Theft or Inequality?
A theoretical and Methodological Proposal for Identifying the Causes of Land Reform in Latin America
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Maria Paula Saffon

Introduction

Land reforms were a central feature of Latin American politics in the 20th century. In most countries, peasants demanded the redistribution of land, and elites mobilized them for support in elections or revolutions by promising land reforms. In the majority of cases, these reforms were actually enacted. And they appear to have played an important role in hindering the conditions of possibility of democratic regimes.

Along with labor regulations, land reforms were the most important manifestation of distributional conflict in the region during the 20th century (Przeworski and Curvale, 2008). However, so far the causes of land reforms have not been satisfactorily explained from a comparative perspective. Land reforms have captured the attention of policy-makers and activists both in Latin America and the US (Welsch, manuscript), but not so much of academics. Most academic studies have been restricted to single-case analyses or, when comparing cases, they have studied different institutional designs for the implementation of land reforms and their impact on various phenomena, but seldom the causes of such variation.

The few exceptions to this, which are recent, have attempted to explain Latin American land reforms as a result of democratization efforts (Lapp, 2004, arguing that suffrage extensions in the 20th century had to do with parties’ interest in mobilizing peasants, whose votes they gained through land reform promises); as a preference of authoritarian regimes (Albertus, manuscript), arguing that authoritarian regimes are more likely to enact land reforms); or as a result of international
political diffusion (Ondetti, manuscript, arguing that land reforms were enacted as a coordinated or contagious preemptive response to the Cuban revolution, largely under the auspices of the US Alliance for Progress).

Although illuminating in many respects, these explanations fail to account for some key facts—for instance, that many land reforms were enacted by non-democratic regimes (the Cuban, Peruvian, Salvadoran, Honduran, Nicaraguan, and Paraguayan), so they cannot be said to coincide with democratization; that most of the authoritarian regimes that enacted land reforms were (at least at some point) social revolutions (the Mexican, Bolivian, Cuban, Nicaraguan), so the driving force behind land reforms seems to have an important social or popular component; and that many, and some of the most important, land reforms in terms of their impact happened before the Cuban Revolution (the Mexican, Bolivian, Guatemalan, Venezuelan), so they cannot be explained as a response to it (as neither can the Cuban itself be), and they can hardly be interpreted as outliers.

More importantly, these explanations swiftly assume that land reforms are from-above processes—mainly explained by the choices made by political elites—thereby neglecting the common view that land reforms respond to peasants’ claims for land redistribution, as well as the extensive evidence in Latin America of peasant mobilization for land reform. Also, existing comparative explanations do not take into account history as a potentially relevant element for understanding the emergence of land reforms—they point to particular political traits at the time of the reforms as their main causal forces. This is not necessarily a shortcoming in itself, but it seems to be an important one in this topic since, as we will see in the next section, land reforms tend to point to a past of land dispossession as a key justification to their enactment.

My dissertation project seeks to fill in this gap, by offering a historical-institutionalist explanation of the emergence of land reforms, and of the variation in their content and implementation, which
focuses on past land disposessions and on the institutional conditions under which they generated peasant grievances. In a nutshell, I argue that during the critical juncture of Latin America’s export boom in the late 19th and early 20th centuries, land values significantly increased, and with them the incentives of landowners to dispossess lands occupied by peasants. The liberal legislation that aimed to the individualization and privatization of land ownership was used to achieve that purpose, thereby generating peasant grievances, which were stronger wherever land dispossessions violated collective legal entitlements over the land (versus mere de facto possessions). Those grievances translated into land reform claims in the 20th century, which were more likely to be addressed through more radical land reform policies, depending on the strength of the grievances.

To test the theory, I propose a methodological framework that focuses on the application of liberal laws by administrative and judicial operators, and on the way in which it impacted future land reform claims and allocations. One of the main contributions that I hope to make with this methodology is to indicate that the implementation of institutions (and not only their formal enactment) can be a cause with long-term or path-dependent effects. Indeed, I will try to show that liberal laws that were very similar in their formulation and explicit goals, were interpreted in significantly different ways by legal operators, depending on the particular context and on the objectives that they sought to achieve therein. This can contribute to the literature’s recent efforts to unpack and render more operational the notion of institutional path-dependency; it may also contribute to the literature on institutional strength, which has shown great variation in the level of enforcement of institutions, but has yet to offer sound theories about the causes of that variation.

In what follows, I summarize the theory and the methodological framework on the basis of which I attempt to substantiate it. The methodological framework contains a discussion of the specific contributions of the qualitative method to that enterprise.
I. Theoretical Framework

Puzzle: Restorative Land Reforms?

Land reform is usually understood (by the social sciences) and justified (as public policy) as a measure of distributive justice. It typically attempts to redistribute land ownership so that it is less concentrated in the hands of a few, more people have access to it, the extensions owned are not so large, etc. Thus, land reform measures are often thought of as mechanisms for diminishing land inequality, providing land access to those in need, and in some cases for making land use more efficient. Further, these measures are often thought of as a solution to conflict over land, therefore also conceived as mainly distributive in nature—i.e. as a conflict between the haves and the have-nots. From this, it is easy to conclude, as a normative prescription, that land reform should be enacted whenever land inequality is too high, and as a positive prediction, that land reform is more likely to be enacted under such circumstances, other things equal.

However, there are several reasons for doubting the common wisdom that land reform is only or mainly a redistributive measure that deals with what is chiefly a distributive conflict over land. To begin with, some of the most famous and radical land reforms in history have been preceded by conflicts generated by the dispossession of lands owned or possessed by one group—often economically or politically weaker—and they have been officially justified as a mechanism to restore those lands and thereby correct the harm caused by their misappropriation.

This is clearly the case of the land reform promoted by Tiberius Gracchus under the ancient Roman Republic (in 134 BC), which sought—or at least claimed to seek—to restore to poor Italian ex-soldiers the lands that had originally been given to them for cultivation, but that had been usurped by the rich through legal stratagems. It also seems to be the case of the agrarian reform enacted by the Mexican Revolution in 1915 (and implemented by the Mexican government up
until 1993), the original justification of which pointed to the need of restoring the lands collectively owned by indigenous/peasant villages\textsuperscript{14} that had been dispossessed by landowners with the complicity of local authorities during the Porfirian dictatorship (1876-1910).

In both cases—as well as in many others where land dispossession figures prominently in the justificatory rhetoric of land reform\textsuperscript{12}—the classic analytical distinction (based on Aristotle’s Nicomachean Ethics) between distributive and corrective justice does not seem fully adequate to understand the causes of land reform. Like typically distributive justice measures, land reforms seek to define a fair way of distributing a scarce good among citizens.\textsuperscript{15} However, they have a strong corrective justice component, since they also seek to solve the conflict between two or more persons or groups claiming to have entitlements over the same piece of land, and to repair the harm caused to the legitimate owner by the usurper. Therefore, land reform programs tend to enmesh both dimensions of justice: they promise a fairer distribution of land property, but they also promise to repair prior dispossessions.\textsuperscript{14}

On the other hand, in contrast with much of the literature on inequality and the demands for redistribution, there does not appear to be a direct link between land inequality and the enactment of land reforms.\textsuperscript{15} In places like Argentina and Uruguay, a land reform has never been implemented, even though the inequality in the distribution of land property has traditionally been quite acute.\textsuperscript{16} In Latin America more generally, claims for land reform were not prevalent during the region’s peak of land inequality (the second half of the 19th century), but rather began to be systematically voiced from the second decade of the 20th century on, when land inequality significantly decreased.\textsuperscript{17} Therefore, land inequality alone does not seem capable of accounting for the enactment of land reforms.\textsuperscript{18} This appears to be in line with the most recent political economy literature, which has found no conclusive evidence for the relation between inequality and demands for redistribution, especially in democracies.\textsuperscript{19}
Theory: Prior dispossession as a necessary cause of land reforms

My dissertation project argues that land inequality alone, without a prior history of land dispossession or land theft, is not a sufficient condition for land reform claims to exist or at least to be strong enough to promote the enactment of land reforms. A prior history of land dispossession is a necessary condition for the emergence of land reforms, whenever those reforms are promoted as a response to peasants’ claims and not simply as a strategy from above. That history is important not only for land reforms to be enacted but also for the way in which they are so. Specifically, we do not only expect to see land reforms enacted in places where land dispossession occurred; we also expect that land reforms are more radical in their content and scope the more land dispossession occurred, and that the land allocations made on the basis of such reforms privilege the victims of prior land dispossession.

The hypothesized causal relationship between land reforms and prior dispossession is grounded on the following mechanism: the existence of prior land owners or long-term possessors implies that land seizures cannot be carried out without the use of force or fraud, since they require the denial of preexistent rights. In turn, the use of force or fraud creates a perception of illegitimacy of the newly acquired rights (and the institutions in charge of their protection), which impedes the allocation of property rights from being completely excluded from the political arena, thereby admitting the possibility of future challenges through land reform.

As Stephen Holmes (partly following Carl Schmitt) argues,

“All historically known systems of private property, including ours, are normatively based on the principle that what is stolen must be returned and, simultaneously, historically based on the refusal to return what was stolen. This paradox does not undermine functioning systems of private property, presumably, because the original sin of violent expropriation is
lost in the mists of time... [However,] When force and fraud have been used to transfer property within living memory, the legitimacy of the property system as a whole is called into question”.

Now, a relevant distinction can be established between the use of force and the use of fraud. Land dispossession can be carried out through sheer force whenever the dispossessed do not have a legal entitlement over the land, but simply a de facto relation to it. In such a circumstance, the dispossessor can easily claim to be the new occupant of the land, and even request a legal title over it—either by buying it to the state or by claiming its allocation on the grounds of her possession. However, force is not enough to carry out land dispossession whenever the potential dispossessed has a legal entitlement over the land, since that entitlement must be overlooked, denied or annulled for the dispossessor to be recognized as the new legitimate possessor or owner.

The use of fraud possibly generates a greater perception of illegitimacy than the use of force alone. Indeed, it implies ignoring, abusing and/or manipulating the laws and institutions that were supposedly intended to protect the rights of the dispossessed. Further, if it leads to an effective dispossession, then it can probably involve the victims’ unsuccessful quest for legal protection or remedy. Therefore, the use of fraud can create a more radical critical view of the laws and institutions in charge of protecting property rights, and/or a more eager desire to transform or subvert them. Consequently, whenever the dispossession of lands involves the denial or overlooking of previous legal entitlements—which we can understand as an indication of fraud—we should expect victims of land dispossession to feel more aggrieved, and therefore to mobilize more eagerly for land reform, as well as to be privileged by land allocations whenever land reforms are enacted.

The previous argument echoes Alexis de Tocqueville (1856)’s and Barrington Moore (1966, 1978)’s reasoning according to which objective inequality alone does not constitute a cause for
redistributive claims; subjective grievances must exist, which allow people under conditions of subordination or exclusion to perceive those conditions as unbearable, and therefore to have a motivation for challenging them. Both de Tocqueville and Moore center their argument on the subjective perception of illegitimacy as a necessary condition for redistributive claims to emerge. However, I do not exclude the possibility that those claims may emerge not only or mainly as a result of a change in the subjective perception of the group in question, but also as a strategic change in their rhetoric for publicly justifying the demand in the political arena in a more appealing or convincing way. In either case, the point is that for the group to decide to demand redistribution, something more than inequality must be at stake, which renders its current situation unbearable either psychologically (to the group’s own eyes) or in their narrative (to the group’s eyes as presented to others).

In the case of claims for land reform, the existence of subjective grievances would hence be the product of a perception (and/or a narrative) of peasants according to which the current regime of property rights is a result of violations to preexisting legal entitlements and is, therefore, illegitimate. Such a perception (and/or narrative) would allow peasants to identify (and/or to publicly present) land dispossession as the main cause of their current condition of deprivation and marginalization. Thus, instead of being the result of forces out of their control or of chance, the socioeconomic condition of peasants could be understood (and/or politically portrayed) as the product of the unjust (and even illegal) actions of particular actors: their disposposers, with the connivance of state institutions. As a result, the transformation of those conditions via land reform would be acknowledged (and again, politically presented) as an urgent task of justice.
Applying the theory to Latin America: critical junctures of land dispossession and forms of legal tenure

In brief, my theory is that variation in the enactment and implementation of land reforms is a function of the extent to which land disposessions preceded them, and particularly of the extent to which those disposessions went against existent legal entitlements. Now, I also argue that those disposessions are more likely to occur in critical junctures for the political economy of land, when agricultural prices increase and make land become highly coveted. Such a critical juncture occurred in Latin America during the last three decades of the 19th century and the first two decades of the 20th century, a period in which the region entered the international market due to a significant increase in the demand for agricultural products explained by industrialization (Knight, 1986; Rueschemeyer et al, 1992; Barsky and Gelman, 2001; Adelman, 1994; Cotsworth, 2008). That demand led to a substantial increase in agricultural production and to a focus on export-crops, which in turn led to (and was also further facilitated by) improvements in the infrastructure necessary to access lands and transport produce—notably railroads (Coatworth, 1981; Barsky and Gelman, 2001). Both the increase in the demand for export crops and the development of transport infrastructure led to a major increase in land values (Knight, 1986; Coatsworth, 2008).

These economic changes generated strong incentives for landed elites to accumulate lands, and to adopt—or, whenever already existent but not consistently applied, to effectively enforce—a liberal regime of property rights that would enable land accumulation and the acquisition of legal entitlements over it. A liberal regime of property rights was suitable because it would enable the privatization (and productive use) of public lands, the individualization (and entrance into the market) of collective lands (notably those of the church and of indigenous communities), and the protection of newly acquired rights against expropriation.
Thus, much as Europe during the advent of commerce in the 18th century—especially Britain, as recounted by Locke [1689], Marx [1845; 1867] and Moore (1966)—19th century Latin America seems to have faced a strong pull for the enclosure of non-individually or non-privately owned lands through the implementation of liberal civil laws. However, in contrast with the European scenario, much of those lands were legally owned or possessed by indigenous/peasant populations, whose rights had been collectively recognized and in many cases effectively protected by the colonial administration and early post-independence governments. Further, some of those lands had been occupied by individual peasants or families under state-sponsored colonization programs that varied ostensibly in the degree of state intervention and small-peasant participation, but that all recognized ownership or possession rights to the first occupants. The legal recognition and enforcement of land rights, especially when they were ownership rights and they were held collectively, seems to have generated a strong sense of legal entitlement and a mechanism of collective action, which—as recounted by many historians—led indigenous/peasant communities to often defend their property rights in courts and before administrative institutions.

Consequently, when the first Latin American export boom began to take place, agricultural producers willing to accumulate lands would find a stronger obstacle to do so than the European enclosure promoters. It is very likely that, despite this resistance, dispossessions attempts ended up being successful even in places where land was collectively owned—due to the magnitude of economic incentives and to the capacity of elites to influence judicial and government institutions. However, it is also very likely that those dispossessions created grievances that translated into claims for land reform in the following decades, which were more prevalent, persistent and significant than those in 19th century Europe because they were based on the notion that enclosures had violated legal and enforceable rights over land. Thus, the existence of prior legal entitlements over the land impeded land dispossessions from becoming an “institutionalized
theft”—as Marx [1867] referred to the establishment of private property and the origin of capitalist accumulation in Europe.

On the other hand, within the Latin American context, land reforms were more likely to emerge and to be more radical in their scope, content and procedure of implementation in places where prior land disposessions were more prevalent and where they denied or subverted legal entitlements (versus mere de facto possessions), especially if those entitlements were collectively held by indigenous/peasant communities. In places where many of the lands coveted for agricultural exploitation in our critical juncture were collectively owned by indigenous/peasant communities (like Mexico, Peru or Bolivia), land disposessions probably generated stronger grievances, which subsequently translated into greater and more radical claims for land reform. This is so because it is likely that land disposessions implied a more prevalent use of fraud or legal manipulation, and also that they were more eagerly resisted by victims through legal means—notably claims of rights protection before the courts and administrative institutions. If unsuccessful, such resistance generated a greater perception of illegitimacy of the distribution of property rights but also, at the same time, the endurance of collective action mechanisms in the hands of communities, which facilitated their subsequent mobilization for land reform.

In contrast, in places where the lands seized during the commercialization of agriculture were simply possessed de facto by individuals or communities (as most clearly in Argentina and Uruguay, but also to an important extent in Colombia, or Brazil), it is likely that dispossession implied a greater dose of force, but that fraud was less important or even absent. Much as what happened with English communal lands, since possessors did not have strong entitlements over their lands, there was not necessarily a need to manipulate the law to carry out dispossession, so brute force (followed by the acquisition of a legal title) was often more than enough to push prior occupiers out. Consequently, in these contexts, we should observe less resistance to dispossession.
through the use of legal means—notably claims of rights protection before the courts and administrative institutions—even if resistance through violence was strong. And, in a subsequent period, we should also observe fewer or weaker claims for land reform, both because the legal allocation of property rights was considered less illegitimate and because there were less mechanisms of collective action to challenge it.

In sum, on the basis of my general theory, I propose a distinction not only between the European and Latin American periods of agricultural commercialization, but also and especially among the different Latin American contexts that faced such commercialization in the late 19th century. This distinction is based on the extent to which the rights of the possessors of seized lands had been previously recognized and enforced by the legal system, and on the particular shape that those rights took. Distributive rural conflict (as epitomized by land reform claims) in 20th century Latin America was particularly acute not only (or mainly) because the commercialization of agriculture and the land seizures that it generated produced dramatic levels of land concentration and inequality (which was probably also the case of 19th century Europe), but also (and especially) because those land seizures violated rights that had been previously enforced by legal institutions, thereby producing much stronger grievances.

In fact, it is very likely that land inequality was, at least in part, a result of land disposessions, and therefore not the only or most important cause of future land reforms in Latin America. Rural conflict was more acute in contexts where the legal entitlements that were ignored or denied by land disposessions were stronger and made collective action easier. Indeed, the recognition of indigenous/peasant collective property in colonial and post-colonial times generated a stronger sense of entitlement among the communities that held it, and an easier and more common way of protecting their legal rights. So, when those rights were trumped against, communities felt (or claimed to feel) more aggrieved, but they also had more easily accessible mechanisms for struggling
against what they perceived as an illegitimate system of property rights. In turn, the dispossession of occupied vacant lands generated less grievances and resistance than those of indigenous/peasant communities, but in the former case those grievances and resistance were more likely to exist and be voiced when there had been a prior legal recognition of ownership or possession rights over vacant lands.

**The theory vis-a-vis other possible explanations**

As argued previously, I attempt to show that land inequality is not the only or main factor driving the emergence, content and scope of land reforms. However, apart from inequality, there are other competing explanations of land reforms. To begin with, one could think that what really drove the variation in land reforms is not the form of legal institutions of property rights and their incidence on land dispossessions, but rather the sheer amount of indigenous/peasant populations that existed in different contexts. Using a similar argument to Mahoney’s concerning variation in economic development, one could hypothesize that the fewer indigenous populations in Spanish colonial times, the fewer or weaker the claims for land reform in the future. Sure, the amount of indigenous population during colonial times quite possibly drove the amount of collective property that the crown allocated to indigenous communities, and even that which persisted until the late 19th century. However, my argument is that the institution of collective property—even if endogenous to some extent—produced effects of its own by influencing the sense of entitlement and the degree of collective action of its beneficiaries, which in turn probably also increased the chances of survival—or at least of resistance against disappearance—of that institution.

In other words, without the institution of collective property, rural conflict would have adopted a different path—probably a much less politically and legally contentious one—even if indigenous/peasant land possessors were quite numerous. Land dispossessions would have implied
more force than fraud, and they would not have generated such a great sense (and/or narrative) of illegitimacy. Nor would they have been so resisted through legal means. Therefore, I argue, land dispossessions, even if great, would not have led to strong claims for land reform. That is, in fact, what I suspect happened in much of Europe and in some Latin American contexts where, even if dispossessed peasants were great in number (such as in Colombia), they did not have strong legal entitlements over the lands they possessed. To show this, I need to look at the nuts and bolts of the implementation of liberal land legislations, and to verify if the processes that I hypothesize did indeed occur.

Now, my theory shares with explanations based on inequality and on the number of indigenous/peasant populations the notion that land reforms are, to an important extent, a “from below” process, in the sense that they respond to popular (and more precisely peasant) claims of land redistribution. Therefore, apart from showing that those claims are mainly driven by prior land dispossessions and by the different ways in which the latter were perceived and struggled against by their victims depending on the legal institutions in place, I also seek to show that those claims are causally relevant in the enactment of land reforms.

Hence, I seek to partially question explanations that portray Latin American land reforms as mainly from above processes, i.e. as elites’ choices to mobilize peasants in exchange for their votes (Lapp, 2007); as policies favored by authoritarian regimes (Albertus, manuscript); or as preemptive counterrevolutionary strategies after the Cuban Revolution (Ondetti, manuscript). My theory does not deny that the emergence of land reform as a relevant policy choice in the political arena is, in part, a result of elite choices and of the current political conditions that influence those choices—such as elite splits caused by democratization processes, or the international diffusion of policies. What I argue is that land reforms are more likely to be enacted and to have radical effects in contexts where there exist peasant claims for land based on prior land dispossessions, and that
those claims are more likely to exist wherever those disposessions infringed property rights, especially when collective.

Therefore, my theory acknowledges that land reform will tend to be proposed by political leaders or parties, and that their timing will depend on these leaders’ strategies for retaining or acquiring power. However, it argues that, when proposed, the enactment and implementation of land reform programs will depend on the extent of peasant grievances (and/or narrative of their grievances) and on their capacity for publicly expressing them and for collectively mobilizing for land reform, both of which will tend to be greater the more land disposessions affected collective property rights.

Thus, according to my theory, even though the timing and concrete reasons for which political elites will promote a land reform program may vary depending on the nature of the political regime, the extent of elite splits, or the degree of diffusion of land reform policies, the latter will only be enacted (and tend to be more radical in their content and implementation) in contexts where peasants’ grievances are strong and relevant enough for elites to strategically need to address them in return for peasant support. And this is the case not only or mainly where peasants are numerous, or where land is unequally distributed, but when there exists a past of land dispossession regarded as illegitimate, as well as a past of legal collective action for the protection of land property rights—both of which are more likely to exist in contexts where indigenous collective property was more widespread. In that sense, according to my theory, strong peasants’ claims for land, based on a prior history of land dispossession, are a necessary cause (though not sufficient, on its own) of the enactment of land reforms in Latin America, and the effectiveness and radicalism of those reforms depends on the extent to which those disposessions affected previously enforceable property rights, especially if collective.

If correct, my theory could explain the enactment of radical land reform programs in places like Mexico, Bolivia, and Peru not only—as Albertus has argued—as a result of the authoritarian nature
of the political regime that enacted them, but also as a response to organized peasants’ claims. This could further suggest that the nature of such political regimes was, at least in their origins, not simply authoritarian, but rather social revolutionary or populist, since those regimes sought to address peasants’ claims from below possibly because of the importance of the peasant class in their power grab. Consequently, my theory would not completely deny that authoritarian regimes are more likely to enact land reform programs and that these programs can be more radical in their implementation because of the existence of fewer veto points. However, I would argue that, in Latin America, the decision of authoritarian regimes to implement extensive land reform programs has more to do with the fact that most of those regimes were, at least in their origins, social revolutions or populist processes, with a strong component of peasant partisans to which authoritarian leaders had to cater.

This could explain why some of the most prototypical (non-revolutionary or populist) authoritarian regimes in Latin America never implemented land reform programs (as in Argentina and Uruguay), and instead in some cases dismounted those enacted by prior democracies (notably Chile and Guatemala). It would also explain that all of the authoritarian regimes that in fact enacted land reforms are commonly described as social revolutions or populist regimes, at least in their origins—Mexico, Cuba, Bolivia, Peru. Contra Albertus (manuscript), then, I would argue that what drives the correlation between authoritarian regimes and the enactment of land reforms in Latin America is the social revolutionary or populist nature of the authoritarian regimes that in fact enact land reforms, and that can be hardly characterized as merely or mainly authoritarian because of the from below demands to which they are subject—which contrasts with the from above or elitist nature of authoritarian projects.

On the other hand, my theory could also fill in the gap left by the explanation of land reforms based on international political diffusion that Ondetti (manuscript) has put forward, which, as
mentioned before, can give no account of the land reforms enacted before or during the Cuban revolution—the Mexican, Bolivian, Guatemalan, Venezuelan and Cuban land reforms—even though such reforms are many (and therefore hardly outliers), and also some of the most significant and radical land reforms in their scope and content. My theory does not discard the relevance of political diffusion for understanding the entrance of land reform proposals into the political agenda, notably its timing. However, I insist on the existence and strength of peasants’ claims related to land dispossession as a necessary condition for the enactment and radicalism of those proposals. I thus argue that, even if proposed, land reforms are not likely to be enacted in places where those claims are few (such as Argentina and Uruguay); and that, even if enacted, land reforms are not very likely to be radical in their content or effective in their implementation in places where those claims are less linked to dispossessions of lands held through private property—notably collective property—than to dispossessions of simply possessed or occupied lands (such as Colombia, or Brazil).

Moreover, my theory may even offer an explanation for the different timings of land reform in the region. Indeed, most of the cases where land reforms were implemented before the Cuban revolution—Mexico, Bolivia, and Guatemala—are also cases where collective land property was probably most extensive, given the high levels of indigenous populations in the 18th and 19th centuries. Therefore, they were probably cases where peasants’ grievances for the prior dispossession of the lands they owned were more pressing, given the perception of illegitimacy that they generated and the level of organization for collective action that peasants retained. Consequently, they were probably the cases where political elites felt most compelled to propose land reform programs under circumstances of political change such as democratization or revolutionary upheaval. In contrast, in contexts where peasants’ claims were less pressing because of their lower levels of organization or perception of illegitimacy of property institutions—such as
Colombia, Brazil, or Chile—it is likely that domestic conditions on their own did not provide a sufficient condition for land reform programs to be proposed by elites, and that international diffusion played a greater role in explaining their timing. But in these cases, too, land reforms were only enacted or effectively implemented if peasants’ claims were strong enough.

II. Research Design

As seen above, in my theory, the main variable driving the land reform outcomes is the form of legal tenure through which individuals or communities held lands that were the target of dispossession attacks during the late 19th and early 20th centuries. As said, the more those lands were held as private ownership, and especially as collective ownership, and not as de facto possessions, the harder it was to dispossess them. Hence, the more fraud would dispossessors use and the more would victims of dispossession resist through legal means. And, once dispossessed, the more would those victims make land reform claims, the more radical in scope and content would those reforms be, and the more the land allocations made on their basis would benefit prior victims of dispossession.

The study’s hypotheses entail two types of comparisons:

1) Across countries, to determine whether (as predicted) greater levels of dispossession of collective property (versus de facto possessions) led to greater levels of political mobilization for land reform and to more radical land reforms.

2) Within countries, to determine whether (as also predicted) land dispossession was greater in regions of export agriculture, and whether (regardless of the way in which it was carried out) land dispossession was causally related to land reform claims and to allocations (wherever land reforms were enacted).
Cross-country comparison

The cross-country comparison requires a selection of cases that present variation in the independent variable, that is, cases that differ in the proportion in which lands that were dispossessed during the critical juncture of Latin America’s export-boom were held through collective property rights (versus through individual property rights, and legal or de facto possessions). I will compare Mexico, Colombia and Argentina, three countries located in different geographical regions of Latin America that, according to the existing literature, seem to have had different shares of collective property and possession of export-prone agricultural lands during the critical juncture—in such a way that they may be said to represent the different possible scenarios in the region during that period. As a matter of fact, the selected countries also exhibit different types of land reform outcomes during the 20th century—something that fits the theory’s predictions of land reform outcomes but that, to be confirmed, requires that I can show that those outcomes were produced through the hypothesized processes and mechanisms.

The following table illustrates the variation of the selected cases both in terms of the independent and dependent variables:

<table>
<thead>
<tr>
<th>Country</th>
<th>Independent variable (late 19th, early 20th centuries)</th>
<th>Outcome (20th century)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mexico</td>
<td>High collective ownership, low occupied waste lands</td>
<td>Radical land reform (1917, but strongly implemented from the 1930s on)</td>
</tr>
<tr>
<td>Colombia</td>
<td>Some collective ownership, high occupied waste lands</td>
<td>Weak or failed land reforms (1936, 1961)</td>
</tr>
<tr>
<td>Argentina</td>
<td>Almost no collective ownership, high (but less occupied) waste lands</td>
<td>No land reform (though some laws improving tenant conditions in 1921, 1932, 1942, and under Peronism)</td>
</tr>
</tbody>
</table>

If my theory is right, we should observe greater levels of dispossession of export-prone lands held under collective property than of waste lands held under possession during the critical juncture in Mexico, followed by Colombia and then by Argentina. Ideally, we should be able to quantify those lands and to identify the places where they were located. It is possible that, for all three countries, we can find calculations of the amount of public waste lands that were privatized during the period under study, and perhaps of the amount of corporate lands that were disentailed. However, on their own, such calculations would not be enough to establish the amount of those lands that were fraudulently or violently dispossessed—since the theory argues that disposessions took place through the fraudulent application (or misapplication) of liberal legislation on land property, we can assume that at least some of that legislation was adequately applied, and we should be able to distinguish adequate from inadequate applications. For that purpose, we need indicators of the way in which the liberal legislation presumably used for land dispossession was implemented in the different countries, as well as in the different regions of those countries.

In each country, I am gathering data on the following indicators for the critical juncture of land disposessions (which varies in each country but roughly goes from the 1870s to the 1920s in the region):

a) National and (in federal countries) state level legislation concerning land ownership, especially legislation referring to the disentailment of collective lands and the privatization
of public lands, as well as legislation concerning the laws of tenancy (which, if strictly applied, could affect the land occupation of non-owner peasants). b) Administrative measures (decrees, ordinances, etc.) interpreting and applying those laws. c) Judicial and administrative cases applying and/or disputing those laws. d) Peasant riots related to land dispossessions.

The previous indicators (especially a, b and c) will allow me to understand the ways in which liberal legislation concerning land was interpreted and applied by state authorities at the time of the 19th/early 20th century export-boom, and therefore to identify if there were particularly abusive interpretations and misapplications that could have led to land dispossession, as well as which were the legal mechanisms that were used more prevalently for that purpose. Furthermore, indicators c and d will allow me to capture (and contrast) the amount and forms of legal and de facto resistance that peasants carried out against the application of such legislation.

The analysis of administrative and judicial complaints concerning land issues will allow me to determine whether (and the extent to which) peasants used legal mechanisms to complain about their lands being misappropriated, and also whether they claimed that such misappropriation was carried out through force or fraud. In turn, the analysis of peasant riots will allow me to determine the extent to which those riots were about land misappropriations, whether they happened especially when such misappropriations were occurring and whether they were used as a complementary or (as I hypothesize) as an alternative strategy to that of legal complaints.

The former indicators will help me show not only that in each country the independent variable manifested itself in the ways predicted by the theory, but also that the independent variable is
causally connected to the land reform outcome occurred therein. As mentioned before, my theory predicts a land reform outcome that actually took place in each country—we know that the Mexican land reform was much more radical than the Colombian ones, and that Argentina never had one. Hence, for the theory to be right, we need to show that the hypothesized processes connecting such outcomes with the independent variable did indeed take place. We should then show that the following phenomena occurred in each country:

Table 2.

<table>
<thead>
<tr>
<th>Country</th>
<th>Liberal legal mechanism used predominantly for land dispossession in export-agriculture regions</th>
<th>Amount of legal resistance against the application of those laws (measured as administrative or judicial complaints of dispossession)</th>
<th>Amount of fraud or violence used in the application of those laws (measured as administrative or judicial complaints including accusations of fraud or violence)</th>
<th>Amount of de facto resistance against the application of those laws (measured as peasant rebellions or riots against dispossession)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mexico</td>
<td>Disentailment laws along with tenancy laws (more than survey of public lands)</td>
<td>High</td>
<td>More fraud than violence</td>
<td>Low</td>
</tr>
<tr>
<td>Colombia</td>
<td>Colonization of the agrarian frontier law (more than disentailment laws, and tenancy laws)</td>
<td>Medium</td>
<td>More violence than fraud</td>
<td>Medium</td>
</tr>
<tr>
<td>Argentina</td>
<td>Colonization of the agrarian frontier, and tenancy laws</td>
<td>Low</td>
<td>Mostly violence</td>
<td>High wherever existent, but low overall</td>
</tr>
</tbody>
</table>
The processes predicted by table 2 can be measured through the indicators for the independent variable identified above (a through d). In turn, to assess whether those processes are causally connected to the land reform outcomes that occurred in each country, I will use the following indicators, which seek to capture the ways in which those outcomes were materialized during the 20th century (roughly between the 1920s and the 1960s, the period in which the enactment of land reforms predominated in the region, though the period has narrower dates in each country):

e) Political mobilization for land reform, measured as the extent to which the programs and rallies of political parties and other political and social organizations (like peasant unions) contained references to land reform, and the amount, importance and success of legislative initiatives concerning land reform.

f) Content and level of radicalism of land reform laws (where enacted), or of other laws aimed at addressing rural discontent and/or ameliorating rural conditions (such as tenancy laws improving the contractual conditions of tenants).

g) Timing, amount and location of land allocations based on land reform laws (where enacted).

The previous indicators will allow me to show in more detail the way in which the land reform outcomes predicted in table 1 occurred, as well as to compare the countries’ outcomes in terms of their greater or lesser level of success and radicalism. I will measure indicator e in all countries, but it will be especially relevant for the Argentine case, which is a null case (of no land reform), and where indicators f and g are therefore mostly inapplicable (with the exception of indicator f’s reference to other laws, which in the Argentine case will consist in tenancy laws).
In turn, I will measure indicator f in the countries where one or more land reforms were enacted, by classifying or ranking the content of such land reforms in terms of its level of radicalism with respect to: (i) the enactment process (extraordinary processes like social revolutions being the most radical); (ii) their redistributive nature (measures significantly reducing land inequality and concentration; granting access to previously landless persons; and/or transforming the socioeconomic profile of landowners would be considered highly radical); (iii) their transformation of the legal system of property rights (ample clauses justifying expropriation, and measures implying changes in types of legal tenure, such as those allowing for corporate property, would be considered more radical); (iii) the execution mechanisms (mechanisms that attempt to avoid inefficacy and to reduce implementation obstacles, such as the creation of specialized and centralized executive agencies, or the requirement of peasant participation, would be considered particularly radical).

Finally, I will measure indicator g, first, by counting the total number of land allocations made on the basis of land reform programs in the countries where those reforms were enacted. Such counting will provide a first rough comparable measure of the scope (and hence the quantitative level of radicalism) of each land reform. However, I will also and more significantly measure indicator g by looking at the timing and location of land reform allocations, which will allow me to see if, within each country, there exists a relation between higher levels of dispossession and early and more abundant allocations—something that I will discuss in more detail in the next section. Lastly, wherever possible, I will also measure indicator g by analyzing the content of land allocation decisions, which include the justification of the decision to allocate a particular piece of land, and therefore offer very relevant information about the prioritization criteria used for allocating land, and even in some cases about the historical background of beneficiaries. For instance, in the
Mexican case, decisions of land allocation by the National Agrarian Commission often referred to whether claimants had been victims of land dispossession, since, according to the 1917 Constitution, this gave them the right to the restitution of their land.

On the basis of the previous indicators, my theory expects the predicted outcome to be materialized in the following way in each of the countries under analysis:

<table>
<thead>
<tr>
<th>Country</th>
<th>Level of political mobilization for land reform (prior to the enactment of the land reform, if enacted)</th>
<th>Content of land reform laws</th>
<th>Amount of lands allocated via the land reform in those regions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mexico</td>
<td>High</td>
<td>Radical</td>
<td>High</td>
</tr>
<tr>
<td>Colombia</td>
<td>Medium</td>
<td>Moderate</td>
<td>Medium</td>
</tr>
<tr>
<td>Argentina</td>
<td>Low</td>
<td>Non-existent</td>
<td>None</td>
</tr>
</tbody>
</table>

**Within country comparisons**

I believe that I can gather a complete sample of the indicators identified in the previous section for all my three cases, which will assure an unbiased sample and also allow me to make certain cross-country comparisons (mainly about national-wide trends). That is the case because I am mostly focusing on indicators related to the national level, which allow me to compare centralized and federal cases—Colombia, and Mexico and Argentina, respectively. However, both because of the great amount of information that such a sample will contain and because it can ignore important
phenomena occurring at the regional and local level—especially in federal countries—I have decided to choose certain regions within each country, which I will analyze in more detail and which will allow me to make relevant within country comparisons.

The selection of regions seeks to satisfy the following criteria: (i) variation in the regions’ geographical location, socioeconomic and/or political traits, and more importantly ethnic composition (which will allow me to compare regions with different levels of collective ownership); and (ii) variation in the regions’ level of participation in the export market (which will allow me to compare regions with similar levels of collective property, but a different degree of involvement in the international market).

For example, in the Mexican case, I selected six states: Coahuila, Sonora, Morelos, Guerrero, Oaxaca and Veracruz. Each pair of those states belongs to the three main geographical areas of the country: North (Coahuila and Sonora); Center (Morelos and Guerrero) and South (Oaxaca and Veracruz). Generally speaking, each of those three regions has different levels of ethnic population, and also different amounts of collective indigenous/peasant land ownership (low, high and mid or high, respectively).

However, the states chosen within each region vary in the extent to which they participated in the agricultural export market during the critical juncture under study: while Coahuila, Morelos and Veracruz were early and active participants in the export market—producing cotton, sugar and vanilla, respectively—and therefore possibly experienced an increase of land values; Sonora, Guerrero and Oaxaca did not significantly participate in the export market either because their main products were for internal consumption (as in the Oaxacan case) or because their territories remained inaccessible through the newly implemented railway system (as in the Guerrero case).
This intra-regional variation will allow me to explore if, as I hypothesize, the participation in the export market generated more land dispossessions (measured in terms of the peasants’ administrative and judicial complaints, and of the extent to which the latter refer to fraud or violence) during the critical juncture. Also, and perhaps more importantly, the inter-regional comparison will allow me to explore whether, as I also hypothesize for the Mexican case, in the regions where collective property was more prevalent (the Center and the South), land dispossessions generated greater grievances and claims for land reform, and also earlier and greater allocations of land in response to those claims. As said above, my case selection contains three cases that participated in the export economy, and hence that possibly faced similar levels of demand for agricultural products and of pressures over the land, but that had different proportions of collective property (Coahuila, Morelos, and Veracruz). So by comparing those cases I will be able to establish if the prevalence of collective property did indeed have an effect on land reform claims, by looking at the relation between the administrative and judicial land complaints during the critical juncture, and at the timing and amounts of land reform allocations in those regions.

For the selected regions, I will analyze the data gathered at the national level at a greater level of detail, and I will also look at more specific data, such as the content of administrative complaints and judicial cases, the content of decisions on land allocations, and possibly judicial cases at the state (not federal) level.

**The Contributions of the Qualitative Method**

Certainly, an important part of the data that I will gather can be profitably used for quantitative analysis. In particular, in each country, it is possible to explore the correlation between land
dispossessions during the critical juncture (measured as administrative and judicial complaints of land dispossession) on the one hand, and land reform claims and/or allocations during the 20th century, on the other hand. Furthermore, the findings of such a regression could be compared with other alternative explanations of land reform allocations, by including in the regression model controls for variables such as land inequality, amount of indigenous populations, suffrage extensions, nature of the political regimes and presence of external circumstances relevant for international diffusion.

However, those correlations alone would not be able to prove or disprove my theory about the causal long-lasting impact of the implementation of property rights institutions on land reform claims. Indeed, my theory focuses on the mechanisms through which institutions produced the effects that it argues were produced. Therefore, the main goal of the research design is to be capable of showing that those mechanisms operated and that it was mainly through them that the alleged effects were generated. Qualitative analysis is crucial to show that the causal mechanisms that I put forward—the extent of dispossessed lands held as collective property, and this institution’s capacity to facilitate collective action and to generate stronger perceptions of illegitimacy—operated.

The operation of those mechanisms can be shown by spelling out the particular paths followed by the implementation of similar liberal property rights (notably disentailment and privatization laws) since the eve of Latin America’s entry into the international market in different institutional contexts (marked by different levels of collective property and possession). And then, by exploring whether those paths are causally connected to the processes of proposals, enactment and implementation of land reform programs.

This can be done, first, by quantifying the extent to which the chosen indicators of the relevant variables presented themselves in different contexts (both across and within cases). Such
quantification does not turn the analysis into a quantitative one, since its main objective is not to establish probabilities or likelihoods of the occurrence of certain events, but their identification as either necessary or sufficient conditions (Goertz & Mahoney, forthcoming). These phenomena are complex and have to do with the degree of implementation of legal institutions, so determining whether they occurred is not an all-or-nothing affair; rather, it requires establishing criteria to analyze the extent to which they took place and also the extent to which their occurrence can be considered causally relevant.

Partly following the methodology of fuzzy-set analysis (Ragin, 2008), I can create scores for assessing the extent to which legal institutions can be considered to have been implemented and for the way in which they were so. As seen above, I already have the criteria on the basis of which I can establish those scores for land reform programs: on the basis of their level of radicalism with respect to their process of enactment, content, mechanisms of implementation, I can formulate scores, such as: radical (7 to 10 in 2 or more of those criteria); intermediary (4 to 7 in 2 or more of those criteria); non-radical (1 to 3 in 2 or more of those criteria), or something of the sort. In turn, I can establish criteria for measuring the way in which liberal reforms in the late 19th and early 20th century were implemented, such as the extent to which collective property was at stake in judicial cases and administrative complaints concerning land—for example, high whenever more than 40% of those cases concerned that type of property—or the extent to which those cases and complaints refer to fraud or violence—again, for example, high whenever more than 40% do so. Even though I can now have a general idea of what those criteria are, it seems important to make a first analysis of the judicial and administrative decisions in question to be sure that the scores used make sense, given their general trends—what Ragin calls calibrating the scores.
Second, the causal connection between land dispossessions and land reform allocations can be analyzed via process-tracing. The latter can be fruitfully carried out through the qualitative analysis of the content of the main instruments of implementation—notably administrative and judicial decisions—of the legislation enacting both liberal policies of property rights in the late 19th and early 20th centuries, and land reform programs throughout the 20th century. Such instruments have the great advantage of referring to specific cases, and therefore of providing enough detail about the conditions of implementation. Those conditions can illuminate whether the hypothesized mechanisms of dispossession (i.e. fraud when the dispossessors were owners; violence when they were possessors) and of resistance against such dispossession (i.e. a more organized collective action with recourse to courts when collective property took place) occurred.

As mentioned before, this is not only the case of administrative and judicial decisions through which the liberal legislation of the late 19th and early 20th centuries was implemented, but also of administrative decisions implementing land reforms, which include the justification of the decision to allocate a particular piece of land, and which sometimes even refer to prior land dispossessions—as in the Mexican case. Land allocation decisions can help establish not only whether a particular community or individual received land from a land reform program and the date in which it did—which are the data most often analyzed by social scientists—but also and much more interestingly what were the reasons why the claimants requested the land and the government considered the land allocation proceeded or not—for instance, in the Mexican case, restoration or simple provision. These reasons can help us causally link land allocations with prior dispossessions either explicitly—when that is the motivation for the government to allocate the land—or implicitly—for instance, when that is the motivation of the community or individual to request it, or when there is other information that allows us to conclude that such a land
dispossession occurred and that it is the reason why the claimants are requesting the land allocation and also why they receive the land with priority. Now since this analysis is quite time-consuming, it may only be done for some regions, or even for some municipalities within those regions. That is one of the main purposes of the subnational case selection proposed earlier.

The qualitative analysis proposed here may be useful not only for showing whether peculiar causal mechanisms were in place. It may also serve to exemplify a way of empirically studying institutional implementation as a cause with path-dependent effects. Indeed, what I am trying to propose here is a method for examining whether institutions in their application—and not merely as they are formulated in the books—can be said to have long-lasting effects that are not attributable to the institutions themselves—which can be very similar in their legal formulation, such as many liberal laws of disentailment or privatization of waste lands were—but to the way in which they are interpreted by legal operators.

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1 As Ondetti (manuscript) shows, with the exception of Argentina and Uruguay, all Latin American countries enacted land reforms during the 20th century, ranging from superficial to structural policies. The peak of the enactment of land reform legislations occurred in the 1960s, and of their implementation in the 1970s. Ondetti defines land or agrarian reform as “any government policy that seeks to make the distribution of land more equitable”. This is a common definition of land reforms, and also the main way in which they have been understood in Latin America. It implies an explicit redistributive view of land reforms, since land reform programs foresee the reallocation of lands with the purpose of reducing land concentration and inequality, and/or of providing access to the landless. It is true that land reform is sometimes used in a wider sense to denote any transformation in the system of property rights or in landholding patterns—for instance, the enclosure or privatization of public lands, or the disentailment of corporate or collective lands, which generally recognize land rights to those who previously occupied it, and hence do not necessarily or immediately reallocate land. I will restrict the definition of land reforms to those directly seeking the reallocation of land even if, as we will see, I will argue that in many cases they also include a justification (and even measures) of land restoration, and hence that they are not so neatly redistributive. I will thus exclude from the term land reform other type of agrarian reform policies that change the system of land property rights without necessarily redistributing land allocation, such as the disentailment of corporate lands and privatization of waste lands. I will refer to the latter type of measures as liberal reforms, given their straightforward liberal objective of unifying the system of land rights into individual, private and absolute property rights, the model of which is the Code Civil of Napoleon.

2 In some contexts, land reforms motivated reactionary coups (e.g., Guatemala and Chile). In others, their enactment by revolutionary or authoritarian regimes with redistributive agendas served to justify their permanence in power to preempt their dismounting (e.g., Mexico, Cuba, Bolivia, and Peru). Yet in others, inefficacious land reforms provoked the emergence of revolutionary armed groups (Colombia and El Salvador).
For this definition and empirical examples of different criteria for distributing goods, see Elster (1992).
war to peace. In some contexts, reparations that seek to redress the harms suffered by victims may exclude the compensation of sumptuary or luxurious assets on the basis of a distributive criterion: to avoid the reconstitution of previous fortunes. Post-World War II Norway and France offer examples of this (see Elster, 2004). In other contexts, reparations may seek not only to redress victims’ harms but also to enhance their socioeconomic wellbeing, especially when victims belong to marginalized populations (see Saffon and Uprimy, 2010).

Many have attempted to causally link inequality with political demands for redistribution. On inequality in general, see Acemoglu and Robinson (2006); on land inequality, Sokoloff (2000); Engerman & Sokoloff (2002); Acemoglu et al (2001).

See Przeworski and Curvale (2008), p. 21, who (on the basis of Vanhanen’s data) measure land inequality in 1850 and 1970 in terms of the average proportion of family farms, in Argentina and Uruguay, that proportion was of 5 and approximately 7 per cent, respectively, in 1850, and of approximately 23 percent in both countries in 1970. Though, as the authors indicate, the proportion of family farms is an imperfect way of measuring income inequality because it does not consider the relative value of land, it can be taken as an indicator of land inequality because family farms tend to be of a small size (thus, the higher their proportion, the less big land extensions one could expect) and because the greater the number of families who own land, the more land is distributed across the national population. This indicator shows that Argentina and Uruguay had few family farms and hence a high land concentration rate in both 1850 and 1970—the rate is similar to the Latin American average, and higher than that of several other countries of the region in both dates.


Ondetti (manuscript) also argues this.

see Kaufman (2008); Albertus and Menaldo (manuscript); Samuels (manuscript).

Here, a relevant distinction might be established between land reforms that are preceded by popular claims for land reform and/or that attempt to address those claims, on the one hand, and land reforms that are carried out as a strictly from above project that seeks to enhance economic efficiency and/or to weaken the landowning class, on the other. Taiwan seems to be a case of the latter. I thank Rodrigo Uprimny for bringing this to my attention. In that sense, the scope of my theory is restricted to land reforms that have been promoted, at least in part, as a response to peasants’ claims or as a way of mobilizing the peasantry. Therefore, my theory does not contend that land reforms can happen without popular claims for land reform, but rather that, if those claims exist, they are likely to be motivated by prior land dispossession rather than by land inequality alone. Further, my theory contends that, in Latin America, popular claims were not causally relevant for the enactment of land reforms, so theories that explain them as exclusively from above projects are incomplete at best. Finally, my hunch is that, apart from Latin America, popular claims were also causally relevant for the enactment of many land reforms in other regions (for instance, in Italy and Ireland, or in Vietnam), so the theory should be generalizable, albeit with some exceptions.

Holmes (2010); for the argument in Schmitt, see Schmitt (2003 [1950]).

There is a lot of circumstantial evidence of peasants going to courts in the late 19th and early 20th centuries in Latin America to request protection against dispossession. See, for instance, Hobshawn (1974) on Peru; Womack (1978) on Mexico; Kalmannovitz and López (2003), Melo (2007), Ocampo et. al. (2007) and Bejarano (2007) on Colombia.

It is possible to empirically inquire which of the two situations (or if both) is more likely to propel redistributive demands, for instance by exploring whether peasants’ land claims were always justified in terms of prior dispossession or only began to be so at some point in time—something that, as I propose below, can be done by looking at the complaints presented by peasants before judicial and administrative authorities through a long period of time.

This argument is inspired in Locke’s ([1689] and later Marx’s [1845; 1867] Moore’s (1966) description of the European (notably the English) land enclosures, and in Womack’s (1978), Knight’s (1980) and Coatsworth’s (2008) description of a not so different process in Latin America, particularly in Mexico. However, in contrast with the Lockean interpretation of economic change offered by neo-classical economists (i.e. North and Thomas, 1973; North, 1981; North, 1990), I argue, following Adelman (1994), that economic incentives do not, on their own, determine outcomes—though I am interested in land reform outcomes, not in economic development, as Adelman is. Preexisting institutions, and notably the prior allocation of property rights, are crucial for explaining the way in which those incentives play out (on this see also Haber et al, 2003; Onoma, 2010). Hence, as I argue below, the difference between the English and the Latin American processes of land appropriation and subsequent enactment of liberal property rights is that, in the latter case peasants had been previously recognized as (often collective) legal owners or possessors, so they constituted a greater opposition than English possessors because they could (and often did) resort to judicial and administrative authorities to request the protection of their prior rights.

On the adoption of civil and commercial codes in the region, see Coatsworth (2008). On the enduring impact of liberal reforms, notably on land issues, in Central America, see Mahoney (2001).
available legal rights). My claim is, then, that these usage or possession rights gave a weaker sense of collective property could be maintained by them through these rights were weaker than private property rights, mainly since they depended on the possessors' factual entitlement and weaker legal mechanisms of protection than private property rights, and notably than collective property rights, which also offered a collective action platform.

European communal lands were public waste lands that could be (and had traditionally been) used by communities; such communities had usage rights but they did not concede—nor were they perceived as—private property rights. In that sense, those rights were similar to those of a few (often nomadic) indigenous groups in Latin America (such as those conquered during the Desert campaign in Argentina during the 1870s and 1880s) and of many colonos in Latin America (who occupied and worked waste lands, and consequently acquired certain possession rights, but who knew that those rights were weaker than private property rights, mainly since they depended on the possessors’ factual capacity to remain on the land). My claim is, then, that these usage or possession rights gave a weaker sense of entitlement and weaker legal mechanisms of protection than private property rights, and notably than collective property rights, which also offered a collective action platform.

Mahoney (2010); see also Lange, Mahoney and vom Hau (2006).

Even with dramatic decreases in indigenous populations, collective property could be maintained by them through several means, including the incorporation into their communities of mestizos so as to keep the minimum number of population required for collective property, as happened in many pueblos de indios in Mexico (see Hernández, 1993; Kouri, manuscript). One can also find interesting examples of indigenous’ legal strategies in Mexico for retaining the collective property of their lands even after liberal laws ordered their disentailment, by using still available legal institutions like the indivisible property of civil associations, or of municipalities (see, among others, Knowlton and Negrete, 1996; Escobar Olmsted, 2001). Now, the phenomenon of endurance of indigenous collective property until the late 19th century is not exclusive to Mexico or to other countries with high amounts of indigenous peoples during colonial times. One can find significant examples in places like Colombia and Chile where indigenous were less numerous but still quite present during the colony, but also even in some Argentine regions like La Rioja, Santiago del Estero, or Tucumán; it has been argued that such endurance was at least in part determined by the capacity of indigenous to avoid inter-group confrontation and remain united (see Barsky and Gelman, 2001).

In this sense, I’m following Thelen (2003), who argues that even if institutions have endogenous origins, they can have effects of their own, and that it is these effects that allow us to meaningfully talk about historical institutionalism and path dependency. See also Mahoney and Thelen (2010).

For the application of this argument to Mexico, see Tannenbaum (1930); Knight (1986).

We may say that Mexico represents other countries where collective property was high in the 19th century and land reform radical in the 20th, such as Bolivia, Peru and Guatemala; that in turn, Colombia represents cases of middle levels of collective property in the 19th century and meager and/or inefficacious land reforms in the 20th century, such as El Salvador, Chile, Nicaragua, Brazil; and that Argentina represents cases of very low collective property in the 19th century and almost non-existent land reform in the 20th, such as Uruguay and Costa Rica. To show this, however, I
would have to analyze in more detail the extent to which these cases do in fact exhibit those traits, something that I will only be able to do at a later stage of generalization of the argument.

"The characterization of the independent variable and the phenomena connecting it to the outcome in each case is based on my preliminary knowledge and data gathering for the period 1860s-1920s. In Mexico, the export boom took place mainly between the 1870s and the 1900s, under the dictatorship of Porfirio Díaz. During that period, there was a sharp increase in the international demand for agricultural products other than corn (mainly for internal consumption), such as sugar, vanilla, coffee, banana and cotton (see Knight, 1986; Haber et al, 2003). The lands where those products were or could be farmed (for both fertility and accessibility issues) seem to have been held by indigenous/peasant communities through collective ownership much more than owned by individuals or possessed by individuals or communities. Indeed, in that period, those products were mostly grown in areas with a lot of indigenous density (especially the center and south of Mexico) and where indigenous pueblos (with collective rights) had existed in greater magnitude at the beginning of the 19th century (Tanck, 2005), and possibly still did—pueblos had been legally eliminated in the 1830s and the legislative order to disentail collective property was issued in the 1850s, but it had remained largely ineffective before the Porfiriato, among other things because of the war against France (see Knight, 1986; Katz, 2004; Kouri, 2002; Kouri manuscript; Melo, 2006; Camacho, 2006, among many others). Hence, the only way to obtain those lands was to seize them from indigenous or peasant communities by implementing the disentailment laws in abusive or fraudulent ways at the state level—indeed, the laws required that collective lands be equally divided among community members, but there is an early and extensive historiographical literature indicating that neighboring hacienda owners and/or indigenous leaders, in conjunction with local politicians, influenced the processes so as to force indigenous beneficiaries to cede or sell the parts that belonged to them, in a way that land was very quickly concentrated in a few hands (for the classics, see Orozco; 1895; Molina Enríquez, 1909; McBride, 1928; Tannenbaum, 1930). At the same time that disentailment laws were being applied, the Mexican federal government attempted to privatize waste lands through concessions to land survey companies in charge of measuring and privatizing the lands. However, the places where waste lands were privatized the most (notably the North of Mexico, but also Chiapas) do not coincide with those where export agricultural products were mostly being produced. Furthermore, as Holden (1994) showed, the privatization of waste lands did not significantly affect indigenous/peasant communities, since survey companies generally respected those communities' preexistent legal holdings. Finally, in Mexico, tenancy and working conditions in Haciendas seem to have become tougher with the export-boom, with Hacendados being less willing to lease lands to autonomous peasants, wishing them instead to work for them as peons, and with labor conditions becoming more dependent through the use of mechanisms like debt-peonage (Knight, 1986; Katz, 2004). However, these changes seem to have been strongly related to land disposessions, since many argue that the latter were in part used to assure labor in Haciendas, and that labor repression was used to hinder peasants from claiming lost lands (Womack, 1978; Knight, 1986; Hernández, 1993).

In Colombia, the export-boom appears to have started in the 1890s and peaked in the 1920s, and to have consisted mainly in coffee production, but also in other agricultural products such as banana and cocoa, as well as some mines (coal and gold). However, in contrast with Mexico, most of the land disposessions appear to have attempted against colonization areas where occupants were either mere de facto possessors or in the process of obtaining legal entitlements by claiming that they had occupied waste lands for more than five years (Le Grand, 1986; Berry, 2002; Kalmanovitz and López, 2005; Melo, 2007; Ocampo et al, 2007; Sánchez et al, 2010) —the most common complaints of peasants seem to be those of colonos dispossessed before the term for claiming ownership, or during the process of claiming such ownership (Saffon and Sánchez, work in progress). Neighboring landowners and companies, as well as speculators, appear to have waited until lands were colonized and made productive, but before legal entitlements were granted, to push possessors out and later claim legal entitlements in their place. There seems to exist a strong correlation between land disposessions of this sort and land allocations through the 1960s land reform, as well as between waste lands and the latter. In contrast, Colombian Indians do not seem to have been as affected by dispossession. Indigenous communities were not so numerous—much less so than in Mexico—but they owned collective lands (in the form of resguardos). However, there does not seem to exist any correlation between the historical presence of indigenous encomiendas or resguardos and complaints of land dispossession, nor between the former and land reform grants (Saffon and Sánchez, work in progress). On the other hand, conflicts between landowners and tenants seem to have also predominated in Colombia during the export boom (Palacios, 2010), with tenants claiming the ownership of the land on which they worked or on which they had made improvements, or at least the recognition of the value of such improvements, and with landowners attempting to kick tenants out to avoid those claims and resist their occupation. However, there seems to exist a quite weak correlation between these types of conflicts and land reform allocations (Saffon and Sánchez, work in progress), which suggests both that those conflicts
were not as important during the export boom as those between colonos and landowners, and that those conflicts did not engender as strong claims for land redistribution.

Finally, in Argentina, the export-boom ranged between the 1880s and 1920s. The export boom led to a significant demand for lamb and wool in its earlier stages, and then to a rather exuberant one of beef and grain. The boom, along with the rapid development of railroads that accompanied and promoted it, certainly generated strong incentives to appropriate lands, as well as steep increases in their values. However, until 1914, the agrarian frontier remained open in the regions where export-production was most intensive—the provinces comprised by the littoral and the humid pampas, notably Buenos Aires and La Pampa. Consequently, the lands that were appropriated were frequently empty lands that were either bought or rented through long-leases to the state (Gelman and Barsky, 2001; Hora, 2002; Palacio, 2002; Girbal, 2011). However, this had important exceptions. To begin with, the renowned conquest of the desert in which the lands to the south of the Buenos Aires province that had been occupied by mostly nomadic indigenous groups were seized by large landowners (estancieros) mainly through the assassination, detention and marginalization of those groups by state forces. Also, in areas where the occupation of waste lands was carried out by small peasants without the support of the state, such as Entre Ríos, large landowners seem to have behaved in similar ways than they did in Colombia, by dispossessing occupied lands and profiting from their uncertain ownership rights (Schmit, 1999). The fact that situations of this kind occurred suggests that land dispossession were more likely to happen in Argentina whenever the agrarian frontier was exhausted (which happened at different times in different regions), and wherever property rights were not clearly delineated (which seems to have been more uncommon in the province of Buenos Aires). Furthermore, with the exhaustion of the agrarian frontier, landowners in the Pampas seem to have begun to impose more stringent conditions to their tenants—charging higher rents, requiring them to use particular intermediaries for their sales, etc.—in order to exploit lands in a more efficient way (Palacios, 2001). Although it is not clear how widespread this phenomenon was, it is likely that it occurred in other regions, where tenants' grievances were politically voiced at the height of the export boom (the clearest example being the Grito de Alcorta in Santafé). It is likely that tenants' grievances led to claims for the improvement of tenant conditions, which were later addressed by tenancy laws.

Where they were numerous, as in Mexico, indigenous/peasant groups seem to have disproportionately settled on productive lands. One could think, for this reason, that their grievances could be traced back not merely to land disposessions and to their prior legal entitlements, but also to the quality of their lands. Since earlier occupation of lands was not random, this could be problematic as compared to countries where it was indeed random. As proposed below, a subnational study of Mexico can control for this by comparing states with equally productive lands but with different commercial uses—some more prone to export agriculture, and others to domestic production of staples like corn.

For data on privatization of waste lands in Colombia see Sánchez et al (2010); for Mexico see Holden (1994).

If pertinent, I might also use indicator f to analyze unsuccessful legislative land reform attempts, especially in Argentina, to see if the level of radicalism of such attempts can be in some way explained by the predicted independent variable and the intervening processes. I will do a similar thing with Argentine tenancy laws.

Indeed, I am gathering, for indicators a and b, the national legislation and the interpretations of it offered by national authorities (I already have all the Mexican legislation and interpretations, and the Colombian legislation); for indicator c, the administrative complaints or requests related to land that reached national authorities (which can be found in the National Archive of each country and which I already have for Colombia and Mexico—for Colombia, the database constructed by Sánchez et al, 2010 of all the complaints concerning lands found in the Colombian Archivo General de la Nación, and for Mexico the database of all requests of copies of land titles made to the Archivo General de la Nación); for indicator c also the judicial cases concerning land issues that were decided by the Supreme Court of Justice of each country (which can likely be found in the courts’ archives, and which I already have for Mexico and Argentina); for indicator d, the peasant riots and rebellions reported by national authorities and/or quantified and analyzed by country experts (I already have those quantified by experts in Mexico); for indicator e, all legislative initiatives on land reform (which I have already gathered for the Mexican case), and all available documentation on parties’ and unions’ agendas at the national level (I already have a lot on Mexico); for indicator f, all enacted land reform programs and tenancy laws at the national level (which I already have for Mexico, Colombia and Argentina); and for indicator g, aggregate data on total number, timing and location of land allocations based on land reform (which I already have for Mexico and Colombia), as well as all administrative decisions granting or refusing a land allocation made by a national authority during the first twenty years after the land reform (which I already have for the Mexican case for the first five years, as they appeared in the Diario Oficial de la Nación, as well as for Colombia’s
second land reform of 1961). Even though the sources for indicators b and c will probably leave out many forms of voiced peasant resistance at the local level that did not reach the level of national administrative and judicial authorities, they will have the benefit of allowing me to construct a largely unbiased sample of the whole country, and also of giving a picture of the types and patterns of conflict regionally.

48 I have not yet made a final selection of the subnational cases for the other two countries. However, following similar criteria, for Colombia, those regions will probably be: Cauca and Putumayo (South); Antioquia and Tolima (Center); Urabá and Bolívar (North). For Argentina, given that during my period of interest the south of the country was not yet fully incorporated politically or economically, I will only choose four cases, each pair corresponding to the main two regions: the North (where there was a higher level of indigenous peoples with collective property at some point in time, as well as an earlier exhaustion of the agricultural frontier and a higher population density), and the region of the Litoral and the Pampas (with lower indigenous peoples who, moreover, were not collective landowners, and with a later exhaustion of the agrarian frontier, a lower population density, and a consequent higher ratio of land to people). Within each region, I will choose two cases varying in their degree of participation in the export market. The most likely candidates are: for the North, Tucumán (which suffered a strong impact of the export market via increasing demands for sugar for internal consumption, which were strengthened by the railroad, and which made the province turn into an almost single-crop farming region) and Jujuy (with a less direct participation in the export market and a more varied agricultural production). For the Litoral and Pampas, the candidates are: the Buenos Aires province (which suffered the greatest and most direct impact of the export boom through an intensive demand for beef and grains, which generated strong incentives for conquering the rest of the agrarian frontier, including combatting and submitting the desert’s Indians), and Entre Ríos (which did not participate so directly in the export economy, but also foresaw an expansion of the agrarian frontier that, however, took a different shape than the Buenos Aires province one because it involved large landowners dispossessing smaller first settlers).

49 The selection of these regions is also useful because each of them seems to have different forms of agricultural exploitation and of local autonomy (prevalence of Haciendas; coexistence of Haciendas and autonomous peasant villages; coexistence of Haciendas and autonomous peasant villages, but predominance of the latter, respectively), as well as different types of labor relations (more free labor; combination of Hacienda peonage and free labor; some servitude). This allows controlling for those factors too.

50 For comparative studies see Albertus (manuscript); Ondetti (manuscript); for Mexico, see Tennenbaum (1930); Walsh Sanderson (1984); for Colombia Saffon and Sánchez (work in progress).