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Working Papers in Economic History

2021-08
ISSN: 2341-2542

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Serie disponible en http://hdl.handle.net/10016/19600

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Land Reform and Rural Conflict: Evidence from 1930s Spain

Sergi Basco* Jordi Domènech† Laura Maravall‡

April 15, 2021

Abstract

We use a novel high-frequency, municipality-level dataset to examine the impact of land reform on rural conflict in 1930s Spain, a classical example of property rights reform in a developing economy. We distinguish between types of implementation and consider five types of conflicts: land invasions, peasant strikes, clashes, petty theft and attacks on landowner assets. By performing a differences-in differences regression analysis, we document three main results. First, overall, land reform only increased the number of reported petty thefts, lasting around two years, followed by a reversion to pre-reform levels. Second, the effects of land reform depend on its implementation. A technical implementation was conducive, if anything, to fewer conflicts (clashes and attacks). In contrast, a more political implementation (which gave, on average, less land per peasant) increased reported petty thefts and, to a lower extent, attacks on owners' assets. Third, we provide suggestive evidence that the fall in income of settlers (the, *a priori*, beneficiaries) explains the increase in social conflict. Our results highlight the importance of the design and implementation of social policies, especially in the context of an agrarian economy.

JEL Codes: N54, O13, P14, Q10, Q24, Q28.
Keywords: property rights, land reform, conflict, interwar Europe, Spain.

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We thank audiences at Carlos III Madrid, Freie Universität Berlin and the Institutions and Political Economy Research Group (IPERG) at Universitat de Barcelona. Basco acknowledges financial support from Catalan Agency for Management of University and Research Grants (2017SGR1301) and Spanish Ministry of Science, Innovation and Universities (PID2019-104723RB-I00). Domènech acknowledges financial support from Spain's Ministry of Science, Innovation and Universities (PID2019-104869GB-100).

1 Introduction

There exists a general consensus that well defined property rights are important for economic development (Soto, 2001; Auerbach and Azariadis, 2015; Besley and Ghatak, 2010). In most agrarian economies, well defined individual property rights on land determine investment and productivity growth (Besley, 1995; Besley et al., 2016; Besley and Burgess, 2000; Goldstein and Udry, 2008; Ho, 2021; Bellemare et al., 2020; Fenske, 2011; Firmin-Sellers and Sellers, 1999; Brasselle, Gaspart and Platteau, 2002). In this context, policymakers regard land reforms, which provide tenure security and decrease ambiguous ownership claims, as valuable instruments to improve rural areas' economic performance. Indeed, land reform may reduce peasant underemployment, increase investment, and intensify the use of land (Lipton, 2009).

However, it is often the case that land reforms are associated with increases in social conflicts, which reduces their productivity-enhancing benefits (Mason (1998); Albertus (2021); Alston, Libecap and Mueller (2000); Simmons (2004)). Often, land reforms also aim at reducing rural inequality and poverty by altering the existing distribution of property rights. Since land is an immobile, illiquid asset, the owners of land and the landless have strong incentives to fight for this asset (Boix, 2008). Moreover, the potential for policy mistakes in land reforms is high and their deployment typically falls short of objectives (Hirschman, 1963; Bardhan and Mookherjee, 2010; Albertus, 2015; Albertus and Popescu, 2020). Defective design and poor enforcement can increase conflict in the short run (Finkel, Gehlbach and Olsen (2015); Finkel and Gehlbach (2021); Alston, Libecap and Mueller (2000); González and Vial (2021)). In many cases, land reform also leads to diffuse and, in some cases, communal or collective property rights, which the literature has also associated to higher conflict (de Janvry and Sadoulet, 2011; Lipton, 1993, 2009; Albertus, 2021; Alston, Libecap and Mueller, 2000).[1]

In this paper, we study the effects of one of the classic land reforms of interwar Europe (Spain in the 1930s) on rural conflict. The general consensus in the historical literature is that Spain's land reform fatally polarized rural areas in Spain, radicalizing landless peasants and landowners alike and leading to democratic breakdown (Malefakis (1970); Simpson and

[1]The literature on communal rights and violence is extensive in Mexico (Murphy and Rossi, 2016; Villarreal, 2004)

Carmona (2020)). We are the first to combine monthly data on rural conflicts at the municipal level before and after the passing of land reform with various municipal characteristics and land reform-related variables to formally analyze the impact of the deployment of reform on rural conflict. Spain's land reform law falls into the general pattern documented by Bhattacharya, Mitra and Ulubaşoğlu (2019), who found that democratic transitions are linked to pro-poor type reforms and inequality reducing motives. Moreover, Spain's land reform was based on farm size ceilings, was aimed to be productivity-enhancing and non-confiscatory. However, for certain types of landowners, typically from a selective group of aristocratic families, compensation did not exist.

We assess the short-run impact of land reform on conflict by exploiting the heterogeneous timing in the deployment across the municipalities of Extremadura (a landlocked region in the South West of the country, bordering with Portugal). Using temporary expropriations in exchange of a rental payment to owners and then using the declaration of social uses of land, land reform was quickly deployed in the region studied here. In 1933, almost 100,000 hectares were seized in the region to settle 32,500 families, almost 80 per cent of the total lands seized in Spain in 1933-34. With these settlements having expired in the Autumn of 1935, the government expropriated 239,000 hectares and settled more than 81,000 families in this region in March 1936, which represented a third of all the expropriated lands in Spain in March-April 1936. In this paper, we analyze the impact of the 1933-34 wave of reform on rural conflict. The effects of the 1936 wave of land reform cannot be studied because of the onset of Civil War in July 1936. We will use the group of municipalities included only in the 1936 wave of land reform as a control group and perform a differences-in-differences analysis. We distinguish between five conflict events: land invasions, strikes, clashes, petty thefts, and attacks. The potential heterogeneity across event types will inform us about on the mechanisms through which land reform affected conflict.

Another contribution of our work is that we can identify two types of land reform implementation in Extremadura: the more "technical" approach carried out by the Institute of Agrarian Reform (henceforth, IRA) and the more executive and "political" intervention implemented by

Luis Peña Novo, the Prefect of Extremadura.[2] While the first responded to local demands for land and cautiously deployed the settlements following bureaucratic procedures, the Peña Novo intervention accelerated the speed of the reform trying to settle as many landless peasants as possible. Drastic policy variation in land reform across the analyzed regions in Extremadura allows us to testing for the differences in its impact depending on the type of land reform designed. For example, Ghatak and Roy (2007) differentiate between the types of land reforms in India after Independence and find that those entailing land ceiling measures (i.e., redistributing land surpluses to the landless), as opposed to those affecting changes in the tenancy contracts, explain the overall negative effect of the land reform on agricultural productivity. In our case study, we use spatial variation by type of land reform implementation to examine the differential impacts of land reform on rural conflicts.

We perform a differences-in-differences regression analysis to identify the impact of the 1933 land reform on rural conflict. The "treated" group includes the municipalities affected by the first and second waves of the land reform (in 1933 and 1936), while the "control" group are the municipalities included only in the second wave (in 1936). After rejecting the existence of different pre-trends, we find no evidence indicating that land reform had a significant effect on conflict, with the exception of thefts, which increased significantly after the reform, before reversing to the pre-treatment levels after two years. In all these regressions, we also control for population in 1930, wheat yield, and several other variables -unionization rates, total area affected by land reform, inequality, presence of aristocratic owners, presence of anarchist unions, and yearly income per hectare - interacted with the year dummies.[3]

The baseline specification points towards an overall non-significant effect of land reform on rural conflict, with the exception of petty thefts. However, these results could be masking substantial heterogeneity across implementation types. To examine if this is the case, we include an interaction term in our baseline specification to capture the differential effect of the political

[2]The Prefect was the highest government authority in each province, typically dealing with issues related to public order. In 1932, the two provinces of the region of Extremadura -Badajoz and Cáceres- were put together under one prefecture.

[3]We prefer this specification to a regression with municipal fixed effects because we can interact the time-invariant characteristics with year dummies and explore the change in the impacts of these variables over time. The regressions with municipal fixed effects give very similar results.

implementation. We find that the results substantially change once we differentiate between the type of implementation (IRA-led vs. those implemented by Peña Novo). In the municipalities treated with the more "technical" IRA-led reform, petty thefts are no longer significant in the treatment period and, if anything, the reform helped decrease social conflict. In contrast, the municipalities that experienced the political, top-down implementation display significantly higher levels of thefts and, to a lower extent, attacks and clashes. These results highlight the importance of taking into consideration the type of implementation in the analysis.

Finally, we report evidence in favor of the hypothesis that a decline in income of the beneficiaries of the land reform generated rural conflict. We proceed in two ways. First, we show that the number (and ratio) of settlers explains social conflict, which suggests that insiders (the peasants getting land, instead of the excluded ones) were causing social conflict. Then, we document the positive effect of the number of settlers per expropriated hectare on social conflict. Our interpretation is that in municipalities intervened by the Prefect, peasants received insufficient amounts of land, which represented a negative income shock and led to an increased number of petty crimes (e.g., steal fruits or harvested wheat) and attacks on trees, agricultural machinery and other landowner assets (in many cases, to artificially increase the demand for rural labor). This suggests that a defective implementation of land reform rather than redistribution per se, or the resistance of landowners, explains the short run increase in conflict. This would be consistent with explanations of the failure of reform based on insufficient land to settle all the landless peasants (Carmona and Simpson (2015); Simpson and Carmona (2017)).

Related Literature Our work relates to the vast literature on property rights and conflict, especially in the case of property rights on land (Gonzalez et al., 2012). To existing studies on Latin America and Africa, we add the case of settlements of landless peasants in an interwar Western agrarian economy. The fact that these settlements were temporary generated "diffuse" and contested property rights. Although by no means a general rule, diffuse property rights have been connected to greater levels of conflict and violence (Andre and Platteau (1998); Alston, Libecap and Mueller (2000); Dell (2012); Fenske (2014); Murphy and Rossi (2016); Castañeda-Dower and Pfutze (2015); Fetzer and Marden (2017); Castañeda-Dower and Pfutze (2020)).

Our results point at the role of more executive reforms in intensifying conflict, compared to more bureaucratic, less litigious land reforms.

We also contribute to the literature on the deployment of public policies in developing, agrarian economies and its impacts (Hirschman (1963); Scott (1998); Williams (2017)). In particular, we focus on the deployment of large-scale, re-distributive policies in agrarian economies (Bardhan and Mookherjee (2010, 2017); Albertus (2015); Kung, Wu and Wu (2012); Jörgensen (2006). In this literature, land reforms often fall short of their objectives, in many cases for lack of political will, insufficient budgets, poor information, or resistance by owners of land. Our paper addresses the costs of fast-tracking land reforms, increasing litigation and contestation of property rights.

Our paper is also related to the literature on the short-run and long-run effects of agricultural reforms. This literature stresses the very negative effects of collectivist land reforms as opposed to more individualistic reforms (Dell (2012); Lin (1990); Naumenko (2021); Castañeda Dower and Markevich (2018)). In the case of land reforms granting individual property rights, it also points at the potentially disruptive impacts on rural capital and labor markets, in turn reducing the incomes of poor peasants (Ramseyer (2015); Guinnane and Miller (1997)). By looking at different types of implementation and conflicts, our results are consistent with the view that poorly designed land reform might reduce the incomes of reform beneficiaries in the short run, triggering an increase in some types of conflicts. It also points at the costs of diffuse property rights typical of more collectivist land reforms.

Our paper has implications for the historical literature on the determinants of democracy and dictatorship in interwar Europe and, in particular, in Spain. Luebbert (1991) considered the role of land reform in generating social-democratic or authoritarian majoritarian coalitions in the 1920s and 1930s, which in turn explains the survival or collapse of democracies emerging after WWI. Gerschenkron (1966) argued that the inability to expropriate Prussian Junkers at the end of WW1 paved the way to dictatorship in Germany. In the case of Spain, land reform has been linked to democratic collapse, authoritarian reaction, revolution, and civil war (Malefakis (1970); Simpson and Carmona (2020)). In our study, we find that the impacts of land reform on peasant collective action were modest and uneven. These results shed light on the role of

top-down, politically-driven policy implementation in comparison with more technical policy implementations, which are less conducive to conflict.

Finally, although we lack granular quantitative evidence on investment and productivity trends, our results have implications for the debate on property rights definitions and broad economic development (see, for example, Soto (2001) or the reviews in Besley and Ghatak (2010) and Auerbach and Azariadis (2015)). In particular, well-defined property rights on land have been connected to agrarian development and has been shown to reduce poverty (see, for example, Besley and Burgess, 2000) and increase investment (see, for example, Fenske, 2011). The effects of land reform on investment and productivity seem more ambiguous and case specific. For example, whereas Adamopoulos and Restuccia (2020) find that the land reform in Philippines was conducive to a fall in productivity, Ghatak and Roy (2007) document that the Indian land reform increased productivity. These ambiguous results point at the importance of implementation, which is one of the main results of our research. Our findings on rural conflict suggest that the technical implementation of land reform in Spain may have also had positive economic effects beyond the attenuation of social conflicts.[4]

2 Historical background

Sharp agrarian inequalities have characterized Spain's modern history (Malefakis, 1970; Diaz del Moral, 1973). In the 19th century, while land reforms, market forces and structural change reduced agrarian inequalities in most Western Europe economies, Spain maintained high levels of inequality, specially in the center and South-west of Spain (Simpson and Carmona, 2020). It was not until the mid 20th century, when booming labor demand in the main industrial centers (in Catalonia, the Basque country and Madrid) vacated these provinces. These sharp and long-lasting agrarian inequalities were the source of generalized social conflict and violent demands of land redistribution, which were eventually granted by the (unsuccessful) land reform law drafted by the new Republican government (1931).

[4]There exists limited evidence on the long-run effects of land reform. One important exception is Besley et al. (2016) that show that land reform in India contributed to increase inequality. We cannot examine the long-run effects on the Spanish land reform since it was interrupted by the onset of the Civil War in July 1936 and the victory of Francoist forces.

Social tensions in 20th century Spain were intense and it is beyond the scope of this paper to summarize them here (see, Casanova, 2010, for a detailed review). However, we briefly state the most relevant events which led to the land reform. Starting with the *Mano Negra* insurgent activities in the early 1880s, landless laborers had turned to revolutionary ideologies, especially towards Anarchism in the early 20th century and later on towards Socialism (Diaz del Moral, 1973). Landless rural laborers had mobilized in 1903 and in 1918-1920 in extensive waves of union recognition strikes during the so-called Bolshevik triennium. Coinciding with the collapse of the Primo de Rivera dictatorship, the provisional Berenguer government, the exile of king Alfonso 13th and the arrival of a democratic Republic in April 1931, an agricultural crisis in 1930 and 1931 brought a new wave of discontent with violent riots and strikes of rural laborers. In the autumn of 1931, the Republican government began to prepare an ambitious law of land reform. In this sense, Spain was no different than many other interwar democracies passing land reform laws in the 1920s and 1930s (Luebbert, 1991). However, as it has been argued elsewhere (Malefakis, 1970), the Spanish reform was to fail spectacularly in meeting the high hopes of the mass of landless families, due to the inadequacy of state capacity and deep society and party cleavages. In this paper, we will examine the effect of this land reform on rural conflict. In addition, we will explore the heterogeneity on the implementation of this reform.

The land reform law set a threshold to select eligible farms. Farms with a size below that threshold were not eligible. For farms above a threshold, typically 250-300 hectares, all cultivated area above the threshold could be seized and given to landless peasants. The law also stipulated other reasons for expropriation like farms leased continuously for more than 12 years, land illegally acquired in the 19th century after the abolition of noble jurisdictions and land not properly irrigated or badly cultivated. After the general Sanjurjo coup, August 1932, the law also incorporated special clauses for land owned by Grandee aristocratic families, the so called *Grandes de España*, who were accused of being key conspirators in the coup. While most targeted owners were to receive compensation for the expropriated land, expropriated land owned by the *Grandeza* aristocrats were to receive no compensation. Because land owned by aristocrats did not require the costly calculation of compensation, this land was expropriated much faster. This is one of the margins of reform that we are going to explore in the paper.

Table 1: Land seizures and settlements in 1933 under temporary occupation schemes.

Province	Land seized (Hectares) 1933	Settled Household heads 1933	Grandee aristocracy land (hectares)
Badajoz	**53,146**	**18,699**	**31,227**
Cáceres	**45,209**	**13,871**	**92,957**
Salamanca	3,719	893	28,876
Toledo	5,106	1,575	38,523
Ciudad Real	4,357	1,852	11,918
Córdoba	0	0	32,986
Seville	3,843	724	26,938
Huelva	0	0	648
Cádiz	7,645	2,394	33,705
Granada	0	0	3,711
Jaén	280	100	40,728

Notes: Badajoz and Cáceres (in bold) are the two provinces in the region of Extremadura. Table adapted from Malefakis (1970), p. 242, table 30, and p. 378, table 37. Grandee aristocratic land data from Robledo (2012).

The government targeted the most unequal 14 provinces (out of 50). These provinces were mostly in the southern western half of the country. Although the government lacked a proper census of landless families, it was thought, at the time, that there was enough land to settle the approximately 570,000 landless families living in the provinces affected by land reform. However, government plans were widely off the mark, especially in relation to the land available for expropriation (Simpson and Carmona, 2017). By December 1933, only 24,203 hectares had been fully expropriated and 123,305 hectares of land had been *temporarily* seized (by the so-called decrees of intensification of cultivation) mostly in the provinces of Badajoz and Cáceres. We will focus our analysis on these provinces, which belong to the region of Extremadura. Table 1 provides the relevant magnitudes for the land seizures in 1933 in the most relevant provinces. Column (1) displays the total area of land seized in temporary occupations (aristocratic and non-aristocratic), column (2) gives the number of settlers in the expropriated land, and column (3) shows total area owned by Grandee aristocratic families in each province. As it can be seen in column (1) and (2), Badajoz and Cáceres (both in Extremadura) concentrated most of the temporary expropriations in 1933 in terms of both land seized and households settled. As it can be seen in column 3, Cáceres stands out for the amount of land owned by Grandee aristocrats, but there was also abundant aristocratic land in Jaén, Toledo or Cádiz.

The implementation of the law reform was heterogeneous across municipalities. In principle, it should have been a technical reform. However, we have evidence of, at least, one ad-hoc, more political implementation. We will compare the outcome of this ad-hoc implementation with the

rest to investigate the importance of the type of implementation. The main technical body in charge of deploying the land reform, the Institute of Agrarian Reform (henceforth, IRA) closely supervised the settlements, provided technical assistance, supplied seeds and fertilizers and gave loans to local peasant unions to be distributed to the settled families. However, the implementation of the Prefect of Extremadura, Luis Peña Novo, a Republican politician from the North-West of Spain, was less planned, did not follow the bureaucratic procedures of the law of intensification of cultivation, and overrode the role of the IRA.[5]

The appointment of Peña Novo came at the peak of Rightist and Leftist threats to the young republic. The Prefect was appointed with the specific aim of deploying land reform fast to stabilize the Republic and stave off potential revolutionary threats. However, it is not at all clear that the settlements ordered by the Prefect had a counterrevolutionary motive. The number of spontaneous invasions in municipalities selected by the Prefect for quick deployment was rather low compared to some municipalities in the neighbouring province of Badajoz. Peña Novo argued that there was no time for legal or bureaucratic rigorism, as, in his view, the situation in the province of Cáceres was desperate and potentially volatile. Yet, when we look at the data, the situation in Badajoz looked far more unstable. For example, this province had one of the most important bouts of violence (in Castilblanco, December 1931), which had a powerful impact on public opinion (Trullen Floría (2015)).

As explained above, we will use the Peña Novo settlements to assess the effects of different types of land reform intervention, the more "political" Peña Novo interventions and the more technical interventions of the IRA. Historical records show that there was a growing tension between the Prefect and the technical personnel of IRA (Riesco, 2005: 189). While the Institute insisted on a cautious deployment of reform following the law, Peña Novo pressured the minister

[5]In December 1932, the government appointed Luis Peña Novo, previously the Prefect of the province of Cáceres (May 1931-August 1932) and later in the province of Seville (August 1932-9 December 1932), the general prefect in the region of Extremadura (*Gaceta de Madrid*, 342, 7th December 1932, p. 1683). This appointment was based on the law of Defense of the Republic (21st October 1931), which gave the minister of the Interior the capacity to appoint delegates in two or more provinces to enforce the law of defense of the Republic, applying special measures to groups or individuals both on the Right and the Left which conspired against the Republican regime organizing coups or revolutionary attempts. In Seville, there had been an attempted coup by the Rightist police and military groups and an anarchist plot of coordinated insurrectional activities. Peña Novo had been appointed as Prefect of Seville after the failed general Sanjurjo coup (starting in this city) and several anarchist insurrectionary attempts in the province.

of Agriculture to accelerate reform as much as possible. In the view of Peña Novo, in absence of quick reform, the landless would have invaded the lands in January 1933. The inspectors and technical personnel of the IRA insisted that the acceleration of reform in the Prefect-initiated settlements, "as bureaucratic procedures were not followed, created a difficult and unstable position." According to these sources, in the municipalities intervened by the Prefect "the number of legal claims attended by the technical personnel is the same as the number of settlements" (Riesco, 2005: 193), suggesting the existence of costly litigation associated with the settlements sponsored by the Prefect.

The initial wave of temporary settlements in late 1932 and early 1933 were to expire in September 1934. Leftist parties had lost the general election of November 1933 and the Republic took a more rightist turn, less favourable to quick land reform. However, a reformist minister of Agriculture, Manuel Giménez Fernández, rolled over the temporary settlements until the 31st of July of 1935. After that date, the settlements were illegal and settled farmers were technically evicted. The historical literature talks of thousands of evicted families. However, we have not been able to find any hard quantitative evidence on this respect, nor is there evidence of major violent conflicts during the evictions. It is difficult to have an idea of the expectations of settled farmers. Given the abrupt and deep regime change in 1931 and the strong emphasis of the new government on giving "land to the tiller", it is very likely that settlers thought these settlements were permanent. However, there is no evidence on this issue, so, we cannot claim it was the case.

The election of February 1936 led to the formation of a Leftist Popular Front government fully committed to land reform. A decree passed on the 5th March 1936 reinstated the temporary rights of evicted settlers (*Gaceta de Madrid*, 5th March 1936, pp. 1849-50, decree signed on the 3rd March). Furthermore, the government signed a decree on the 20th March 1936 allowing the IRA to accelerate settlements on land affected by land reform (*Gaceta de Madrid*, 28th March 1936, p. 2470). On the 25th March 1936, a large wave of land invasions swept Extremadura. According to figures provided by the IRA, in 1936, more than 80,000 landless families invaded the land of farms affected by land reform (Boletín del Instituto de Reforma Agraria, 46, 47). Using the declarations of social use of land, 573,190 hectares were temporarily seized to settle

114,343 household heads and their families across the affected provinces, again with particular intensity in Extremadura. In this region, 65 per cent of eligible families were settled in 1936 between late March and July 1936. On the 18th July of 1936 there was a military coup which led to the Civil War (1936-39).

In this paper, we study the impacts of the 1933 wave of land reform on rural conflicts. The 1936 wave is not analyzed. We will use the affected municipalities in this second wave to create a control group. The main reason for not analyzing the 1936 wave is that it coincides with the onset of Civil War, which puts a severe limit to the number of observations after treatment. Moreover, since it was almost universal in the targeted provinces, we cannot exploit municipality variation within the region. Finally, the potential effects of the 1936 reform are affected by the resolution of the first wave of land reform, confusing the effects of the first and second wave. For these reasons, our study will compare a "treated" group of municipalities, those included in both the first (1933) and second wave of reform (1936), and a control group of municipalities included in the latter (1936). Almost all municipalities included in the first wave of reform were also include in the second. According to our data, in Extremadura, the exception were 8 municipalities. Most of them were really small. The only important municipality was Alburquerque, one of the largest municipalities in the province of Badajoz. The historiography has not provided any rationale for the exclusion of this municipality in the 1936 reform. As a robustness check, we include Alburquerque in the group of "treated municipalities" and show that our main results hold.

3 Empirical Predictions, Data and Empirical Strategy

3.1 Main empirical predictions

In this section we discuss how the land reform in Spain may have affected rural conflict. In particular, we emphasize three different types of empirical predictions: (i) the overall effect of the land reform, (ii) the heterogeneous effects across implementation types, and (iii) the heterogeneous effects across types of conflicts. Finally, we discuss how we can test the channel though which the land reform affected the number of conflicts.

3.1.1 Overall Effect

Theoretically, there is no clear consensus on the overall effects of redistributive policies on conflict. In most models of political transitions (e.,g Acemoglu and Robinson, 2006; Boix, 2008; Collier and Hoeffler, 2004), the poor fight to obtain redistribution because their opportunity cost of violence is very low. In this context, a land reform, which redistributes resources towards the poor, should appease them by increasing the opportunity cost of engaging in violence, crime or conflict and, thus, reduce conflict (Gillezeau, 2015; Fishback, Johnson and Kantor, 2010; Jah and Shayo, 2019; Fetzer, 2020). However, many factors can lead to greater conflict during the implementation. This is especially the case for land reforms, which affects poor and rural economies. For example, poorly informed and low-capacity governments have to rely on local collective action to deploy reform, which may give incentives to local unions to protest or use violence to catch the attention of policy makers (Dasgupta, Gawande and Kapur, 2017; González and Vial, 2021). This in turn implies that land reform can help organize landless peasants, increasing mobilization and conflict in the short run. Finally, low capacity states might not be able to enforce land reform if they face a stern opposition by landowners (Finkel and Gehlbach, 2021).

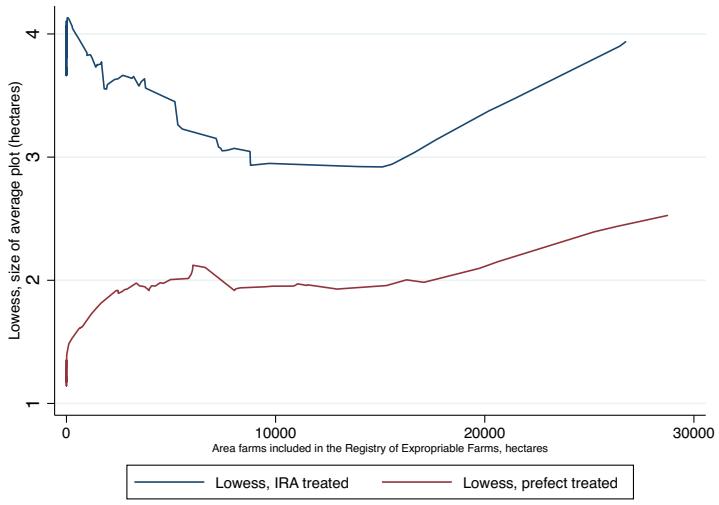
3.1.2 Heterogeneity Across Types of Implementations

To examine the importance of implementation, we will compare the effect of the ad-hoc political implementation of land reform by Prefect Peña Novo with the IRA-led technical land reform. Theoretically, we expect the more executive, top down Peña Novo settlements to be more conducive to conflict.

One important difference of the Peña Novo settlements is that they were on, average, more crowded than the IRA-led settlements, meaning the average size of plots given to landless families in the Peña Novo settlements was much smaller. In Figure 1, we plot Lowess functions with average settlers' plot size in the vertical axis and the total area of farms included in the Registry of Expropriable Property in each municipality. Farm sizes for settlers in the Peña Novo settlements are consistently smaller. The average size of each family plot in the ordinary IRA-

led settlements was 3.75 hectares. In contrast, in the municipalities intervened by Prefect Peña Novo, the average size was only half of it (1.89 hectares per family). It is very possible that other sources of income from casual work or tenancy contracts dried up with land reform. For many families, land reform could have reduced their already meagre incomes. This negative income shock could directly translate into more theft and attacks, but also could have an impact on protests. More generally, a defective land reform can reduce the incomes of very poor peasants in the short run beyond the average size of the area. Indeed, settlements may require large investments in buildings, equipment, seeds, fertilizers and machinery that might not be supplied in the short run. Poor knowledge of local conditions means that sometimes plots were too small to be viable or were located in unsuitable land. Finally, inadequate administrative capacity or poor targeting of eligible peasants implies that many deserving peasants could be excluded from the settlement plans, which could generate conflict among the groups of potential beneficiaries. As discussed above, we think that the settlements of Peña Novo are more likely to fall into this category and, thus, be conducive to an increase in rural conflict.

Figure 1: Average size of settlements in 1933, Lowess functions.



Notes: This figure plots Lowess functions for the average size of settlements (in hectares) in municipalities intervened by the IRA (blue) and by the Prefect (maroon). The horizontal axis is the total hectares of farms affected by land reform in each municipality.

3.1.3 Heterogeneity across types of conflicts

The effects of land reform need not to be homogeneous across all types of conflicts and this heterogeneity may help us to understand the source of conflict. We will look at five types of conflicts: (i) illegal land invasions, (ii) peasant strikes, (iii) violent clashes with the rural police (*Guardia Civil*), (iv) petty theft and (v) attacks on landowner's assets, typically trees (probably a way of increase the area of arable) or machinery (as peasant unions tried to limit the use of machinery to maximize employment during the harvest and other periods of high labor demand). An increase in the number of strikes and violent clashes would be consistent with greater landowner opposition or peasants looking for the attention of the government (González and Vial, 2021). In addition, since settlements were, theoretically, temporary, we could observe more conflicts as the date to roll over temporary settlements was about to expire. An increase in petty theft and attacks on landowners' assets might be consistent with a negative income shock. Peasants may end up poorer with the settlement, if they are given too little. It could also be the case that landowners were more willing to denounce landless peasants for engaging in practices like gleaning, which had been tolerated in previous periods. Finally, we mostly disregard the results on invasions because it may be endogenous. Indeed, land invasions might have intensified in some municipalities before the passing of land reform, which could have increased the probability of the reform being implemented in that municipalities. Thus, we will abstain from making a causal interpretation when using land invasions as dependant variable.

3.1.4 Mechanism: Insiders vs Outsiders

One potential channel generating conflict is the targeting of landless peasants. If settlements did not reach all landless peasants, then the group of excluded peasants could be the one generating acts of violence. In contrast, it could be that the settled peasants were experiencing a negative income shock (as explained above) and reacted to this shock by creating conflicts. To identify the group generating conflict, we will use two measures: (i) the log of settled household heads in each municipality and (ii) the ratio of settled household heads to eligible household heads in

each municipality (according to the Peasant Census[6]). If the beneficiaries of land reform were the main perpetrators, these two variables, especially, the ratio of settlers, should be positively related to the rise in conflict. If the excluded peasants were behind the rise in conflict, the relationship between conflict and the ratio of settled to eligible should not be significant.

3.1.5 Mechanism: Income Effect

As discussed above, if we find evidence supporting the idea that insiders were generating conflict and conflicts were exacerbated when the reform was implemented by Peña Novo, it is suggestive that the income effect was driving the increase in rural conflict. To better document this mechanism, we will consider the number of settlers per expropriated hectare. We argue that this variable is a proxy for the potential negative income shock withstood by settlers. In municipalities with a high number of settlers per hectare, average plot sizes were small. In the dry farming conditions of Extremadura, farm sizes below 4 hectares were probably insufficient (Simpson and Carmona, 2017). In addition, the expropriations and settlements necessarily contracted the demand for labor and the Republican government constraint of temporary migrant workers[7] contributed to considerably reduce the other potential sources of income of peasants. Thus, according to this income effect, we expect a larger increase in conflicts in municipalities with more crowded settlements.

3.2 Data Description

This section describes our data on social conflict, how we measure the implementation of the land reform and the main control variables.

3.2.1 Data on Social Conflict

We have collected monthly data on social conflicts at the municipality level. The main original sources are García Pérez (1982) and Méndez (2015). These historians compiled the counts of

[6]The Peasant Census (*Censo de Campesinos*) was compiled in 1933 by the government to gather evidence on the number of landless peasants in each municipality who were to be given land with land reform. The census has been digitized by González and Brel (2013).

[7]The government introduced a law,*ley de términos municipales*, which prohibited the hiring of laborers from other municipalities in each local labor market.

conflict events from newspapers and Spanish Ministry of Interior sources (generally, communications on conflicts sent by the prefects of each province to the ministry), grouping conflicts following the categorization of conflicts used at the time. To the best of our knowledge, we are the first to have compiled a longitudinal database of conflict in the region, using the very detailed secondary literature. Even though some of the conflicts are self-explanatory, we briefly define them below.

- Strikes: number of strikes exclusively organized by peasant unions or general strikes in which peasants participate. We use arrests of peasants during the general strike of June 1934 reported in Méndez (2015) to complement the information for Badajoz.[8]
- Violent clashes: includes all events related to local protests (not strikes), riots, and clashes with the rural police (*Guardia Civil*).[9]
- Petty Theft: denounced acts of theft of fruits (frequently acorns, also olives), harvested wheat, and/or animals like pigs.[10]
- Attacks: includes all the attacks on machinery, trees, and draft animals.[11]
- Land invasions: counts of denounced land invasions.[12]

Our dataset covers 384 municipalities, for 57 consecutive months between April 1931 and December 1935, which amounts to 21,888 municipality-month observations. Our initial date coincides with the regime change in Spain from a non-democratic monarchy to a democratic Republic. We choose to finish the sample in Decemeber 1935 to avoid our results being distorted by the second wave of land reform in late March 1936 and the start of the Civil War (the coup of the 18th July of 1936).

[8] Méndez (2015), p. 450. We assume there was a peasant strike in the municipality if at least one peasant was arrested.

[9] In "Violent Clashes" we include "*Alteraciones del orden público*", "*Movimientos Revolucionarios*", "*Enfrentamientos con la Guardia Civil*" from Méndez (2015) and "*Alteraciones del orden*", "*Manifestaciones y desmanes*" and "*Choques con la Guardia Civil*" from García Pérez (1982)

[10] "*Robos y Hurtos*" in Méndez (2015) and "*Robo frutos*", "*Robo frutos (bellotas*", "*Robo frutos y animales*", "*Robo animales*" and "*Robo mieses*" from García Pérez (1982)

[11] "*Destrozos*" and "*Destrozos de máquinas*" in Méndez (2015), "*Destrozos*" in the case of García Pérez (1982)

[12] This category combines "*Invasiones de Fincas*" and "*Roturaciones arbitrarias*" in Méndez (2015) and "*Roturar arbitrariamente*" typically, but also "*trabajar sin permiso*", "*Repartirse terrenos*" and "*Asalto de fincas*" in García Pérez (1982).

3.2.2 Data on Land Reform Implementation

We use information on whether a given municipality had temporary settlement to construct our variable of interest (i.e., treated vs control group). We obtain this information using different sources (BIRA, 16, pp. 52-8; Riesco (2005), 477-8). According to our data, 146 municipalities had temporary settlements in 1933, 298 municipalities reported temporary settlements in 1936 (77 per cent of all municipalities), 8 municipalities reported settlements in 1933 but not in 1936, and 74 municipalities did not report settlements neither in 1933 nor in 1936.

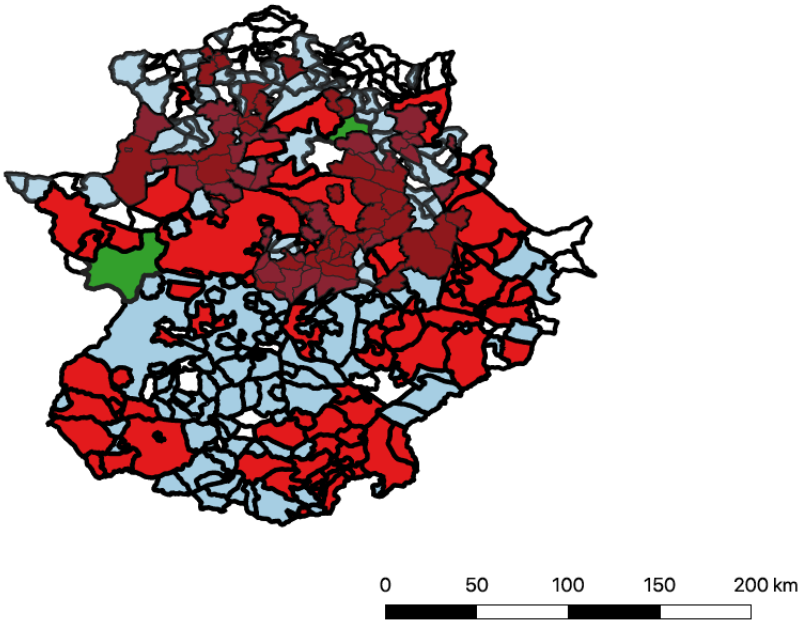
Figure 2 provides the spatial distribution of all categories. We label "treated group" as those municipalities which had settlements both in 1936 and 1933 (red and maroon in the map). Our "control group" are municipalities that had settlements only in 1936 (blue). Thus, we exclude from the sample the 74 municipalities that never had land reform in the 1930s (white in the map). Table 2 shows that this latter group of municipalities had very different observed characteristics. In our baseline, we also drop the 8 municipalities having reform in 1933 but not in 1936 (green in the map). It could be argued that these municipalities were different for observed but also unobserved characteristics.

Lastly, when we compare implementations of Peña Novo with the IRA technical intervention, we know the 62 municipalities where the prefect implemented the reform in 1933 (maroon in the map). In addition, we will explore the intensive margin of reform by looking at the number of settlers, the ratio of settled household heads with respect all eligible household heads (typically landless), and the number of settlers per expropriated hectare of land (a proxy for overcrowding).

3.2.3 Additional Control Variables

Our database is completed with information on municipality characteristics. We have collected a series of demographic, agricultural, geographic and political-institutional variables. We include the population in 1930 from Spain's Population Census to control for the effects of municipality size on conflict (we would expect larger municipalities to have more conflicts). To capture structural characteristics of agriculture in each municipality, we use total area (in hectares) of the municipality, the area covered by farms affected by land reform (total area of farms included

Figure 2: Municipalities with temporary settlements in 1933 (red and maroon municipalities)



Notes: Treated municipalities shaded in red and maroon. Maroon municipalities are the municipalities intervened by the Prefect. Light blue municipalities are the control group (municipalities *not* having land reform in 1933 and having it in 1936). The municipalities in green are the municipalities included in the first wave (1933) but not in the second. Municipalities in white are the municipalities *without reform in the two waves* (1933 and 1936). Both the Green and White municipalities are excluded from the sample of municipalities in the baseline regressions.

in the Registry of Expropriable Property from Riesco (2005) and Rosique (1988)). From Carrión (1975), we compute income per hectare in the 1920s, the ratio between large landowners and total number of landowners, and the ratio between very high taxable agricultural incomes (above 5,000 *pesetas*) in total agricultural income. We collect information at the municipality level on the presence and size of farms owned by Grandee aristocratic families (*Grandes de España*) from IRA (1934: 45, 134-135).

Finally, we have also collected several institutional characteristics for each municipality. We have computed estimates of peasant unionization in October 1931, one year before the passing of land reform (union members of local peasant unions as percentages of the local population in 1930, original data from the *Gaceta de Madrid*, 293, 20th October 1931: 426-7). This information also provides the year in which the local union was created. We add a

dummy variable for the presence of the most radical anarchist unions (from Méndez, 2015 and Ayala Vicente, 2002) and we also have information on the existence of a branch of the Socialist Peasant Union (*Federación Nacional de Trabajadores de la Tierra*) in the Summer of 1932 (the main source is FNTT, 2000).

3.2.4 Control, Treated and Excluded Municipalities

We now briefly discuss the observational characteristics of the four groups discussed above and why we exclude some municipalities from our baseline specification. Table 2 reports the means and standard error of the means of the most relevant variables for four groups: (i) settlement in 1933 and 1936 (treated), (ii) settlements only in 1936 (control), (iii) no reform (excluded) and (iv) only reform in 1933 (excluded). The number of observations in each group-variable bin varies due to data availability.

First, by looking at group 3 (no settlements either 1933 nor 1936), we can see that these municipalities are different. On average, they were much less populated (less than half of the control group, group 2 in the table), they were smaller (around half of the control group) and had much less expropiable area (roughly, one fifth of the control group). In addition, levels of peasant unionization were half of the control group. Lastly, although there is a lot of missing information from the Land Registry, the municipalities with some information on agricultural taxable incomes show relatively low levels of inequality ('share largest landowners') and lower levels of agricultural income per hectare.

Then, the 8 municipalities included in land reform plans in 1933 but not in 1936 (group 4), which we also exclude from our baseline regression, are also special in many dimensions. First, we want to emphasize that looking at the means is misleading because the sammple is very small (8) and it includes one large municipality (Alburquerque) and several very small ones. In addition, two of these municipalities had abundant expropriable land, pushing the average upwards and were quite unequal (as measured by the share of total yearly agricultural income controlled by the largest landowners, according to the Land Registry) and they have lower levels of income per hectare. These municipalities had really zero peasants unionized except in one

municipality (Higuera).[13]

Finally, we note that municipalities in group 1 (treated) and group 2 (control) were much more similar. For example, population difference were not statistically significant between the two groups. The share of Leftist vote was very similar in 1931 and the difference was not statistically significant. In addition, neither the income per hectare, nor local inequality (measured by the share of total agricultural income taken by those with highest estimated incomes) show statistical significant mean differences between group 1 and group 2. However, for the rest of the variables, the two means are different. For example, the size of the municipality (in hectares) and the area of farms affected by land reform were larger in the treated group. Lastly, levels of unionization were significantly larger in the treated group. Thus, we cannot conclude that the two groups were the same, but we will control for these observational differences in our estimation. In addition, we will make sure that the parallel trend assumption on the outcome variable is satisfied. Given that we have observations for 19 pre-treatment months, giving 5,548 pre-treatment municipality-month observations, we have enough statistical power to test for the existence of different pre-trends in the treatment and control groups.

To conclude, Table 3 provides summary statistics for the baseline sample (control and treated municipalities). There are 292 municipalities and we consider 57 months. Except for the conflict variables, all the other variables are time- invariant. The variables "inequality of taxable income", "Inequality owners" and "Income per hectare" have 83 observations that are imputed with multiple imputation.[14]

3.3 Suggestive Evidence on the Effects on Social Conflict

Before formally estimating the effect of land reform on rural conflict, we discuss the evolution of the dependent variables for the treated and control groups. Figure 3, shows the monthly evolution of the five events discussed above between April 1931 and December 1935. The blue

[13] In unreported regressions, our results are robust to the inclusion of these municipalities. These regressions are available upon request.

[14] We have performed 10, 20, and 30 multiple imputations using the Stata command mi impute mvn, which fills missing data using multivariate normal regression, in this case we use longitude, latitude, altitude, the log of population in 1930 and the municipal average of the wheat yield, from FAO-GAEZ, wheatyield-lowinput-rainfed estimates. Results do not change when more imputations are added. In the online Appendix, sections J to N, we also display the results without multiple imputation. Results are not altered.

Table 2: Observable characteristics, differences by municipality group.

	Pop 1930	Area Hec	Area expr hec	% Union Peasants	% Left 1931	Income per hec	Share largest landowners
Group 1 Reform in 1933 AND 1936							
N	138	139	139	105	137	108	103
Mean	3,655	14,860	7,658	.225	.59	37.27	.545
se (mean)	309	1,654.2	1,386	.041	.023	1.66	.027
Group 2 Reform ONLY 1936							
N	160	158	160	132	148	110	107
Mean	3,199	9,617	3,224	.11	.57	40.47	.513
se (mean)	374.8	1,209	774.5	.035	.024	1.73	.044
Group 3 No reform							
N	74	75	75	65	70	17	16
Mean	1,503	5,327	675.1	.059	.371	30.74	.351
se (mean)	154.79	542.75	235.7	.02	.033	3.64	.054
Group 4 Reform in 1933 No reform in 1936							
N	8	8	8	8	8	7	7
Mean	1,826	13,554	7,624	.049	.19	18.87	.51
se (mean)	1,185	8,307	5,629	.049	.11	5.71	.071

Notes: This table gives the means of the selected variables for each group of municipalities. The *wave 1933* group includes all municipalities affected by land reform in 1933. N is the number of municipalities, mean is the group mean and se(mean) is the standard error of the mean. Pop1930 is the population in 1930, Area is the total area of the municipality expressed in hectares, 'Area expr' is the area covered by farms affected by land reform and included in the Registry of Expropriable Property. % unionized peasants in 1931 is the number of peasants affiliated to peasants union divided by the estimates of landless peasants from the Peasant Census (*Censo de Campesinos*). % Left 31 is the percentage of vote cast in favour of Leftist parties in the general election of 1931, % 'Income per hec' is the total taxable agricultural income per hectare (total taxable agricultural income as estimated by the Land Registry divided by the total area in hectares of the municipality. Note this is total area, not total agricultural area. "Share largest landowners" is the ratio between the total taxable income of the largest landowners (above 5,000 *pesetas* of yearly income) divided by total taxable income as estimated by the Land Registry. Population Census for population, Carrión (1975) for area and income, *Gaceta de Madrid* for union presence, voters and potential electors from Boletín de la Provincia de Badajoz and Ayala Vicente (2001). Data from the Peasant Census (*Censo de Campesinos*) compiled in 1933-35 from González and Brel (2013).

line represents the control group, the red line the treated one. Month 20, November 1932, is marked with a vertical blue line to represent when the land reform was approved in the selected provinces[15]. We can see how rural conflict was generally higher after the passing of the land reform in both groups. This pattern seems clearer for land invasions, petty theft and attacks.

We briefly describe, looking at Figure 3, the evolution of each of these conflicts. For inva-

[15] *Gaceta de Madrid*, 297, 23rd October 1932, p. 522; *Gaceta de Madrid*, 308, 3rd November 1932, p. 762)

Table 3: Summary statistics.

	Obs	Mean	Std Dev	Min	Max
Invasions	16,664	.019	.191	0	6
Strikes	16,664	.007	.116	0	1
Violence	16,664	.007	.09	0	1
Petty Theft	16,664	.019	.216	0	7
Attacks	16,664	.007	.088	0	1
1933 Treatment	16,664	.46	.499	0	1
Pop 1930	16,664	3,470	4,325	151	43,726
Log Pop 1930	16,664	7.706	.91	5.01	10.69
Area (hect)	16,664	12,079	17,511	360	175,033
Expropr area	16,664	5,302	13,410	0	145,893
Wheat yield	16,664	2.11	.17	1.83	2.64
Ineq. income	13,514	51.7	36.8	0.06	421
Ineq. owners	13,514	2.8	3.9	0.18	45.31
Income hect.	14,036	37.7	17.8	0.286	112.9
Grandee arist.	16,664	.47	.5	0	1
% union	16,664	.03	.06	0	.28
Anarchism	16,664	.11	.31	0	1
Longitude	16,664	39.2	.62	38.07	40.3
Latitude	16,664	-6.1	.52	-7.5	-4.9
Altitude	16,664	430.9	.138.7	188	941.8

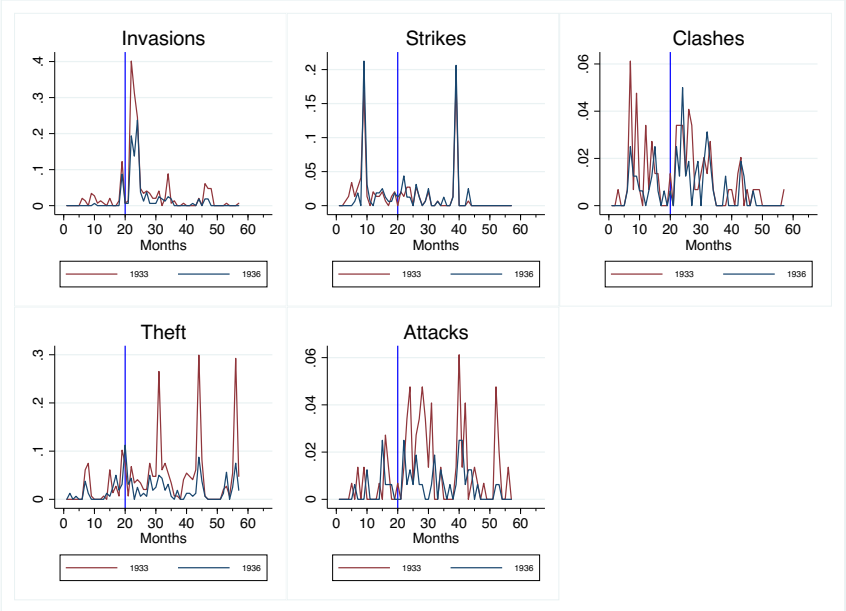
Notes: *Expropr area* is the area covered by farms affected by land reform included in the Registry of Expropriable Farms. Data Sources: Conflicts: Méndez (2015); García Pérez (1982). 1933 treatment: BIRA, Riesco (2005), p. 477-8. Pop 1930: Spain's Population Census. Area: Carrión (1975). Expropriable Area: Rosique (1988); **?**); Riesco (2006). Wheat yield: municipal averages calculated from FAO-GAEZ, wheat, low input, rainfed. Inequality income, inequality owners, income per hec: Carrión (1975). Grandee aristocracy: Suplemento del Boletín del Instituto de Reforma Agraria, Datos recopilados sobre las provincias de Badajoz, Cáceres y Huelva, Madrid, 1934. Percentage unionized in October 1931: *Gaceta de Madrid*, 293, 20th October 1931: 426-7. Anarchism: Méndez (2015), Ayala Vicente (2002). Longitude, latitude, altitude: IVIE.

sions, there is an uptick for both groups of municipalities in October 1932. However, the largest peak for both groups is after November 1932, especially in January 1933. After this outburst, the two series collapses to close to zero with some increases in 1934 and 1935. Something similar happens with strikes, there is a sharp increase in strikes for both groups of municipalities after November 1932 but there is also an intensification of conflict before November 1932. Violent clashes, on the other hand, display a different trend over time, generally at much lower levels since these events are generally more exceptional. Violent conflicts were high in 1931 in these provinces, the most famous episode being the events of Castilblanco (Badajoz), in December 1931, during which four Guardia Civil members were lynched by a mob of peasants. In this case, both groups of municipalities display high levels of violent conflicts in the months before the deployment of land reform. After November 1932, both series maintain high levels, the decline phase happens only during 1935. Finally, for theft and attacks, we also see an increase in conflict after the passing of land reform. Theft typically happened in the last months of the year, as peasants invaded the largest farms to glean acorns and sometimes olives, which were important alternative sources of food or income, especially during the months in which they were unemployed. Attacks also display a cyclical pattern, being typically higher during the harvest, normally in June.

For our identification strategy, it is important that the trend of conflicts in the two groups before the passing of the land reform was similar. Although we will formally test this assumption below, graphically, the series of strikes and attacks are fairly similar between both groups, and the series for theft are also very close, before November 1932. A potential concern may be for invasions and clashes, but we will test formally if the pre-trends are different.[16]

[16]In any event, to have an idea of the effect of the land reform we need to compare the difference between the red and blue line. We do this exercise in Figure 11 in the Appendix, in which we only report the difference in the outcome variable of the treated and control group. Note that values close to zero imply that there is no different between both groups. In contrast, high values post-reform imply that the land reform increased social conflict. Looking at the figure, it would seem that only for petty thefts and attacks the difference were higher after the land reform. That is, it seems that there is a differential increase in the number of these events in the treated municipalities after the trade reform. We will formally perform this diff-in-diff estimation in the next section.

Figure 3: Evolution of average monthly conflicts in treated and untreated municipalities.



Notes: This figure plots the time series evolution of the monthly average of conflicts in "treated" and "untreated" municipalities. The vertical blue line marks the passing of the legislation on temporary occupations of land

3.4 Empirical Strategy: Differences-in-differences

We will identify the effect of the land reform by comparing the performance of the treated municipalities with our control group after the shock. To formally estimate this effect, we consider a differences-in-differences strategy. In the next section, we discuss the validity of the parallel trend assumption.

In particular, we consider the following baseline specification,

$$C_{i,t} = \alpha + \beta_t * R_i * Y_t + \beta * R_i + X_i' * \gamma_1 + X_i' * Y_t * \gamma_{n,t} + Y_t + m_t + \epsilon_{i,t}, \quad (1)$$

where $C_{i,t}$ is the number of rural conflicts in municipality i in month t. We will run this regression for each type of social conflict: (i) illegal land invasions, (ii) peasant strikes, (iii) violent clashes between peasants and the police, (iv)petty theft and (v) attacks on machinery and other landowner assets. R_i is the treatment variable, which takes value 1 for municipalities being selected in the the reform of 1933 and 1936 (treated group) and 0 for those being selected only in 1936 (control group). X_i is a set of time-invariant municipality controls, Y_t and m_T are

year and month fixed effects. We include m_T to capture the seasonality of conflicts (invasions more typical in January and February, petty theft in November, attacks in June). We take 1931 as our reference year, so β_t should be interpreted as the increase in year t relative to the reference year 1931. Year 1932 was only affected by land reform at the very end, so we consider it a transition year, whereas years 1933, 1934 and 1935 are the years affected by the first wave of reform (or lack of it). We estimate all the models with OLS with population weights and we cluster the standard errors at the municipality level to avoid the problems of serial auto-correlation in differences-in-differences designs (Bertrand, Duflo and Mullainathan, 2004).

The vector of time invariant municipality controls, X_i, includes the log of the population in 1930, wheat yield, the log of total expropriable area, a dummy for the presence of Grandee aristocratic land (IRA, 1934), and a series of geographical controls. We include longitude, latitude and an interaction between longitude and latitude to control for hidden spatial patterns and altitude, for unobserved characteristics like ruggedness or characteristics of the terrain that might affect conflict. Note that we also include the interaction between these municipality controls and the vector of year fixed effects. We argue that this specification is superior to include only municipality fixed effects because we allow the effect of this municipality variables to be different over time. We are confident that our municipality controls absorb most of municipality specific effect. Indeed, in the Online Appendix, sections O to S, we also show that our results can be replicated in a specification with municipality fixed effects. Note that, in this case, we cannot include the interaction between municipality and year fixed effects because it would be co-linear with our variable of interest.

One important assumption to correctly estimate the effect of land reform is that the two groups had parallel trends before the treatment. In other words, when estimating our baseline specification we should find that β and β_{1932} are not significantly different from zero. As we said, 1931 is our reference year, so β represents the differential number of conflicts between both groups in 1931. We report these coefficients in the next section and confirm that this is the case.

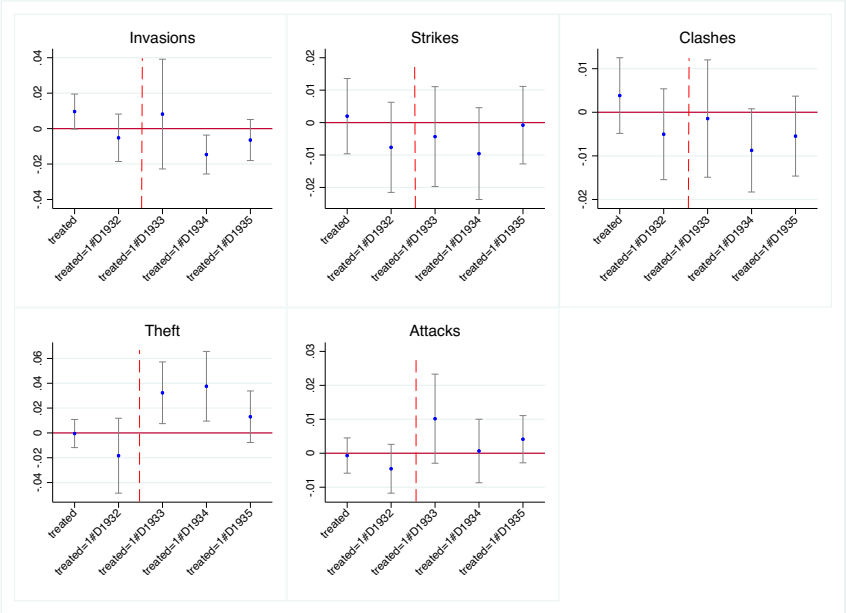
4 Effects of the Land Reform on Social Conflict

In this section we report the main results of the paper. First, we examine the overall effects of land reform on social conflict. Then, we exploit the heterogeneity on the implementation of the reform to investigate whether the effects differed in both cases.

4.1 Overall Effects

Figure 4 reports the coefficients of β and the four β_t from equation 1 (one for each year other than the reference year) along with their confidence intervals. The treatment effect of the land reform is captured by coefficients β_{1933}, β_{1934} and β_{1935}. Each panel of the figure reports the effects for a different type of social conflict. The values of the point estimates along with the coefficients of other variables can be found in Table E in the Appendix.

Figure 4: Treatment effects of first wave of land reform. β_t coefficients from equation [1]



Notes: The figure plots coefficients from regression using equation 1 for each of the conflict types.

The first thing that we want to emphasize is that β and β_{1932}, the pre-reform coefficients, are not different from zero in any of the five types of conflicts. As argued above, it means that these two groups of municipalities did not have significantly different number of conflict before the reform. The only concern is on the coefficient for invasions in 1931, which is positive

and significant at the 10 per cent level. However, in 1932, right before the reform, it was not significant. Thus, we consider that the pre-reform parallel trend assumption is satisfied for all types of social conflict.

We now discuss the effects of the land reform on rural conflict. Looking at all panels, we can see that there was no a generalized increase in conflict after the implementation of the land reform. Indeed, β_{1933} is not statistically different from zero in four out of the five types of conflict. β_{1933} is only positive and statistically significant for the case of petty theft. More importantly, the increase in petty theft was not only significant in 1933 but also in 1934. That is, the effects of the land reform had some persistence. In 1935 the coefficient is positive but it is not significant at conventional levels. This result implies that the increase in theft was temporal (two years) and it returned to pre-reform levels after that. The fact that the increase in social conflict is only on theft suggests that a decline in income may be driving this result. Remember that petty thefts were denounced acts of stealing fruits, harvested wheat or animals, which could be performed by peasants in greater need of food. Quantitatively, the increase in petty thefts was .032 in 1933 and .038 in 1934. The average counts of Petty Thefts were .017 in 1931-1935, .024 in 1933 and .032 in 1934. Thus, compared to the petty theft averages, the estimated effects of the reform are quantitatively large.

Lastly, we turn our attention to the effects on invasions. As explained above, we need to be careful interpreting these results because land invasions may be endogenous to land reform. We can see that the coefficient for invasions is only statistically different from zero in 1934. In that year, the number of land invasions was lower in the treated municipalities. However, it was not different from zero either before or after, so, we do not have an explanation for this particular year.

Summing up, the coefficients presented in Figure 4 suggest that, with the exception of petty thefts, land reform did not increase rural conflict. However, if the effect of land reform depends on its implementation, this overall result may be masking substantial heterogeneity across municipalities. In the next section, we attempt to disentangle this overall effect by analyzing the effects of the ad-hoc political implementation of Peña Novo.

4.2 Heterogeneous Effects Across Implementations

We now examine how the effects of the land reform may vary with the type of implementation. As discussed in the historical section, there were two very different implementations of land reform in this region. On the one hand, the more technical IRA-led implementation. On the other hand, the more ad-hoc (or political) implementation of the General Prefect of Extremadura, Luis Peña Novo. The effect of land reform in the municipalities where Peña Novo implemented it may be different for a variety of reasons. One important reason is that, as shown in Figure 1, the settlements ordered by Peña Novo were typically smaller, which might have reduced the incomes of landless peasants. Given the evidence on petty thefts reported above, it may be that all the increase in social conflict may be explained by this political implementation of reform.

In order to decompose the impacts of the political vis-à-vis the technical reform, we include an interaction with a "prefect" municipality dummy. In particular, we consider the the following specification,

$$C_{i,t} = \alpha + \delta_t * R_i * P_i * Y_t + \delta * R_i * P_i + \beta * R_i + \beta_t * R_i * Y_t + X_i' * \gamma_n + Y_t + m_t + \epsilon_{i,t}, \quad (2)$$

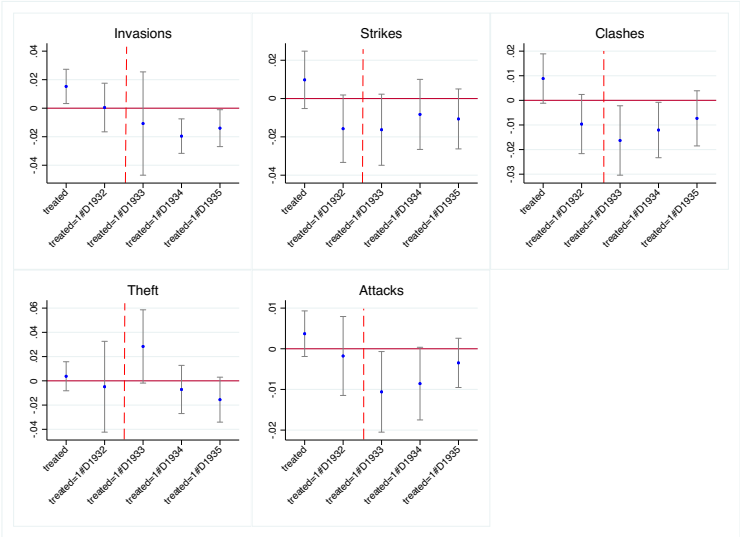
where $C_{i,t}$ represents the monthly count of conflicts, R_i is a dummy for a municipality having reform in both 1933 and 1936 (treatment group in the baseline specification), P_i is a dummy for a municipality with the prefect-initiated settlements, X_i are the usual time invariant municipal characteristics, Y_t and m_t are the year and month fixed effects. Analogous to our baseline specification, δ and β are the effects in the reference year (1931). Note that the effect of IRA-led land reform is β_t, the differential effect of prefect-initiated reforms is δ_t and, thus, the total effect of prefect-initiated land reform is given by the sum, $\beta_t + \delta_t$.

Figure 5 reports the β_t coefficients, which represent the effects of the technical IRA-led land reform on social conflict.[17] Compared to our previous results, Figure 4, the most importance difference is that the coefficient on petty thefts is not longer positive and statistically significant

[17] Values of coefficients and coefficients on the other variables in the regression can be found in Table F in the Appendix.

for years 1933 and 1934. In other words, by removing the effect of the expropriations of Peña Novo, we observe that the land reform did not lead to an increase in the number of petty thefts. This result is supportive of the idea that decline in income associated to the implementations of Peña Novo, which gave smaller areas to the peasants, explains the increase in thefts. For the rest of coefficients, we want to emphasize the negative and statistically significant coefficients for attacks and clashes in 1933. That is, it seems that these technical land reforms were, if anything, conducive to short-run decline in rural conflict.

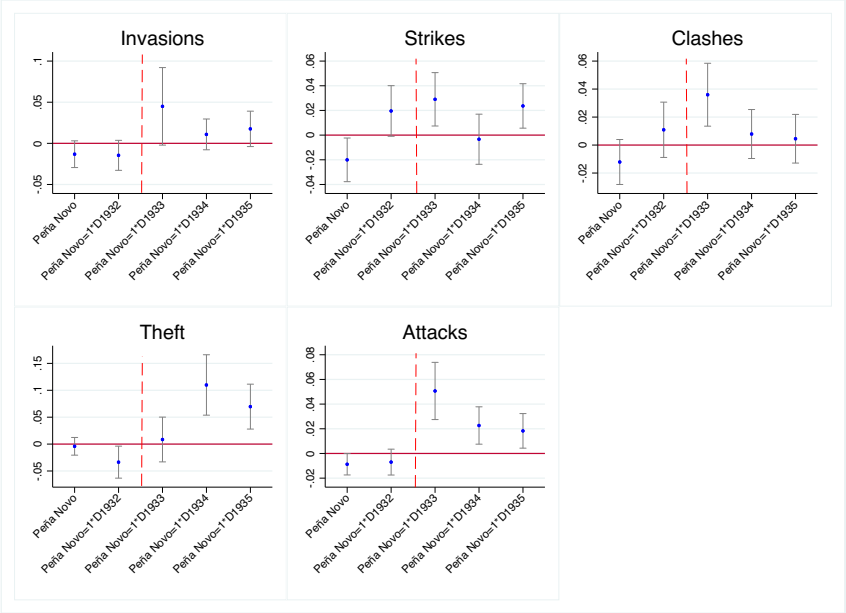
Figure 5: Treatment effects of 'technical' land reform. β_t coefficients from equation [2]



Notes: This figure plots the β_t coefficients from equation [4].

The effects are radically different in the case of the prefect-led settlements. In Figure 6, we plot the δ_t coefficients, which report the differential effect of the implementation of Peña Novo. The coefficients displayed in Figure 6 point at the exact opposite effects of technical land reform reported in Figure 5. Compared to the technical reforms, the municipalities intervened by Peña Novo had significantly more strikes, clashes and attacks in 1933. These differential effects were persistent until 1935 for the case of attacks (the coefficient for strikes is not significant in 1934 and it is positive and significant in 1935). For petty theft, we observe the there is a differential increase in the number of thefts in 1934 and 1935 in the municipalities where Peña Novo implemented the reform. The coefficient for 1933 is positive but not statistically significant.

Figure 6: Treatment effects of Prefect-initiated land reform. δ_t coefficients in equation [2]



Notes: This figure plots the δ_t coefficients from equation [4].

One concern with our estimates is that the municipalities chosen by Peña Novo were in the province of Cáceres. Even though we control for most of the observable differences between municipalities, it could be that unobserved differences between provinces may explain some of our findings. To address this concern, we re-estimate equation [2] only for the municipalities in the province of Cáceres. Figure 12 in the Appendix reports the estimated δ_t coefficients. Note that we are decreasing a lot the size of the sample and, thus, the power of our coefficient also shrinks. In any event, we note that the results are very similar. In particular, the positive and significant δ_t coefficients for theft and clashes survive.

Lastly, we examine the total effect of land reform in the prefect-initiated settlements. As discussed above, we need to consider the sum of the β_t and δ_t coefficients. We want to emphasize that the overall effect of theft are positive and statistically significant for all post-reform years. This is in stark contrast with the effect of the technical implementation (Figure 5), which was not different from zero in any year. Quantitatively, the sum of the β and δ coefficients is 0.036 for 1933, 0.101 for 1934, and 0.053 for 1935. The overall effects on attacks are also positive in 1933 and 1935 (in 1934 they are significant at 10 percent). For the remaining types

of conflict, the overall effect of prefect-initiated settlements is not statistically different from zero at 5 percent. For example, the effect on clashes in 1933 is positive and significant only at 10 percent. The same for strikes in 1935. For none of the years, the effect on invasions is different from zero. In any event, it is clear that the implementation of Peña Novo exacerbated rural conflict.

To summarize, the evidence reported in this section underlines the importance of the implementation of the reform. Indeed, we have documented that technical land reform was, if anything, conducive to reduce social tensions and conflicts in the short-run (for example, clashes and attacks in 1933). On the other hand, the ad-hoc political implementation of Peña Novo tended to increase thefts and, to a lower extent, attacks and clashes. Thus, it follows that how land reforms are implemented matters to mitigate an outburst of conflicts. In the next section, we attempt to identify the mechanism through which some types of land reform implementation increased conflict.

5 Mechanism

The fact that conflicts were exacerbated when the expropriations were led by Peña Novo points towards a possible income effect. That is, we know that the average size of areas that Peña Novo gave to peasants were relatively smaller, maybe insufficient, which may reduce the income of peasants and result in an increase in petty thefts. We will use data on settlers to test this hypothesis. An alternative explanation is that the increase in social conflict may be due to a different attitude of expropriated owners. To examine this hypothesis, we will exploit the presence of aristocracy in the municipality. We note that the presence of aristocracy was larger in the municipalities selected by Peña Novo and this aristocracy were behind the failed coup.

5.1 Income Effect

In this section we will investigate whether the fall in income can explain the increase in social conflict. We proceed in two parts. First, we discuss whether insiders are more likely to be behind the increase in social conflict. Second, we report the effects of the number of settlers by

hectare expropriated (overcrowding). If we find that both (i) the effect is caused by insiders and (ii) the effect increases in overcrowded municipalities, we will argue that the decline in income of peasants is the main mechanism through which the land reform affected conflict.

5.1.1 Insiders vs Outsiders

When discussing the income effect, we have been assuming that insiders were responsible for the increase in social conflict. That is, peasants received insufficient amount of land and this explained the increase in, for example, petty theft. However, it could also be that outsiders, who did not receive any land, had incentives to create conflict. The excluded peasants were not a small group. The average settlement rate was 65 per cent, but this average includes several outliers in very small municipalities.[18] The average (weighted by the population in 1930) is only 26 per cent. In most of the largest municipalities, many landless families were left out in the settlement plans. In order to examine whether insiders explain the increase in social conflict, we replace the land reform dummy in equation [1] for (i) number settlers and (ii) ratio between settlers and eligible peasants (according to the Peasant Census). In both cases, we normalize the values of the proxy for the intensive margin with the inverse hyperbolic sine function.[19] If any of these two variables is related with an increase in the number of conflicts, it would be supportive of the idea that insiders were behind the increase in the number of conflicts.

Figure 7 reports the coefficient of the interaction between settlers in 1933 and the year dummies.[20] Note that these coefficients are qualitatively very similar to our baseline specification (Figure 4). In particular, note the positive and significant coefficient for petty thefts in 1933 and 1934. The coefficients are not significantly different from zero pre-reform. That is, municipalities which had more settlers had more petty thefts after the reform (however, they did not have more thefts before the reform). This evidence suggest that insiders were generating rural conflict after the reform.

As complementary evidence, Figure 13 in the Appendix reports the same type of coefficient

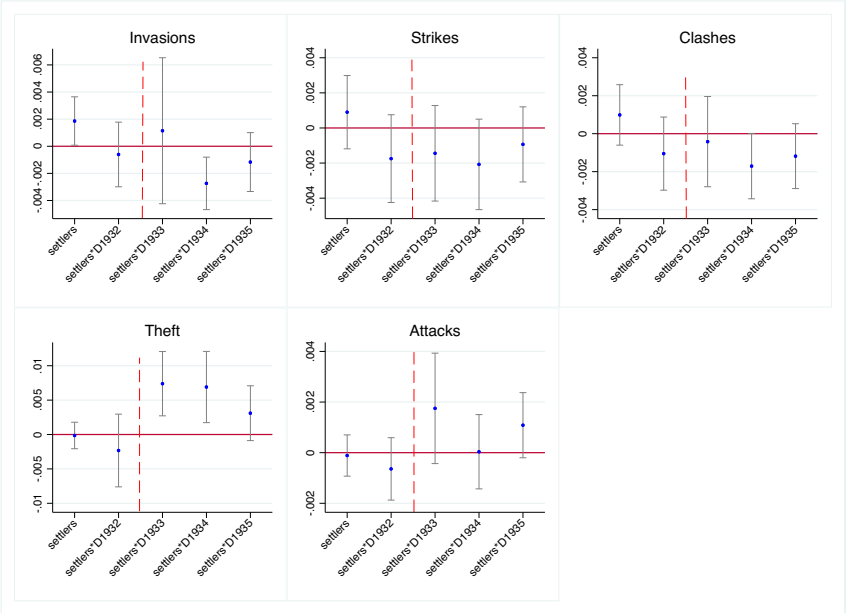
[18] We calculate the settlement rate as the number of settlers divided by the landless peasants included in the Peasant Census (from González and Brel (2013)) (expressed in percentages).

[19] We use this function instead of the log for the presence of zeros in the distribution of both variables.

[20] Full results can be found in Table G in the Appendix.

but using the interaction between the ratio of settled to eligible peasants and the year dummies.[21] A very similar pattern emerges. Once again, if we focus on thefts, we observe that the coefficient is positive and statistically significant for 1933 and 1934 and zero otherwise. Thus, we conclude that the rise in conflicts mainly involved the participation of beneficiaries, rather than the excluded landless families.

Figure 7: Intensive margin, number of Settlers.



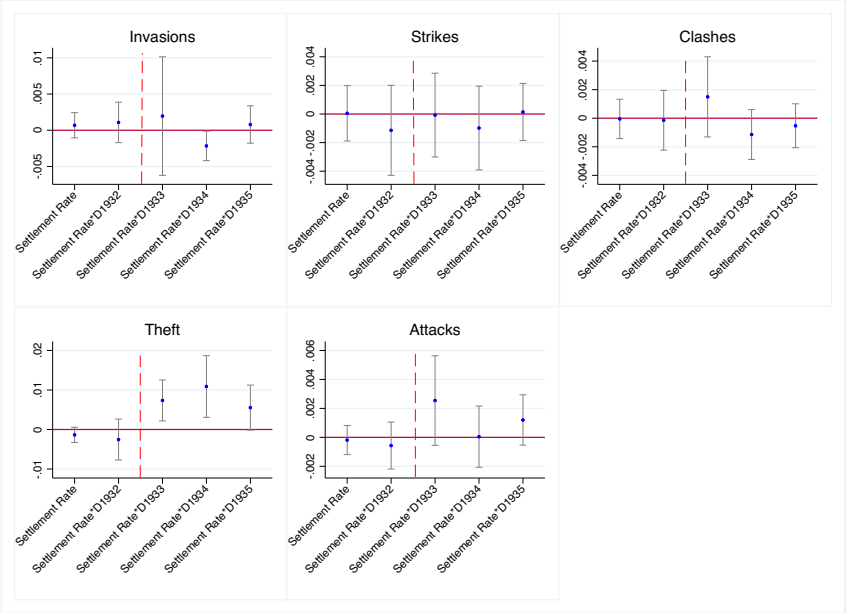
Notes: Coefficients on the interaction settlers * dummies year. We normalize the number of settlers with the inverse hyperbolic sine function.

5.1.2 Overcrowding

To conclude that the income effect was driving the rise in social conflict, we use settlers per expropriated hectare. If there are more settlers per hectare, it means that each settler has less land available and it is thus less likely to make enough income. We also discussed above that there is a minimum size below which the land becomes unprofitable. Thus, if there is an increase in the number of conflicts correlated with the increase in the number of settlers per hectare, it would be evidence in favor of the income effect. Figure 9 reports the coefficients of the interaction between number of settlers per hectare (normalized with the inverse hyperbolic

[21] Full results can be found in Table H in the Appendix.

Figure 8: Intensive margin, Settlers/Eligible.



Notes: CCoefficients on interaction (Settlers/Eligible) * dummies year. We normalize the ratio with the inverse hyperbolic sine function.

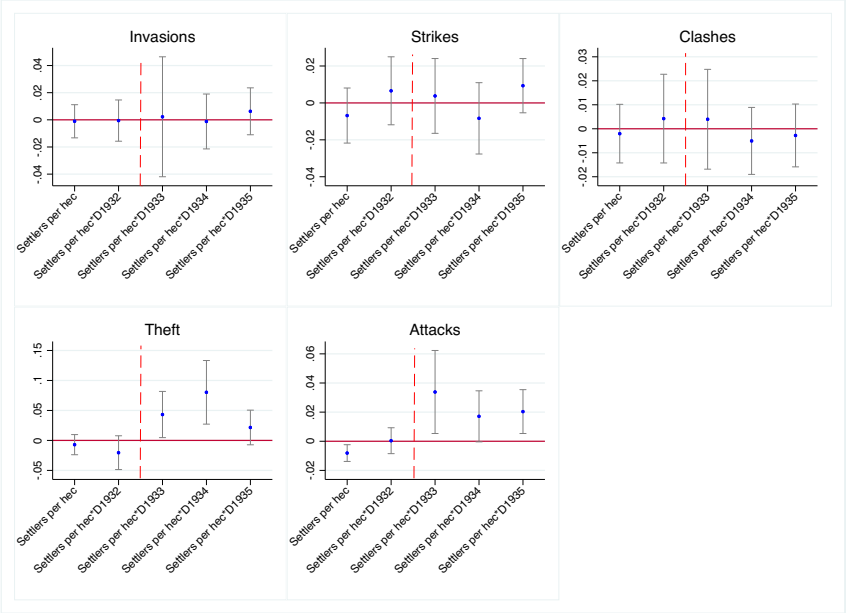
sine function) and the year fixed effects.[22] We want to emphasize the positive and significant coefficients for thefts and attacks. The effect on petty thefts is positive in all years post-reform but only significant in 1934, whereas all post-reform coefficients on attack are positive and significant. For the rest of conflicts, we do not observe any significant effect (except in the invasion regression in 1934 and in 1935, both at the 5 per cent level). The fact that the significant effect is concentrated on petty thefts and attacks also adds supporting evidence for the income effect hypothesis. Indeed, as we mentioned, petty theft could be related to stealing food. In addition, attacks also include damaging trees, which peasants may use to produce firewood.

5.2 Alternative Explanation: Heterogeneous Landowners

In our previous section, we have argued that settlers (the, a priori, beneficiaries) were generating rural conflict because their income decreased after the land reform. We have also argued that this can explain the differential effect that the ad-hoc implementation of Peña Novo had. An

[22] Full results can be found in Table I in the Appendix.

Figure 9: Effects of settler per hectare.



Notes: Coefficients on interaction settlers per expropriated hectare and the year dummies .

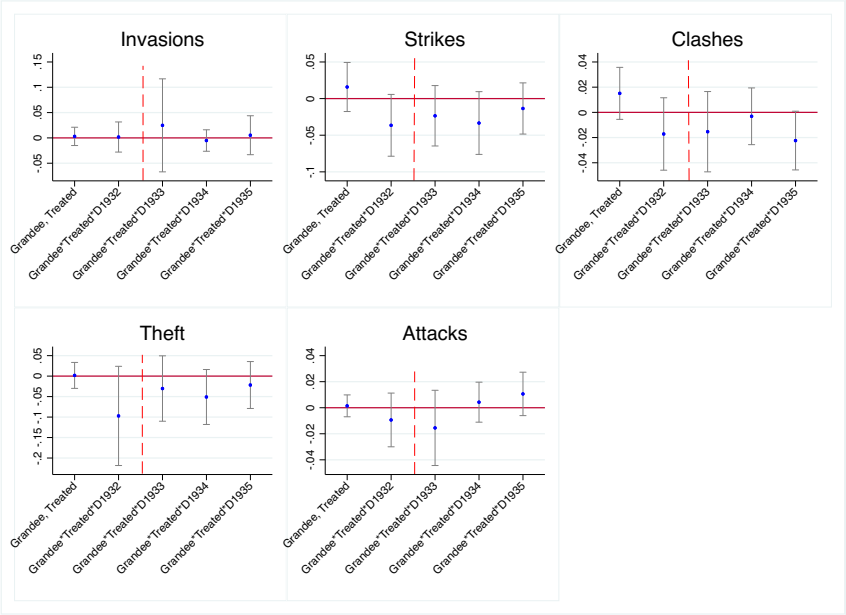
alternative explanation is that the behavior of landowners were different in some municipalities. A prominent example is the heterogeneous presence of Grandee aristocracy. As explained above, this aristocracy was thought to be behind the failed coup and, in contrast to the rest of landowners, they would not receive compensation for the expropriated land. Thus, it is plausible that these municipalities could be more polarized or landowners could resist and boycotted settlements, thereby generating more conflicts. To explore this possibility, we consider an specification with a triple interaction. That is, the reform dummy R_i, the dummy variable for the presence of farms owned by Grandee aristocrats in the municipality and the year dummies. The coefficient of this triple interaction represents the differential effect of the reform in municipalities in which the aristocracy owns land, with respect to the rest of municipalities.

Figure 10 reports the coefficients for this triple interaction for all five types of conflict. We note that none of the coefficient is statistically different from zero (neither before nor after the reform). It means that the presence of aristocracy did not have a differential effect on the effect of the land reform on social conflict.[23] Thus, we can reject the hypothesis that the effect of the

[23]For completeness, Figure 14 in the Appendix, represents the β_t coefficients of the baseline equation to show that they are robust to the inclusion of the triple interaction containing the dummy for Grandee aristocratic presence.

land reform was different in municipalities in which Grandee aristocracy had owned the land.

Figure 10: Effects of expropriations of Grandee aristocratic land. Effects in addition to the ordinary treatment effect of land reform.



Notes: Coefficients on interaction of treatment, the dummy for aristocratic presence and the year dummies.

6 Conclusions

Land reforms are policy tools used in several developing countries to increase the efficiency of agriculture. A related aim of reform is, in many cases, to preserve public order and placate revolutionary threats. In this paper, we analyze if land reforms increase conflict in the short run, which might offset the alleged positive effects of land reform on efficiency and social peace.

We document three main results. First, the land reform only increased the number of petty thefts in the short-run (1933 and 1934) but did not change the occurrence of the other types of conflict (invasions, strikes, clashes and attacks). Second, the effects of land reform on social conflict depend on its implementation. We show that social conflict, if anything, declined after the "technical" implementation (for example, clashes and attacks in 1933). On the contrary, conflicts were exacerbated with the *ad-hoc* political implementation of Peña Novo (especially for thefts and attacks). Third, the mechanism that explains the effect of the land reform on

social conflict seems to be the income channel. Indeed, we document that (i) settlers (the, a priori, beneficiaries) were behind the increase in social conflict and (ii) conflicts increased in municipalities where settled households were allocated less land.

To conclude, our work underscores the heterogeneous impacts of land reform and the importance of its implementation. In this reform, it seems that one of the main problems was that the prefect-led settlements did not give a sufficient amount of land to settlers. Settlers with less income and constrained to obtain other sources of revenue (for example, the government also drafted a law that prohibited to hire peasants from outside the municipality), committed a larger amount of petty thefts (mainly, food) and attacks (for example, destroy machinery to artificially increase labor demand during the harvest and other peak labor demand periods). Our results are consistent with the view that the defective design of land reforms and other interventions in agrarian factor markets might reduce poor peasants' incomes in the short run (Guinnane and Miller (1997); Ramseyer (2015)), thereby increasing rural conflict. A more positive interpretation of our results is that well-designed land reform does not necessarily increase rural conflict and might even mitigate it.

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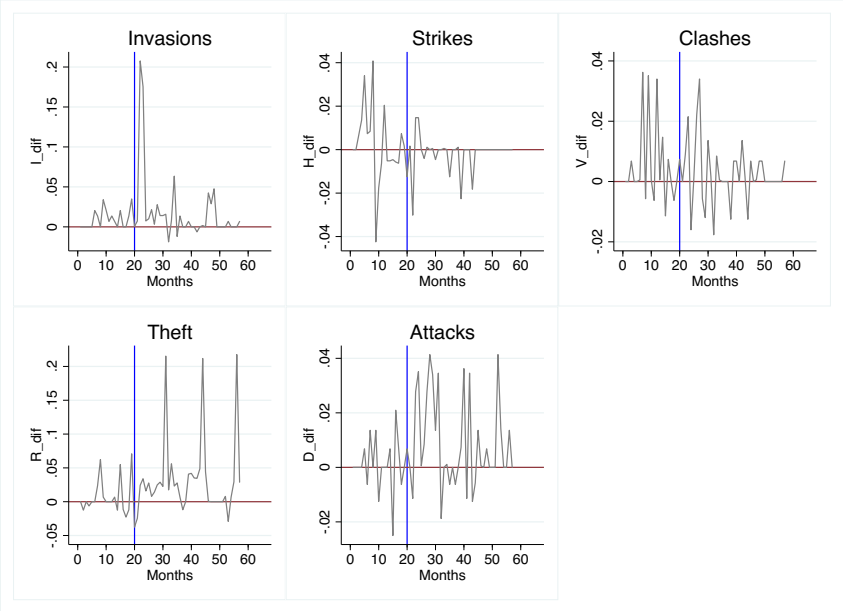
A Time-trend evolution of average number of conflict. Differences between control and treated groups of municipalities.

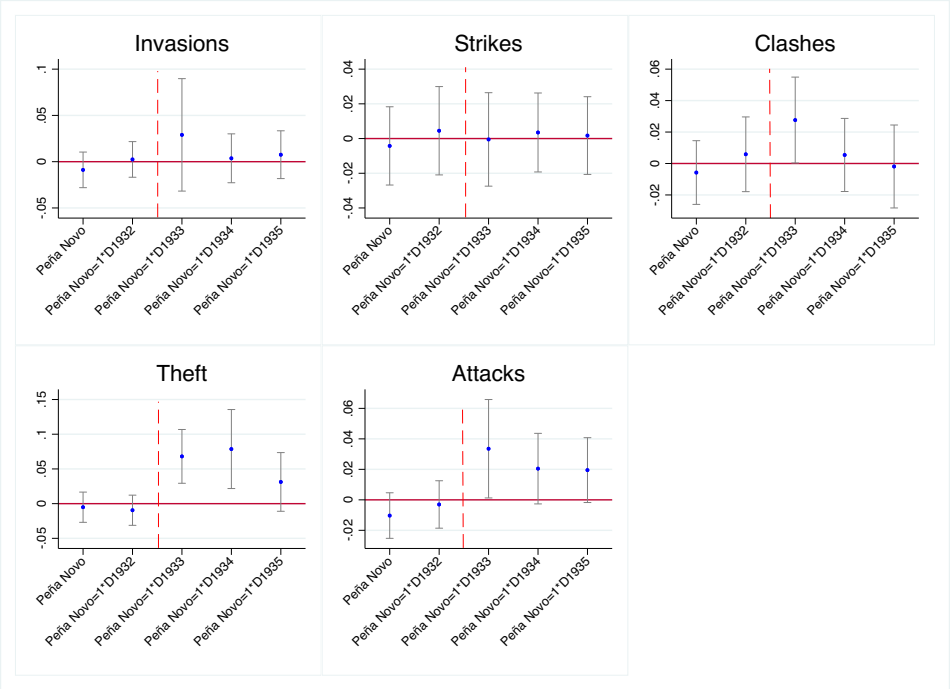
Figure 11: Time trend of differences in monthly averages of conflicts between treated and untreated municipalities.



Notes: This figure plots the time series evolution of the monthly average of the difference of average monthly conflicts in "treated"and "untreated" municipalities. The vertical blue line marks the passing. of the legislation on temporary occupations of land.

B Prefect regressions, Cáceres province only

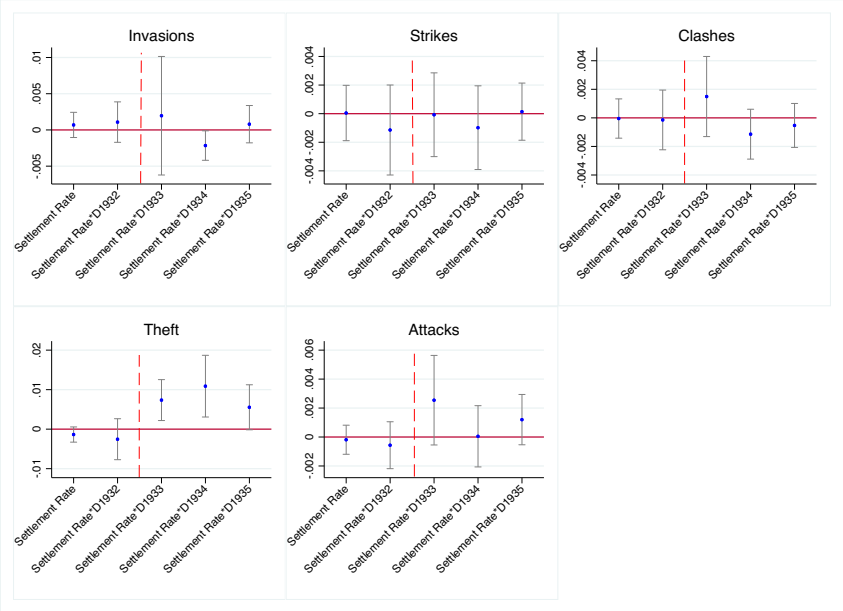
Figure 12: Effects of Peña Novo interventions. Cáceres province only.



Notes: Coefficients on the triple interaction Treated*Prefect-initiated*Year Dummies.

C Intensive margin, regressions using the ratio of settled peasants to eligible peasants as proxy for the intensive margin of reform

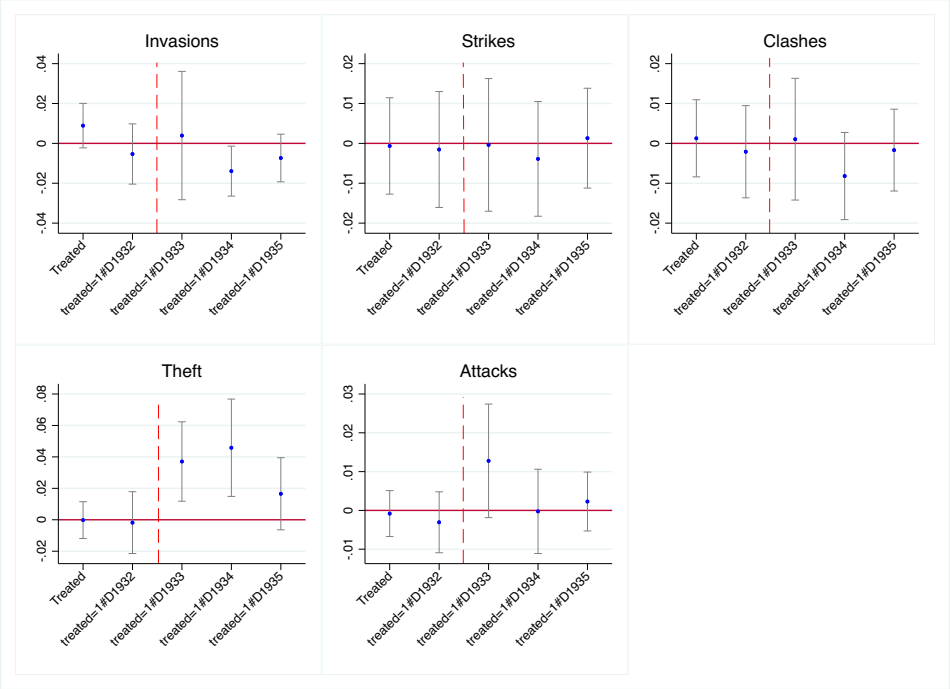
Figure 13: Intensive margin, Settlers/Eligible.



Notes: Coefficients on interaction (Settlers/Eligible) * dummies year. We normalize the ratio with the inverse hyperbolic sine function.

D βt coefficients in the regressions containing a triple interaction treatment, year dummies and the dummy for the presence of Grandee aristocratic owners.

Figure 14: Effects of land reform in municipalities *without* Grandee aristocratic owners.



Notes: Coefficients on the interaction Treated*Year Dummies

E Regression output underlying Figure 4

	(1) Invasions	(2) Strikes	(3) Clashes	(4) Theft	(5) Attacks
Log Pop1930	0.0118***	0.0106***	0.00783***	0.0195***	0.00706***
	(3.74)	(7.00)	(6.01)	(5.22)	(4.67)
Wheat Yield	0.0530**	0.00809	0.00373	0.0409*	0.000408
	(2.38)	(0.92)	(0.48)	(1.95)	(0.04)
D1932	-0.00678	-0.00749	-0.00170	0.00320	-0.000872
	(-0.78)	(-1.34)	(-0.40)	(0.35)	(-0.27)
D1933	0.0514***	0.000515	0.0153**	0.0101	0.00574
	(3.94)	(0.08)	(2.57)	(1.13)	(1.44)
D1934	0.00471	-0.00132	-0.00428	0.0217**	0.00806*
	(0.66)	(-0.25)	(-1.16)	(2.04)	(1.70)
D1935	-0.00893	-0.0119**	-0.00676*	0.0151	-0.00143
	(-1.29)	(-2.38)	(-1.92)	(1.54)	(-0.52)
Treated =1	0.00979**	0.00201	0.00390	-0.000277	-0.000482
	(1.97)	(0.34)	(0.89)	(-0.05)	(-0.18)
Treated=1*D1932	-0.00534	-0.00778	-0.00513	-0.0187	-0.00484
	(-0.79)	(-1.10)	(-0.97)	(-1.19)	(-1.32)
Treated=1*D1933	0.00790	-0.00433	-0.00153	0.0319**	0.00998
	(0.50)	(-0.55)	(-0.22)	(2.52)	(1.49)
Treated=1*D1934	-0.0150***	-0.00953	-0.00875*	0.0374***	0.000481
	(-2.70)	(-1.32)	(-1.81)	(2.63)	(0.10)
Treated=1*D1935	-0.00674	-0.000924	-0.00555	0.0129	0.00394
	(-1.15)	(-0.15)	(-1.20)	(1.20)	(1.11)
Cáceres=1	0.0165*	-0.00961***	0.0166***	0.0286***	0.0224***
	(1.93)	(-2.92)	(4.40)	(3.16)	(4.79)
Longitude	-0.167***	-0.0192	0.00707	-0.0371	-0.0133
	(-3.41)	(-0.72)	(0.30)	(-0.57)	(-0.60)
Latitude	0.998***	0.140	-0.0639	0.152	0.0513
	(3.43)	(0.87)	(-0.46)	(0.40)	(0.38)
Longitude × Latitude	-0.0253***	-0.00364	0.00167	-0.00373	-0.00127
	(-3.41)	(-0.89)	(0.47)	(-0.39)	(-0.37)
Altitude	-0.0000301	-0.0000104	-0.0000179*	-0.0000480**	-0.0000230**
	(-1.40)	(-1.30)	(-1.89)	(-2.13)	(-2.38)
Constant	6.484***	0.659	-0.327	1.259	0.483
	(3.40)	(0.63)	(-0.35)	(0.50)	(0.57)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	16,644	16,644	16,644	16,644	16,644

t statistics in parentheses

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

F Regression output underlying Figure 5

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0118***	0.0109***	0.00780***	0.0178***	0.00650***
	(3.73)	(7.22)	(5.85)	(5.11)	(4.30)
Wheat Yield	0.0528**	0.00731	0.00376	0.0446**	0.00164
	(2.36)	(0.84)	(0.47)	(2.12)	(0.15)
Treated=1	0.0152**	0.00973	0.00888*	0.00348	0.00369
	(2.49)	(1.28)	(1.74)	(0.57)	(1.29)
Treated=1*D1932	0.000280	-0.0159*	-0.00966	-0.00467	-0.00184
	(0.03)	(-1.78)	(-1.58)	(-0.25)	(-0.37)
Treated=1*D1933	-0.0108	-0.0162*	-0.0163**	0.0286*	-0.0105**
	(-0.59)	(-1.72)	(-2.28)	(1.86)	(-2.10)
Treated=1*D1934	-0.0195***	-0.00820	-0.0120**	-0.00660	-0.00855*
	(-3.16)	(-0.88)	(-2.10)	(-0.66)	(-1.88)
Treated=1*D1935	-0.0140**	-0.0106	-0.00740	-0.0152	-0.00349
	(-2.13)	(-1.33)	(-1.30)	(-1.61)	(-1.13)
Treated Gobernador=1	-0.0136	-0.0201**	-0.0121	-0.00342	-0.00830*
	(-1.64)	(-2.23)	(-1.48)	(-0.41)	(-1.89)
Treated Gobernador=1*D1932	-0.0138	0.0199*	0.0111	-0.0343**	-0.00733
	(-1.47)	(1.91)	(1.11)	(-2.30)	(-1.38)
Treated Gobernador=1*D1933	0.0458*	0.0290***	0.0362***	0.00809	0.0503***
	(1.89)	(2.64)	(3.16)	(0.38)	(4.25)
Treated Gobernador=1*D1934	0.0110	-0.00333	0.00801	0.108***	0.0221***
	(1.15)	(-0.32)	(0.90)	(3.80)	(2.87)
Treated Gobernador=1*D1935	0.0178	0.0237**	0.00455	0.0688***	0.0182**
	(1.62)	(2.59)	(0.51)	(3.21)	(2.55)
Longitude	-0.167***	-0.0190	0.00707	-0.0387	-0.0138
	(-3.40)	(-0.72)	(0.30)	(-0.61)	(-0.66)
Latitude	0.997***	0.134	-0.0636	0.184	0.0620
	(3.43)	(0.85)	(-0.46)	(0.50)	(0.49)
Longitude × Latitude	-0.0253***	-0.00348	0.00166	-0.00458	-0.00156
	(-3.40)	(-0.87)	(0.47)	(-0.49)	(-0.48)
Altitude	-0.0000303	-0.0000119	-0.0000177*	-0.0000403*	-0.0000204**
	(-1.43)	(-1.49)	(-1.92)	(-1.84)	(-2.14)
Constant	6.481***	0.648	-0.327	1.324	0.503
	(3.40)	(0.63)	(-0.35)	(0.54)	(0.63)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	16,644	16,644	16,644	16,644	16,644

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G Regression output underlying Figure 7

	(1) Invasions	(2) Strikes	(3) Clashes	(4) Theft	(5) Attacks
Log Pop1930	0.0115***	0.0106***	0.00775***	0.0186***	0.00695***
	(3.63)	(6.94)	(5.86)	(5.18)	(4.61)
Wheat Yield	0.0514**	0.00879	0.00384	0.0375*	0.00000276
	(2.32)	(1.00)	(0.50)	(1.84)	(0.00)
D1932	-0.00746	-0.00663	-0.00134	0.000788	-0.00129
	(-0.87)	(-1.19)	(-0.32)	(0.09)	(-0.40)
D1933	0.0519***	0.00202	0.0156***	0.00629	0.00589
	(4.02)	(0.32)	(2.66)	(0.69)	(1.45)
D1934	0.00481	-0.000484	-0.00394	0.0210*	0.00820*
	(0.68)	(-0.09)	(-1.07)	(1.98)	(1.75)
D1935	-0.00894	-0.0103**	-0.00633*	0.0133	-0.00220
	(-1.29)	(-2.06)	(-1.80)	(1.37)	(-0.79)
settlers (asinh)	0.00184**	0.000913	0.000998	-0.000211	-0.0000811
	(2.01)	(0.86)	(1.23)	(-0.22)	(-0.19)
settlers (asinh)*D1932	-0.000628	-0.00178	-0.00108	-0.00223	-0.000676
	(-0.52)	(-1.40)	(-1.10)	(-0.85)	(-1.08)
settlers (asinh)*D1933	0.00119	-0.00147	-0.000430	0.00746***	0.00171
	(0.43)	(-1.06)	(-0.36)	(3.17)	(1.54)
settlers (asinh)*D1934	-0.00272***	-0.00209	-0.00171*	0.00703***	0.0000205
	(-2.73)	(-1.59)	(-1.96)	(2.66)	(0.03)
settlers (asinh)*D1935	-0.00119	-0.000939	-0.00119	0.00316	0.00106
	(-1.07)	(-0.86)	(-1.37)	(1.54)	(1.62)
Longitude	-0.165***	-0.0191	0.00821	-0.0294	-0.0123
	(-3.36)	(-0.72)	(0.35)	(-0.45)	(-0.55)
Latitude	0.987***	0.140	-0.0692	0.114	0.0463
	(3.39)	(0.87)	(-0.50)	(0.30)	(0.34)
Longitude × Latitude	-0.0250***	-0.00364	0.00180	-0.00279	-0.00115
	(-3.37)	(-0.89)	(0.51)	(-0.29)	(-0.33)
Altitude	-0.0000289	-0.0000107	-0.0000176*	-0.0000447**	-0.0000226**
	(-1.35)	(-1.31)	(-1.88)	(-2.00)	(-2.36)
Constant	6.408***	0.651	-0.373	0.962	0.444
	(3.35)	(0.62)	(-0.40)	(0.38)	(0.52)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	16,644	16,644	16,644	16,644	16,644

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

H Regression output underlying Figure 13

	(1) Invasions	(2) Strikes	(3) Clashes	(4) Theft	(5) Attacks
Log Pop1930	0.0155***	0.00975***	0.00772***	0.0134***	0.00590***
	(4.24)	(5.71)	(5.37)	(3.69)	(3.54)
Wheat Yield	0.0680**	0.0153	0.00796	0.0255	-0.0121
	(2.47)	(1.56)	(0.88)	(1.10)	(-1.15)
D1932	-0.00930	-0.00693	0.00114	-0.00620	-0.00324
	(-0.99)	(-1.12)	(0.25)	(-0.55)	(-0.89)
D1933	0.0610***	-0.00162	0.0122*	0.00208	0.00295
	(4.07)	(-0.24)	(1.90)	(0.20)	(0.70)
D1934	0.00516	-0.00000309	-0.00319	0.0185	0.00953*
	(0.62)	(-0.00)	(-0.76)	(1.49)	(1.79)
D1935	-0.0104	-0.0109**	-0.00528	0.00987	-0.00302
	(-1.32)	(-2.02)	(-1.44)	(0.86)	(-0.91)
Settlement Rate	0.00174	0.00101	0.000345	-0.00105	0.0000838
	(1.27)	(0.67)	(0.31)	(-0.77)	(0.12)
Settlement Rate*D1932	-0.0000774	-0.00207	-0.000606	-0.00293	-0.000907
	(-.05)	(-1.09)	(-.46)	(.1.02)	(-1.00)
Settlement Rate*D1933	0.000773	-0.000990	0.00107	0.00688**	0.00221
	(0.18)	(-0.50)	(0.66)	(2.46)	(1.35)
Settlement Rate*D1934	-0.00344**	-0.00189	-0.00159	0.0104***	-0.000280
	(-2.55)	(-0.98)	(-1.31)	(2.60)	(-0.24)
Settlement Rate*D1935	-0.000491	-0.000787	-0.000968	0.00509*	0.000869
	(-0.32)	(-0.51)	(-0.84)	(1.70)	(0.90)
Longitude	-0.244***	-0.0279	0.00804	-0.0276	-0.00208
	(-4.19)	(-0.90)	(0.29)	(-0.37)	(-0.09)
Latitude	1.470***	0.193	-0.0650	0.0744	-0.0130
	(4.30)	(1.04)	(-0.41)	(0.17)	(-0.09)
Longitude × Latitude	-0.0374***	-0.00505	0.00171	-0.00180	0.00032
	(-4.28)	(-1.07)	(0.42)	(-0.16)	(0.09)
Altitude	-0.0000345	-0.00000508	-0.0000195*	-0.0000319	-0.0000151
	(-1.42)	(-0.60)	(-1.82)	(-1.29)	(-1.57)
Constant	9.445***	0.971	-0.376	0.944	0.0627
	(4.18)	(0.81)	(-0.35)	(0.32)	(0.07)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	16,644	16,644	16,644	16,644	16,644

t statistics in parentheses

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

I Regression output underlying Figure 9

	(1) Invasions	(2) Strikes	(3) Clashes	(4) Theft	(5) Attacks
Log Pop1930	0.0121***	0.0106***	0.00789***	0.0193***	0.00683***
	(3.82)	(7.00)	(6.09)	(5.20)	(4.64)
Wheat Yield	0.0514**	0.00777	0.00338	0.0417*	0.00156
	(2.32)	(0.88)	(0.43)	(1.97)	(0.14)
D1932	-0.00881	-0.0118**	-0.00453	-0.00000232	-0.00279
	(-1.01)	(-2.21)	(-1.04)	(-0.00)	(-0.86)
D1933	0.0540***	-0.00195	0.0138**	0.0135	0.00281
	(4.33)	(-0.32)	(2.40)	(1.49)	(0.69)
D1934	-0.000788	-0.00344	-0.00667*	0.0200*	0.00484
	(-0.11)	(-0.67)	(-1.90)	(1.78)	(1.04)
D1935	-0.0127*	-0.0142***	-0.00836**	0.0156	-0.00400
	(-1.78)	(-2.95)	(-2.44)	(1.55)	(-1.30)
Settlers per hec	-0.00101	-0.00687	-0.00205	-0.00770	-0.00789***
	(-0.16)	(-0.91)	(-0.33)	(-0.89)	(-2.69)
Settlers per hec*D1932	-0.0000697	0.00654	0.00423	-0.0199	0.000166
	(-0.01)	(0.70)	(0.45)	(-1.40)	(0.04)
Settlers per hec*D1933	0.00207	0.00389	0.00418	0.0443**	0.0337**
	(0.09)	(0.38)	(0.39)	(2.24)	(2.32)
Settlers per hec*D1934	-0.00142	-0.00785	-0.00493	0.0806***	0.0170*
	(-0.14)	(-0.80)	(-0.69)	(2.99)	(1.91)
Settlers per hec*D1935	0.00608	0.00940	-0.00280	0.0223	0.0202***
	(0.70)	(1.26)	(-0.42)	(1.50)	(2.63)
Caceres=1	0.0199**	-0.00930***	0.0172***	0.0283***	0.0203***
	(2.47)	(-2.73)	(4.65)	(2.97)	(4.71)
Longitude	-0.174***	-0.0174	0.00699	-0.0465	-0.0132
	(-3.54)	(-0.65)	(0.30)	(-0.71)	(-0.60)
Latitude	1.029***	0.129	-0.0649	0.207	0.0552
	(3.52)	(0.80)	(-0.47)	(0.54)	(0.42)
Altitude	-0.0000310	-0.0000104	-0.0000180*	-0.0000483**	-0.0000226**
	(-1.46)	(-1.30)	(-1.91)	(-2.13)	(-2.34)
Longitude × Latitude	-0.0261***	-0.00335	0.00170	-0.00513	-0.00139
	(-3.49)	(-0.82)	(0.48)	(-0.52)	(-0.41)
Constant	6.760***	0.591	-0.322	1.627	0.475
	(3.54)	(0.57)	(-0.35)	(0.64)	(0.56)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	16,644	16,644	16,644	16,644	16,644

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

J Regression output main regressions using equation [1]

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0123***	0.0124***	0.00829***	0.0199***	0.00709***
	(3.71)	(6.08)	(5.03)	(4.09)	(3.49)
Wheat Yield	0.0434	0.0207*	0.00864	0.0394	-0.00639
	(1.57)	(1.82)	(0.73)	(1.34)	(-0.40)
D1932	-0.00626	-0.000820	0.00172	-0.0306*	-0.0131*
	(-0.57)	(-0.07)	(0.16)	(-1.77)	(-1.95)
D1933	0.0273	0.0210	0.0176	-0.0149	0.0129
	(1.42)	(1.53)	(1.24)	(-0.78)	(1.24)
D1934	0.0124	-0.0109	-0.00405	0.00980	-0.00402
	(1.55)	(-0.99)	(-0.40)	(0.50)	(-0.61)
D1935	0.00483	0.00684	-0.00654	0.0237	-0.0107
	(0.54)	(0.64)	(-0.66)	(1.37)	(-1.32)
Treated	0.00926	0.00577	0.00426	-0.00443	-0.00544*
	(1.56)	(0.80)	(0.79)	(-0.66)	(-1.90)
Treated=1*D1932	-0.00727	-0.00953	-0.00466	-0.0204	-0.00241
	(-0.84)	(-1.08)	(-0.73)	(-1.38)	(-0.60)
Treated=1*D1933	0.00900	-0.00876	0.0000992	0.0332**	0.0168**
	(0.57)	(-0.96)	(0.01)	(2.36)	(2.17)
Treated=1*D1934	-0.0130**	-0.0109	-0.00960	0.0344**	0.0103**
	(-1.99)	(-1.26)	(-1.58)	(2.33)	(2.34)
Treated=1*D1935	-0.00773	-0.00336	-0.00685	0.0164	0.00855**
	(-1.11)	(-0.45)	(-1.19)	(1.45)	(2.08)
Cáceres=1	0.0184*	-0.0179***	0.0171***	0.0366***	0.0242***
	(1.91)	(-4.54)	(3.50)	(3.11)	(3.77)
Longitude	-0.133**	-0.0155	-0.00556	-0.0829	0.00563
	(-2.20)	(-0.50)	(-0.19)	(-1.05)	(0.19)
Latitude	0.814**	0.163	0.0256	0.407	-0.0644
	(2.29)	(0.86)	(0.15)	(0.85)	(-0.36)
Longitude × Latitude	-0.0208**	-0.00417	-0.000665	-0.0102	0.00165
	(-2.29)	(-0.87)	(-0.15)	(-0.83)	(0.36)
Altitude	-0.00000683	-0.0000107	-0.0000208	-0.0000513	-0.0000191
	(-0.25)	(-0.93)	(-1.65)	(-1.62)	(-1.43)
Constant	5.070**	0.479	0.139	3.068	-0.252
	(2.17)	(0.40)	(0.12)	(1.01)	(-0.22)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	9,291	9,291	9,291	9,291	9,291
r2	0.047	0.052	0.018	0.068	0.025

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

K Regression output main regressions using equation [2]

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0125***	0.0127***	0.00824***	0.0193***	0.00652***
	(3.82)	(6.22)	(5.06)	(4.11)	(3.40)
Wheat Yield	0.0439	0.0213*	0.00855	0.0381	-0.00765
	(1.60)	(1.94)	(0.73)	(1.32)	(-0.48)
D1932	-0.00450	-0.00350	0.000495	-0.0259	-0.0119*
	(-0.43)	(-0.29)	(0.05)	(-1.50)	(-1.85)
D1933	0.0223	0.0181	0.0141	-0.0161	0.00676
	(1.20)	(1.34)	(1.01)	(-0.87)	(0.70)
D1934	0.0111	-0.0107	-0.00461	-0.000730	-0.00658
	(1.40)	(-0.97)	(-0.48)	(-0.04)	(-1.03)
D1935	0.00282	0.00355	-0.00655	0.0158	-0.0132
	(0.31)	(0.34)	(-0.66)	(0.97)	(-1.64)
Treated	0.0152**	0.0138	0.00778	0.00196	-0.00202
	(2.01)	(1.43)	(1.26)	(0.28)	(-0.82)
Treated=1*D1932	-0.00127	-0.0187*	-0.00883	-0.00452	0.00159
	(-0.11)	(-1.66)	(-1.24)	(-0.25)	(0.32)
Treated=1*D1933	-0.00808	-0.0188	-0.0119	0.0289	-0.00413
	(-0.44)	(-1.64)	(-1.38)	(1.64)	(-0.79)
Treated=1*D1934	-0.0176**	-0.0101	-0.0115	-0.00144	0.00151
	(-2.39)	(-0.86)	(-1.61)	(-0.13)	(0.34)
Treated=1*D1935	-0.0146*	-0.0146	-0.00688	-0.0105	-0.0000755
	(-1.84)	(-1.45)	(-0.97)	(-1.00)	(-0.03)
Prefect =1	-0.0156*	-0.0209*	-0.00813	-0.0113	-0.00435
	(-1.65)	(-1.82)	(-0.87)	(-1.23)	(-1.17)
Prefect =1*D1932	-0.0143	0.0218	0.00998	-0.0379**	-0.00956**
	(-1.27)	(1.64)	(0.87)	(-2.13)	(-1.98)
Prefect=1*D1933	0.0408*	0.0239*	0.0287**	0.0103	0.0500***
	(1.71)	(1.89)	(2.13)	(0.41)	(3.53)
Prefect=1*D1934	0.0111	-0.00193	0.00459	0.0858***	0.0209**
	(0.99)	(-0.14)	(0.43)	(3.60)	(2.37)
Prefect=1*D1935	0.0164	0.0268**	0.0000643	0.0644***	0.0206***
	(1.32)	(2.27)	(0.01)	(3.39)	(2.69)
Cáceres=1	0.0205**	-0.0150***	0.0166***	0.0304***	0.0182***
	(2.09)	(-3.56)	(3.22)	(2.79)	(3.16)
Longitude	-0.131**	-0.0133	-0.00589	-0.0875	0.00114
	(-2.16)	(-0.44)	(-0.20)	(-1.11)	(0.04)
Latitude	0.803**	0.148	0.0277	0.438	-0.0346
	(2.25)	(0.82)	(0.16)	(0.92)	(-0.21)
Longitude × Latitude	-0.0205**	-0.00379	-0.000721	-0.0110	0.000876
	(-2.24)	(-0.82)	(-0.16)	(-0.90)	(0.20)
Altitude	-0.00000766	-0.0000118	-0.0000206*	-0.0000489	-0.0000167
	(-0.29)	(-1.05)	(-1.67)	(-1.59)	(-1.25)
Constant	5.008**	0.394	0.153	3.258	-0.0693
	(2.13)	(0.34)	(0.13)	(1.07)	(-0.07)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	9,291	9,291	9,291	9,291	9,291
r2	0.048	0.053	0.019	0.073	0.032

t statistics in parentheses

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

L Regression output main regressions using intensive margin, settlers (asinh)

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0120***	0.0123***	0.00807***	0.0193***	0.00689***
	(3.67)	(5.95)	(4.85)	(3.98)	(3.43)
Wheat Yield	0.0421	0.0210*	0.00886	0.0375	-0.00662
	(1.56)	(1.85)	(0.76)	(1.31)	(-0.42)
D1932	-0.00719	-0.000628	0.00200	-0.0311*	-0.0131**
	(-0.66)	(-0.05)	(0.19)	(-1.80)	(-1.99)
D1933	0.0297	0.0221	0.0181	-0.0176	0.0139
	(1.57)	(1.63)	(1.28)	(-0.92)	(1.31)
D1934	0.0125	-0.0106	-0.00346	0.01000	-0.00287
	(1.56)	(-0.97)	(-0.34)	(0.51)	(-0.42)
D1935	0.00405	0.00865	-0.00577	0.0234	-0.0113
	(0.45)	(0.81)	(-0.58)	(1.36)	(-1.41)
settlers (asinh)	0.00198*	0.00149	0.00124	-0.000745	-0.000733
	(1.80)	(1.13)	(1.27)	(-0.67)	(-1.63)
settlers (asinh)*D1932	-0.00104	-0.00185	-0.000968	-0.00362	-0.000456
	(-0.67)	(-1.17)	(-0.82)	(-1.34)	(-0.68)
settlers (asinh)*D1933	0.000846	-0.00201	-0.000144	0.00716***	0.00280**
	(0.31)	(-1.24)	(-0.10)	(2.73)	(2.17)
settlers (asinh)*D1934	-0.00246**	-0.00216	-0.00200*	0.00638**	0.00153**
	(-2.08)	(-1.36)	(-1.86)	(2.54)	(2.18)
settlers (asinh)*D1935	-0.00118	-0.00126	-0.00155	0.00317	0.00182**
	(-0.88)	(-0.94)	(-1.46)	(1.53)	(2.33)
Cáceres=1	0.0176*	-0.0184***	0.0160***	0.0352***	0.0236***
	(1.84)	(-4.70)	(3.35)	(2.99)	(3.71)
Longitude	-0.129**	-0.0145	-0.00306	-0.0769	0.00769
	(-2.15)	(-0.47)	(-0.10)	(-0.99)	(0.26)
Latitude	0.795**	0.157	0.0124	0.376	-0.0751
	(2.25)	(0.83)	(0.07)	(0.80)	(-0.42)
Longitude × Latitude	-0.0203**	-0.00403	-0.000333	-0.00942	0.00192
	(-2.25)	(-0.84)	(-0.07)	(-0.78)	(0.42)
Altitude	-0.00000523	-0.0000108	-0.0000205	-0.0000489	-0.0000185
	(-0.20)	(-0.92)	(-1.65)	(-1.56)	(-1.40)
Constant	4.926**	0.438	0.0400	2.837	-0.333
	(2.12)	(0.36)	(0.03)	(0.94)	(-0.30)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	9,291	9,291	9,291	9,291	9,291
r2	0.047	0.052	0.018	0.069	0.025

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

M Regression output main regressions using intensive margin, settlers'ratio (asinh)

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0140***	0.0106***	0.00809***	0.0149***	0.00621***
	(3.19)	(4.68)	(4.84)	(2.86)	(2.74)
Wheat Yield	0.0568	0.0317**	0.0169	0.0209	-0.0300*
	(1.64)	(2.22)	(1.11)	(0.56)	(-1.90)
D1932	-0.0233*	0.00425	0.000859	-0.0421**	-0.0157**
	(-1.96)	(0.30)	(0.07)	(-2.19)	(-2.02)
D1933	0.0361	0.0197	0.00475	-0.0288	0.00830
	(1.52)	(1.24)	(0.32)	(-1.42)	(0.62)
D1934	0.00775	-0.0110	-0.00761	0.00193	-0.00477
	(0.88)	(-0.79)	(-0.59)	(0.08)	(-0.53)
D1935	-0.00341	0.0113	-0.0101	0.0167	-0.0141
	(-0.35)	(0.96)	(-0.84)	(0.79)	(-1.43)
Settlement Rate	0.00219	0.00184	0.0000425	-0.00245	-0.00110
	(1.26)	(0.96)	(0.03)	(-1.43)	(-1.40)
Settlement Rate*D1932	-0.000257	-0.00228	0.000217	-0.00155	-0.000502
	(-0.12)	(-0.95)	(0.13)	(-0.45)	(-0.51)
Settlement Rate*D1933	-0.000342	-0.00223	0.00257	0.00731**	0.00363*
	(-0.09)	(-0.96)	(1.32)	(2.11)	(1.80)
Settlement Rate*D1934	-0.00313**	-0.00200	-0.00131	0.00949**	0.00212*
	(-2.03)	(-0.82)	(-0.86)	(2.56)	(1.87)
Settlement Rate*D1935	-0.000868	-0.00134	-0.000896	0.00557*	0.00222*
	(-0.45)	(-0.70)	(-0.62)	(1.75)	(1.87)
Cáceres=1	0.0194*	-0.0219***	0.0174***	0.0412***	0.0275***
	(1.67)	(-5.10)	(3.42)	(3.11)	(3.67)
Longitude	-0.205***	-0.0248	0.00258	-0.0777	0.0358
	(-2.70)	(-0.65)	(0.07)	(-0.73)	(1.06)
Latitude	1.254***	0.232	-0.0196	0.344	-0.249
	(2.77)	(1.02)	(-0.09)	(0.53)	(-1.21)
Longitude × Latitude	-0.0321***	-0.00597	0.000504	-0.00859	0.00630
	(-2.77)	(-1.03)	(0.09)	(-0.52)	(1.19)
Altitude	-0.00000569	-0.00000243	-0.0000259*	-0.0000370	-0.00000672
	(-0.19)	(-0.19)	(-1.81)	(-1.03)	(-0.50)
Constant	7.867***	0.820	-0.193	2.945	-1.398
	(2.67)	(0.56)	(-0.13)	(0.72)	(-1.08)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	9,291	9,291	9,291	9,291	9,291
r2	0.047	0.055	0.022	0.079	0.026

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

N Regression output main regressions using intensive margin, settlers per hec (asinh)

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
Log Pop1930	0.0129***	0.0125***	0.00831***	0.0195***	0.00662***
	(3.91)	(6.14)	(5.12)	(4.11)	(3.49)
Wheat Yield	0.0408	0.0201*	0.00845	0.0440	-0.00304
	(1.46)	(1.74)	(0.71)	(1.45)	(-0.19)
D1932	-0.0102	-0.00810	-0.00255	-0.0332*	-0.0137**
	(-0.94)	(-0.68)	(-0.24)	(-1.79)	(-2.17)
D1933	0.0309	0.0158	0.0170	-0.0102	0.0105
	(1.63)	(1.22)	(1.20)	(-0.54)	(1.02)
D1934	0.00369	-0.0149	-0.00681	0.00231	-0.00557
	(0.44)	(-1.36)	(-0.68)	(0.13)	(-0.84)
D1935	-0.00246	0.00204	-0.00822	0.0246	-0.0130
	(-0.26)	(0.20)	(-0.79)	(1.37)	(-1.63)
Settlers per hec	-0.00606	-0.00577	0.00107	-0.0120	-0.0111***
	(-0.79)	(-0.54)	(0.12)	(-1.03)	(-2.87)
Settlers per hec*D1932	-0.0000124	0.00871	0.00720	-0.0343*	-0.00276
	(-0.00)	(0.67)	(0.55)	(-1.74)	(-0.63)
Settlers per hec*D1933	0.00528	0.00211	0.00293	0.0546**	0.0468**
	(0.20)	(0.16)	(0.21)	(2.21)	(2.50)
Settlers per hec*D1934	0.00691	-0.00791	-0.00995	0.107***	0.0291***
	(0.51)	(-0.58)	(-0.98)	(3.37)	(2.77)
Settlers per hec*D1935	0.0128	0.0122	-0.00830	0.0325*	0.0284***
	(1.13)	(1.19)	(-0.87)	(1.71)	(2.89)
Cáceres=1	0.0223**	-0.0171***	0.0173***	0.0309**	0.0197***
	(2.35)	(-3.88)	(3.54)	(2.50)	(3.32)
Longitude	-0.139**	-0.0145	-0.00559	-0.0921	0.00438
	(-2.30)	(-0.47)	(-0.19)	(-1.14)	(0.15)
Latitude	0.847**	0.154	0.0250	0.485	-0.0435
	(2.35)	(0.81)	(0.14)	(0.99)	(-0.24)
Altitude	-0.00000637	-0.0000110	-0.0000208	-0.0000491	-0.0000182
	(-0.24)	(-0.95)	(-1.65)	(-1.53)	(-1.37)
Longitude × Latitude	-0.0216**	-0.00394	-0.000649	-0.0122	0.00110
	(-2.35)	(-0.81)	(-0.15)	(-0.98)	(0.24)
Constant	5.336**	0.447	0.142	3.418	-0.210
	(2.27)	(0.37)	(0.13)	(1.10)	(-0.19)
Monthly dummies	yes	yes	yes	yes	yes
Unionization rate ×Year dummies	yes	yes	yes	yes	yes
Expropriable area ×Year dummies	yes	yes	yes	yes	yes
Anarchist union×Year dummies	yes	yes	yes	yes	yes
Grandee aristocracy×Year dummies	yes	yes	yes	yes	yes
Income inequality×Year dummies	yes	yes	yes	yes	yes
Income per hectare×Year dummies	yes	yes	yes	yes	yes
N	9,291	9,291	9,291	9,291	9,291
r2	0.046	0.052	0.018	0.070	0.028

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

O Regression output, Fixed Effects regressions, extensive margin.

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
D1932	-0.00180	-0.00830**	-0.000591	0.0189**	0.00337*
	(-0.44)	(-2.06)	(-0.23)	(2.16)	(1.73)
D1933	0.0503***	-0.0104**	0.0114***	0.0200***	0.00753***
	(5.53)	(-2.22)	(2.94)	(3.73)	(2.90)
D1934	-0.000305	0.000433	-0.00281	0.0126**	0.00818***
	(-0.12)	(0.10)	(-1.09)	(2.26)	(3.07)
D1935	-0.00753***	-0.0229***	-0.00632***	0.0111**	0.000242
	(-3.09)	(-6.27)	(-2.83)	(2.17)	(0.16)
Treated=1*D1932	-0.00551	-0.00990	-0.00420	-0.00964	-0.00305
	(-0.92)	(-1.51)	(-0.82)	(-0.91)	(-0.90)
Treated=1*D1933	0.0257*	-0.00781	-0.00239	0.0331***	0.0138**
	(1.66)	(-1.09)	(-0.36)	(2.92)	(2.32)
Treated=1*D1934	-0.0134***	-0.0122*	-0.00688	0.0356***	0.00148
	(-2.82)	(-1.89)	(-1.42)	(2.82)	(0.34)
Treated=1*D1935	-0.00278	-0.00790	-0.00626	0.0132	0.00367
	(-0.49)	(-1.31)	(-1.38)	(1.34)	(1.06)
Constant	0.0872***	0.0267***	0.0112***	0.00999	0.00725**
	(7.96)	(8.01)	(3.25)	(1.49)	(2.10)
Monthly dummies	yes	yes	yes	yes	yes
N	17,043	17,043	17,043	17,043	17,043

t statistics in parentheses

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

P Regression output, Fixed Effects regressions, extensive margin and Prefect effects.

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
D1932	-0.00753	-0.0116	-0.00412	0.0152	0.00208
	(-1.79)	(-3.11)	(-1.59)	(1.74)	(1.10)
D1933	0.0446	-0.0137	0.00786	0.0162	0.00625
	(4.83)	(-3.31)	(2.04)	(3.09)	(2.40)
D1934	-0.00604	-0.00288	-0.00634	0.00884	0.00689
	(-2.23)	(-0.66)	(-2.56)	(1.64)	(2.58)
D1935	-0.0133	-0.0262	-0.00985	0.00738	-0.00105
	(-4.91)	(-9.05)	(-4.75)	(1.48)	(-0.74)
treated=1*D1932	0.0164	-0.000525	0.000451	0.0180	0.00336
	(2.51)	(-0.10)	(0.14)	(1.45)	(0.86)
treated=1*D1933	0.0234	-0.00266	-0.00731	0.0423	-0.00292
	(1.28)	(-0.57)	(-1.47)	(3.03)	(-0.77)
treated=1*D1934	-0.00384	0.00716	-0.00236	0.00194	-0.00488
	(-1.74)	(1.17)	(-1.06)	(0.24)	(-1.31)
treated=1*D1935	0.00421	-0.00176	0.000906	-0.00474	-0.000901
	(1.03)	(-2.25)	(0.41)	(-0.80)	(-0.63)
treated Gobernador=1*D1932	-0.0222	-0.00519	0.00683	-0.0454	-0.00843
	(-3.62)	(-0.92)	(1.30)	(-4.85)	(-2.35)
treated Gobernador=1*D1933	0.0340	0.00458	0.0290	-0.00268	0.0451
	(1.41)	(0.80)	(3.70)	(-0.14)	(4.30)
treated Gobernador=1*D1934	0.00632	-0.0284	0.00711	0.0967	0.0212
	(1.21)	(-5.45)	(1.45)	(3.93)	(3.32)
treated Gobernador=1*D1935	0.0124	0.00229	0.000989	0.0602	0.0170
	(1.50)	(2.61)	(0.34)	(3.31)	(3.15)
Constant	0.0872	0.0267	0.0112	0.00999	0.00725
	(7.18)	(7.06)	(3.14)	(1.79)	(2.06)
Monthly dummies	yes	yes	yes	yes	yes
N	17,043	17,043	17,043	17,043	17,043

t statistics in parentheses

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

Q Regression output, Fixed Effects regressions, intensive margin (asinh(Settlers)).

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
D1932	-0.00289	-0.00753	-0.000142	0.0159	0.00265
	(-0.73)	(-1.90)	(-0.05)	(1.85)	(1.39)
D1933	0.0503	-0.00867	0.0116	0.0142	0.00744
	(5.56)	(-1.89)	(3.01)	(2.62)	(2.83)
D1934	0.000197	0.000877	-0.00234	0.0112	0.00831
	(0.08)	(0.20)	(-0.92)	(1.96)	(3.15)
D1935	-0.00733	-0.0208	-0.00577	0.00884	-0.000746
	(-2.97)	(-5.79)	(-2.57)	(1.73)	(-0.51)
settlers (asinh)	0.00272	0.00214	0.00165	0.00180	0.000429
	(3.25)	(1.96)	(2.14)	(2.32)	(1.30)
settlers (asinh)*D1932	-0.000546	-0.00199	-0.000892	-0.000540	-0.000262
	(-0.49)	(-1.64)	(-0.94)	(-0.29)	(-0.45)
settlers (asinh)*D1933	0.00444	-0.00199	-0.000501	0.00785	0.00241
	(1.66)	(-1.56)	(-0.43)	(3.70)	(2.45)
settlers (asinh)*D1934	-0.00251	-0.00228	-0.00136	0.00668	0.000208
	(-2.83)	(-1.91)	(-1.54)	(2.69)	(0.30)
settlers (asinh)*D1935	-0.000552	-0.00214	-0.00129	0.00312	0.00100
	(-0.52)	(-1.96)	(-1.54)	(1.61)	(1.59)
Constant	0.0799	0.0210	0.00680	0.00515	0.00609
	(7.02)	(4.74)	(1.85)	(0.91)	(1.72)
Monthly dummies	yes	yes	yes	yes	yes
N	17,043	17,043	17,043	17,043	17,043

t statistics in parentheses

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

R Regression output, Fixed Effects regressions, intensive margin (asinh(Settlement rate)).

	(1)	(2)	(3)	(4)	(5)
	Invasions	Strikes	Clashes	Theft	Attacks
D1932	-0.0000510	-0.00835	-0.000183	0.0164	0.00196
	(-0.01)	(-1.85)	(-0.06)	(1.59)	(0.94)
D1933	0.0607	-0.0112	0.00850	0.0191	0.00715
	(5.74)	(-2.28)	(2.08)	(3.03)	(2.50)
D1934	0.00243	-0.000325	-0.00254	0.0126	0.00893
	(0.86)	(-0.06)	(-0.86)	(1.85)	(2.92)
D1935	-0.00702	-0.0229	-0.00663	0.00899	-0.000520
	(-2.83)	(-5.65)	(-2.61)	(1.47)	(-0.30)
Settlement Rate	0.00315	0.00208	0.00136	0.00101	0.000646
	(2.56)	(1.36)	(1.32)	(0.95)	(1.21)
Settlement Rate*D1932	-0.000104	-0.00251	-0.000627	0.0000424	-0.000318
	(-0.07)	(-1.43)	(-0.49)	(0.02)	(-0.38)
Settlement Rate*D1933	0.00390	-0.00160	0.000500	0.00903	0.00295
	(0.95)	(-0.90)	(0.32)	(3.14)	(1.99)
Settlement Rate*D1934	-0.00326	-0.00223	-0.00148	0.0102	-0.000321
	(-2.77)	(-1.28)	(-1.30)	(2.58)	(-0.31)
Settlement Rate*D1935	0.0000406	-0.00208	-0.00128	0.00559	0.000764
	(0.03)	(-1.36)	(-1.20)	(1.84)	(0.85)
Constant	0.0840	0.0233	0.00498	0.00713	0.00652
	(6.29)	(4.66)	(1.54)	(1.12)	(1.65)
Monthly dummies	yes	yes	yes	yes	yes
N	17,043	17,043	17,043	17,043	17,043

t statistics in parentheses

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

S Regression output, Fixed Effects regressions, intensive margin (asinh(Settlers per hectare)).

	(1) Invasions	(2) Strikes	(3) Clashes	(4) Theft	(5) Attacks
Log Pop1930	0.0156	0.0131	0.00513	0.0266	0.00488
	(6.35)	(10.71)	(5.23)	(5.22)	(4.50)
Wheat Yield	0.0246	-0.0136	0.0113	0.0272	0.0115
	(2.61)	(-3.00)	(2.28)	(1.78)	(2.29)
D1932	-0.00407	-0.0139	-0.00364	0.0203	0.00172
	(-1.03)	(-3.63)	(-1.31)	(2.63)	(0.90)
D1933	0.0587	-0.0145	0.00970	0.0273	0.00617
	(6.90)	(-3.37)	(2.57)	(4.63)	(2.08)
D1934	-0.00679	-0.00302	-0.00582	0.0135	0.00467
	(-2.48)	(-0.72)	(-2.22)	(2.07)	(1.77)
D1935	-0.0112	-0.0280	-0.00873	0.0127	-0.00282
	(-4.10)	(-8.10)	(-3.54)	(2.32)	(-1.71)
Settlers per hec	0.00287	-0.00791	0.00388	0.0000673	-0.00232
	(0.67)	(-1.10)	(0.74)	(0.01)	(-1.37)
Settlers per hec*D1932	-0.000680	0.00352	0.00437	-0.0212	0.000944
	(-0.10)	(0.41)	(0.51)	(-1.68)	(0.24)
Settlers per hec*D1933	0.0145	0.000829	0.00200	0.0404	0.0350
	(0.70)	(0.09)	(0.20)	(2.23)	(2.56)
Settlers per hec*D1934	-0.000393	-0.00878	-0.00425	0.0730	0.0159
	(-0.04)	(-0.97)	(-0.66)	(3.02)	(1.93)
Settlers per hec*D1935	0.00929	0.00511	-0.00319	0.0208	0.0196
	(1.11)	(0.65)	(-0.53)	(1.55)	(2.64)
Constant	-0.0848	-0.0431	-0.0527	-0.253	-0.0546
	(-2.83)	(-2.80)	(-3.77)	(-4.14)	(-3.63)
Monthly dummies	yes	yes	yes	yes	yes
N	17,043	17,043	17,043	17,043	17,043

t statistics in parentheses

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