage) so the effect that we find in the second stage must be associated with the mobilisation of Arab farmers who are driven by poor harvests to the alternative activity of conflict. These Arab farmers, however, are unlikely to be experienced militants, and so are not expected to be very effective; instead, they are likely to suffer from greater odds of injury or death, which we believe explains the non-result on Jewish casualties and positive effect on Arab casualties here.

5.2 Channels

We have now established that Jewish-Arab income inequality is a strong predictor of Arab-initiated violence, where the underlying variation in inequality is driven by changes in Arab agricultural income. This, however, raises an important question regarding the mechanisms that connect inter-ethnic income inequality to violence.

To be clear, a *resentment* effect could only materialise through relative income, whereas *opportunity cost* or *appropriation* effects would stem from income levels but could nonetheless work their way through relative income. Thus, an examination of the effect of income levels on conflict intensity can help determine the relevance of *opportunity costs* and *appropriation*, but an investigation of the *resentment* channel would require a different approach, for example, by using supplementary data. Below, we examine each of the three channels in turn.

First, could our results be driven by Arabs wanting to appropriate Jewish assets? This of course seems unlikely since we know that our current IV specification relies on variation in rainfall-induced changes in (agrarian) Arab income rather than in (non-agrarian) Jewish income. Nevertheless, we can test this possibility by considering the effect of Jewish per capita income on conflict intensity, holding constant Arab per capita income. To extract exogenous variation in Jewish income, we use the interaction of the annual world steel price – obtained from Jacks (2013) – and a pre-Mandate (1912) district-level employment share of manufacturing. We know that Palestine was an importer of these raw materials for manufacturing (of which steel is the main input), and that Jews (but not Arabs) were mostly engaged in manufacturing, so the logic for this instrument is that exogenous surges in steel import price would have negatively impacted Jewish income, particularly for districts that were engaged in more manufacturing. Indeed, we expect Jewish income to respond negatively to this instrument in the first stage, and this is what we observe (Table 5). The first-stage partial F-statistic is reasonably large at 11.53 and, in the second stage we find no effect of Jewish per capita income on Arab-initiated violence (or indeed on any measure of violence), ruling out the appropriation channel.

Next, we attempt to isolate the effect of Arab per capita income on conflict intensity, while controlling for Jewish per capita income. In this case, we use the interaction of Palestine-wide annual rainfall shocks and district-level crop intensity – i.e. the same instrument in Tables 3 and 4 – to instrument for Arab per capita income. The results are shown in Table 6. We see in the first stage that Arab income responds positively to rainfall shocks in agrarian districts as expected, but

the specification seems to suffer from a weak instrument problem (F-statistics = 4.306). Hence, even though we find that Arab per capita income is associated with lower conflict intensity in the second stage, which might be interpreted as evidence in favour of the *opportunity costs* channel, the results must be interpreted cautiously and can only be considered suggestive at best.

Finally, to examine the resentment channel, we make use of our rich conflict data to identify episodes of violence that were clearly motivated by resentment. Specifically, we decompose our current count of conflict events into three types: (i) resentment-motivated, (ii) appropriation-motivated, and (iii) other. Out of 430 events in our sample, 146 (33.9 percent) are deemed resentment-motivated, and 26 (less than 1 percent) appropriation-motivated. Resentment-motivated events include religious altercations (such as attacks on religious installations) and terrorism-type episodes of violence (such as bombs in market squares, shootings and kidnappings).[34] Appropriation-motivated events, on the other hand, involve looting or grabbing of assets. We then repeat our conflict events and casualties regressions with these breakdowns. The results, reported in Table 7 demonstrate that Jewish-Arab income inequality only affects resentment-motivated violence, but not the other categories of violence (including appropriation-motivated ones).

Overall, the weight of the evidence leans toward the conclusion that the inequality-driven violence, as identified in this paper, was most likely an expression of resentment. As for the other two channels, opportunity costs remain a possibility but appropriation is ruled out definitively. In other words, it is the increased sense of ethnic hatred and grievance – not greed or the cost of taking up arms – that underlies our estimate of income inequality on violence.

5.3 IV Confounders

There are several potential objections to our instrumental variable strategy. First is the possibility that our instrument may violate the exclusion restriction if it is also correlated with per capita income which we did not account for hitherto. To address this, we verify that the conditional correlation between our instrument and per capita income is in fact statistically insignificant, so it is unlikely that the omission of per capita income in our regressions will bias our estimates.[35]

Next, we provide more reassurances about the exclusion restriction by demonstrating that crop intensity is not proxying for district-level conflict proneness other than rainfall exposure. Specifically, we look at four categories of possible district-level confounders: land ownership inequality, demographic composition, Jewish communal agricultural settlement, and pre-existing

[34] Indeed, many of our events are resentment-motivated. Some examples are: the kidnapping of three Jewish youths on 23 June 1938 in Givat Ada, Haifa (*The Reports to the League of Nations on Palestine and Transjordan, 1938*); and the Jewish-initiated market bombing at Haifa in June 1939, which resulted in 20 fatalities of which eight women and two children, and 24 wounded (Hoffman, 2016). Notice that, in the context of the Jew-Arab conflict, inter-ethnic kidnappings were likely resentment rather than appropriation-motivated, since we observe from the data that all the kidnappings – there were four instances in our data – led to deaths.

[35] It is also worth pointing out that income is generally thought to be an important determinant of conflict (Collier and Hoeffler, 2004; Miguel, Satyanath, and Sergenti, 2004), so one might want to include it as a control variable in equation (2). However, we ultimately decided not to include per capita income in our main regressions because it is potentially endogenous.

anti-Jewish sentiments. In each case, we assemble the associated data, and additionally control for the interaction of district averages with demi-decade dummies to tease out the effect of those confounders.[36]

The first category is land ownership inequality. As the Jewish diaspora made large land purchases which dispossessed many Arabs of their farmland, it is possible that crop intensity is correlated with Jewish land purchases, which in turn influences inter-ethnic hostility, especially in years of low rainfall. Alternatively, if Jewish farmers react to low rainfall by land grabbing from their Arab counterparts, then again crop intensity may become a proxy for propensity to land grab. Either scenario would raise the possibility that our climatic instrument is generating exogenous variation not in Jewish-Arab income inequality but in land ownership inequality, which then influences conflict. To ascertain this, we additionally control for the interaction of district averages of land ownership inequality (measured by Jewish-Arab land ratio) with demi-decade dummies, and find that this does not significantly change the coefficients of Jewish-Arab income inequality [columns (1)-(2) of Table 8].

The second category is demographic composition. For example, one might expect that districts with markedly large (or small) representations of Jews be more prone to conflict, due to heightened hostilities (or ease of attack). Indeed, this is especially worrisome in our sample as we know that Jews were underrepresented in districts with higher crop intensity, for example. To account for the possible interference of demographics, therefore, we include the district-averages of the Jew and Arab population, interacted with demi-decade dummies, as additional controls. These inclusions do not overturn our main results qualitatively [columns (3)-(6) of Table 8].

The third category is Jewish communal agricultural settlements (kibbutzim). Since their first establishment in the late Ottoman period, kibbutzim were systematically built in rural areas to grab land that would create a stronger claim for a Jewish state (Near, 1992). The early settlements (pre-Mandate) embraced the ideology of the "conquest of labour", according to which Jews should become agrarian labourers and Jewish farmers had a duty to employ Jewish instead of Arab workers.[37] Backed by the financial and political support of Zionist authorities, subsequent waves of *aliyah* continued the expansion of kibbutizm in the spirit of nation-building, and their exponential growth could even be regarded as a response to inter-ethnic violence during the nationwide riots

[36] Since these four categories of district-level characteristics may each be correlated with conflict as well as Jewish-Arab income ratio, we use the aforementioned interaction variables as additional controls instead of directly controlling for district-time variation, as the latter may be "bad" controls. Ideally, these district-level characteristics should be interacted with time dummies at a finer level, e.g. year, but our sample size is not sufficiently large for us to do so.

[37] The first Jewish agricultural settlements in Palestine took place during the first *aliyah*: 28 agricultural colonies were established, whose structure ranged from villages to other forms of social organisation, such as co-operatives (moshav) and communal settlements, the precursors of the kibbutz (Near, 1992). Migrants from the second *aliyah* consolidated such process of agricultural colonisation: the first kibbutz, Degania, was founded in 1909 by Russian migrants. According to the Jewish Agricultural Census, 14 kibbutzim/moshavim were established during the pre-Mandate period.

(1929) and the Arab Revolt (1936-1939).[38] Since kibbutzim, by design, influenced agricultural land ownership which could have amplified Jewish-Arab animosity, our measure of crop intensity may be picking up such effects instead of climatic exposure to rainfall. In other words, the concern is that our instrument may reflect the impact of the kibbutz movement (which may be coincidentally stronger in low rainfall years) rather than the impact of rainfall-induced inequality on conflict intensification. To tackle this problem directly, we collected panel data [from Muenzner (1947) and the Jewish Agricultural Census] on the number of kibbutzim (and moshavim) before and during the Mandate, for each district. We then use the data in the following way. In Table 8, columns (7)-(8), we additionally control for the interaction of pre-Mandate (i.e. pre-1920) kibbutz presence with demi-decade dummies, while in columns (9)-(10) we additionally control for the interaction of Mandate kibbutz presence with demi-decade dummies. These controls should help account for the impact of the kibbutz movement over time, both before and during the Mandate. In each case, kibbutz presence is a district-level count of the number of kibbutzim, and both sets of results demonstrate that the main effect of income inequality on conflict remains unaltered.

The fourth category is pre-existing anti-Jewish sentiments. It may be argued that districts that are historically populated by Arab farmers and void of Jewish farmers may be so because of deliberate Jewish exclusion from agricultural land, which itself may be a manifestation of anti-Jewish attitudes. To this end, we construct a proxy for pre-Mandate anti-Jewish sentiments using detailed evidence provided by Mandel (1965) on various (violent and non-violent) expressions of anti-Jewish attitudes.[39] We interact the total number of anti-Jewish expressions at the district level with demi-decade dummies, and use this variable as an additional control. The results, presented in columns (11)-(12) of Table 8 highlight that our main findings are not confounded by pre-Mandate anti-Jewish sentiments.

5.4 Robustness Tests

Having dealt with objections to our instrumental variable strategy, we go on to conduct a battery of robustness tests. First, districts that exhibit high income inequality (i.e. high Jewish-Arab income ratio) appear to have small Jewish populations and higher volatility in income inequality, as evidenced by Tables 1 and 2. Such districts may raise concern since one might be worried that

[38] Kibbutzim were a key target during the years of the Arab Revolt, which prompted them to introduce new strategies for defence against prolonged and concerted attacks (Near, 1992). Since Dec. 1936 kibbutzim started being strategically placed in politically sensitive areas, using a new method of expansion, based on the so-called tower-and-stockade enterprise. This exploited a clause in Ottoman law preventing an illegal building or settlement to be demolished once the roof had been completed. This settlement style led to the rapid establishment of more than 50 kibbutzim between 1936 and World War II. Several tower-and-stockade settlements were the target of Arab attacks, though all of them survived. This settlement policy was aimed to extend the potential boundaries of the future Jewish state: the majority of kibbutzim established in the 1930s and 1940s were instrumental in the definition of the borders of the future state of Israel (Abramitzky, 2018; Near, 1992).

[39] There were 27 distinct manifestations of anti-Jewish sentiments between 1882 and 1914, taking place across eight different districts: Gaza, Haifa, Jaffa/TelAviv, Jerusalem, Nablus, Nazareth, Ramleh, Tiberias. These manifestations, most of which took place in urban areas, include the emergence of anti-Zionist societies, the organisation of protests against Jewish immigration and land sale, or episodes of violence.

the changes in income inequality that our rainfall instrument elicits are mechanically driven by a small Jewish population. While this is already addressed to some extent by controlling for Jewish population size in our regressions, it is reassuring to know that our results are additionally robust to removing districts – Nablus and Ramallah – with the lowest (bottom decile) Jewish population share [columns (1)-(2) of Table 9].[40]

Second, while we have carefully deflated wage earnings using price indices that vary across Jewish and Arab consumption baskets, the procedure may be seen as ad hoc and so in columns (3)-(4) of Table 9 we also demonstrate that our findings are qualitatively robust to using a common deflator for both groups. On a related note, our current normalisation of income relies on labour force rather than population data, so our income measure reflects wage differences across Arab and Jewish workers but not necessarily economic inequality between Arabs and Jews (since the two groups may have different household size and labour force composition). To this end, we repeat our analyses by normalising income by population instead of by worker. This adjustment produces qualitatively similar results from before, but with smaller magnitudes [columns (5)-(6) of Table 9].

Third, we test the validity of our moving average approach in the construction of the dependent variables. Our conflict variables have been defined as three-year moving averages of future conflict, as we sought to capture lagged effects of inequality. In doing so, we were mindful not to select leads far into the future as eventually the effects will diminish. Given that this approach may appear ad hoc, we therefore use alternative two- and four-year moving averages [Table 9, columns (7)-(8) and (9)-(10) respectively], and find that indeed the choice of leads do affect slightly the magnitude of the effects. Conceptually, we expect grievances to be more salient and so are more likely to drive violence in the years immediately following the income shock rather than in years further down the track, and this is indeed what we observe as the coefficient size decreases as the number of lead years increases.

Fourth, we tackle the concern that our instrument may not adequately capture rainfall shocks to the agrarian economy. To this end, we test several alternative measures of rainfall shocks that are commonly used in the weather shocks and conflict literature (Burke, Miguel, Satyanath, Dykema, and Lobell, 2009; Dell, Jones, and Olken, 2014; Anderson, Johnson, and Koyama, 2016; Harari and La Ferrara, 2018; Grosfeld, Sakalli, and Zhuravskaya, 2020) in Table A3. Specifically, we identify extreme rainfall shocks as (i) a dummy variable that takes the value of one for years in which rainfall is above the eightieth percentile (or below the twentieth percentile) of the overall rainfall distribution in our sample period (Sarsons, 2015), (ii) deviation from previous year's rainfall (Miguel, Satyanath, and Sergenti, 2004), and (iii) a dummy for whether rainfall in a given year exceeds one standard deviation of historical rainfall. All the second-stage results indicate that our earlier conclusions are robust to using these alternative instruments, but our existing rainfall shock

[40]Moreover, one should be mindful that districts with fewer Jews are those where the Jewish-Arab income gap is more salient when Arab income is hit by a rainfall shock. Hence, the effect of income inequality on conflict in such districts is nonetheless likely to be consistent with the resentment channel.

instrument generates the strongest first-stage by far.

5.5 Violence Outliers

Although our conflict data do afford variation in violence across space and time, we can see from Table 1 that conflict events and casualties are concentrated in a small number of districts and years. To examine the potential influence of outliers, we therefore perform several exercises below (results shown in Table 10).

First, our conflict variable may be misspecified if low-casualty hostilities are erroneously considered to be systematic violence by one group on the other. To address this, we exclude events with less than five casualties (lowest quintile) from the sample and find that the coefficients of income inequality again remain qualitatively unchanged [columns (1)-(2)].

Next, we identify districts with the most casualties – Jaffa/Tel-Aviv and Jerusalem. These two districts experienced over 45 percent of all conflict events in our sample (211 out of 430). Moreover, they were more populous and ethnically-diverse, held larger representations of Jews, and contained larger shares of manufacturing workers (relative to other districts). Thus, by several accounts, they were "different" from the rest of Mandate Palestine. We therefore run regressions that exclude them from the sample and confirm that our earlier estimates remain robust [columns (3)-(4)].

Finally, because certain periods were extremely violent according to the data, we remove the violence data from such periods to examine the potential role of time outliers. These periods are: the Arab Revolt (1936-1939) [columns (5)-(6)], the first nationwide riots (1929) [columns (7)-(8)], and the Haganah-Irgun conflict (1944-1945) [columns (9)-(10)]. In each case, we replace the conflict events and casualties with zeros for the corresponding years and recompute three-year moving averages of future conflict. The results indicate that none of these periods influence our main findings qualitatively.

5.6 Crime

Could our empirical results reflect the classic relationship between poverty and crime, rather than being specific to inter-ethnic conflict? That poverty is an important determinant of crime is well-established (Ludwig, Duncan, and Hirschfield, 2001; Kling, Ludwig, and Katz, 2005), and it is possible that as Arabs become poorer (due to rainfall shocks) they may turn to crime which in this case could include hostilities against Jews. The question for us, however, is whether such a phenomenon could explain our empirical results. In this section, we address this possibility.

As discussed in Section 3.1, our conflict data only reflect violent events that we can directly verify to be related to the broader Arab-Jewish conflict during the Mandate. In particular, we made sure that the conflict data do not include petty theft, burglary, and assault, for example, to ensure that our conflict events and casualties are as far as possible detached from general crime.

While we do not have data at the district-year level to examine the potential effect on general crime, one proxy for general crime may be intra-group violence, and in this regard we can look at the relationship between Jewish-Arab income inequality and intra-group violence. To be clear, our data sources do not explicitly distinguish intra-group from inter-group conflict, but we can employ an objective criterion to do so by flagging conflict events as being intra-group if they

generated casualties from only the initiator group (that is, where Arabs (Jews) initiated violence which resulted only in Arab (Jewish) casualties). While this method is admittedly imperfect – since such events could also reflect inter-ethnic fights in which the initiator group was overwhelmingly inferior – the evidence in Table 11 supports the notion that Jewish-Arab income inequality does not affect either intra-Arab or intra- Jewish violence. All evidence considered, the hostilities motivated by Jewish-Arab income inequality are inter-group, not intra-group, so it is unlikely that our results are a reflection of the poverty-crime relation.

5.7 Retaliation Attacks

Our analysis of initiator identity in columns (4)-(9) of Table 3 raises yet another issue. Specifically, we have so far considered each violent incident from our data sources as a conflict event, but this approach of grouping incidents ignores the possibility that some of the observed attacks may be responses to recent incidents that were initiated by the opposing ethnic group. Since our intent is to examine how local economic factors affect communal-level violence, it is not unreasonable to consider avengers to be equally responsible for conflict intensification at the local level, and therefore to define each violent incident as a separate (local) event. Nonetheless, one could also argue that retaliation attacks should be grouped together with the initial attack, and be considered as one (global) event. The choice between these two approaches has important implications for interpreting the empirical results. For example, by classifying incidents of Arab attacks as Arab-initiated events, if in reality they were a collective response to a previous Jewish-initiated attack, we would overestimate the role of Arabs in starting conflict, and thus mistakenly attribute Arab involvement to be driven by economic factors when in fact they may simply be acts of revenge.

To address such concerns, we examine whether economic factors are correlated with retaliation attacks. First, we objectively consider all violent incidents that occur in any location following a week/fortnight of an initial incident as retaliation attacks. We then create two dependent variables: total retaliation, which is simply a count variable of all retaliation attacks, and average retaliation, which normalises total retaliation by the number of incidents in a district-year. Of the two, total retaliation represents an intentional "double-count" and can be thought of as an upper-bound measure of retaliation attacks, whereas average retaliation perhaps better reflects retaliation intensity. Regardless, the results in Appendix Table A5 show that the coefficients of Jewish-Arab income ratio are not significant across all specifications, which suggest that our main results are not driven by mechanisms related to revenge.

5.9 Heterogeneous Effects

While our results consistently show that economic inequality had a strong impact on conflict intensification, it can be argued that non-economic factors also played a key role in exacerbating hostilities between Arabs and Jews, and that such forces were more important. Indeed, economic inequality might cause conflict intensification simply because it makes existing relations of hatred more salient or meaningful (Cramer, 2003). To this end, we test in this section for the presence of heterogeneous effects; in particular, whether the effects of economic inequality are stronger in contexts where the relationship between Jews and Arabs are particularly strained. We look at two

such contexts, both affording us over-time variation: pro-Zionism tendencies under British rule and Jewish immigration (*alyiah*). Our estimating equation is therefore:

$$\ln(Conflict_{dt}) = \beta_1 Inequality_{dt} + \beta_2 Inequality_{dt} \times I_t + \theta X_{dt} + \alpha_d + \gamma_t + \varepsilon_{dt} \quad (4)$$

where I_t denotes a particular time-varying context (an indicator variable), and all other variables are as defined previously for equation (2). To establish causal identification, we instrument $Inequality_{dt}$ and $Inequality_{dt} \times I_t$ with: annual rainfall shock × district-level crop intensity, crop intensity × I_t, and the triple interaction: annual rainfall shock × crop intensity × I_t.

To construct over-time variation that indicate the degree of British colonial support for Zionism, we first consider whether the British Prime Minister in office in any given year is pro-Zionist or not. While all the British Prime Ministers who were in power during the Mandate period had accepted the Balfour Declaration and thus the de facto fulfilment of a Zionist agenda, some were stronger advocates of Zionism's ultimate political goal, namely the establishment of a Jewish state on Palestine's soil. Among them, Baldwin (1924-1929 and 1935-1937) and Churchill (1940-1945) gave unconditional support to Zionists' demands, while MacDonald (1929-1935) and Chamberlain (1937-1940) embraced a set of policies that tried to reconcile Zionists' goals with Arab aspirations for self-government. MacDonald, in particular, passed the Passfield White Paper in 1930, which limited official Jewish immigration and criticised some Zionist institutions, such as the Histadrut and the Jewish Agency, for their anti-Arab discriminatory setup. Chamberlain then echoed similar policies, as he legislated in favour of the White Paper of 1939 which limited Jewish immigration and land purchase.

An additional variation in colonial policies at the time stemmed from the High Commissioner, the highest ranking authority of the British Mandate in Palestine. Importantly, while British policies on Palestine were ratified by the British Parliament, they were also influenced by the High Commissioner. There were six High Commissioners in our sample period, all of whom exhibited different degrees of support to Zionism. Lord Plumer (1925-1928) and Wauchope (1931-1938) actively backed Zionists' demands, Chancellor (1928-1931) and MacMichael (1938-1944) implemented relatively pro-Arab policies, while Gort (1944-1945) and Cunningham's (1945-1948) administrations were widely viewed as neutral.[41]

Our construction of the pro-Zionism context combines variation from Prime Ministers as well as from High Commissioners. As one might imagine that the appointment of pro-Zionist High Commissioners may be a result of changes in conflict intensity, we construct an indicator for pro-Zionism in years where the Prime Minister in office is pro-Zionist but the High Commissioner is not. This approach makes use of plausibly exogenous variation from the timing of British Parliamentary elections, and assumes that the emergence of a pro-Zionist winner is uncorrelated with events in Palestine, while avoiding concerns arising from pro-Zionist High Commissioners being appointed

[41] See Birnbaum (1990) and the Jewish virtual library (http://www.jewishvirtuallibrary.org/high-commissioner-for-palestine) for a classification of the High Commissioners' political support for Zionism.

by pro-Zionist Prime Ministers for reasons associated with Palestinian conflict.

Defining a proxy for the Jewish immigration (*aliyiah*) context is more straightforward, insofar as the annual variation in Jewish immigration stemmed from political events outside of Palestine. We make use of Metzer's (1998) data to first determine the annual growth rate of Jewish immigration, and construct an indicator for *aliyiah* years where the change in growth was greater than one hundred percent (1929, 1932-1933, 1939, and 1943).

Our results are reported in Table 12. We appear to detect heterogeneous effects, though one must interpret them cautiously as the instruments are weaker than before. In particular, we find that Jewish-Arab income inequality had a larger impact on conflict events and casualties in years where the British exhibited stronger support for Zionism [columns (1)-(2)], in years where Jewish immigration rose sharply [columns (3)-(4)]. These findings are sensible as both indicators reflect years in which inter-ethnic relations were particularly strained.[42] In addition, the baseline effect of Jewish-Arab income inequality on conflict events and casualties remains substantial, suggesting that it is an important determinant of conflict intensity even in years where Jewish-Arab hatred was less salient.

6 Conclusions

The Mandate period was crucial in setting the foundations of the Israeli-Palestinian conflict. Indeed, it was during this time that Palestine witnessed an intensification in ethnic violence which eventually led to the first Arab-Israeli war. This paper offers empirical evidence to show that Jewish-Arab income inequality over this period did escalate inter-ethnic hostilities, and that the effects are especially strong when considering institutional settings where the relationship between Arabs and Jews were particularly strained, such as periods of pro-Zionist British rule or high Jewish immigration.

Our findings point to a large effect of inter-ethnic income inequality on conflict intensification. Specifically, a 10-percent increase in Jewish-Arab income inequality led to almost twice as many conflict events and two times more casualties. While Jewish-Arab income inequality induced more violence on average, when identifying the ethnic identity of victims and aggressors, we find that the effect was driven by Arab-initiated events which correspondingly resulted in more Arab casualties. To be clear, our results do not imply that the Jewish-Arab conflict in Mandate Palestine is inherently driven by Arabs; rather, they demonstrate that the part of income inequality that is moved by rainfall shocks affecting Arab agricultural income did intensify conflict.

It is important to note that our instrumental variables strategy generates a LATE estimator. In this case, identification relies on changes in Jewish-Arab income inequality due to rainfall shocks affecting Arab agricultural income in districts with higher crop intensity. In other words, rural areas in which many Arab farmers reside are the ones that contribute directly to LATE. Since economic institutions are segregated along ethnic lines, when Arab farmers face an adverse rainfall shock,

[42] These results are also reassuring in the sense that they suggest that our instrumental variable strategy are less likely to be driven spuriously by other coincidental trends.

they are unlikely to be able to find alternative work which leaves violence as a very likely activity to substitute into. This would explain why we obtain such substantial effects and, more generally, confirm that economic shocks coupled with existing economic segregation can create some very bad outcomes.

The LATE interpretation also clarifies the non-result on Jewish casualties and positive effect on Arab casualties, as the Arab farmers who are mobilised by poor harvests toward conflict are unlikely to be experienced militants, and are thus not expected to be very effective and instead quite likely to sustain injury or death.

Using original conflict data that allow us to distinguish perpetrators from victims, we conduct further analyses to uncover the distinct channels that motivate violence. In particular, we find that inequality-driven violence, especially Arab-initiated violence, was most likely an expression of resentment, rather than the result of opportunity costs or appropriation.

Our analyses confirm that increasing the income gap between warring ethnic groups can bring about adverse consequences for violent conflict. Moreover, inter-ethnic income inequality may be driven by different factors in different contexts; therefore, one must first uncover the reasons for inter-ethnic income gaps in order to determine the underlying mechanisms that explain its impact on conflict. As such, our efforts here help reconcile the mixed evidence in the existing empirical literature.

Finally, while the case of Mandate Palestine is of course distinct, the overlap of existing inter-ethnic hatred with economic inequality is certainly not unique to Palestinian Jews and Arabs. In this regard, our work represents an important step towards a better understanding of the relationship between inter-ethnic economic inequality and conflict that may be applicable in many other settings.

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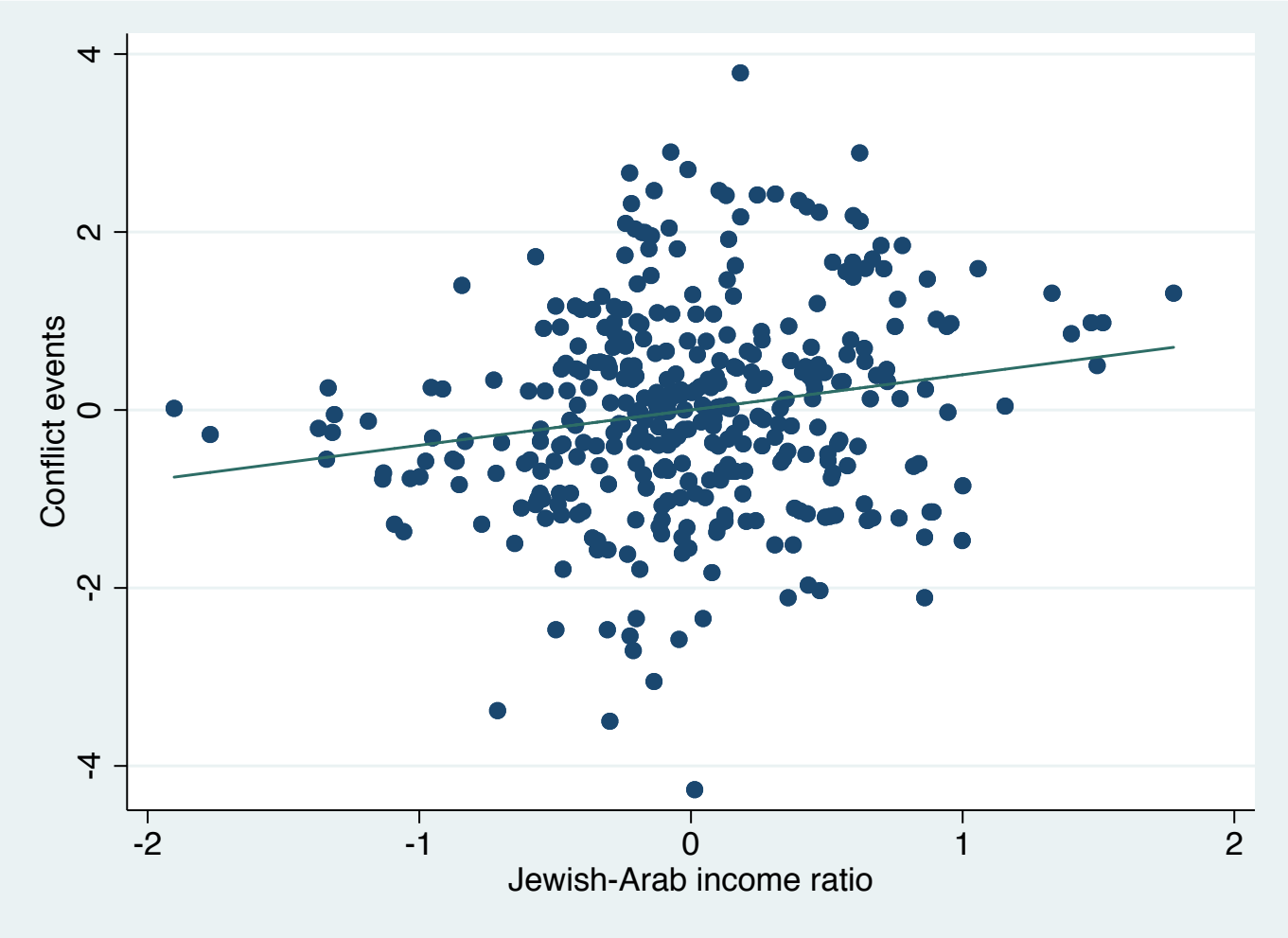
Figure 1: Jewish-Arab Income Ratio and Conflict Events



Note: This sample consists of 18 districts in the period 1926-1945. The Figure depicts the empirical relationship between conflict events and Jewish-Arab income ratio, controlling for district and year effects. Each observation depicted above is a district-year. Jewish-Arab income ratio is computed using labour force and wage earnings data from six reported occupations: agriculture, construction, printing and stationary, metal works, wood works, and tobacco. The slope coefficient is 0.396 (p=0.001).

Figure 2: Crop Intensity Map

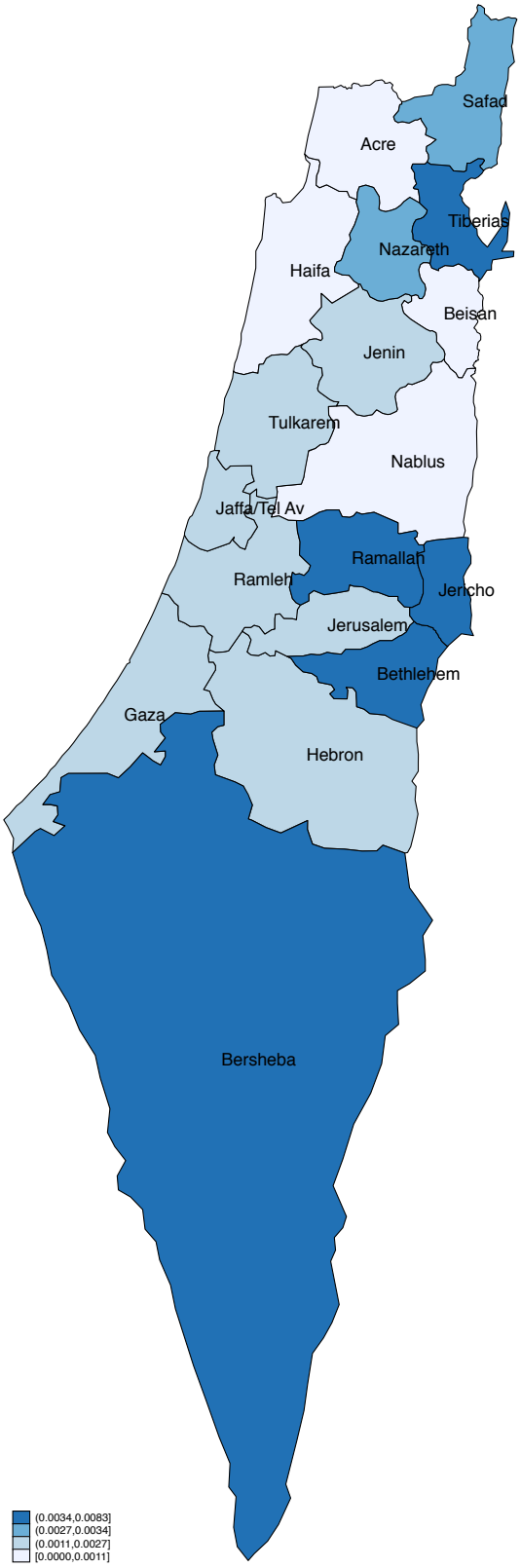
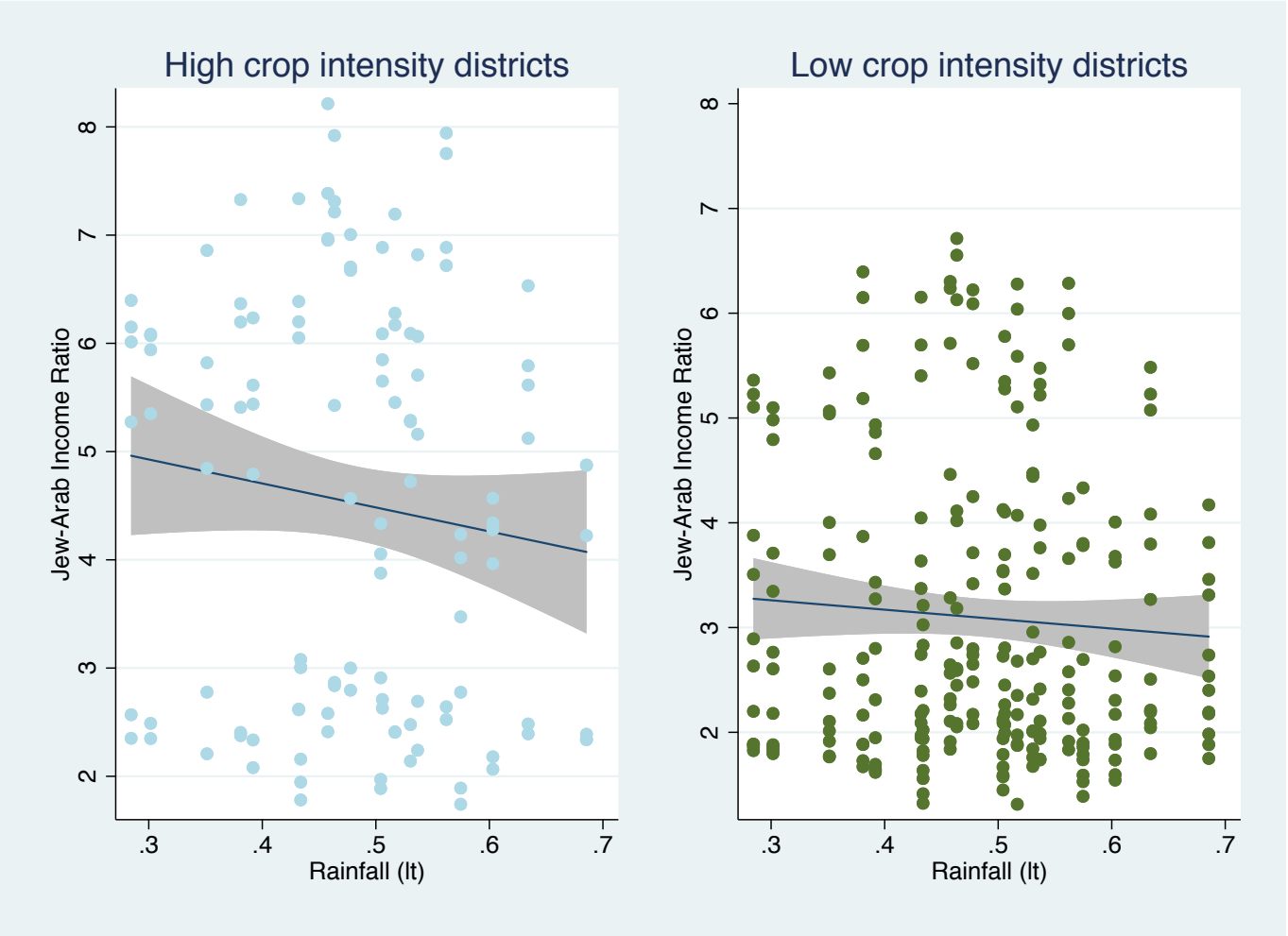


Figure 3: Jewish-Arab Income Ratio and Rainfall Shocks



Note: This sample consists of 18 districts in the period 1926-1945. Each observation depicted above is a district-year. The left panel depicts the relationship between Jewish-Arab income ratio and rainfall shocks for the 7 high crop intensity districts that produced more than the mean of 0.003 kilotonnes of cereals per dunam (120 observations), whereas the right panel does the same for the 11 low crop intensity districts (240 observations). The slope coefficients are -2.216 (p=0.014) for high crop intensity districts, and -0.900 (p=0.027) for low crop districts.

Table 1: Summary Statistics – Conflict

District	Number of conflict events per year			Number of casualties per year		
	Total	Arab-initiated	Jewish-initiated	Total	Arab	Jewish
Acre	0.73	0.32	0.11	9.32	8.50	0.27
	(1.86)	(0.95)	(0.31)	(41.06)	(39.20)	(0.88)
Beisan	0.27	0.11	0.02	3.14	1.45	1.59
	(0.77)	(0.43)	(0.11)	(9.41)	(5.63)	(5.44)
Bersheba	0.14	0.02	0.00	0.18	0.14	0.00
	(0.47)	(0.11)	(0.00)	(0.66)	(0.64)	(0.00)
Bethlehem	0.18	0.05	0.00	0.18	0.05	0.09
	(0.59)	(0.21)	(0.00)	(0.59)	(0.21)	(0.29)
Gaza	0.27	0.09	0.00	1.68	1.14	0.45
	(0.77)	(0.29)	(0.00)	(5.60)	(5.11)	(2.13)
Haifa	3.23	1.91	0.91	48.64	30.91	15.86
	(6.73)	(5.12)	(2.07)	(95.44)	(63.54)	(32.23)
Hebron	0.27	0.23	0.02	9.00	3.50	5.45
	(0.88)	(0.86)	(0.11)	(31.43)	(12.76)	(25.58)
Jaffa/Tel Aviv	4.09	2.77	0.82	38.95	20.36	16.95
	(11.90)	(9.78)	(1.63)	(87.64)	(42.77)	(48.33)
Jenin	0.41	0.32	0.00	4.32	3.95	0.05
	(1.22)	(0.78)	(0.00)	(16.70)	(15.21)	(0.21)
Jericho	0.09	0.11	0.00	0.09	0.05	0.00
	(0.43)	(0.43)	(0.00)	(0.43)	(0.21)	(0.00)
Jerusalem	4.86	2.91	1.73	41.27	25.32	13.41
	(11.44)	(9.11)	(2.98)	(73.53)	(49.69)	(33.97)
Nablus	0.45	0.25	0.05	3.27	3.05	0.05
	(1.34)	(0.87)	(0.21)	(8.88)	(8.23)	(0.21)
Nazareth	0.50	0.30	0.07	1.91	1.36	0.45
	(1.19)	(0.88)	(0.23)	(6.21)	(5.74)	(1.01)
Ramallah	0.32	0.23	0.00	6.27	3.86	0.00
	(1.29)	(0.87)	(0.00)	(29.20)	(17.90)	(0.00)
Ramleh	1.27	1.07	0.02	4.55	1.00	3.32
	(4.28)	(3.72)	(0.11)	(9.46)	(2.69)	(8.32)
Safad	0.68	0.20	0.09	11.14	6.86	3.86
	(1.64)	(0.85)	(0.25)	(28.11)	(21.33)	(12.78)
Tiberias	1.09	0.23	0.00	4.18	1.95	2.23
	(3.85)	(0.87)	(0.00)	(15.01)	(9.17)	(7.21)
Tulkarem	0.68	0.45	0.00	6.55	3.86	1.86
	(2.25)	(1.60)	(0.00)	(20.92)	(14.94)	(7.89)
Average	1.09	0.64	0.21	10.81	6.52	3.66
	(4.65)	(3.59)	(1.03)	(41.19)	(26.32)	(18.06)

Note: Based on conflict data in the period 1926-1948. Figures shown are averages over this period by district (standard deviations in parentheses). Conflict event is a count variable depicting violent incidents, while casualties denote the total number of killed and injured individuals (Arab and Jewish).

Table 2: Summary Statistics – Income Inequality and Other District Characteristics

District	Jewish-Arab income ratio	Jewish population %	Total population	Per capita net revenue
Acre	3.25	1.69	51.69	0.07
	(0.88)	(1.27)	(8.76)	(0.20)
Beisan	2.46	17.28	17.28	0.05
	(0.30)	(6.47)	(3.27)	(0.18)
Bersheba	5.56	0.22	51.11	0.01
	(1.09)	(0.58)	(11.28)	(0.04)
Bethlehem	4.89	0.11	25.81	0.06
	(1.41)	(0.06)	(2.61)	(0.23)
Gaza	3.58	0.93	102.96	0.01
	(0.58)	(0.56)	(15.25)	(0.02)
Haifa	1.94	35.00	142.85	0.10
	(0.25)	(9.62)	(51.90)	(0.27)
Hebron	5.19	0.16	73.93	0.02
	(0.98)	(0.14)	(8.97)	(0.08)
Jaffa/Tel Aviv	2.02	60.10	235.60	-0.04
	(0.35)	(10.46)	(94.17)	(0.41)
Jenin	4.89	0.02	46.89	0.02
	(0.92)	(0.03)	(6.95)	(0.04)
Jericho	5.78	10.03	4.18	0.02
	(1.41)	(5.59)	(1.40)	(0.11)
Jerusalem	2.94	43.36	160.10	0.12
	(0.75)	(2.81)	(35.69)	(0.44)
Nablus	5.13	0.01	75.94	0.05
	(1.04)	(0.01)	(9.66)	(0.10)
Nazareth	2.15	12.34	33.49	0.06
	(0.28)	(2.67)	(6.48)	(0.22)
Ramallah	6.14	0.003	42.01	0.01
	(1.35)	(0.004)	(4.47)	(0.04)
Ramleh	1.82	16.79	89.68	0.04
	(0.19)	(4.59)	(21.98)	(0.06)
Safad	2.45	9.73	42.48	0.03
	(0.30)	(1.54)	(6.56)	(0.11)
Tiberias	2.33	30.19	30.49	0.10
	(0.31)	(2.01)	(4.50)	(0.34)
Tulkarem	1.78	9.36	59.22	0.01
	(0.22)	(6.86)	(15.11)	(0.06)
Average	3.57	13.74	71.43	0.04
	(1.73)	(17.66)	(62.81)	(0.21)

Note: This sample comprises of observations in the period 1926-1945, and the figures shown are averages over this period by district (standard deviations in parentheses). Jewish-Arab income ratio is constructed using wage earnings per worker for each ethnic group. Population data are reported in thousands. Revenue data are in Palestinian Pounds.

Table 3: Conflict Events

	Conflict events			Arab-initiated events			Jewish-initiated events		
	FE (1)	FE (2)	IV (3)	FE (4)	FE (5)	IV (6)	FE (7)	FE (8)	IV (9)
Jewish-Arab	0.396	0.247	1.820	0.030	-0.017	1.249	0.085	0.035	0.335
income ratio	(0.143)	(0.161)	(0.531)	(0.111)	(0.122)	(0.414)	(0.099)	(0.110)	(0.541)
	{0.015}	{0.144}	{0.014}	{0.795}	{0.879}	{0.006}	{0.388}	{0.655}	{0.560}
District and Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y
Observations	360	360	360	360	360	360	360	360	360
First stage:			Jewish-Arab income ratio						
Rainfall shock ×			-0.348			-0.348			-0.348
Crop intensity			(0.062)			(0.062)			(0.062)
			{0.028}			{0.028}			{0.028}
First-stage partial F-stat			31.85			31.85			31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 4: Conflict Casualties

	Total casualties			Jewish casualties			Arab casualties		
	FE (1)	FE (2)	IV (3)	FE (4)	FE (5)	IV (6)	FE (7)	FE (8)	IV (9)
Jewish-Arab	0.428	0.403	2.975	0.020	0.189	1.039	0.085	0.213	2.330
income ratio	(0.236)	(0.261)	(0.803)	(0.259)	(0.269)	(0.800)	(0.234)	(0.253)	(0.889)
	{0.084}	{0.171}	{0.012}	{0.929}	{0.472}	{0.175}	{0.703}	{0.449}	{0.052}
District and Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y
Observations	360	360	360	360	360	360	360	360	360
First stage:			Jewish-Arab income ratio						
Rainfall shock ×			-0.348			-0.348			-0.348
Crop intensity			(0.062)			(0.062)			(0.062)
			{0.028}			{0.028}			{0.028}
First-stage partial F-stat			31.85			31.85			31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 5: Appropriation

	Conflict events			Conflict casualties		
	Total	Arab-initiated	Jewish-initiated	Total	Jewish	Arab
	IV(1)	IV(2)	IV(3)	IV(4)	IV(5)	IV(6)
Jewish	0.001	-0.003	-0.006	0.007	-0.008	0.013
Income	(0.005)	(0.003)	(0.004)	(0.010)	(0.006)	(0.008)
	{0.979}	{ 0.707}	{0.146}	{0.548}	{0.338}	{0.158}
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	360	360	360	360	360	360
First stage:			Jewish income			
Steel import price ×			-33.254			
Manufacturing share			(9.793)			
			{0.014}			
First-stage partial F-stat			11.53			

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab income, Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish income ratio is instrumented by the interaction of the annual world steel price and the district-level employment share of manufacturing in 1912. The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 6: Opportunity Costs

	Conflict events			Conflict casualties		
	Total	Arab-initiated	Jewish-initiated	Total	Jewish	Arab
	IV(1)	IV(2)	IV(3)	IV(4)	IV(5)	IV(6)
Arab	-0.011	-0.009	-0.001	-0.019	-0.010	-0.016
income	(0.006)	(0.005)	(0.003)	(0.008)	(0.007)	(0.007)
	{0.037}	{0.050}	{0.864}	{0.019}	{0.782}	{0.031}
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	360	360	360	360	360	360

First stage:	Arab income
Rainfall shock	53.325
× Crop intensity	(21.643)
	{0.009}
First-stage partial F-stat	4.306

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Jewish income, Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Arab per capita income is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 7: Resentment

	Conflict events			Conflict casualties		
	Resentment-motivated	Appropriation-motivated	Other	Resentment-motivated	Appropriation-motivated	Other
	IV (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Jewish-Arab	1.625	0.189	-0.304	2.072	-0.059	-0.766
income ratio	(0.548)	(0.197)	(0.589)	(0.975)	(0.278)	(0.957)
	{0.010}	{0.503}	{0.644}	{0.031}	{0.763}	{0.493}
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	360	360	360	360	360	360
First-stage partial F-stat	31.85	31.85	31.85	31.85	31.85	31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Resentment-driven events include religious altercations (attacks to religious places) and terrorism-type episodes of violence (such as bombs in market squares, non-targeted shooting, kidnapping random individuals) [columns (1) and (4)]. Appropriation-motivated events include events driven by economic appropriation (looting or grabbing of assets) [columns (2) and (5)]. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 8: IV Confounders

	Events IV (1)	Casualties IV (2)	Events IV (3)	Casualties IV (4)	Events IV (5)	Casualties IV (6)
Jewish-Arab	1.989	3.275	1.884	2.733	1.634	2.618
income ratio	(0.508)	(0.767)	(0.546)	(0.809)	(0.519)	(0.901)
	{0.007}	{0.003}	{0.009}	{0.010}	{0.033}	{0.060}
Demi-decade dummies ×:						
Jewish-Arab land ratio	Y	Y	N	N	N	N
Jewish population	N	N	Y	Y	N	N
Arab population	N	N	N	N	Y	Y
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	342	342	360	360	360	360
First-stage partial F-stat	38.38	38.38	33.63	33.63	24.34	24.34

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. Demi-decade dummies are indicators for 1926-1930, 1931-1935, 1936-1940, and 1941-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 8: IV Confounders (continued)

	Events IV (7)	Casualties IV (8)	Events IV (9)	Casualties IV (10)	Events IV (11)	Casualties IV (12)
Jewish-Arab	1.971	3.070	1.765	2.782	1.989	3.113
income ratio	(0.588)	(0.934)	(0.639)	(1.010)	(0.536)	(0.887)
	{0.017}	{0.018}	{0.028}	{0.025}	{0.011}	{0.013}
Demi-decade dummies ×:						
Pre-Mandate kibbutz	Y	Y	N	N	N	N
Mandate kibbutz	N	N	Y	Y	N	N
Anti-Jewish sentiments	N	N	N	N	Y	Y
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	360	360	360	360	360	360
First-stage partial F-stat	36.29	36.29	27.26	27.26	33.89	33.89

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Demi-decade dummies are indicators for 1926-1930, 1931-1935, 1936-1940, and 1941-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 9: Robustness Tests

	Exclude low Jewish population		Common price deflator		Income normalised by population		Average conflict in next two years		Average conflict in next four years	
	Events IV (1)	Casualties IV (2)	Events IV (3)	Casualties IV (4)	Events IV (5)	Casualties IV (6)	Events IV (7)	Casualties IV (8)	Events IV (9)	Casualties IV (10)
Jewish-Arab income ratio	1.772 (0.561 {0.030}	2.828) (0.834) {0.027}	2.091 (0.929) {0.070}	3.418 (1.372) {0.027}	1.234 (0.360) {0.011}	2.017 (0.545) {0.013}	1.424 (0.545) {0.040}	2.219 (0.744) {0.044}	2.209 (0.852) {0.014}	3.537 (1.010) {0.009}
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	320	320	360	360	360	360	378	378	342	342
First-stage partial F-stat	29.81	29.81	8.41	8.41	31.85	31.85	31.85	31.85	20.91	20.91

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945, unless otherwise stated. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Columns (1)-(2) exclude the districts with low Jewish population (bottom decile). Columns (3)-(4) use a common price index to deflate wage earnings for Arab and Jewish workers. Columns (5)-(6) use a measure of income normalised by population instead of by worker. Columns (7)-(8) and (9)-(10) use dependent variables that are moving averages of conflict in the next two and four years, respectively; the sample period thus increase or decrease by one year accordingly. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 10: Violence Outliers

	Exclude low casualty events		Exclude high conflict districts		Exclude Arab Revolt (1936-1939)		Exclude nationwide riots (1929)		Exclude Haganah-Irgun conflict (1944-1945)	
	Events IV (1)	Casualties IV (2)	Events IV (3)	Casualties IV (4)	Events IV (5)	Casualties IV (6)	Events IV (7)	Casualties IV (8)	Events IV (9)	Casualties IV (10)
Jewish-Arab income ratio	1.090	2.317	2.168	3.076	1.487	2.325	1.989	3.356	2.430	4.142
	(0.651)	(0.939)	(0.623)	(0.860)	(0.598)	(0.980)	(0.502)	(0.763)	(0.892)	(1.496)
	{0.191}	{0.044}	{0.009}	{0.011}	{0.024}	{0.026}	{0.006}	{0.006}	{0.020}	{0.017}
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	360	360	300	300	288	288	342	342	324	324
First-stage partial F-stat	31.85	31.85	22.79	22.79	18.06	18.06	32.02	32.02	17.39	17.39

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945, unless otherwise stated. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Columns (1)-(2) exclude events with less than 3 casualties (lowest quintile). Columns (3)-(4) exclude the high conflict districts (Haifa, Jaffa/Tel-Aviv, and Jerusalem). Columns (5)-(6) exclude violence data from years of the Arab Revolt (1936-1939). Columns (7)-(8) exclude violence data from 1929, year of the nationwide riots. Columns (9)-(10) exclude violence data from years of the Haganah-Irgun conflict, 1944-1945. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 11: Intra-Ethnic Violence

	Intra-Arab violence		Intra-Jewish violence	
	Events IV(1)	Casualties IV(2)	Events IV(3)	Casualties IV(4)
Jewish-Arab income ratio	-0.519	0.112	-0.708	0.206
	(0.400)	(0.100)	(0.522)	(0.185)
	{0.240}	{0.349}	{0.199}	{0.365}
District and Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	360	360	360	360
First-stage partial F-stat	31.85	31.85	31.85	31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Intra-ethnic violence is identified as such when it generates casualties from only the initiator group (e.g. Arab initiated violence resulting in only Arab casualties). Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table 12: Heterogeneous Effects

	Events IV(1)	Casualties IV(2)	Events IV(3)	Casualties IV(4)
Jewish-Arab income ratio	2.412	4.350	1.205	1.863
	(0.538)	(0.994)	(0.357)	(0.492)
	{0.007}	{0.005}	{0.006}	{0.008}
Jewish-Arab income ratio	1.112	2.292		
× Zionism indicator	(0.618)	(1.122)		
	{0.148}	{0.129}		
Jewish-Arab income ratio			0.400	0.849
× *aliyah* indicator			(0.094)	(0.166)
			{0.039}	{0.032}
First-stage partial F-stats	12.54; 3.43		18.36; 5.96	
District and Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	360	360	360	360

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam), district-level crop intensity × indicator variable, and the triple interaction of annual rainfall shocks × district-level crop intensity × indicator variable. Kleibergen-Paap first-stage partial F-statistics are reported above; the first F-statistic is from the first-stage regression with Jewish-Arab income ratio as the dependent variable, and the second F-statistic is from the first-stage regression with Jewish-Arab income ratio × indicator variable as the dependent variable. The Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.18 and 13.43 respectively.

Table A1: Jewish and Arab souces reporting bias

	Jewish initiator	Arab initiator	Jewish casualties	Arab casualties
Jewish source bias	1.153	10.042	49.143	25.334
	(0.743)	(7.148)	(28.544)	(25.964)
Arab source bias	0.018	-7.225	-12.821	4.026
	(0.494)	(6.707)	(21.685)	(19.149)
District and Year FE	Y	Y	Y	Y
N	430	430	430	430

Note: The unit of analysis is the event. Robust standard errors, clustered by district, are in parentheses.

Table A2: Conflict Casualties (Killed/Injured)

	Total casualties			Total killed			Total injured		
	FE (1)	FE (2)	IV (3)	FE (4)	FE (5)	IV (6)	FE (7)	FE (8)	IV (9)
Jewish-Arab	0.428	0.403	2.975	0.271	0.250	1.900	0.106	0.201	2.933
income ratio	(0.236)	(0.261)	(0.803)	(0.227)	(0.252)	(0.790)	(0.273)	(0.290)	(0.981)
	{0.084}	{0.171}	{0.012}	{0.230}	{0.375}	{0.043}	{0.684}	{0.470}	{0.019}
District and Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y
Observations	360	360	360	360	360	360	360	360	360
First stage:			Jewish-Arab income ratio						
Rainfall shock ×			-0.348			-0.348			-0.348
Crop intensity			(0.062)			(0.062)			(0.062)
			{0.028}			{0.028}			{0.028}
First-stage partial F-stat			31.85			31.85			31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table A3: Alternative Rainfall Instruments

	Extreme rainfall shocks		Deviation from previous year's rainfall		Deviation from historical rainfall	
	Events IV(1)	Casualties IV(2)	Events IV(3)	Casualties IV(4)	Events IV(5)	Casualties IV(6)
Jewish-Arab income ratio	2.417	4.402	2.012	4.470	1.996	3.556
	(1.431)	(2.456)	(0.898)	(1.764)	(0.691)	(1.214)
	{0.072}	{0.064}	{0.055}	{0.029}	{0.006}	{0.007}
District and Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Observations	360	360	342	342	360	360
First-stage partial F-stat	6.34	6.34	3.75	3.75	6.78	6.78

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). In columns (1)-(2), rainfall shocks are defined as a dummy variable that takes the value of one for years in which rainfall is above the eightieth percentile or below the twentieth percentile of the overall rainfall distribution during the sample period (Sarsons, 2015). In columns (3)-(4), rainfall shocks are defined as the deviation from the previous year's rainfall (Miguel et al, 2004); we lose one year of observations as a result. In columns (5)-(6), rainfall shocks are defined as a dummy for whether rainfall in a given year exceeds one standard deviation of historical rainfall. The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table A4: Linear Probability Model (without logs) and Conflict Onset

	LPM (without logs)		Conflict Onset	
	Event	Casualties		
	IV (1)	IV (2)	FE (3)	IV (4)
Jew-Arab	0.485	0.547	0.179	2.018
income ratio	(0.171)	(0.115)	(0.314)	(0.284)
	{0.022}	{0.003}	{0.606}	{0.000}
District and				
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	360	360	300	300
First-stage				
partial F-stat	31.85	31.85		31.85

Note: The dependent variable is a conflict onset indicator for at least one conflict event in the next three years. Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Table A5: Retaliation

	Total retaliation attacks				Average retaliation attacks			
	Jewish retaliation		Arab retaliation		Jewish retaliation		Arab retaliation	
	Week IV(1)	Fortnight IV(2)	Week IV(3)	Fortnight IV(4)	Week IV(5)	Fortnight IV(6)	Week IV(7)	Fortnight IV(8)
Jewish-Arab income ratio	-0.068	-0.025	-0.001	0.013	-0.072	-0.049	-0.001	-0.034
	(0.128)	(0.080)	(0.020)	(0.042)	(0.101)	(0.073)	(0.020)	(0.031)
	{ 0.797}	{0.809}	{ 0.934}	{0.713}	{ 0.780}	{0.778}	{0.927}	{0.217}
District and Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	360	360	360	360	360	360	360	360
First-stage partial F-stat	31.85	31.85	31.85	31.85	31.85	31.85	31.85	31.85

Note: Robust standard errors, clustered by district, are in parentheses and wild cluster bootstrap p values are in curly brackets. This sample comprises of observations in the period 1926-1945. Controls include Arab and Jewish population sizes, ethno-religious fractionalisation and polarisation indices, per capita net revenues, the share of Arab and Jewish data sources, and the interaction between annual Jew-Arab land ownership inequality and district-level crop intensity. Jewish-Arab income ratio is instrumented by the interaction of (Palestine-wide) annual rainfall shocks and the district-level crop intensity in 1920 (measured by cereal production in kilotonnes per dunam). Total retaliation attacks refer to the sum of retaliation attacks within a week/fortnight of an initial event; average retaliation attacks refer to the above normalised by the number of initial events in a district-year. The Kleibergen-Paap first-stage partial F-statistics are reported above; for comparison, the Stock-Yogo critical values for 15% and 10% maximal bias in size are 8.96 and 16.38 respectively.

Data Appendix

Price indices: 1923-32, Arab from the *Commercial Bulletin of the Department of Commerce and Industry*; Jewish from (Metzer, 1998, pp.238-240). 1933-35 and 1940-47, Arab and Jewish, from the *General Monthly Bulletin of Current Statistics of Palestine*. 1936-39, Arab and Jewish, from the *Retail prices and cost of living bulletin*.

Wages: Agriculture, 1922-47 from the *General Monthly Bulletin of Current Statistics of Palestine*. Manufacturing (wages for metal, wood, tobacco, print and construction): 1926-32 from *Blue Books*; 1933-1947 from the *General Monthly Bulletin of Current Statistics of Palestine*. When the sources did not report wage for a specific district/year we assigned it the share of the wage of the closest district based on past shares.

Landownership and population: Jewish, 1927: *Report and general abstracts of the Census of Jewish Agriculture*, 1928; 1929: *Report and general abstracts of the censuses of Jewish agriculture, industry and handicrafts and labour taken in 1930*; 1930: *Report on Immigration, Land Settlement and Development by Sir John Hope Simpson* ; 1936 and 1937: *Report and General Abstract of the Census of Jewish Agriculture (1936/7)*; 1944: gurevich1947. Jewish and Arab, 1922: *Report and general abstracts of the census of 1922. Taken on the 23rd of October 1922*; 1938, 1943, 1945: *Village statistics*; 1941-1942 *Statistical Abstract of Palestine*; 1947 (McCarthy, 1990). For the years in which data on Arab landownership was not available, the data was constructed by subtracting the amount of land owned by Jews and of land used for roads, rail, lakes and rivers to total land in each district.

Labour force: 1922 from *Report and general abstracts of the census of 1922. Taken on the 23rd of October 1922*; 1927 from *Report and general abstracts of the Census of Jewish Agriculture, 1928*; 1929 from *First census of industries*; 1930 from *Report and general abstracts of the censuses of Jewish agriculture, industry and handicrafts and labour taken in 1930*; 1931-7 from *Report and General Abstract of the Census of Jewish Agriculture (1936/7)*, and from *Report and general abstracts of the census of Jewish manufacture, transportation and commerce taken in 1937*; 1939 and 1943 from *Statistical Abstract of Palestine*; 1941 from *Census of Jewish Agriculture in Palestine, 1941/2*; 1942 *Statistics of wage rates, wage census. March 1942*. 1943-47 from *General Monthly Bulletin of Current Statistics of Palestine*. Missing years have been computed by linear interpolation, exploiting available data for benchmark years between 1922 and 1947 from the above mentioned sources.

Conflict: see primary sources, conflict section. Secondary sources: (Cohen, 2015; Kayyali, 1978; Shindler, 2015; Swedenburg, 1999; Hoffman, 2016; Bell, 1976; Gannon, 2008; Chazan, 2003; Walton, 2013; Morris, 2008; Pappe, 2015; Tal, 2004; Khalidi, 1988).

Revenue and expenditure: 1926-1938: *Blue Books*; 1939-1944: *Statistical Abstract of Palestine*. 1945: (Anglo-American Committee of Inquiry, 1990). Missing data for years 1946 and 1947 were filled by interpolation.

Rainfall: *Statistical Abstract of Palestine* and *Annual Report* of the Department of Agriculture and Fisheries.