LAND REFORM AND PEASANT REVOLUTION: EVIDENCE FROM 1930s SPAIN

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Abstract: We analyze the impact of failed land reform on peasant conflict in Spain before the Civil War using a novel, municipal data set with monthly observations of peasant conflict from April 1931 to July 1936. We find temporary occupations of land were rare and not correlated with either organized reaction to land reform or the existence of a large pool of beneficiaries. Potential beneficiaries of reform struck more often in the first period of land reform. There is some evidence that effective land reform implementation reduced strikes, in towns with a legacy of domination by a noble family. We argue both sets of evidence suggest faster re-distribution would have reduced conflict.

Keywords: land reform, conflict, revolution, re-distribution, property rights, peasantry, agrarian economies

1 Introduction

Does land reform cause greater levels of rural conflict? According to the literature on political regimes and transitions, re-distributive policies can appease bottom-up revolutionary pressures and social conflict (Acemoglu and Robinson, 2005). While developed economies re-distribute by taxing wealth and on income and giving away

social transfers, in developing economies characterized by a large presence of the agricultural sector, re-distributive policies have generally taken the form of land reform.

However, it has been argued that drastic land ownership re-distribution can sometimes create more conflict. Democratization and the deployment of pro-poor policies generally go hand in hand with a reduction of repression favoring the collective action of peasants. Land reform, in addition, rises the expectations of peasants, which leads to more demands from this social group that, if not met, can increase the levels of conflict (Finkel, Ghelbach, Olsen, 2015: 985).

However, it is also the case that conflict arises out of the state's inability to deploy land reform. As land is the main asset in agrarian economies and land is illiquid and immobile, land reform creates sharply defined groups of winners and losers (Luebbert, 1991; Boone, 2014). In this context, winners will do whatever it takes to speed up reform, while losers have every incentive to block reform. When this is the case, the logic of collective action favors landowners, who form a cohesive, small and wealthy group able to co-ordinate collective action more effectively. The ability of landed elites to block reform means that, unless revolution, conquest or unconstrained executive power precede land reform, land reform can be unenforceable. With the patience of the landless wearing thin, failed land reform can trigger peasant rebellions, revolution, and civil wars.

The existing empirical studies point at the conflict enhancing effects of incomplete land reforms. In Colombia, the positive effects of land reform on rural insurgency were largely circumscribed to large-scale reform in lands threatened by

guerrilla activity, with incomplete land reform increasing conflict elsewhere (Albertus and Kaplan, 2013). In Russia after 1861, local elites captured land reform causing dissatisfaction with the terms and pace of reform and widespread conflict (Finkel, Ghelbach and Olsen, 2015: 985). In Brazil, invasions of farms by squatters and other forms of violent conflict were a mechanism to force state actors to enforce or speed up land reform (Alston, Libecap and Mueller, 1999, 2000). In all cases, partial land reform increases peasant conflict.

Spain in the 1930s has been included among the cases of "failed" and "incomplete" land reform, associated with landlord resistance, bottom-up mobilization of the landless peasantry, revolution, and civil war. The classic historical study on the period bears the self-explanatory title *Agrarian Reform and Peasant Revolution. Origins of the Civil War* (Malefakis, 1970). More recently, a leading expert on 1930s Spain argued that peasant radicalization happened "when it became clear how slowly agrarian reform was progressing (Casanova, 2010: 47)." Expectations generated by land reform led to conflict and violence in those areas where peasants would have benefitted the most from reform.

Yet, to our knowledge, no systematic test of the impact of land reform on rural conflict and peasant collective action in 1930s Spain has been so far undertaken. In this paper, we present a novel data set of local rural strikes and conflict in two *latifundia* provinces of Spain, the provinces of Jaén and Córdoba, in the period 1931-1936. Both experienced substantial variation in local levels of conflict and degrees of local exposure to land reform legislation and land reform implementation.

We contribute to the literature on the direct impacts of partial land reforms on rural conflict in two ways. Firstly, we add to this literature a case of land reform under democracy. The comparative literature on land reform under democracies shows that a minimal degree of institutional quality and democratic rules slow down ambitious re-distributive democratic agendas (Albertus, 2015; Bardhan and Mokherjee, 2010). At the same time, greater protection of workers' rights and softer repression means various segments of the working class, including the rural laborers, could organize more easily (Domenech, 2013). In addition the position of the owners of land was much weaker than in the other cases in the literature. Second, land reform happens at various stages from low- to mid-levels of development with important variation in levels of state capacity. Here we address the case of a state at higher levels of capacity than is usually the case in countries after revolution or war, and also with higher capacity than the Russian state in the second half of the 19th century or many Latin American states in the 20th.

2 Land Reform in 1930s Spain

Democratization in April 1931 resulted in the weakening of landed elites and the emergence of a dominant coalition favorable to large-scale land ownership re-distribution towards tenants and laborers. As a result, article 44 of the December 1931 Spanish constitution claimed "national wealth (...) is subordinated to the interests of the national economy (...) Property can be socialized." Some of the initial laws of the new government offered greater protection to rural tenants. There were several interventions in rural labor markets like laws limiting the mobility of laborers during harvest months (Domenech, 2013). A Land Reform law was passed in September 1932.

The Law was circumscribed to 14 provinces in Central and Southern Spain, more specifically in Andalusia and Extremadura and in the provinces of Ciudad Real, Toledo, Albacete (in New Castile) and Salamanca (in Old Castile). The Institute of Agrarian Reform (IRA, *Instituto de Reforma Agraria*) was the government body in charge of implementing land reform. In the paper, we exploit characteristics of the law as to which farms were to be expropriated and who was going to benefit for re-distribution to generate variation in the intensity of land reform treatment.

Article 5 gave a detailed description of the types of farms that could be confiscated. Clauses 12 and 13 of article 5 were the most consequential. Clause 12 stipulated that farms that had been leased for 12 or more consecutive years could be expropriated. Clause 13 established upper limits to the size of farms, with these sizes finally determined by each IRA provincial committee on the basis of soil characteristics and crops. Articles 6 and 7 of the law defined the exceptions (communal lands) and created the register or inventory of farms to be expropriated giving each IRA provincial committee a year to complete the task of identifying and registering the farms to be confiscated. We use local information from the Inventory of Expropriable Land, as well as information on farm sizes and land ownership inequality to proxy the local level of land re-distribution, as well as the level of pre-reform local inequality.

Articles 10 of the law stipulated the creation of a Peasant census, which would be the basis for settlements on expropriated land. The Census counted the number of laborers, tenants and small owners in the towns affected by the law. We construct local measures of the number of beneficiaries from land reform using information from this Census.

In the paper, we will exploit differences in land reform implementation to analyze the effect of land reform deployment on conflict. The work of the *Juntas Provinciales* started without significant delays in 1933, so that by late 1933 an Inventory of Expropriable Property was completed. In Andalusia, and more specifically in the provinces studied in this paper, land reform had the potential to alter dramatically the existing distribution of ownership. In the province of Córdoba Malefakis estimated 47 per cent of cultivated lands was affected by land reform (Malefakis, 1970: 210). In Córdoba, 88 per cent of the land earmarked for expropriation fell under clause 13, i.e. farms exceeding the thresholds of maximum farm size (López and Mata, 1993: 42; Pérez Yruela, 1979: 260). This ratio was 79 per cent in Jaén.

In the immediate years after the passing of reform, progress was modest. In the provinces targeted by reform, only 8,600 families had settled on expropriated properties by the end of 1934. Only 211 landless families had been settled by the end of 1933 in Córdoba and probably 205 in Jáen (Malefakis, 1970: 281). In 1934, only an extra 534 peasant families were settled (López and Mata, 1993: 102). In Jaén, the progress of land reform was even more limited.

Temporary seizures of land via decrees of intensification of cultivation (*laboreo forzoso*) were often used in Extremadura (South West of Spain), with temporary settlements of 32,570 families on 98,355 hectares by October 1933. But intensification of cultivation was very sparsely used in Córdoba and Jaén, we only find 100 peasant families settled on meager 280 hectares under temporary seizures of land (Malefakis, 1970: 242).

In February 1936 a coalition of Leftist parties won the general election and started in earnest to accelerate expropriations. Only in March and April of 1936, more than 400,000 hectares were seized and over 94,000 families were settled. From April to July, an extra 111,000 families were settled on 572,000 hectares of land. But, as in 1933, the largest number of settlements happened in Extremadura (83,767 peasant families and an 85 per cent of the peasant census). In comparison, there was a more modest number of settlements in Andalusia. The expropriation of 34,395 hectares in Córdoba made possible the settlement of 5,300 families, about 10 % of landless household heads in the Peasant census of the province. In Jaén, 693 families settled on 8,271 hectares in Jaén, only 2 per cent of the recorded number of landless households in the Census of Peasants (Malefakis, 1970: 378; Robledo, 2014: 77; Garrido, 1979: 25).

Such low figures for Córdoba and Jaén underline the very limited progress of land reform in Andalusia until the Civil War (1936-1939). Things changed quickly in the first months of the war with quick collectivizations of land in areas controlled by the Republican government. Some historical studies estimate 65 per cent of land was expropriated in Jaén and 24 per cent in Córdoba. In the case of Jaén, about 80 per cent of the confiscated land was exploited collectively by peasant co-operatives (Martínez Ruiz, 2006: 130).

3 Data

This paper studies the Southern provinces of Jaén and Córdoba to understand the determinants of rural conflict in Spain before the Civil War. High land ownership inequality, extensive plans to confiscate the lands of the largest landowners, and high

rural conflict characterize both provinces, although there is substantial local variation in both rural conflict (the dependent variable) and in the local impact of land reform. Both provinces have been the subject of very detailed historical studies on rural conflict (Pérez Yruela, 1979; Cobo Romero, 1992, 2003). We use these to compute monthly local indices of conflict.

Our analysis necessarily stops in July 1936, when the civil war broke out. After the general strike of peasants organized by the FNTT in June 1934, the number of strikes collapsed to almost zero, as many union offices were closed. Strikes and conflicts resumed at a lower level after the Popular Front victory of February 1936. We exclude from the analysis the period of complete repression of peasant collective action from July 1934 to January-February 1936.

3.a Dependent variable. We measure local rural conflict from April 1931 (the start of the Second Republic) to July 1936. We have a continuous panel of 39 consecutive months from April 1931 to June 1934 (both included) and a second period from March 1936 to July 1936 (both included). We first consider land invasions and other attacks on established property rights property, from temporary seizures of land to trespassing by groups of organized peasants. We find no evidence of illegal squatting and temporary occupations of land, so our first conflict measure considers short invasions of farms by groups of peasants with the purpose of damaging crops, enforcing picket lines, performing unsolicited tasks in a farm, or gleaning.

Our second variable of interest is peasant strikes. We first look at the extensive margin, with the variable taking value 1 if a strike or more started in a dyad month-town

and 0 otherwise. We also look at the intensive margin by looking at the number of days on strike in each municipality-month dyad and the number of times peasant strikes in every municipality-month dyad appeared in newspapers. We compute monthly estimates of impact of peasant strikes in each town-month dyad as a weighted average of the number of hits in each month using the following formula:

Total impact in municipality *i* in month *t*= (.25*hits in the provincial press) + (.75*hits in newspapers in other provinces) for strikes of municipality *i* in month *t*.

Our main sources are the detailed historiography on 1930s rural conflict in the two provinces and Boolean searches for each town in digitized contemporary newspapers of the period (see online appendix, section A.1).

Maps 1 to 4 display the spatial variation in the dependent variables . To construct those maps, we add up all instances of conflict at the extensive and intensive margin to construct local estimates of conflict in the period 1931-19136. Map 1 shows the pattern of land invasions and related conflicts at the extensive margin. Map 2 displays local count of strikes for the period. Maps 3 and 4 display intensive margins of strike intensity (cumulative counts for each municipality of days on strike and newspaper hits). Compared to "invasions", strikes at both the extensive and intensive margin were more concentrated in several areas in the province of Córdoba (left half of the map), especially in the towns surrounding the city of Córdoba and in the so-called *Campiña* region to the South-East of the capital. Other towns in the province away from this cluster also throw a large number of conflicts (like in Villanueva de Córdoba in the

North East of the province or Palma del Río in the South west). In the case of Jaén, strikes cluster in the area of Villanueva del Arzobispo and Villacarrillo in the North East of the province and Alcalá la Real and Alcaudete in the South Western part of the province.

INSERT MAPS 1-4 HERE

3.b Independent variables.

Land reform treatment intensity: The main hypothesis in the literature is that land reform triggers high levels of conflict, especially when it is not comprehensively deployed and enforced. We consider two treatment periods. A first one starts with the deployment of the land reform law of September 1932. The second with the acceleration of land reform after the victory of the Popular Front in the elections of February 1936. Defining the window for the first period is fairly arbitrary and we have experimented with several treatment windows. We present results using a window from April 1933, when provincial IRA committees were constituted (López and Mata, 1993: 95) to June 1934. The second treatment window (of quickly deployed land reform) goes from March 1936 to July 1936, coinciding with the months of the Popular Front government. In the case of the first period, our main results are robust to the choice of different treatment windows (in the online appendix we offer results with alternative treatment windows October 1932-June 1934 and December 1933-June 1934).

We start by assuming there was a general effect of partial land on the landless. We use several proxies of the intensity of land reform treatment. In all cases, the intensity of treatment during the period in which land reform was active is in fact very

strongly related to inequality, both in terms of land ownership inequality and social structure. We can use pre-treatment estimates of inequality as a proxy for expected land reform in the treatment period. We will label the various proxies of inequality and land reform intensity in town i '$INEQUALITY_i$'.

Firstly, we identify intensity of treatment to the potential supply of expropriable land. We expect in these towns with a larger share of available land could have more invasions and trespassing than in towns with a smaller supply of available land. At the same time, the presence of a small and cohesive group of landowners in the most unequal towns meant unequal municipalities could have seen greater landowner collective action and more resistance to land reform implementation (Albertus, Brambor and Ceneviva, 2016). This could at the same time depress and spur the organization of the landless peasantry.

Secondly, we proxy the intensity of land reform treatment via the demand-side by looking at the share of potential beneficiaries of land reform in the local population. Because beneficiaries were empowered by reform or perhaps, because they were not satisfied with the pace of reform, we expect that towns with a larger share of beneficiaries to see more conflict, both in terms of strikes and short invasions.

Finally, we interact both supply-side and demand-side measures of treatment intensity to capture the degree of polarization, expecting conflict to be highest where there is a greater proportion of beneficiaries of land reform and a small number landowners. Typically, polarization leads to more conflict (Esteban and Schneider, 2008; Esteban and Ray, 2008).

In a second set of regressions, we allow for the existence of two different treatment effects. To a first effect of land reform intensity, we add a second effect of land reform deployment. The expectation is that conditional on the *ex ante* level of land re-distribution, the deployment of re-distribution reduced conflict vis-à-vis the towns with no deployment. We code a dummy variable taking value 1 if the town saw some land reform deployment during the period of preparation and implementation of land reform and 0 otherwise. This was generally the case in towns with farms owned by Grandee families in 1933-1934 and 1936 and with towns in which some farms were temporary seized under the decrees of *laboreo forzoso* (compulsory cultivation) during the government of the Popular Front. We circumscribe the analysis to the province of Córdoba because there were very few families settled in the province of Jaén throughout the period.

To capture measurements of the intensity of the land reform treatment based on the amount of potentially expropriable land, we look at various dimensions of pre-reform land ownership inequality. We start with cadastral information given in Carrión (1975 [1932)]. The Cadastre was compiled in the provinces analyzed here in the early 1920s (Pro, 1992) and Carrión (1975) used this information to compile the share of local area taken by farms of more than 250 hectares (‘*% area*’). Because the lower bound of maximum farm size was established by the 1932 law at 300 hectares for cereal growing areas, we can use Carrión’s estimates to measure pre-treatment inequality and the expected level of reform intensity in each municipality. Still from Carrión (1975), we retrieve the share of total taxable agricultural income taken by landowners with a taxable income from land above 5,000 pesetas per year (‘*% tax’*). Carrión (1975)

estimated medium-sized properties had a taxable income of 1,000 to 3,000 *pesetas* per year. Therefore, '*% tax*' would be an estimate of local land ownership inequality, which also takes into account variation in the productivity of land.

In addition, we use information from the Inventory of Expropriable Farms – *Registro de la Propiedad Expropriable*- compiled in 1933 by IRA agronomists (Pérez Yruela, 1979, 255-60; Garrido González, 1990: 382-4). The Inventory was a register of farms earmarked for confiscation on the basis of the various causes of confiscation established in article 5 of the law of the land reform of September 1932. Using this data we construct indices of local exposure of land reform as the ratio of total expropriable area to total local area.

We approximate the intensity of exposure to land reform by looking at the demand for land reform proxied by the number of potential beneficiaries in each town. The IRA collected a Peasant Census in 1933-35 to establish the number of landless or near landless families that had to be settled. This Census gives a count of household heads in various peasant groups (laborers, tenants, and small owners) (Brel and González, 2013). We extract the number of peasants and calculate the local ratio of poor peasant household heads as a proportion of the overall population (‘*% poor’*). We do the same with the share of household heads classified as rural laborers in the total population (‘*% laborers*’). Both are measures of local inequality and the share of potential beneficiaries of land reform in the local population.

Finally, we look at interactions of the supply- and demand-side to capture the effect of polarization (high land inequality and a large number of beneficiaries). Here we present results with the interaction ('*%expropriable*') and ('*%laborers*').

The three sources (Cadastre, Inventory, and Peasant Census) have gaps, with information missing in some towns. Of 164 towns in the data set, the Peasant Census does not report information on 16. In Carrión (1975), there is missing information on 33 towns. There is no information collected for the 34 towns in the Inventory of Expropriable Farms. In section A.2. of the on-line appendix A2, we discuss the potential biases introduced in our database from missing sources. We conclude that there are selection biases but that the most unequal towns are not excluded from the data set, suggesting that the assets and income of the wealthiest families were not being hidden. Because selection biases most probably eliminate the most egalitarian towns from the analysis. In order to avoid losing observations and introducing selection biases, we give a 0 value to towns with missing information for the "exposure" variables collected from incomplete sources. In addition, we have coded a dummy variable taking value 1 if the town was missing in the source used to compile that variable.

3.c Controls. The analysis takes into account several controls. We include local population in 1930 to take into account that there might be reporting bias in favor of larger towns. In addition, peasants living in larger towns might have spillovers from the collective action in other sectors, translating into better organization or more capacity for collective action. Greater population is also correlated with observed and unobserved locational advantages like better land, more water, or greater access to markets. We also include the productivity of land as a control, proxying with average

soil quality in the town. We construct a Soil Quality Index using information from FAO's Harmonized World Soil Database, which gives information on average soil qualities for grids of 30-arc seconds (a horizontal grid spacing of 30-arc seconds represents 0.008333 degrees or approximately 1 km).[1] Using the formula by Brady and Weil (2008), the index is the average of 5 topsoil properties normalized to an index that ranges from 0 to 10. The entire index is then multiplied by 10 to vary between 0 and 100.

$$SQI = \frac{1}{5} \times \sum_{t=1}^{5} S_t \times 10$$

Finally, we throw in several geographical controls like average altitude, longitude and latitude, to control for unobserved characteristics plausible correlated with these variables. This for example could the case of the propensity of local collective action to be repressed (since repression is more difficult in isolated and rugged terrain) or unobserved income and wealth effects associated with variation in crops and water supply.

4 Panel regressions, 1931-1936.

We now turn to the statistical analysis. We start with simple difference-in-difference regressions in the panel April 1931-July 1936, with the period of harsh repression from July 1934 to February 1936 excluded from the panel. Our first model looks at the effect of expected land re-distribution on peasant protest, without taking into account the very limited amount of land reform deployment in 1933-34 and 1936 for towns with Grandee property.

[1] http://www.fao.org/soils-portal/soil-survey/soil-maps-and-databases/harmonized-world-soil-database-v12/en/

$$Y_{i,t} = \alpha + \beta_0 INEQUALITY_i + \beta_1 (INEQUALITY_i \times period\ 1)$$

$$+\beta_2 (INEQUALITY_i \times period\ 2) + \beta_3 period\ 1 + \beta_4 period\ 2 + \gamma X_i + \delta Z_t + \mu_{i,t} \quad [1]$$

Where $Y_{i,t}$ is the conflict variable, measured at the extensive (one conflict event or more observed in a given month) or at the intensive margin (number of days on strike per month or number of newspaper hits per month), $INEQUALITY_i$ is the variable measuring the level of inequality before the treatment period and the intensity of land reform treatment. Period 1 and 2 are dummy variables capturing the treatment windows. Period 1 is a dummy variable taking value 1 for all towns between April 1933 and June 1934 (both included) and 0 in other months. Period 2 is a dummy variable taking value 1 for all observations between March 1936 and July 1936 and 0 otherwise. Regression results with alternative treatment windows in the first period can be found in the online appendix (section A.5).

X_i captures time-invariant characteristics of towns, such as population in 1930, average soil quality, and geographical controls like altitude, latitude and longitude. Z_t is a linear trend for the 44 months included in the analysis from April 1931 to July 1936 and 11 dummies for each month except January. $\mu_{i,t}$ is an error term.

In the case of time-series, cross-sectional data like the ones used here, serial autocorrelation and the structure of lags are serious problems. Beck (2001) and Beck and Katz (1995) recommend lagging the dependent variable and using unit and time dummies and clustered standard errors as quick fixes. Plümper, Troeger and Manow (2005) dispute this as the included variables absorb potentially important time-series

variation. In the appendix, section A.4, we report estimates with extra controls for serial and spatial correlation.

It is important to clarify that coefficients β_1 and β_2, the treatment effects of land reform intensity in periods 1 (April 1933-June 1934) and period 2 (March-July 1936). do not measure the impact of land reform in towns with high inequality and a large share of landless peasantry relative to a period in which potential beneficiaries did not anticipate land reform. Rather, we have to see the pre-treatment period as one in which potential beneficiaries expected land reform, but were not aware land reform was going to fail. It is generally the case that peasant collective action is repressed in periods with no land reform, therefore comparisons between periods with land reform, when repression is often lifted, and others with repressed peasant collective action would require the estimation of latent conflict propensities in the period of delayed reform.

In addition, the validity of our differences-in-differences framework would be compromised if peasants anticipated in 1931-1932 (pre-treatment period) a slow and incomplete land reform. However, there is no reason to think this was the case. The 2nd Republic was welcomed euphorically by the working classes. There were no comparable historical periods of democratic governments trying to implement ambitious plans of land reform. Most peasants were confident that the time of "*reparto*" (re-distribution) had come.

Before we move on to the estimation of equation [1], we display the conflict data to see if the hypothesis that land reform increased conflict in the treatment period has some real empirical basis. Figures 1 and 2 plot strike propensities (extensive

margin) for the second and top quartiles of our measure of land reform intensity, $INEQUALITY_i$, measured from the demand-side (*% laborers*) and the supply side *(% expropriable*). Both figure 1 and figure 2 suggest that the various estimates of $INEQUALITY_i$ were largely irrelevant to explain protest in the pre-treatment period (1931-32). There is an intensification of conflict in 1933 and 1934 and conflict in this period intensified more prominently at higher levels of $INEQUALITY_i$. However, the differences of mean strike propensities at different quartiles of land reform are not statistically significant. In the second treatment period in March-July 1936, peasant conflict remained at low levels and measures of expected re-distribution were irrelevant to explain protest. We have not reported figures with the intensive margin of strikes because they look similar to those based on the extensive margin.

Figure 3 reports the mean propensity to invade measured at the second and fourth quartiles of $INEQUALITY_i$. Data here tell a slightly different story than in figures 1-2, although in this case the choice of months May-June might not be ideal for invasions. As in the previous cases, expected land reform does not seem to affect conflict in the pre-treatment period, nor does it affect invasions in the Popular Front period (period 2, March 1936-July 1936). For May and June in 1931 and 1936, our data set reports zero cases of "invasion". In treatment period 1, 1933 does not see an intensification of conflict, whereas 1934 saw a large increase with respect to the other years. Invasion propensity is higher the higher the potential for land reform, yet standard errors of mean invasion probabilities are very large, especially for the second quartile of '*% laborers*'. On the basis of figures 1 and 2, it would seem there is some basis for the claim that the credible promise of land reform in 1931-1932 and in 1936 reduced peasant conflict and that the delays in land reform in 1933-1934 intensified

conflict. However, this needs to be qualified by the visual evidence for invasions and trespassing displayed in figure 3, which lends much weaker support to the hypothesis linking failed land reform and peasant conflict.

INSERT FIGURES 1, 2 AND 3

We now turn to the statistical analysis. In table 1, presents the summary statistics of the main variables used in the regressions. Table 2 displays the main correlations. There is a high correlation between the various definitions of strikes and the extensive and intensive margins, as well as the various definitions of the land reform exposure variable ($INEQUALITY_i$), especially in the case of our supply-side measures of treatment intensity are *'% area'*, *'% tax'* and *'% expropriable'*, as defined in section 3.a.:

INSERT TABLES 1 AND 2 PLEASE

In table 3, we present estimations of a linear probability model with a dummy variable taking value 1 if peasants entered illegally one or more large estates in town *i* in month *t* and zero otherwise. Despite the difficulties in collecting instances of invasions and the necessarily arbitrary definition of these events, we consider invasions a more genuine example of explicit challenges to authority than strikes.

When analyzing the extensive margin of invasions (and peasant strikes below), the variable is dichotomous, so that panel logit or probit models could be used. However, the hypothesis tested here requires that our estimated equations have several

interacted variables that compromise the interpretation of marginal effects (Ai and Norton, 2003). Table 3 displays the regressions with several approaches to measuring $INEQUALITY_i$: in columns I, II, III we proxy intensity of treatment from the supply of land, columns IV and V from the demand for land reform and in columns VI and VII with an interacted supply-demand term capturing polarization. We expect the coefficients on the different measurements of $INEQUALITY_i$ for the pre-treatment period, β_0, to be positive, although the link between land ownership inequality and conflict has been elusive (Albertus, Brambor and Ceneviva, 2016; Biswanger, Deininger, and Feder, 1995). β_1 and β_2 test the main hypothesis of the paper for period 1 and 2 respectively: did the slow and defective deployment land reform increase conflict in towns with a high level of expected re-distribution of land? In this case, do we observe more "invasions" in towns with a high level of expected re-distribution because there was a large percentage of underexploited land in large estates, because there was a large number of beneficiaries or because there was a lot of underexploited land *and* a large number of beneficiaries? In the case of invasions, the answer to these questions is no, for every measurement of land reform treatment intensity. Table 3 gives the coefficients.

PLEASE INSERT TABLE 3 HERE

The explanatory power of the model in the case of invasions is limited, perhaps because invasions were less common than assumed in the literature. Our main hypothesis cannot be confirmed in the case of invasions. Invasions were rare in the period and they did not follow a pattern related to the intensity of expected land reform. None of the specifications throws statistically significant coefficients β_0, β_1 and β_2. β_0,

which measures the effect of $INEQUALITY_i$ on invasions in the pre-treatment period, is generally positive in line with the expectation that inequality increases conflict, but always statistically insignificant (columns I to VII in table 3). β_1, the estimate of the treatment effect in period 1 (April 1933-June 1934), is always negative in the case of supply side measures of $INEQUALITY_i$ (columns I, II, III of table 3), positive in the case of demand side measures (columns IV and V) and positive and negative in the case of polarization (columns VI and VII). β_2, the estimate of the treatment effect of land reform in period 2, is in most cases negative (decreasing conflict), especially in the case of demand-side measures of the intensity of land reform treatment, although the coefficients remain statistically insignificant in all specifications. Perhaps the acceleration of land reform under the Popular Front slightly reduced the propensity of peasants to invade, yet the effect is small and not distinguishable from zero. Finally, across specifications, only the coefficient on variable 'population in 1930' is consistently positive and statistically different from zero, perhaps reflecting genuine effects of larger towns (more militancy, more information) or reporting biases in the evidence.

What do we make of these coefficients? Negative β coefficients do not lend support to the view that local landowner collective action to resist land reform drove peasants to more invasions. The estimated effects of polarization on invasions do not cohere. Finally, potential beneficiaries invaded more often, despite the statistically insignificant results, which would be partially consistent with explanations of peasant protest based on a change of landless peasants' expectations caused by land reform.

Table 4 displays the evidence for the extensive margin of peasant strikes. In this case, our dependent variable is a dichotomous variable taking value 1 if there was at least one recorded peasant strike in town *i* and month *t* and 0 if there was none. As in table 3, we use a linear probability model to predict the occurrence of strikes.

PLEASE INSERT TABLE 4

The explanatory power of models displayed in table 4 is higher than in the case of invasions. In the pre-treatment period, regressions throw negative coefficients β_0 when the variable is measured using the supply of expropriable land (columns I, II and III of table 3). Isolated towns with a large share of underexploited, expropriable land were far from ideal hotbeds of peasant collective action in the period preceding land reform. In contrast, β_0 is positive (although statistically insignificant) when $INEQUALITY_i$ is measured from the demand side (potential beneficiaries of reform) (columns IV and V), whereas the signs of coefficients are less coherent when the intensity of treatment is proxied by polarization (columns VI and VII).

The coefficient β_1, capturing the treatment effect of delayed land reform in treatment period 1, is negative for the supply-side measures of $INEQUALITY_i$, in line with the results of table 3. Moreover, as in table 3, when the intensity of treatment is measured from the demand side (the share of potential beneficiaries of future re-distribution), β_1, the treatment effect of delayed land reform in period 1, flips to positive and in the case of *'% laborers'* statistically significant at the 1 per cent level. For the polarization measures, regressions throw both a positive and a negative β_1 coefficient. Estimated β_1 coefficients are consistent with table 3, local resistance to

reform was not correlated with an increase in peasant strikes, whereas the fact that the share of laborers predicts more strikes in the periods of land reform deployment strongly suggests an explanation based on changing expectations of the landless peasantry are not off the mark.

Continuing with other results in table 4, for period 2, from March 1936-July 1936, the estimated coefficients send a noisier signal. The estimate of the treatment effect in the second period, β_2, bounces more often from negative to positive sign and is not statistically significant. As in table 3, we find the coefficient on population is always statistically significant at the 1 per cent level in all specifications, suggesting there were large reporting biases favoring big towns or that there was something specific about larger towns (spillovers from other sectors perhaps, more information, greater market access, and unobserved locational advantages -water, access to transport routes, etc.) leading to higher strike propensity. The positive impact of size appears in other studies of rural protest, although reporting biases with this kind of information would not be surprising (Hobsbawm and Rudé, 1973; Markoff, 1985, 1986; Blair, Blattman, Hartman, 2015). The fertility of the soil has also a positive relationship with strike propensity and other forms of conflict (Finkel, Ghelbach, Olsen, 2015: 1010).

Going back to our estimates of the coefficient β_1, the estimated coefficient when $INEQUALITY$ is proxied by the share of laborers in the first treatment period is .0031. When performing the various robustness checks, coefficients ranges between .0016 and .004, in most cases being able to reject the null hypothesis of the coefficient being zero with a confidence level above 95 %.

This effect is robust to the use of different treatment windows and is closely related to land reform, not to changes in the dominant political coalition and a more pro-landowner policy after the general election of November 1933. This is apparent when we use different treatment windows. Using a first treatment window starting right after the passing of reform in October 1932 until June 1934, our estimate of the treatment effect using the share of laborers is .0024 and is statistically significant at the 5 per cent level (table A5.1 in the online appendix). For a window for period 1 going from December 1933 to June 1934, which would capture the change towards a political coalition opposed to reform, we get an estimate of .0018 with a p-value of .26 (table A5.2 in the online appendix).

Despite the robustness of the coefficient estimates, the size of the effect is not at first sight large. A coefficient of .003 in table 4 column V means a one standard deviation increase in the share of laborers ('*% laborers*') brings only an increase in 1.6 probability points in the probability of striking, which is only 6 % of the standard deviation in the extensive margin of strikes. However, this small coefficient is in fact caused by a compositional effect. For almost three quarters of the year, there is no relationship between the share of laborers and strikes, because, in several months of the year, municipalities report zero strikes. In contrast, in months in which strikes and the share of laborers are linked, the implicit coefficient is much higher. This is immediately obvious if we estimate the treatment effect of the share of laborers in period 1, β_1, for each month separately. From January to June (included) the coefficient is close to zero, in the second half of the year it is much higher, especially in July and August. Figure 4 displays the coefficients estimated for each month separately. A coefficient of .01-.013 estimated for July and August, means an increase in one standard deviation increase in

the share of laborers increases the probability of strikes by 6-7 probability points, which is almost a third of the standard deviation in the extensive margin of strikes for those months.

INSERT FIGURE 4

In table 5, we report the coefficient estimates of equation [1] using the intensive margin of strikes. In this case, our dependent variable is the number of days peasants were on strike in town *i* in month *t*. Coefficient estimates confirm to a large extent the conclusions of table 4 now for the intensive margin. As in table 4, coefficients β_0 in the pre-treatment period are negative when we consider the share of expropriable land and positive for the share of potential beneficiaries, although as in table 4 the coefficients are not statistically significant. The treatment effect in the first period (April 1933-June 1934), β_1, is generally negative for the supply-side measures of the intensity of land reform treatment and positive for the measurements of treatment intensity based on the demand side. The coefficient for the share of laborers is positive and significant at the 1 per cent level.

For estimations of β_1 using the share of laborers to proxy $INEQUALITY_i$, we get point estimates ranging from .015 to 0.017 in tables 5 and table A3.2 in the appendix. These estimates imply a standard deviation increase in the share of laborers increases the number of days on strike by a tenth of a day (.09). As in the case of the extensive margin of strikes, the small number responds to a compositional effect as many months register very low strike activity, suggesting effects in particular months might be large.

When we restrict our observations to the month of August, the estimated effect is .071, meaning a one standard deviation increase in the share of laborers brings almost an extra half day on strike per month and 25 per cent of the standard deviation in '*days*'.

Similar to the regressions with the extensive margin of strikes, β_1 does not have a coherent direction for measures of polarization. In line with coefficients in table 4, the sign of the treatment effect in the second period, β_2, goes in different directions without a clear pattern and is not statistically significant. Table 5 reinforces the conclusion that potential beneficiaries of land reform struck more often and for longer in the first period of implementation. Unreported regressions using the weighted number of newspaper hits on peasant strikes in town *i* and month *t* instead of the number of days of strikes also tell a very similar story (these regressions can be found in the online appendix).

INSERT TABLE 5

Results in tables 3, 4, and 5 show that the first period of implementation of land reform saw greater number of strikes in. For both 1931-32 and 1936, perhaps a credible promise of re-distribution meant there was a reduction in levels of conflict. In the case of strikes and invasions, the negative coefficients on the supply side measurements of $INEQUALITY_i$ mean that peasant collective action was not stronger where landowners had greater capacity to resist land reform (Domenech, 2015). The fact that demand side proxies of $INEQUALITY_i$ get a positive coefficients, especially in the case of strikes, could suggest land reform can increase conflict by changing the expectations of beneficiaries of reform. However, what tables 3, 4, and 5 do not clarify is whether land reform by itself lifted the expectations of peasants or whether it was its failure what

galvanized the collective action of peasants. In other words, would the deployment of reform have appeased peasants? In the next section, we attempt to clarify this point.

4 Limited Land reform implementation in Córdoba

In section 2 we have shown how land reform implementation was very limited in Córdoba and Jaén.. However, in towns with surviving estates from *Grandes de España*, peasants saw the promise of land reform did in fact materialize, irrespective of the slow deployment of settlements. There, some large estates were expropriated , tenants were expelled plots were assigned to landless families. A credible promise of re-distribution should have appeased landless peasants' protests.

The problem with the analysis of deployment the very limited number of experiments with expropriations, which were only tried in towns with *Grandeza* property that could be expropriated quickly without a lengthy and costly compensation process. In the early modern period, Grandee noble families held sway over large tracts of land in Andalusia, as did the King, the Church and religious-military orders (like the orders of Santiago or of Calatrava). The Church and the religious-military orders were dispossessed of their lands in the mid nineteenth century, especially after the land reform of Pascual Madoz in 1854-56. Despite the formal abolition of jurisdictions controlled by noble families, or *señoríos,* in the early nineteenth century, many noble families retained their economic, social and political clout over many towns and villages through their large holdings of land, their control over the electoral process and their actions as main employers or landlords of the local peasantry. However, there had been a progressive break-up of latifundia in the 19th and early 20th centuries (Bernal, 1988: 91-93; Díaz del Moral, 1973) and relatively efficient land markets also re-distributed

land to the most efficient producers (Carmona, Rosés, Simpson, 2015). As a result, land owned by *Grandeza* in both provinces represented 6 % in the province of Córdoba and 7.4 % in the province of Jaén (Robledo, 2012: 383) and was clearly insufficient to settle all peasants.

We examine the effect of implementation by focusing on the limited set of towns with expropriated *Grandeza* property. It was characteristic of these towns that they also had abundant land relative to the number of landless laborers, perhaps making settlements technically feasible. Table 6 displays all towns with a legacy on being ruled by a noble family in the early modern period (noble jurisdiction) comparing towns with estates owned by *Grandes de España* before expropriation and those without. This second group would reflect the experience of municipalities that had historically been under the jurisdiction of some *Grandeza* family. Table 6 shows that towns with surviving Grandee farms had similar social structure (% laborers) than towns with a legacy of being under the jurisdiction of a noble family, but crucially they also had abundant expropriable land. Compared to other towns with past noble jurisdiction, towns with expropiated *Grandeza* ownership had higher means of the share of large estates, share of tax paid by the largest landowners and share of expropriable land, they were also more polarized. These differences, despite small sample sizes, are statistically significant.

INSERT TABLE 6

In order to show there is something going on with land reform deployment and conflict, we plot the rates of growth in protests for different quartiles of $INEQUALITY_i$

comparing towns with and without land reform implementation in the province of Córdoba. In Figure 5, we display strike propensities only for the months of May-June before and during the deployment of land reform. As in the previous cases, the confidence intervals are very large for the means of treated towns (because we have to rely on a small set of month-town observations to make years comparable). Taking towns in the top two quartiles of $INEQUALITY_i$, figure 5 displays the levels of protest for towns without settlement plans and with settlement plans before treatment and during the first treatment period (April 1933-June 1934). It looks as if protest was always lower in towns with land reform deployment before treatment and during treatment, although these differences are not statistically significant due to large standard errors.

INSERT FIGURE 5

In order to substantiate these impressions with statistical analysis, we modify equation [1] to introduce triple interactions in the two treatment periods to take into account implementation of land reform or its absence on peasant protest.

$$Y_{i,t} =$$

$$\alpha + \beta_0 INEQUALITY_i + \gamma_{0,1} SETTLEMENT1_i + \gamma_{0,2} SETTLEMENT2_i + \beta_1 (INEQUALITY_i \times period\ 1) + \gamma_1 (INEQUALITY_i \times SETTLEMENT1_i \times period1) + \beta_2 (INEQUALITY_i \times period\ 2) + \gamma_2 (INEQUALITY_i \times SETTLEMENT2_i \times period\ 2) + \beta_3 period\ 1 + \beta_4 period\ 2 + \delta X_i + \varepsilon Z_t + \mu_{i,t} \quad [4]$$

where $INEQUALITY_i$ is the measure of land reform intensity used in the previous sections, X_i the set of time-invariant characteristics of each town or village and Z_t the monthly dummies and the time trend. $SETTLEMENT1_i$ is a dummy variable taking value 1 for all monthly observations if the town had a settlement plan drawn up in 1933-34 and 0 otherwise (López and Mata, 1993: 98, 102). $SETTLEMENT2_i$ is a dummy variable taking value 1 for all monthly observations of *i* if the town had land reform deployed after February 1936 in the form of temporary confiscations or settlement plans on Grandee property and 0 otherwise (López and Mata, 1993: 107, 110). Our main coefficients of interest are β_1, γ_1, β_2, and γ_2, with the expectation of finding that the intensity of expected land reform in the implementation period increased conflict in towns with no implementation (both β_1 and β_2, but especially β_1) and reduces it in towns with implementation (γ_1 and γ_2). Table 7 reports the coefficients of estimating equation [4] using various dependent variables (extensive margin of strikes and invasions and extensive margin of strikes –days and impact) with our main independent variable ($INEQUALITY_i$) proxied by the share of laborers ('*%laborers*').

INSERT TABLE 7

In column I of table 7, using 'invasion' as our dependent variable, we get a positive, non-significant coefficient for the share of laborers in the first and second treatment periods (β_1 and β_2), a positive effect of settlement on land invasions (γ_1) in the first period and a negative effect in the second period (γ_2). These results are only slightly altered by robustness checks, especially re-estimating equation [4] excluding towns with missing information from the sample (table A2.4).

In the case of strikes, coefficient β_1 is statistically significant (columns, II, III, IV), with bigger size than in tables 4 and 5, (.008 as opposed to .003 and .005 in previous tables). In column II, with the extensive margin of strikes as dependent variable, the coefficient is .027 and statistically significant, also much larger for the province of Córdoba than in previous regressions from the two provinces. Coefficient γ_1, the treatment effect of intensity of land reform conditional on deployment of land reform, is negative in the case of strikes and 'impact' but, in absolute value, smaller than β_1 (columns II and IV). It is however positive in the case of using 'days' as the intensive margin of strikes. In addition, standard errors on estimates of γ_1 are big, making the coefficient statistically non-significant. Finally, negative estimated γ_1 flip to positive when we exclude towns with no information in the Peasant Census from the sample. All in all, despite some negative coefficients, there is little evidence that land reform deployment appeased peasants

In treatment period 2, results are even more inconclusive. We get positive coefficients on the intensity of land reform treatment in the second period, β_2, in the case of invasions and strikes (intensive and extensive margin). But land reform implementation in the second treatment period gets positive, not statistically significant coefficients for strikes and negative for invasions.

It could be the case that the comparisons between towns with settlement and towns without settlement performed in table 7 underestimate the effect of settlement. Because Republican land reform had a strong anti-nobility bias, perhaps the effects of absent deployment of reform were only felt in towns that had been under the

jurisdiction of a noble family in the past (which in general also had larger shares of laborers), meaning the right comparison is between towns with expropriated farms owned by *Grandeza* families and towns with a history of noble domination (a group we label "Historical *Grandeza*" towns). The latter towns could have a legacy of greater polarization and peasants in these towns could expect higher levels of land reform. For this reason, we code a variable taking value 1 if the town had been under the jurisdiction of a Grandee family in the early modern period but did not have large tracts of land owned by *Grandeza* (and therefore did not see quick deployment of land reform) (past jurisdiction of towns from *España dividida*, 1789). We then 'Historical *Grandeza*' with the share of laborers in both periods of land reform implementation (period 1 and period 2). So we estimate the following regression:

$$Y_{i,t} =$$

$$\alpha + \beta_0 INEQUALITY_i + \gamma_{0,1} SETTLEMENT1_i + \gamma_{0,2} SETTLEMENT2_i +$$

$$+\theta_0 (Historical\ Grandeza_i) + \beta_1 (INEQUALITY_i \times period\ 1) + \gamma_1 (INEQUALITY_i \times$$

$$SETTLEMENT1_i \times period1) + \theta_1 (INEQUALITY_i \times Historical\ grandeza_{i,1})$$

$$+ \beta_2 (INEQUALITY_i \times period\ 2) + \gamma_2 (INEQUALITY_i \times SETTLEMENT2_i \times period\ 2) +$$

$$\theta_2 (INEQUALITY_i \times Historical\ grandeza_{i,2}) + \beta_3 period\ 1 + \beta_4 period\ 2 + \delta X_i + \varepsilon Z_t +$$

$$\mu_{i,t} \quad [5]$$

with $Historical\ grandeza_i$ being a dummy variable taking value 1 in towns with a past noble jurisdiction but no surviving large estates from *Grandeza* noble, $Historical\ grandeza_{i,1}$ is a dummy variable taking value 1 for towns that were under the jurisdiction of a Grandee family in the early modern period only for period 1 and 0 otherwise and $Historical\ grandeza_{i,2}$ does the same for period 2. The remaining variables are defined as in equation [4]. Table 8 reports the coefficients of various

estimations of equation [5] for 'invasion', 'strike', 'days' and 'impact', only reporting the γ, β, and θ coefficients.

INSERT TABLE 8 HERE PLEASE

In column I of table 8, using 'invasion' as our dependent variable, we get a positive, non-significant coefficient for the share of laborers in the first and second treatment periods (β_1 and β_2), a positive, now significant effect of settlement on land invasions (γ_1) in the first period and a negative effect in the second period (γ_2). The effect of *Grandeza*'s legacies is inconclusive, with a positive effect in period 1 (θ_1) and a negative effect in period 2 (θ_2).

In the case of strikes, coefficient β_1 is statistically significant in columns II and IV, with slightly bigger sizes than in tables 4 and 5. Coefficient γ_1, the treatment effect of intensity of land reform conditional on deployment of land reform, is negative in the case of strikes and 'impact' but, in absolute value, smaller than β_1 (columns II and IV). Standard errors on γ_1 are large, making the coefficient statistically non-significant. Coefficient θ_1 on the interaction between '*%laborer*', period 1 and the 'Historical *Grandeza*' dummy gets statistically significant, positive, larger coefficients. We also get a positive, large and statistically θ_2 coefficient (treatment effect of historical *Grandeza* in the second period) suggesting towns with a past of being under noble jurisdiction protested more often perhaps expecting greater and faster re-distribution. These results are very similar if we exclude the towns with missing information from the sample (table A2.4).

Results in tables 7 and 8 need to consider the small number of observations treated with land reform implementation, however coefficients in table 7 suggest potential beneficiaries not treated with implementation struck more often , but we do not find a robust coefficient for the triple interaction between land reform intensity, settlement and both time periods. However, table 8 suggests the impact of settlement is perhaps underestimated in table 7. Comparing towns with past *Grandeza* presence with those with surviving *Grandeza* presence and expropriation, the coefficients on the interactions between historical *Grandeza* presence, the extent of potential land reform and the first treatment period throws positive and in many case significant coefficients in the first and second treatment periods. If we accept that this is the right comparison, then expropriation and land deployment reduced strikes and invasions. If this were the case, the peasant conflict in the case studied here, was endogenous to land reform and its glacial progress. Quicker land reform maybe would have reduced strikes, although the empirical basis to make this claim is still thin.

5 Conclusions

This paper contributes to the literature on land reforms and rural conflict by analyzing one of the classic cases of failed land reform under democracy. Because the minimal protection of property rights meant lengthy assessments of costly compensations to be paid to landowners, the deployment of reform was slower and more difficult than expected by landless peasants. Did the glacial pace and uneven deployment land reform cause greater levels of peasant conflict in 1930s Spain? Our answer to the question is a qualified yes. We find that a group of potential beneficiaries of reform (laborers) struck more often in the period in which land reform was slow and partial, compared to an initial treatment period. There is also suggestive evidence that implementation reduced

protest. Our regressions show that the mechanics of rural protest in the provinces studied here were not caused by the interactions between recalcitrant owners of land and frustrated peasants. Rather, in a context of very slow land reform implementation, our results would be consistent with model of rural protest that emphasizes interactions with state actors and state policy (Alston, Libecap, Mueller, 1999), especially in the case of rural laborers. This protest was generated by frustrated expectations of land ownership re-distribution created by the same process of land reform.

There were many reasons for the absence of invasions in Andalusia. Towns with an abundance of landless laborers were typically more egalitarian and therefore had less land to settle peasants, with the only exception being the case of towns with surviving farms owned by *Grandeza* families. This mismatch meant settlement of large groups of rural laborers and their families was maybe technically difficult, as it meant expropriating farms away from the main population centers and settling peasants in distant and unfertile areas where settlements were most probably only viable for short periods of the year.

Perhaps one important issue not considered by the literature on land reform and rural conflict relates to the type of peasant affected by reform. Very mobile workers facing a sharply seasonal demand for labor like rural laborers in Andalusia were perhaps poorly adapted to reform. In contrast, land reform perhaps had greater effects in regions were farms could sustain tenants and their families year-round, for example where tenancy rather than wage labor was the norm, meaning tenants and sharecroppers were perhaps better equipped to take advantage of land reform quickly. The slow pace of land

reform in Andalusia and the apparent lack of appetite of Andalusian peasants for invading farms were perhaps not at all surprising.

The analysis of land reform in Córdoba and Jaén suggests a one-size-fits-all land reform was perhaps poorly adapted to the variety of agrarian problems in 1930s Spain (Dobby, 1936). The lack of fit of land reform to Andalusian conditions perhaps reflected a very defective knowledge of agriculture and agrarian conditions by policy makers in the period. It was only during the implementation of reform that the state started to collect hard evidence to understand the causes of the various agrarian problems in Spain . But this process of problem discovery was not happening quickly enough to avoid reform paralysis. Attempts at solving the problem of rural poverty preceded the adequate understanding of the problem at hand.

More than 50 years ago, Albert Hirschman studied the evolution of various important policies in Latin America, including land reform in Colombia (Hirschman, 1963). In developed economies, he observed it was typically the case that the advances in the understanding of a particular problem preceded the motivation to tackle that problem. In developing economies, however, the impulse or need of governments to act generally precedes the understanding of the problem at hand. In this context, potentially large mistakes, fast learning and continuous trial and error characterize policy-making. Hirschman did not have a negative view of trial and error and discovery, in part seeing them as inevitable. Yet, in the case of Spain, civil war and authoritarian reaction, not peasant revolution, put an end to a most ambitious project of social transformation.

Main abbreviations:

CNT: *Confederación Nacional del Trabajo*, National Confederation of Labor.

FNTT: *Federación Nacional de Trabajadores de la Tierra*, National Federation of Rural workers.

IRA: *Instituto de Reforma Agraria*. Institute of Agrarian Reform

UGT: *Unión General de Trabajadores*, Workers' General Union.

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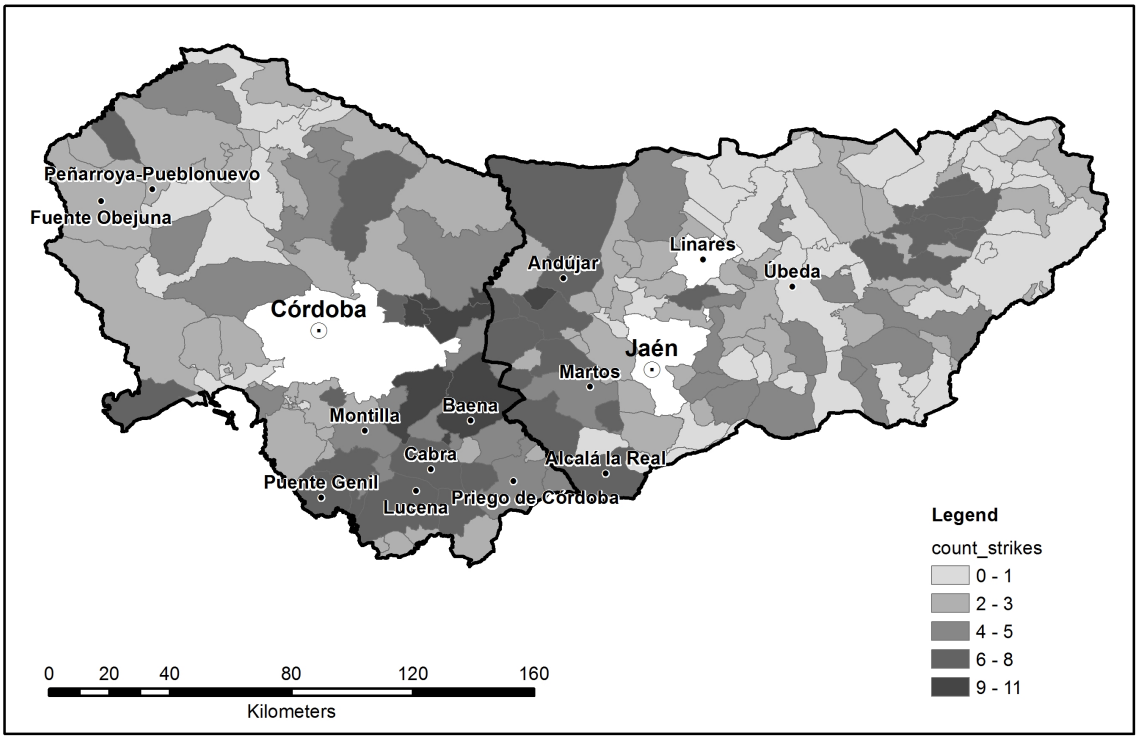
GRAPHS

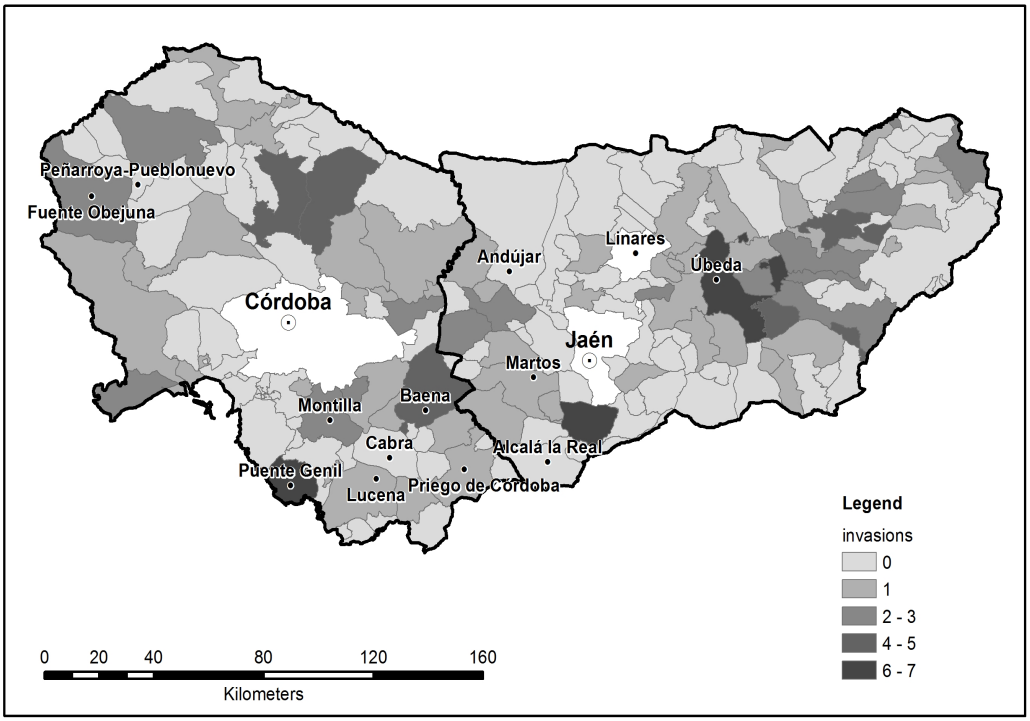
Map 1
Count of peasants strikes in Córdoba and Jaén

This map displays the total count of strikes in each municipality adding the number of recorded events from April 1931 to June 1934 (both included) and from March 1936 to July 1936. The two capital cities and Linares are excluded from the universe of towns in the two provinces. Darker means a higher number of strikes.



Map 2
Count of invasions and collective trespassing in Córdoba and Jaén

This map displays the total count of recorded events of invasion and collective trespassing in each municipality from April 1931 to June 1934 and from March 1936 to July 1936. The two capital cities and Linares are excluded from the universe of towns in the two provinces. Darker means higher counts of events.



Map 3
Total number of days on strike in Córdoba and Jaén

This map displays the total number of days on strike from April 1931 to June 1934 and from March 1936 to July 1936. The two capital cities and Linares are excluded from the universe of towns in the two provinces. Darker means a higher number of days on strike.

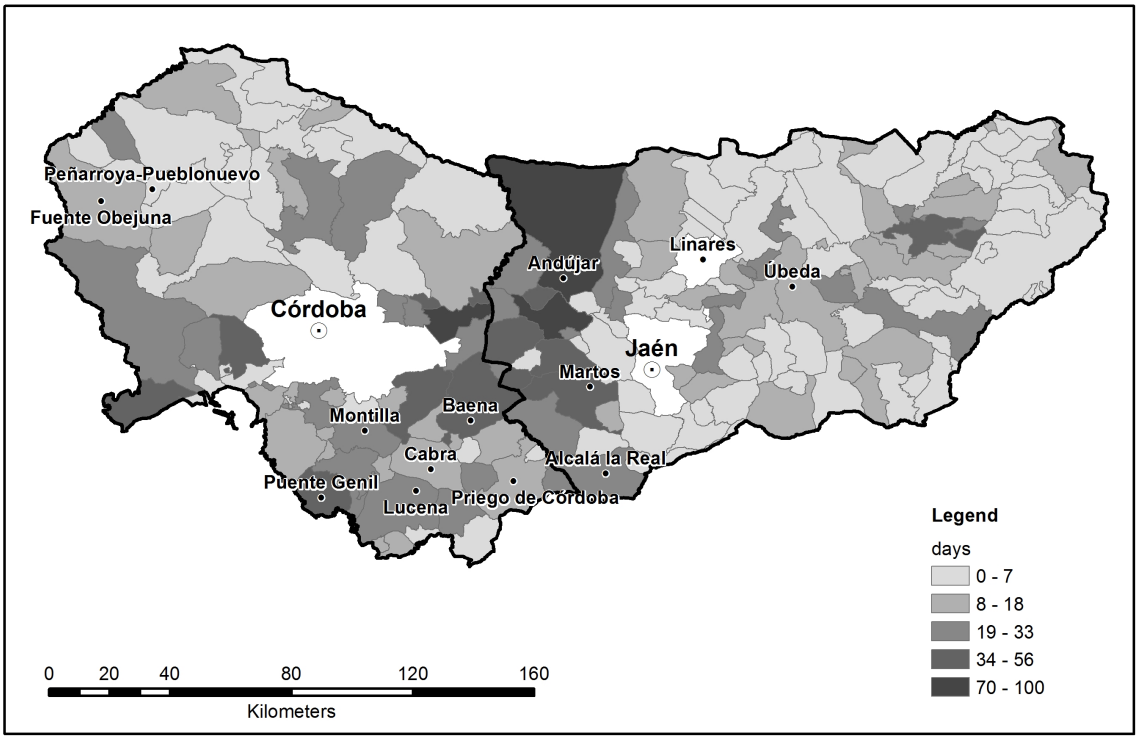


Figure 1
Mean probability of strikes in May and June for the 2nd and 4th quartiles of the share of laborers in the local population, '% laborers'

The figure shows the mean probability of recording at least one peasant strike event (extensive margin) in May-June 1931, May-June 1932, May-June 1933, May-June 1934 and May-June 1936 in the second and fourth quartile of municipalities in Córdoba and Jaén (except the two provincial capitals and Linares) ranked by from lower to higher percentage of laborers in the local population (% laborers, the demand-side proxy of $INEQUALITY_i$). Strikes in May and June are considered to make years comparable. Averages are calculated for the second and fourth quartiles of % laborers, Vertical bars display the 95 % confidence intervals of the estimated mean propensities. The vertical striped line separates the pre-treatment and treatment periods. There are no strikes recorded for year 1935.

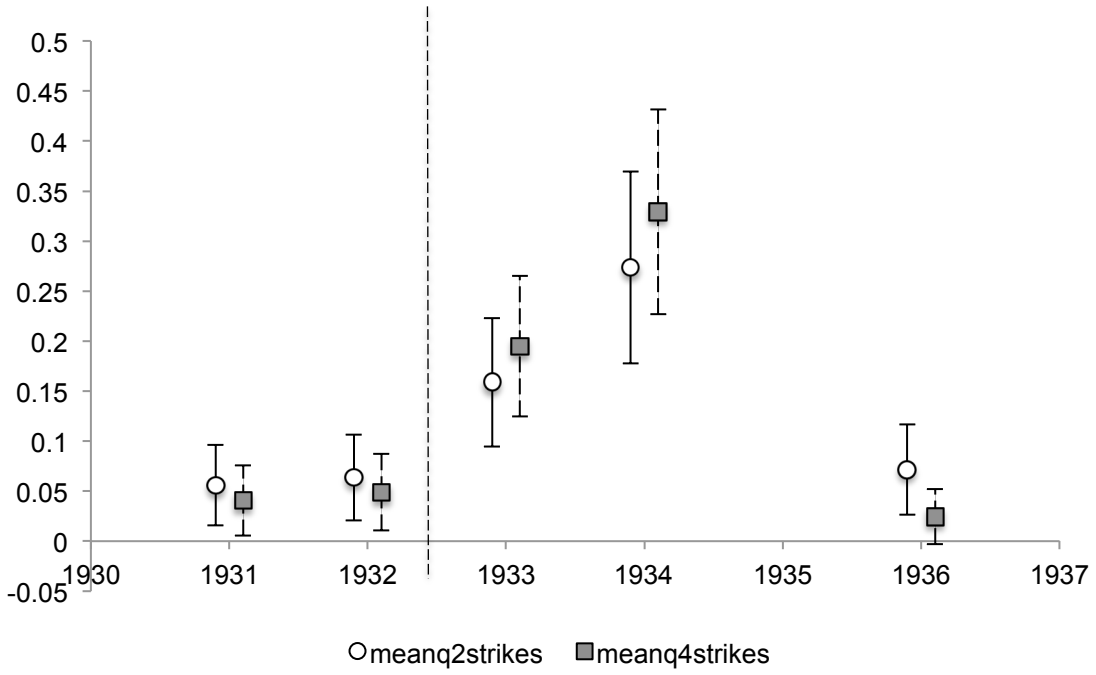


Figure 2
Mean probability of strikes in May and June for towns in the 2nd and 4th quartile of the share of expropriable land (% expropriable)

The figure shows the mean probability of recording at least one peasant strike event (extensive margin) in May-June 1931, May-June 1932, May-June 1933, May-June 1934 and May-June 1936 in the second and fourth quartile of municipalities in Córdoba and Jaén (except the two provincial capitals and Linares) ranked by from lower to higher percentage of the share of expropriable land in the area of the municipality (% expropriable, one of the supply-side proxies of $INEQUALITY_i$). Strikes in May and June are considered to make years comparable. Averages are calculated for the second and fourth quartiles of % expropriable. Vertical bars display the 95 % confidence intervals of the estimated mean propensities. The vertical striped line separates the pre-treatment and treatment periods. There are no strikes recorded for year 1935.

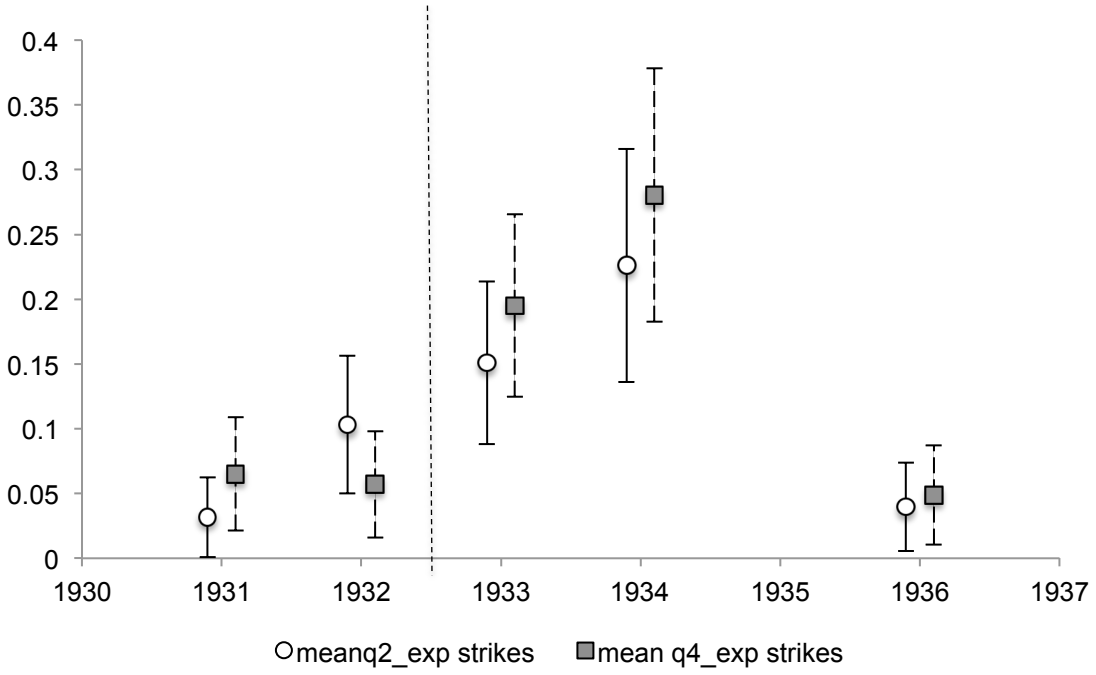


Figure 3
Mean probability of 'invasion' events May-June for the 2nd and 4th quartiles of the % laborers

The figure shows the mean probability of recording at least one invasion or collective trespassing event (extensive margin) in May-June 1931, May-June 1932, May-June 1933, May-June 1934 and May-June 1936 in the second and fourth quartile of municipalities in Córdoba and Jaén (except the two provincial capitals and Linares) ranked by from lower to higher percentage of the share of laborers in local population (% laborers, one of the demand-side proxies of $INEQUALITY_i$). Only invasions in May and June are considered to make years comparable. Averages are calculated for the second and fourth quartiles of % laborers, Vertical bars display the 95 % confidence intervals of the estimated mean propensities. The vertical striped line separates the pre-treatment and treatment periods. There are no invasions recorded for year 1935.

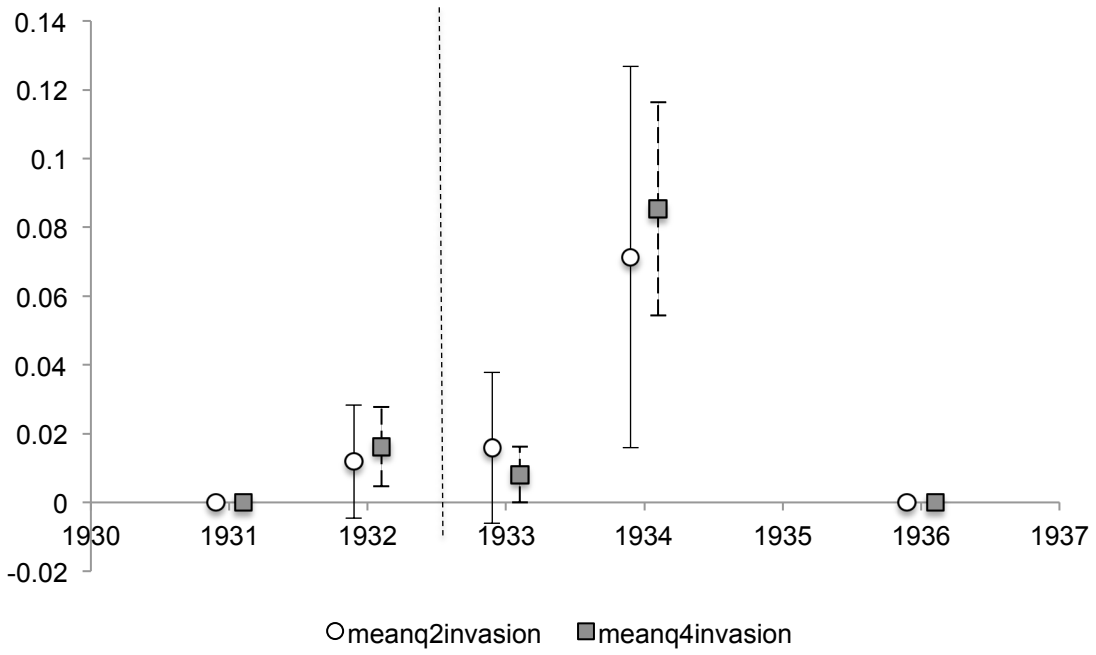


Figure 4
Coefficient estimates of β_1 for different months

This figure displays the point estimates of coefficient β_1 when estimating equation [1] separately for each month of the year. Dependent variable is the extensive margin of strikes in each municipality and the main independent variable is the share of laborers ('*% laborer*'). The estimated coefficients capture the effect of '*% laborer*' in the first treatment period (April 1933 to June 1934).

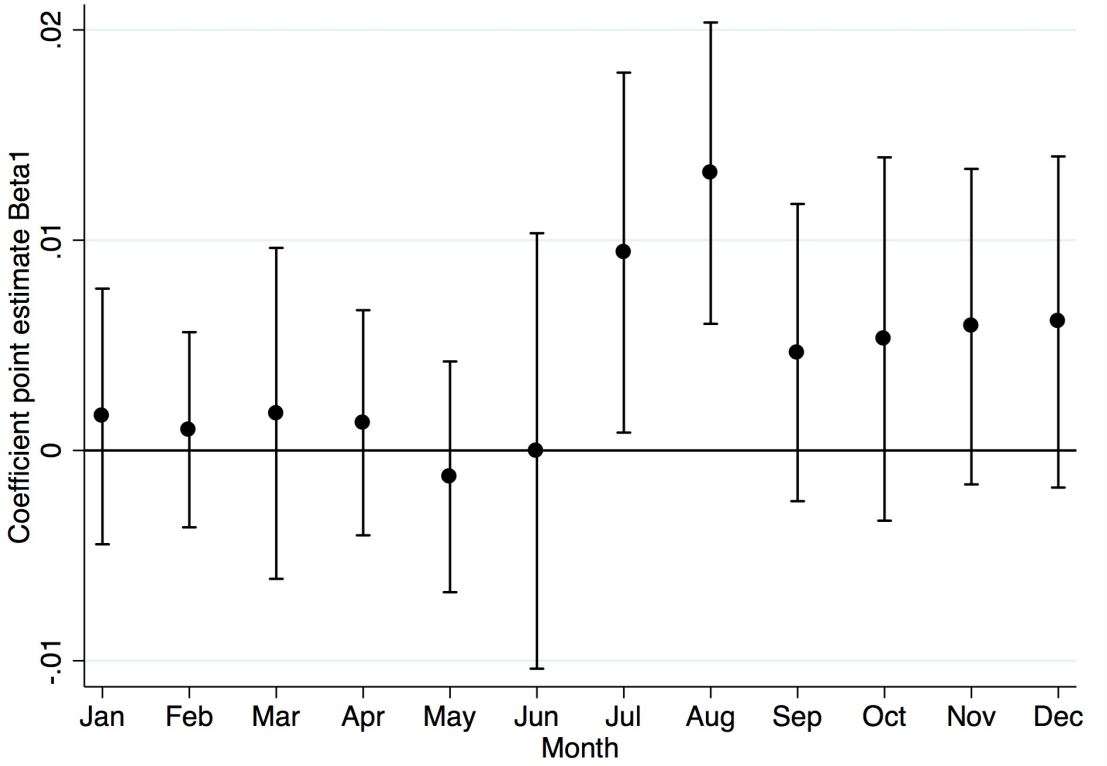


Figure 5

Mean strikes prevalence May-July in the pre-reform and first treatment period

This figure displays estimated average propensities to strike (extensive margin) for towns in Córdoba ranked in the top two quartiles of the share of landless laborers. We only consider May and June to make the pre-reform and reform periods comparable. The pre-reform period starts in April 1931 and finishes in March 1933 and, therefore, includes strikes in May-June 1931 and May-June 1932. The reform period starts in April 1933 and finishes in June 1934, it therefore includes strikes in May-June 1933 and May-June 1934. Vertical bars represent the 95 % confidence intervals of the mean.

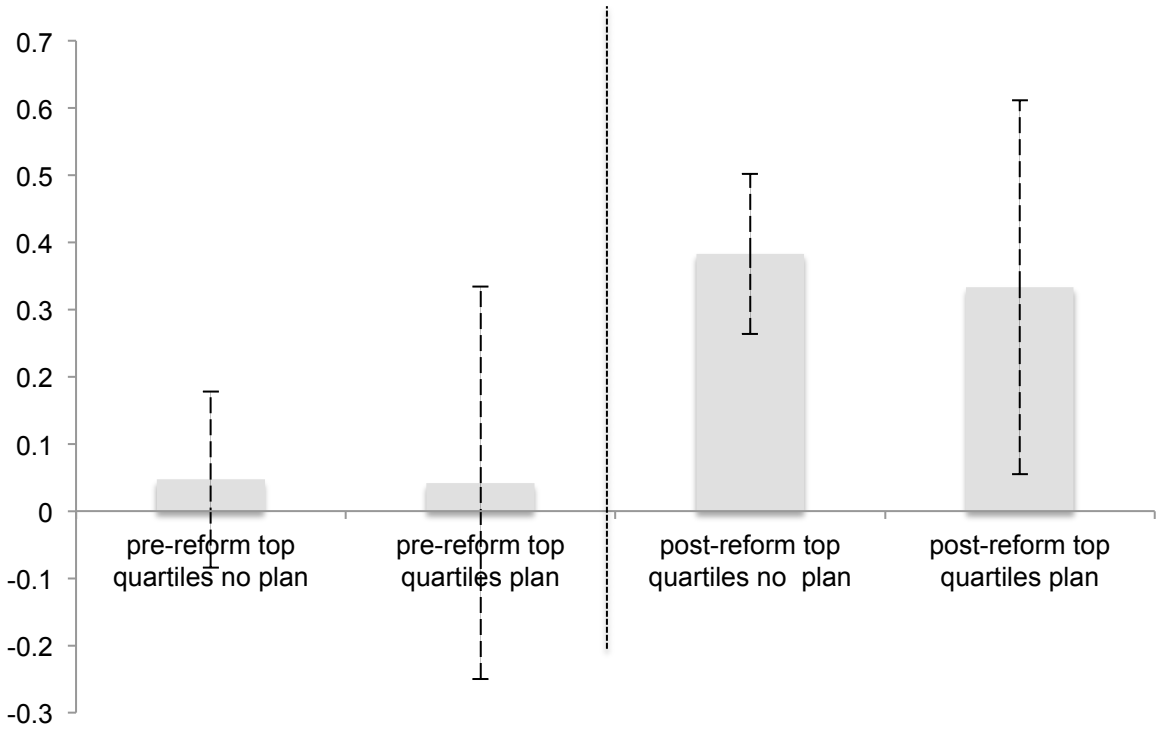


Table 1
Summary statistics

This table gives the summary statistics of the dependent and independent variables used in regressions estimating equation [1] on the monthly panel data set of observations at the municipal level. Variables [1] to [4] are the dependent variables used in the regressions. *'Strike'* is a dichotomous variable taking value 1 if a town registered at least one peasant strike in a given month and 0 if it registered none, using the counts of peasant strikes from the historiography. *'Invasion'* is a dichotomous variable taking value 1 if the town registered at least on episode of collective trespassing and taking value 0 if it registered none. *'Days'* is the total number of days on strike in each town in a given month (intensive margin). *'Impact'* is the weighted number of newspaper hits referring to peasant strikes in a town in a given month. Variables [5] to [16] are the independent variables used in equation [1], with variables [5] to [10] being our main variable of interest proxying the degree of exposure to land reform (intensity of treatment), $INEQUALITY_i$. *'% area'* is the share of farms over 250 hectares in the total area of the town according to cadastral information, in percentage terms. *'% taxable'* is the share of the sum of taxable incomes above 5,000 *pesetas* in the total taxable agricultural income of the town (assessed by the Cadastre), in percentages. *'% expropriable'* is the share of the area of all expropriable farms in total local area, in percentage terms (from the Inventory of Expropriable Farms). *'% poor'* is the proportion of household heads counted in the Peasant Census as a proportion of the population in each town in percentage terms (the Peasant Census included laborers, tenants, and owners of small owners). *'% laborers'* is the proportion of household heads who were classified as laborers in the total population of the town in percentage terms. '% expropriable' × '% laborers' is the interaction between the supply- and demand-side proxies of land reform intensity. Variables [5] to [10] take value 0 for the towns with missing information. Variables [11] to [16] are the remaining controls. *'Population in 1930'* is the population of the town according to the Spanish Population Census of 1930. *'Soil quality'* is an index of average soil quality in each town using the formula by Brady and Weil (2008) using FAO's Harmonized World Soil Database. *'Altitude'* is average altitude in the town. Variables [14] to [16] are dummy variables used to mitigate the problem of missing information. *'No Cadastre'* is a dummy variable taking value 1 if Carrión (1932) did not report cadastral information from the village or town and 0 otherwise. *'No Inventory'* is a dummy variable taking value 1 when there is missing information on the town in the Inventory of Expropriable Property and 0 otherwise. *'No Census'* is a dummy variable taking value 1 if there is missing information on town from the Peasant Census and 0 otherwise.

	Mean	Std Deviation	Min	Max
[1] Strike	.072	.258	0	1
[2] Invasions	.016	.126	0	1
[3] Days	.314	1.757	0	36
[4] Impact	.124	.516	0	7.75
[5] % area	28.63	25.698	0	100
[6] % taxable	32.14	21.817	0	100
[7] % expropriable	18.81	21.003	0	98.57
[8] % poor	11.9	7.5	0	35.6
[9] % laborers	7.9	5.4	0	31.3
[10] % expropriable×%laborers	159.06	245.25	0	1394.5
[11] Population in 1930	6854.71	5904.46	664	27657
[12] Soil Quality	65.03	7.369	47.21	75.92
[13] Altitude	559.47	229.41	60.92	1229.57
[14] No Cadastre	.198	.398	0	1
[15] No Inventory	.203	.403	0	1
[16] No Census	.102	.004	0	1

Table 2
Correlations

This table gives the correlations between the dependent and independent variables used in regressions estimating equation [1] on the monthly panel data set of observations at the municipal level. This table gives the summary statistics of the dependent and independent variables used in regressions estimating equation [1] on the monthly panel data set of observations at the municipal level. Variables [1] to [4] are the dependent variables used in the regressions. *'Strike'* is a dichotomous variable taking value 1 if a town registered at least one peasant strike in a given month and 0 if it registered none, using the counts of peasant strikes from the historiography. *'Invasion'* is a dichotomous variable taking value 1 if the town registered at least on episode of collective trespassing and taking value 0 if it registered none. *'Days'* is the total number of days on strike in each town in a given month (intensive margin). *'Impact'* is the weighted number of newspaper hits referring to peasant strikes in a town in a given month. Variables [5] to [16] are the independent variables used in equation [1], with variables [5] to [10] being our main variable of interest proxying the degree of exposure to land reform (intensity of treatment), $INEQUALITY_i$. *'% area'* is the share of farms over 250 hectares in the total area of the town according to cadastral information, in percentage terms. *'% taxable'* is the share of the sum of taxable incomes above 5,000 *pesetas* in the total taxable agricultural income of the town (assessed by the Cadastre), in percentages. *'% expropriable'* is the share of the area of all expropriable farms in total local area, in percentage terms (from the Inventory of Expropriable Farms). *'% poor'* is the proportion of household heads counted in the Peasant Census as a proportion of the population in each town in percentage terms (the Peasant Census included laborers, tenants, and owners of small owners). *'% laborers'* is the proportion of household heads who were classified as laborers in the total population of the town in percentage terms. '% expropriable' × '% laborers' is the interaction between the supply- and demand-side proxies of land reform intensity. Variables [5] to [10] take value 0 for the towns with missing information. Variables [11] to [16] are the remaining controls. *'Population in 1930'* is the population of the town according to the Spanish Population Census of 1930. *'Soil quality'* is an index of average soil quality in each town using the formula by Brady and Weil (2008) using FAO's Harmonized World Soil Database. *'Altitude'* is average altitude in the town. Variables [14] to [16] are dummy variables used to mitigate the problem of missing information. *'No Cadastre'* is a dummy variable taking value 1 if Carrión (1932) did not report cadastral information from the village or town and 0 otherwise. *'No Inventory'* is a dummy variable taking value 1 when there is missing information on the town in the Inventory of Expropriable Property and 0 otherwise. *'No Census'* is a dummy variable taking value 1 if there is missing information on town from the Peasant Census and 0 otherwise.

	[1] Strike	[2] Invasions	[3] Days	[4] Impact	[5] % area	[6] % tax	[8] % expropriable	[9] % poor	[10] % laborers	[11] % expr× %lab	[12] Pop 1930	[13] Soil quality	[14] Altitude	[15] No Cad	[16] No Invent
Strike															
Invasions	.16														
Days	.64	.13													
Impact	.87	.2	.67												
% area	-.04	-.04	-.02	-.03											
% tax	.04	.03	.05	.04	.54										
% expropriable	-.02	.01	.01	-.01	.55	.62									
% poor	-.04	-.04	-.01	-.04	.21	.07	.1								
% laborers	-.01	-.03	.00	-.01	.19	.08	.09	.86							
| % expropriable× aborers	-.01	.005	.01	-.01	.51	.54	.84	.38	.43						
| Population in 1930	.1	.11	.08	.1	-.01	.13	.02	-.39	-.27	-.11					
| Soil Quality	.04	.01	.03	.04	-.4	-.03	-.17	.01	-.003	-.14	.07				
| Altitude	-.08	.04	-.08	-.07	-.06	-.38	-.36	.02	-.18	-.34	-.05	.13			
| No Cadastre	.01	-.04	.006	.01	-.55	-.32	-.25	-.08	-.1	-.23	-.22	.22	-.02		
| No Inventory	.02	-.02	.002	.004	-.23	-.29	-.43	.03	.06	-.31	-.17	.06	.08	.27	
| No Census	.02	.001	-.003	.02	-.21	-.12	-.07	-.54	-.49	-.22	-.01	-.05	-.04	.23	.08

Table 3
Land reform and land invasions

This table tests the relationship between land reform exposure (intensity of treatment) and the probability of land invasions. The data set is a monthly panel of municipal observations running from April 1931 to July 1936 (with months from July 1934 to February 1936 excluded). We estimate differences-in-differences linear probability models with the intensity of the land reform treatment interacted with two time period dummies of land reform implementation. *Period 1* runs from April 1933 to June 1934 and *Period 2* runs from March 1936 to July 1936. The dependent variable '*land invasion*' is a dichotomous variable taking value 1 if there was at least an episode of collective trespassing or similar recorded in the town or village in a given month and 0 if there was none. In the first three regressions (columns I, II, III) we proxy the intensity of treatment with three measurements of the supply of expropriable land. These proxies are expressed as shares of each town's total area. These supply-measures ('*% area*', '*% tax*', '*% expropriable*') are interacted with dummy variables for the two periods of land reform treatment (period 1 and 2). In columns IV and V, we look at the demand for land reform by looking at the share of peasants who would benefit from reform. In column IV, we use the number of household heads included in a census of potential beneficiaries and express it as a percentage of the local population ('%poor'). In column V, we take the share of local population classified as laborers ('*%laborers*'). In Columns VI and VII, we include interactions of the supply ('*% expropriable*') and demand ('*% poor*' or '*% laborers*'). In all regressions, we add extra controls affecting the probability to invade land or report invasions. This the case with population in 1930, local soil quality, average altitude, latitude and longitude at the centroid of the town, dummies for the month of the year, and a time trend. We also add dummy variables taking value 1 if there is missing information on the town in the relevant source to proxy intensity of treatment. Regressions included in this table are simpler regressions without correction for spatial or serial correlation, regressions with different treatment windows and with corrections for serial and spatial correlation can be found in the online appendix.

Land reform treatment measured as the amount of land available Supply side				**Land reform treatment measured as the local estimated of potential beneficiaries Demand side**			**Interaction of supply-side and demand-side measurements of intensity of treatment Interaction Supply-Demand**		
	I	**II**	**III**		**IV**	**V**		**VI**	**VII**
Intercept	-.384* (.224)	-.338 (.223)	-.358 (.218)	**Intercept**	-.382* (.219)	-.389* (.22)	**Intercept**	-.34 (.22)	-.361 (.22)
% area	-.0001 (.00009)			**% poor**	.00007 (.00032)		**% poor× % expropriable**	9.4e-06* (5.7)	
% area ×period 1	-6.2e-06 (.0001)			**% poor × period1**	.0004 (.0004)		**% poor × %expropriable× period1**	-1.9e-06 (8.5e-06)	
% area × period 2	-.035 (.011)			**% poor ×period 2**	-.0007 (.0007)		**% poor×%expropriable × period 2**	-2.2 (.00001)	
% tax		.0001 (.0001)		**% laborers**		.00003 (.00042)	**% laborers ×% expropriable**		.00001 (9e-06)
% tax × period 1		-.0002 (.0001)		**% laborers × period 1**		.0008 (.0006)	**% laborers×%expropriable×period 1**		-1.7e-06 (.00001)
% tax × period 2		-.0002 (.0002)		**% laborers ×period 2**		-.001 (.001)	**% laborers×%expropriable×period 2**		-4.3e-06 (.00002)
% expropriable			.0001 (.0001)						
% expropriable×period 1			-.00008 (.0002)						
% expropriable×period 2			.0002 (.0002)						
Population in 1930	2.5e-06*** (2.68e-07)	2.5e-06*** (2.7e-07)	2.6e-06*** (2.61e-07)	**Population in 1930**	2.7e-06*** (2.9e-07)	2.6e-06*** (2.8e-07)	**Population in 1930**	2.7e-06 (2.7e-07)	2.7e-06*** (2.7e-07)
Soil quality	.0001 (.0003)	.0002 (.0003)	.0002 (.0003)	**Soil quality**	.0002 (.0003)	.0002 (.0003)	**Soil quality**	.0002 (.0003)	.0002 (.0002)
Altitude	-8.1e-06 (7.93e-06)	-4.66e-06 (8.70e-06)	-3.34e-06 (8.24e-06)	**Altitude**	-7.4e-06 (7.9e-06)	-6.2e-06 (8e-06)	**Altitude**	-5.1e-06 (8.1e-06)	-3.7e-06 (8.3e-06)
No Cadastre (=1)	-.008* (.005)	-.003 (.004)							
No Inventory (=1)			.0002 (.004)				**No Inventory (=1)**	-.0001 (.004)	-.0005 (.004)
				No Peasant Census (=1)	.005 (.006)	.004 (.006)	**No Peasant Census (=1)**	.004 (.005)	.004 (.005)
Geographical controls	✓	✓	✓	**Geographical controls**	✓	✓	**Geographical controls**	✓	✓
Time trend	✓	✓	✓	**Time trend**	✓	✓	**Time trend**	✓	✓
Period 1 and 2 dummies	✓	✓	✓	**Period 1 and 2 dummies**	✓	✓	**Period 1 and 2 dummies**	✓	✓
Monthly dummies	✓	✓	✓	**Monthly dummies**	✓	✓	**Monthly dummies**	✓	✓
Lagged dependent variable				**Lagged dependent variable**			**Lagged dependent variable**		
28 judicial district FE				**28 judicial district FE**			**28 judicial district FE**		
Observations	7,216	7,216	7,216	**Observations**	7,216	7,216	**Observations**	7,216	7,216
Adjusted R^2	.03	.03	.03	**Adjusted R^2**	.03	.03	**Adjusted R^2**	.03	.03

Notes: Standard errors in parentheses. * p<.10, ** p<.05, *** p<.01.

Table 4
Land reform and peasant strikes (extensive margin)

This table tests the relationship between land reform exposure (intensity of treatment) and the probability of strikes at the extensive margin. The data set is a monthly panel of municipal observations running from April 1931 to July 1936 (with months from July 1934 to February 1936 excluded). We estimate differences-in-differences linear probability models with the intensity of the land reform treatment interacted with two time period dummies of land reform implementation. *Period 1* runs from April 1933 to June 1934 and *Period 2* runs from March 1936 to July 1936. The dependent variable 'peasant strike' is a dichotomous variable taking value 1 if there was at least one peasant strike recorded in the town or village in a given month and 0 if there was none. In the first three regressions (columns I, II, III) we proxy the intensity of treatment with three measurements of the supply of expropriable land. These proxies are expressed as shares of each town's total area. These supply-measures ('*% area*', '*% tax*', '*% expropriable*') are interacted with dummy variables for the two periods of land reform treatment (period 1 and 2). In columns IV and V, we look at the demand for land reform by looking at the share of peasants who would benefit from reform. In column IV, we use the number of household heads included in a census of potential beneficiaries and express it as a percentage of the local population ('%poor'). In column V, we take the share of local population classified as laborers ('*%laborers*'). In Columns VI and VII, we include interactions of the supply ('*% expropriable*') and demand ('*% poor*' or '*% laborers*'). In all regressions, we add extra controls affecting the probability to invade land or report them. This the case with population in 1930, local soil quality, average altitude, latitude and longitude at the centroid of the town, dummies for the month of the year, and a time trend. We also add dummy variables taking value 1 if there is missing information on the town in the relevant source to proxy intensity of treatment. Regressions included in this table are simpler regressions without correction for spatial or serial correlation, regressions with different treatment windows and with corrections for serial and spatial correlation can be found in the online appendix.

Land reform treatment measured as the amount of land available **Supply side**				**Land reform treatment measured as the local estimated of potential beneficiaries** **Demand side**			**Interaction of supply-side and demand-side measurements of intensity of treatment** **Interaction Supply-Demand**		
	I	**II**	**III**		**IV**	**V**		**VI**	**VII**
Intercept	-.239 (.44)	-.126 (.438)	-.102 (.429)	**Intercept**	.078 (.429)	-.13 (.432)	**Intercept**	-.1 (.43)	-.07 (.43)
% area	-.0002 (.0002)			**% poor**	.00028 (.00063)		**% poor× % expropriable**	-.00001 (.00001)	
% area × period 1	-.0003 (.0003)			**% poor *×period1**	.00106 (.00085)		**% poor × %expropriable× period1**	-6.9e-06 (.00002)	
% area × period 2	.0004 (.0004)			**% poor ×period 2**	.00053 (.000133)		**% poor × %expropriable × period 2**	-9.4 (.00002)	
% tax		-.0002 (.0002)		**% laborers**		.00006 (.00083)	**% laborers× % expropriable**		-.00003* (.00002)
% tax × period 1		-.0003 (.0003)		**% laborers×period 1**		.0031*** (.0012)	**% laborers×%expropriable×period 1**		3.1e-06 (.00003)
% tax × period 2		-.0004 (.0004)		**% laborers×period 2**		-.0012 (.0018)	**% laborers×%expropriable×period 2**		-.00003 (.00004)
% expropriable			-.0004* (.0002)						
% expropriable×period 1			-.0003 (.0003)						
% expropriable×period 2			-.0002 (.0004)						
Population in 1930	4.00e-06*** (5.07e-07)	4.09e-06*** (5.31e-07)	4.05e-06*** (5.13e-07)	**Population in 1930**	4.37e-06*** (5.79e-07)	4.28e-06*** (5.41e-07)	**Population in 1930**	4.0e-06*** (5.3e-07)	4.0e-06*** (4.2e-07)
Soil quality	.001*** (.0005)	.002*** (.0005)	.001*** (.0005)	**Soil quality**	.002*** (.0005)	.002*** (.0005)	**Soil quality**	.002*** (.0005)	.001** (.0005)
Altitude	-.00008*** (.00002)	-.00007*** (.00002)	-.00009*** (.000002)	**Altitude**	-.00008*** (.00002)	.00007 (.00002)	**Altitude**	-.00009 (.00002)	-.0001*** (.00002)
No Cadastre (=1)	.003 (.009)	.011 (.009)							
No Inventory (=1)			.011 (.008)				**No Inventory (=1)**	.016** (.008)	.014* (.008)
				No Peasant Census (=1)	.029** (.012)	.028*** (.011)	**No Peasant Census (=1)**	.012 (.01)	.01 (.01)
Geographical controls	✓	✓	✓	**Geographical controls**	✓	✓	**Geographical controls**	✓	✓
Time trend	✓	✓	✓	**Time trend**	✓	✓	**Time trend**	✓	✓
Period 1 and 2 dummies	✓	✓	✓	**Period 1 and 2 dummies**	✓	✓	**Period 1 and 2 dummies**	✓	✓
Monthly dummies	✓	✓	✓	**Monthly dummies**	✓	✓	**Monthly dummies**	✓	✓
Lagged dependent variable				**Lagged dependent variable**			**Lagged dependent variable**		
28 judicial district FE				**28 judicial district FE**			**28 judicial district FE**		
Observations	7,216	7,216	7,216	**Observations**	7,216	7,216	**Observations**	7,216	7,216
Adjusted R^2	.1	.1	.1	**Adjusted R^2**	.1	.1	**Adjusted R^2**	.1	.1

Notes: Standard errors in parentheses. * p<.10, ** p<.05, *** p<.0

Table 5
Land reform and peasant strikes (intensive margin)

This table tests the relationship between land reform exposure (intensity of treatment) and the number of days peasants struck per month in each municipality. The data set is a monthly panel of municipal observations running from April 1931 to July 1936 (with months from July 1934 to February 1936 excluded). We estimate differences-in-differences models with the intensity of the land reform treatment interacted with two time period dummies of land reform implementation. *Period 1* runs from April 1933 to June 1934 and *Period 2* runs from March 1936 to July 1936. The dependent variable '*days on strike*' records the number of days peasants were on strike in a town or village in a given month. Our main explanatory variable, 'intensity of land reform treatment' is measured from the supply side, the demand side and as an interaction of supply and demand. In the first three regressions (columns I, II, III) we proxy the intensity of treatment with three measurements of the supply of expropriable land. These proxies are expressed as shares of the total area of each town. These supply-measures (‘*% area*’, ‘*% tax*’, ‘*% expropriable*’) are interacted with dummy variables for the two periods of land reform treatment (period 1 and 2). In columns IV and V, we look at the demand for land reform by looking at the share of peasants who would benefit from reform. In column IV, we use the number of household heads included in a census of potential beneficiaries and express it as a percentage of the local population (‘%poor’). In column V, we take the share of local population classified as laborers (‘*%laborers*’). In Columns VI and VII, we include interactions of the supply (‘*% expropriable*’) and demand (‘*% poor*’ or ‘*% laborers*’). In all regressions, we add extra controls affecting the probability to invade land or report them. This the case with population in 1930, local soil quality, average altitude, latitude and longitude at the centroid of the town, dummies for the month of the year, and a time trend. We also add dummy variables taking value 1 if there is missing information on the town in the relevant source to proxy intensity of treatment. Regressions included in this table are simpler regressions without correction for spatial or serial correlation, regressions with different treatment windows and with corrections for serial and spatial correlation can be found in the online appendix.

Land reform treatment measured as the amount of land available **Supply side**				**Land reform treatment measured as the local estimated of potential beneficiaries** **Demand side**			**Interaction of supply-side and demand-side measurements of intensity of treatment** **Interaction demand-supply**		
	I	**II**	**III**		**IV**	**V**		**VI**	**VII**
Intercept	-4.467	-4.204	-3.905	**Intercept**	-4.016	-3.948	**Intercept**	-3.64	-3.75
	(3.09)	(3.086)	(3.016)		(3.019)	(3.036)		(3.02)	(3.01)
% area	-.001			**% poor**	.004		**% poor×% expropriable**	.00002	
	(.002)				(.0045)			(.0001)	
% area ×period 1	.001			**% poor × period1**	.009		**% poor×%expropriable×period1**	.0001	
	(.002)				(.006)			(.0001)	
% area × period 2	.00007			**% poor ×period 2**	-.007		**% poor×%expropriable×period 2**	-.0001	
	(.003)				(.009)			(.0002)	
% tax		.002		**% laborers**		-.003	**% laborers×% expropriable**		-.00008
		(.001)				(.006)			(.0001)
% tax × period 1		.001		**% laborers×period 1**		.017**	**% laborers×%expropriable×period 1**		.0003
		(.002)				(.008)			(.0002)
% tax × period 2		-.003		**% laborers×period 2**		-.0015	**% laborers×%expropriable×period 2**		-.0002
		(.003)				(.0013)			(.0003)
% expropriable			-.0014						
			(.0014)						
% expropriable×period 1			.0013						
			(.0021)						
% expropriable×period 2			2.65e-06						
			(.0031)						
Population in 1930	.00002***	.00002***	.00002***	**Population in 1930**	.00003***	.00002***	**Population in 1930**	.00002***	.00002***
	(3.71e-06)	(3.74e-06)	(3.60e-06)		(1.07e-06)	(3.80e-06)		(3.7e-06)	(.094)
Soil quality	.01***	.01***	.011***	**Soil quality**	.011***	.011***	**Soil quality**	.011***	.011***
	(.004)	(.004)	(.004)		(.004)	(.004)		(.004)	(.004)
Altitude	-.0006***	-.0006***	-.0007***	**Altitude**	-.0006***	.0006***	**Altitude**	-.0006***	-.0006***
	(.0001)	(.0001)	(.0001)		(.0001)	(.0001)		(.0001)	(.0001)
No Cadastre (=1)	.047	.1							
	(.064)	(.061)							
No Inventory (=1)			.046				**No Inventory (=1)**	.082	.066
			(.058)					(.055)	(.055)
				No Peasant Census (=1)	.077	.009	**No Peasant Census (=1)**	-.017	-.032
					(.084)	(.08)		(.07)	(.07)
Geographical controls	✓	✓	✓	**Geographical controls**	✓	✓	**Geographical controls**	✓	✓
Time trend	✓	✓	✓	**Time trend**	✓	✓	**Time trend**	✓	✓
Period 1 and 2 dummies	✓	✓	✓	**Period 1 and 2 dummies**	✓	✓	**Period 1 and 2 dummies**	✓	✓
Monthly dummies	✓	✓	✓	**Monthly dummies**	✓	✓	**Monthly dummies**	✓	✓
Lagged dependent variable				**Lagged dependent variable**			**Lagged dependent variable**		
28 judicial district FE				**28 judicial district FE**			**28 judicial district FE**		
Observations	7,216	7,216	7,216	**Observations**	7,216	7,216	**Observations**	7,216	7,216
Adjusted R^2	.05	.05	.05	**Adjusted R^2**	.05	.05	**Adjusted R^2**	.05	.05

Notes: Standard errors in parentheses. * p<.10, ** p<.05, *** p<.01.

Table 6
Characteristics of towns with past noble jurisdiction in Córdoba with and without plans

This table computes the mean of the various proxies of inequality and expected re-distribution with land reform ($INEQUALITY_i$) for towns that had been under noble jurisdiction in the eighteenth century. We compute the means with towns that recorded no expropriations in 1933-34 and 1936 (column I) and those that did record expropriation (column II). There are 30 towns in the first group and 18 in the second. '*% area*' is the share of large estates over 250 hectares in the total area of the town. '*% tax*' is the share of tax contributions over 5,000 *pesetas* in the total agricultural taxes paid in the town. '*% expropriable*' is the share of expropriable land in the total land of the town. '*% poor*' and '*%laborers*' measure the share of beneficiaries of land reform in each town. '*% poor*' is the share of household heads included in the Peasant Census in the total population of the town. '*% laborers*' is the share of laborers included in the Peasant Census in the total population of the town. Finally, the expression ['*% expropriable*' × '*% laborers'*] is an interacted supply- and demand-side land reform measure of the intensity of the land reform treatment, $INEQUALITY_i$, which is intended to measure polarization. Column (III) gives the p-value under the null of no difference in means between the two groups. Column IV gives the statistical significance in two-tailed tests.

Variable	**Towns with past noble jurisdiction** **N=41** **(I)**	**Mean in towns with expropriated Grandeza property** **N=8** **(II)**	**p-value of the difference in means. Null hypothesis mean =0** **(III)**	**Significance** **1% ***** **5% **** **10 % *** **(IV)**
% area	28.4	46.9	.08	*
% tax	36.7	61.4	.006	***
% expropriable	19.6	47.1	.002	***
% poor	12.4	14.3	.53	
% laborers	8.7	11.2	.28	
% expropriable×%laborers	152.1	529.3	.0003	***

Table 7
Land reform implementation and rural conflict in Córdoba

This table tests the relationship between land reform implementation and rural conflict. The data set is the monthly panel of municipal observations running from April 1931 to July 1936 (with months from July 1934 to February 1936 excluded) *for towns and villages in the province of Córdoba only*. To compare towns equally exposed to land reform but at different stages of the implementation of land reform, we estimate triple-differences models with the intensity of the land reform treatment interacted with a dummy variable taking value 1 if there were settlement plans approved for the town and 0 otherwise. We test for this relationship in two periods, period 1 from April 1933 to June 1934 and period 2 from March 1936 to July 1936. As dependent variable, column I uses a dichotomous variable taking value 1 if there was at least one recorded instance of collective trespassing in the town or village in the month and 0 if there were none. Column II uses a dichotomous variable taking value 1 if there was at least one recorded strike in the town or village in a given month and 0 if there were none (extensive margin of peasant strikes). Both column I and column II report coefficients of linear probability models. Column III uses the estimate of days on strike in each town or village in a given month as dependent variable. Column IV uses the weighted number of newspaper hits in local and national newspapers referring to peasant strikes in a given town or village in a month. Our main independent variable are the interactions between $INEQUALITY_i$ (in this case, '*% laborers*') and each of the treatment periods, interactions between $INEQUALITY_i$ and a dummy variable ('*settlement*') taking value 1 if preparatory work to settle peasants had started in period 1 or in period 2 and 0 otherwise. In all regressions, we add extra controls affecting the probability of peasants striking or invading land or increasing the intensity of strikes. This the case with population in 1930, local soil quality, average altitude, latitude and longitude at the centroid of the town, dummies for the month of the year, and a time trend. We also add dummy variables taking value 1 if there is missing information on the town in the relevant source to proxy intensity of treatment. Regressions included in this table are simpler regressions without correction for spatial or serial correlation, regressions with different treatment windows and regressions with corrections for serial and spatial correlation can be found in the online appendix.

Dep variable:	I Invasion	II Strike	III Days on strike	IV Impact
Intercept	-.625	-1.244	-4.393	-1.994
	(.386)	(.815)	(5.032)	(1.466)
Settlement plan in 1933-34 yes=1	-.014	.006	-.06	.052
	(.016)	(.034)	(.212)	(.062)
Settlement plan in 1936 yes =1	.003	.007	.02	-.004
	(.01)	(.02)	(.124)	(.036)
% laborers	-.0004	-.002	-.012	-.005**
	(.0007)	(.001)	(.009)	(.002)
% laborers×settlement 1	.0012	-.002	-.005	-.007
	(.0015)	(.003)	(.02)	(.006)
% laborers×settlement 2	.0001	-.0006	-.002	.0007
	(.001)	(.002)	(.012)	(.004)
% laborers×period 1	.0009	.0083***	.027***	.014***
	(.0007)	(.001)	(.008)	(.002)
% laborers×period 1×settlement 1	.0022	-.005	.014	-.008
	(.0017)	(.004)	(.022)	(.007)
% laborers×period 2	.002	.0005	.001	.002
	(.001)	(.002)	(.014)	(.004)
% laborers×period 2×settlement 2	-.0011	.0008	.008	.004
	(.0015)	(.003)	(.02)	(.006)
Population in 1930	2.6e-06	3.5e-06	.00002***	7.3e-06***
	(3.9e-07)	(8.2e-07)	(5.1e-06)	(1.5e-06)
Soil quality	.0007	.002**	.006	.003*
	(.0005)	(.001)	(.006)	(.002)
Altitude	-.00002	-.0001***	-.0008***	-.0002***
	(.00001)	(.00003)	(.0002)	(.00006)
No Peasant Census (=1)	.009	.031*	.021	.04
	(.008)	(.017)	(.105)	(.031)
Geographical controls	✓	✓	✓	✓
Time trend	✓	✓	✓	✓
Period 1 and 2 dummies	✓	✓	✓	✓
Month dummies	✓	✓	✓	✓
Lagged dependent variable				
28 judicial district FE				
Observations	3,271	3,271	3,271	3,271
Adjusted R^2	.03	.12	.06	.08

Notes: Standard errors in parentheses. * p<.10, ** p<.05, *** p<.01.

Table 8
Land reform implementation and rural conflict in Córdoba

This table tests the relationship between land reform implementation and rural conflict, taking into account the past legacy of historical *Grandeza* lands. The data set is the monthly panel of municipal observations running from April 1931 to July 1936 (with months from July 1934 to February 1936 excluded) *for towns and villages in the province of Córdoba only*. To compare towns equally exposed to land reform but at different stages of the implementation of land reform, we estimate triple-differences models with the intensity of the land reform treatment interacted with a dummy variable taking value 1 if there were settlement plans approved for the town and 0 otherwise. We test for this relationship in two periods, period 1 from April 1933 to June 1934 and period 2 from March 1936 to July 1936. As dependent variable, column I uses a dichotomous variable taking value 1 if there was at least one recorded instance of collective trespassing in the town or village in the month and 0 if there were none. Column II uses a dichotomous variable taking value 1 if there was at least one recorded strike in the town or village in a given month and 0 if there were none (extensive margin of peasant strikes). Both column I and column II report coefficients of linear probability models. Column III uses the estimate of days on strike in each town or village in a given month as dependent variable. Column IV uses the weighted number of newspaper hits in local and national newspapers referring to peasant strikes in a given town or village in a month. Our main independent variables are the interaction between the $INEQUALITY_i$ (in this case, '*% laborers*') and a dummy variable ('*settlement*') taking value 1 if preparatory work to settle peasants had started in period 1 or in period 2 and 0 otherwise and an interaction between % laborers and a dummy variable taking value 1 if the town had been under a *Grandeza* noble family in the past interacted with periods 1 and 2. In all regressions, we add extra controls affecting the probability of peasants striking or invading land or increasing the intensity of strikes. This the case with population in 1930, local soil quality, average altitude, latitude and longitude at the centroid of the town, dummies for the month of the year, and a time trend. We also add dummy variables taking value 1 if there is missing information on the town in the relevant source to proxy intensity of treatment. Regressions included in this table are simpler regressions without correction for spatial or serial correlation, regressions with different treatment windows and regressions with corrections for serial and spatial correlation can be found in the online appendix.

Dep variable:	I Invasion	II Strike	III Days on strike	IV Impact
Settlement plan in 1933-34 yes=1	-.015	.017	.01	.072
	(.017)	(.035)	(.218)	(.064)
Settlement plan in 1936 yes =1	.004	-.0007	-.04	-.02
	(.01)	(.021)	(.132)	(.038)
***Historical Grandeza* =1**	-.0004	.002	-.009	-.004
	(.009)	(.018)	(.111)	(.032)
% laborers	-.0002	-.0013	-.017	-.006
	(.001)	(.002)	(.014)	(.004)
% laborers×Historical *grandeza*	-.0003	-.0014	.0037	-.0006
	(.001)	(.002)	(.014)	(.004)
% laborers×settlement 1	.0001	-.0033	-.008	-.009
	(.002)	(.0033)	(.02)	(.006)
% laborers×settlement 2	.0002	.00003	.006	-.009
	(.001)	(.0023)	(.014)	(.006)
% laborers×period 1	-.00008	.0046**	.013	.0081**
	(.001)	(.0022)	(.013)	(.004)
% laborers×period 1×settlement	.003*	-.0026	.022	-.0041
	(.002)	(.0038)	(.023)	(.0068)
% laborers×Historical *grandeza* period 1	.0014	.0053**	.019	.0083*
	(.0011)	(.0023)	(.015)	(.004)
% laborers×period 2	.0027	-.0053	-.0098	-.005
	(.0017)	(.0036)	(.0225)	(.007)
% laborers×period 2×settlement	-.002	.006	.024	.011
	(.002)	(.004)	(.026)	(.007)
% laborers×Historical *grandeza* period 2	-.002	.008**	.016	.009
	(.002)	(.0038)	(.024)	(.007)
Population 1930, soil quality, altitude, no peasant census (=1) included	✓	✓	✓	✓
Geographical controls	✓	✓	✓	✓
Time trend	✓	✓	✓	✓
Period 1 and 2 dummies	✓	✓	✓	✓
Month dummies	✓	✓	✓	✓
Lagged dependent variable				
28 judicial district FE				
Observations	3,271	3,271	3,271	3,271
Adjusted R^2	.03	.12	.06	.08

Notes: Standard errors in parentheses. * p<.10, ** p<.05, *** p<.01.