

The impact of land property rights interventions on investment and agricultural productivity in developing countries: a systematic review

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Abstract

We conducted a systematic review on the effects of land tenure recognition interventions on agricultural productivity, income, investment and other relevant outcomes. We synthesise findings from 20 quantitative studies and nine qualitative studies that passed a methodological screening. The results indicate substantial productivity and income gains from land tenure recognition, although gains differ markedly by region. We find that these effects may operate through gains in perceived tenure security and investment; we find no evidence for a credit mechanism. The qualitative synthesis highlights potential adverse effects. A conclusion emphasises the need for further research on interregional differences and on the role of customary tenure arrangements.

1. Introduction

Secure access to land as a productive resource is key to the livelihoods of farmers around the world. Secure land rights enable farmers to work and invest in their farms with the expectation that they will reap the benefits without fear that their land may be confiscated arbitrarily. Formal and informal land rights are therefore seen as key to improving the conditions of the poor in developing countries in terms of economic growth, agricultural production, food security, natural resource management, gender-related inequalities, conflict management and local governance processes more generally (Bruce 2012; de Soto 2000; Deininger 2003; Feder and Feeny 1991).

Leading multilateral and bilateral development agencies accord high priority to reforms that strengthen tenure security, especially in strategies to reduce poverty among women and other traditionally disadvantaged members of society. According to a 2003 World Bank study,

Providing secure tenure to land can improve the welfare of the poor, in particular, by enhancing the asset base of those, such as women, whose land rights are often neglected. At the same time, it creates the incentive needed for investment, a key element underlying sustainable growth. (Deininger 2003, ix) Other agencies, including USAID and

FAO, have put tenure security at the centre of their funding strategies (see especially USAID and MCC (2012) and FAO (2011)).

Land rights may include a wide range of rights to use, own and/or transfer land, as well as enforce rules and exclude outsiders. Strengthening of land rights can take a variety of forms, from documenting customary uses to formalising individual rights. Some forms may engage directly with the rights holder, for example through farm-by-farm land titling. Other forms of strengthening rights may be enacted at the national level, for example constitutional reforms to recognise customary land rights. National-scale or even community-level interventions that seek to strengthen rights may have differing impacts within populations, for example many interventions seeking to improve rights may lead to elite capture of benefits and subsequent loss of rights for poor and vulnerable subpopulations, particularly in the absence of safeguards (Besteman 1990). The socially embedded nature of customary rights means the land rights of many women depend on social entitlements that can be eroded due to reforms that make land rights marketable, resulting in a de facto transfer of a greater share of rights to (typically) male title holders. Observed impacts of tenure reforms likely vary across individuals, communities, regions and countries (Place and Swallow 2000).

Conversion of communal or nondemarcated rural land to freehold title and registration of such rights in an official registry has become a standard approach, under the presumption that communal land tenure rights are inherently insecure. Such conversion typically consists of adjudicating and assigning land rights, physically surveying boundaries, and registering rights and boundary demarcations in an official land registry. Conversion of customary tenure systems to a system based on registration of individual parcels has resulted in concerns about the high costs of title adjudication and registration.

A variety of factors are likely to influence the effectiveness of land property rights interventions on productivity. Figure 1 presents the basic elements of a theory of change (causal chain) that draws on the theoretical literature (Atwood 1990; Bruce 2012; de Soto 2000; Deininger 2003; Feder and Feeny 1991; Place 2009). The figure sketches out moderating factors, mechanisms of change and intermediate outcomes, and final outcomes that, prior to carrying out our review, we saw as being important in understanding the effects of land property rights on productivity. The final outcomes of interest include the following:

- Productivity of land use;
- Welfare of pre-policy landholders, measured in terms of income and consumption;
- Domestic violence and gender equity and
- Welfare of post-policy landholders, measured as noted above.

In theory, it is important to distinguish between the welfare of pre-policy and post-policy landholders in evaluating the welfare impacts of these interventions. To the extent that

these groups differ, one should consider the potential for adverse consequences for pre-policy landholders.

Various moderating factors may be important in determining the nature of the effects that are likely to follow land property rights interventions:

- Governance, including the nature of interests represented by those controlling policy;
- Social norms and practices, specifically ways in which gender, age, community standing and other characteristics influence the other three moderating factors and individuals' ability interact with interventions in a particular social context;
- Land use, including population pressure on land, whether land is subject to mixed use (pastures and forests), and whether cash crops are grown and
- Markets, including the presence of credit markets and market demand for crops as well as demand for agricultural land, resulting from both local and international factors.

Interventions may operate through a number of intermediate drivers of change, including the following:

- Recognition of alienation right by those holding registered rights. This is presumed by conventional economic theory to provide collateral and, therefore, enable access to credit.

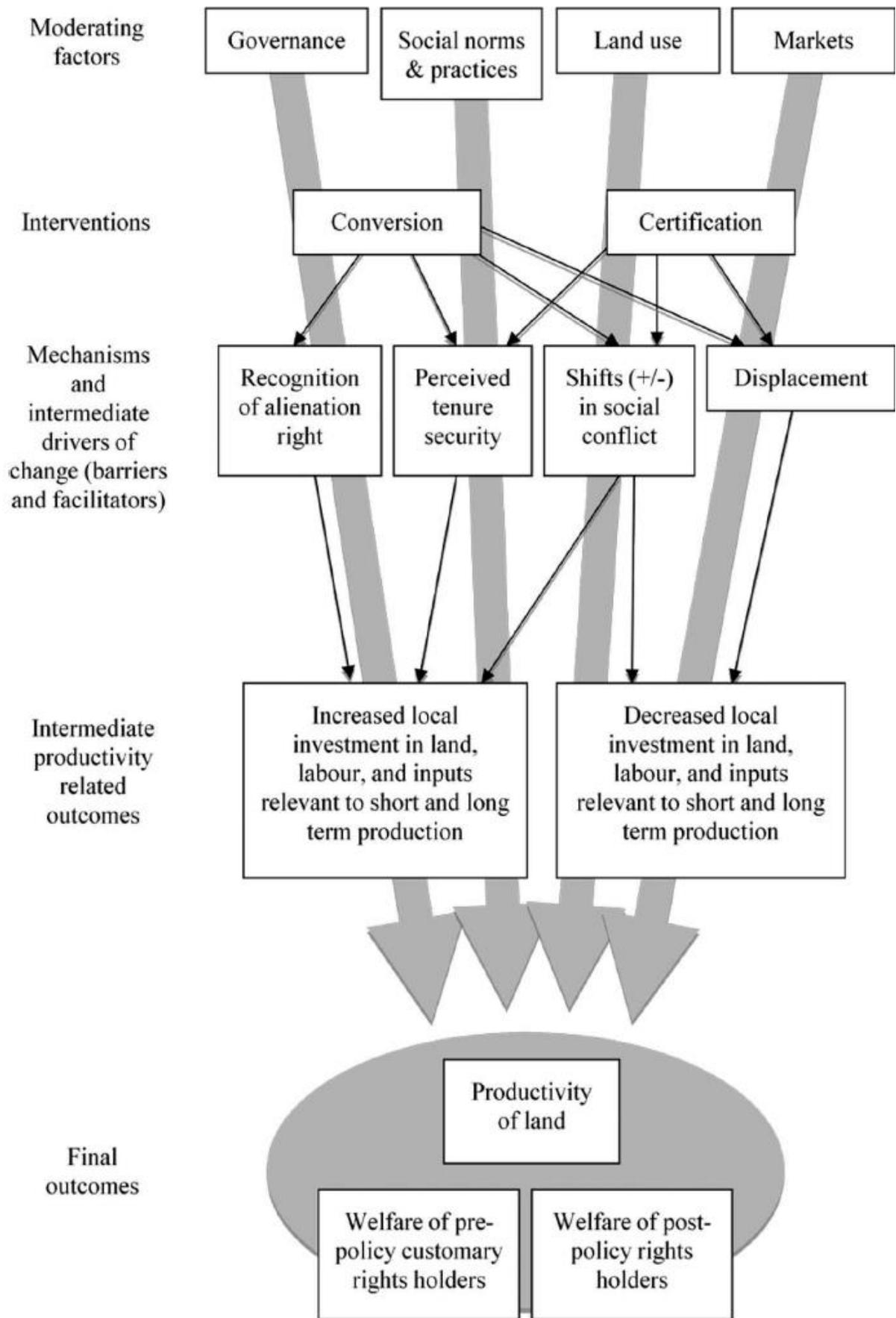


Figure 1. Theory of change path diagram.

- Perceptions of tenure security, which is presumed by conventional economic theory to motivate investment of personal resources into production.
- Social conflict, including reducing amounts of land held in dispute and therefore not being used productively due to inheritance disputes, boundary demarcation disputes, or land use conflicts between, for instance, pastoralists and agriculturalists. At the same time, to the extent that such registration changes who has access to land, these interventions may indeed trigger violence or other forms of contention over these changes.
- Displacement of tenants whose rights to land are denied as a result of the intervention.

Intermediate outcomes of an intervention include shifts in land, labour and agricultural inputs relevant to both short- and longer-term production. More specifically, these would include changes in the following:

- Investments of resources into short-term production and land (fertiliser, pesticides and so on)
- Investment of resources into longer-term production and land (for example, soil conservation, tree crops and so on) and
- Fuller employment of land through leasing-out or sharecropping.

Existing evidence on the effects of land property rights interventions is mixed and to a considerable degree dependent upon the initial land rights conditions. In many cases where existing rights are already secure through stable informal and customary systems, the formalisation of rights through land titling, one form of strengthening rights, may have little impact (Pickney and Kimuyu 1994; Atwood 1990). In other cases, as in the Brazilian Amazonian frontier in the early 1990s, mechanisms for formalising property rights where no formal institutions had previously existed are argued to have increased productivity and slowed forest loss (Alston, Libecap, and Schneider 1996). Alternatively, if strengthening land rights simply results in formalising a bundle of overlapping rights customarily distributed through a community into private property, this 'strengthening' could lead to the exclusion and marginalisation of large sections of the community, including the poor, as is argued to have occurred alongside Kenyan tenure reform (Meinzen-Dick and Mwangi 2008). Thus, it is important to understand to what extent the strengthening of rights in any context leads to new institutional realities and who bears the costs and benefits of changes in how land rights are assigned (Fort 2008; Bellemare 2013).

The inconsistent conclusions from studies on the relationship between strengthening land rights and productivity have led scholars and policy-makers in recent years to try to understand how differing theoretical assumptions and different approaches to the empirical study of tenure may explain these differences (Brasselle, Gaspard, and Platteau 2002). A literature review published by Dickerman and Barnes (1989) on efforts to formalise and register customary land rights in Africa found that formalisation had

significant positive effects on investment and agricultural productivity in only a small number of particularistic contexts where customary systems had broken down or were absent. Rarely did the benefits associated with surveying land parcels, adjudicating and assigning rights and maintaining official registers outweigh the costs. The authors suggested that registration in many settings had deleterious effects on the poor and on women farmers, particularly where women were not listed as joint title-holders.

Much of the literature underscores the complexity of attribution and the importance of context (Place 2009) to understanding relationships between security, registration and productivity, and to understanding gender dimensions. They also suggest that tenure security alone is not the single factor 'silver bullet' leading directly to higher farmer incomes attributed to tenure reforms by writers such as de Soto (2000). Context matters, including whether markets and credit institutions are in place and input and other costs are at levels conducive to competitive pricing of agricultural products (Bruce 2012). Relevant questions have recently been raised about the extent to which much of the available empirical research on the effects of tenure security has a handle on tenure security as a concept (Arnot, Lukert, and Boxall 2011).

Despite the importance of these debates, we are unaware of any systematic review or meta-analyses on the relationships between land property rights and productivity or welfare. In addition, Fenske (2010) highlights study design limitations in many of the studies that have not found significant impacts of tenure security. The concerns about inconsistent effects and design limitations provided a strong motivation for this systematic review.

2. Methods

We use a systematic review methodology that follows the guidelines of Campbell Collaboration (2011) and Waddington et al. (2014). For a detailed explanation of our methodology, readers may consult the full systematic review report (Lawry et al. 2014).¹ Here, we summarise the key elements of the search strategy and methods for synthesis.

2.1 Criteria for considering studies for this review

We assessed the eligibility of a study in terms of the population investigated, intervention types, the types of comparisons undertaken, the outcomes variables and the research design.² As for eligible populations, we included studies on smallholders and communities in rural farming systems in low- and middle-income countries. For interventions, we included studies examining the interventions that recognised individual tenure rights for holders of communal or nondemarcated rural land.³ We accepted studies that compared farmers and communities where, formal activities to strengthen land rights have been implemented, to control groups where these efforts have not been undertaken. We accepted studies that estimated effects on either productivity of land or income/consumption of land tenants (our 'final' outcomes of interest) or on access to credit, perceptions of tenure security, social conflict,

displacement or investments ('intermediate' outcomes of interest). We used separate criteria to determine eligibility for quantitative versus qualitative synthesis. For the quantitative synthesis, we accepted studies that used either a randomised experiment or quasi-experimental design. For the qualitative synthesis, we accepted studies with clearly defined research objectives and links to relevant literature, and that provided details on context, sample selection and data collection methods (Kuper, Lingard, and Levinson 2008; Spencer et al. 2003; Waddington et al. 2010).

2.2 Search methods for identification of studies

We searched all prominent academic literature databases, journals or working paper series that cover economic development.⁴ We also searched grey literature on websites of leading institutions working on tenure rights. We also consulted bibliographies and contacted land tenure researchers to obtain references to work that did not come up from our database searches.

2.3 Data collection

For studies that met the inclusion criteria, we collected publication information as well as information on the interventions, study design, estimated effects on 'final' and 'intermediate' outcomes and moderator data. The quantitative studies were assessed in terms of 'risk of bias' using the Campbell Collaboration IDCG Risk of Bias Tool (March 2012 version). The risks that we assessed include potential for selection bias due to nonrandom assignment to intervention conditions, potential for spill-over or unmeasured differences in the nature of treatments, selected outcome or analysis reporting, and potential for noncompliance, attrition or other missing data to bias the analysis.

We used measures of treatment effects that are conventional in the development economics literature. For effects on value of productive output or consumption, we report treatment effects on the scale of the natural logarithm. For other continuous outcome measures, we use a standardised difference that standardises the outcome relative to the control group standard deviation ('Glass's delta'), which specifies the treatment effect in terms of the no-treatment regime 'counter-factual' outcome distribution (Kling, Liebman, and Katz 2007). For binary outcomes, we report treatment effects in terms of risk differences (cf. Angrist and Pischke 2009, chap. 3).⁵ When a study included multiple estimates of the same treatment effect, we used the one judged to have minimal risk of bias.

Some of our cases are statistically independent because they estimate effects for the same intervention over the same time period (within one year) in the same location, albeit with different analysis samples. This applies to studies that we include from Ethiopia 1998–2006, Ethiopia 2003–2006/7, Nicaragua 1981–1998, Peru 1992/3–2004 and Vietnam 1993–2004/6. We use a hierarchical approach where, in the first step, we aggregate estimates from such sets of overlapping studies into single inverse-variance weighted

random effects mean. Then, in the second step, we perform the meta-analysis across cases using these synthesised estimates along with the estimates from the nonoverlapping cases in the quantitative analysis.

2.4. Data synthesis

We carried out quantitative synthesis for sets of comparably measured outcomes: productivity of land, consumption/income, probability of formal borrowing, amount of formal credit obtained, probability of perceiving land may be expropriated and investment in long-term production. Given high inter-study heterogeneity in effect sizes, we used random effects synthesis and random effects meta-regression on moderator variables.⁶ We assessed publication bias via funnel plots and funnel plot regression (Egger et al. 1997).

We carried out a very limited moderator analysis for the productivity and investments effects. Based on our theory of change, we studied the following moderator variables: Polity IV democracy scores (from 0 to 10, least to most democratic, respectively) as a measure of governance conditions,⁷ indicator variables for the regions of the world (Latin America, Middle East/North Africa, South Asia, Southeast Asia/Oceania or sub-Saharan Africa) as measures of norms and institutional differences, GDP per capita as a measure of market conditions,⁸ rural population density as a measure of intensity of land use (taken from the World Bank Development Indicators database), an indicator for cash crop farming determined on the basis of information given in each study and an indicator for mixed land use determined on the basis of information given in the study. We also study how years since intervention moderates productivity and investment effects. Given the small number of studies, we only performed bivariate meta-regressions.

Our qualitative synthesis used the qualitative ‘metasummary’ methodology of Sandelowski, Barroso, and Voils (2007). Metasummaries involve a process of extracting findings, grouping them into themes and then establishing the frequency and intensity of findings corresponding to the themes. The results are put into a matrix to convey the themes and their frequency and intensity.

3. Results

We identified 20 quantitative studies and nine qualitative studies that met our substantive and methodological inclusion criteria. The studies document a wide range of tenure interventions. At the most individualised end of the spectrum is the allocation of formal titles to individual land-holders, through registration on a national cadastre (Foltz, Larson, and Lopez 2000; on Nicaragua, Ali, Deininger, and Goldstein 2011; on Rwanda, Do and Iyer 2007; on Vietnam). The studies also depict varying systems of recording, registering and mapping individual and household claims to land (Fort 2008; on Peru, Deininger et al. 2007; on India). At the more informal end of the spectrum, they address mechanisms for recording household claims through local institutions (Holden,

Deininger, and Ghebru 2009; Deininger, Ali, and Alemu 2009; Deininger, Ali, and Alemu 2011). Table 1 summarises reasons for exclusion at the full text screening stage.

Table 1. Reasons for study exclusion after full text analysis.

Experimental and quasi-experimental studies: reasons for rejection (not exclusive)	Number	Percent
Did not study a clearly defined and relevant tenure intervention or policy change	43	81
Did not assess an outcome of interest	5	6
Did not assess outcomes at appropriate level of analysis	2	2
Did not examine a developing country	0	0
Did not provide adequate methodological information	4	6
Did not use acceptable experimental or quasi-experimental method	11	9
Non-experimental and qualitative studies: reasons for rejection (not exclusive)	Number	Per cent
Did not study a clearly defined and relevant tenure intervention or policy change	52	22
Did not assess an outcome of interest	53	22
Did not offer qualitative analysis	72	30
Did not examine a developing country	0	0
Did not provide post-intervention analysis	5	2
Was not based on primary research	35	15
Did not assess outcomes at appropriate level of analysis	21	9

Figure 2 displays funnel plots of productivity and long-term investment effect estimates. The funnel plot regression test fails to reject the null of no publication bias; however, the presence of the two, large, positive effects with large standard errors (bottom right of the productivity graph) is quite typical for situations where low-powered tests are screened for statistically significant positive effects (Gelman and Weakliem 2009). The fact that so many estimates reside outside the confidence region (the white triangles) on each plot is indicative of the high degree of heterogeneity.

Figure 3 displays results from our ‘risk of bias’ assessment based on the Campbell Collaboration IDCG Risk of Bias Tool. A weakness of many studies is that they provide little clarity on why certain households or land parcels received tenure recognition while others did not and therefore leave as unclear the extent of selection bias that may be present (the top left histogram in Figure 3). Only a handful of studies provided clarity on this point: Ali, Deininger, and Goldstein (2011) discussed how the programme that they were evaluating was a geographically limited pilot by the Rwandan government, while Do and Iyer (2007) explained that the variation in access to tenure recognition was due to phased province-level implementation of tenure recognition policies in Vietnam. For other studies, concern about selection biases remains. Quite likely, unmeasured factors that determine households’ or producers’ expected gains from tenure recognition continue to confound the analysis.

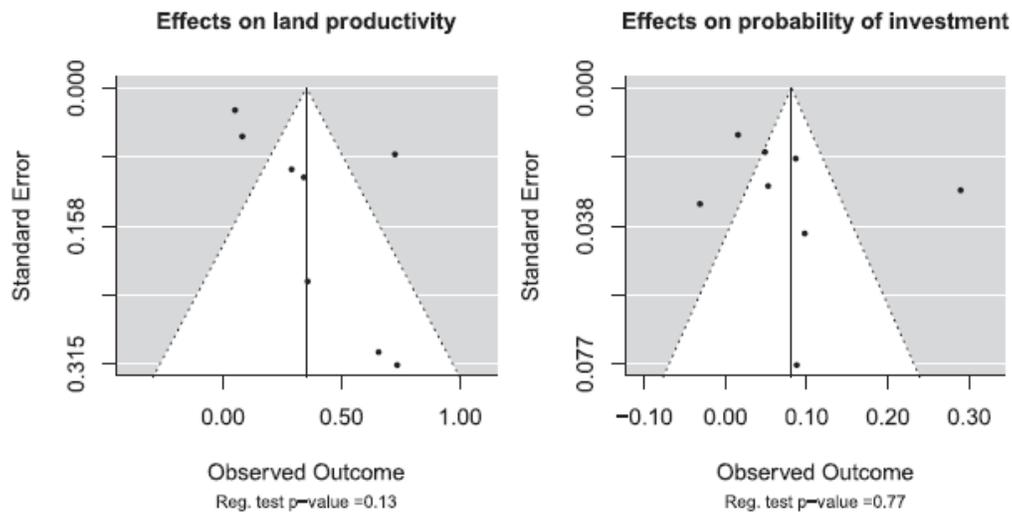


Figure 2. Funnel plots of productivity and long-term investment effect estimates. The white triangular regions in each plot show the 95 per cent confidence region under the assumption that all studies are measuring the same underlying treatment effect.

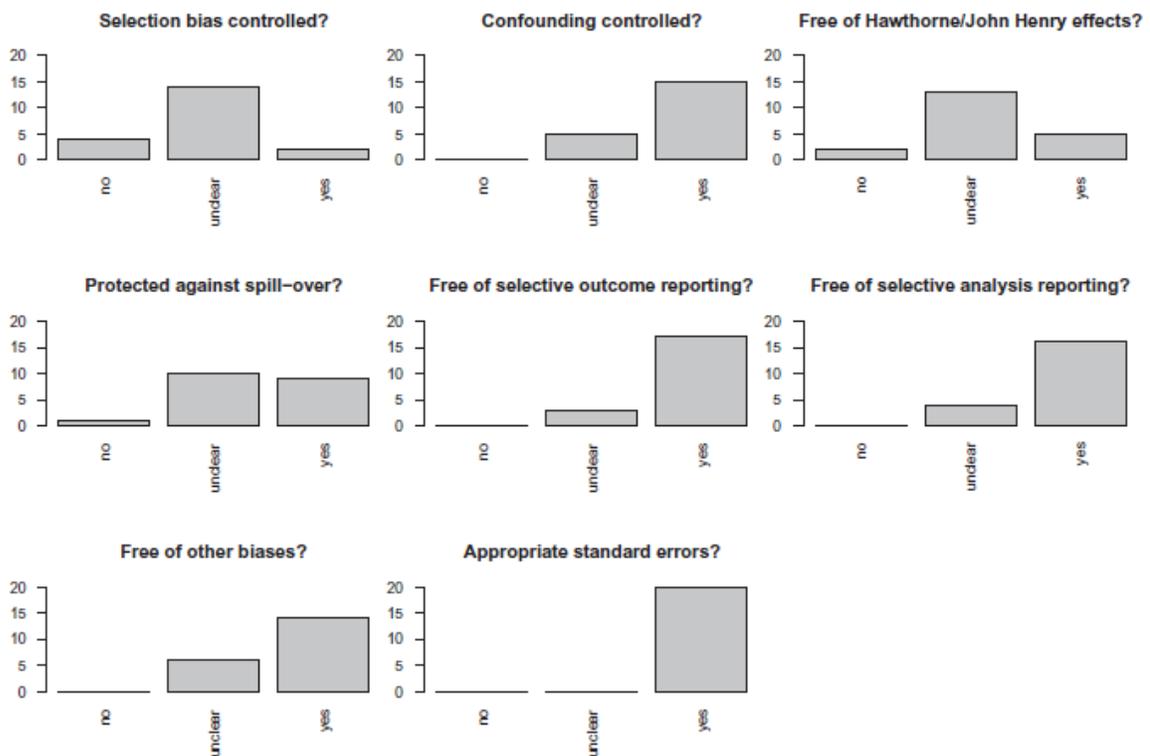


Figure 3. Distribution of studies according to risk of bias criteria.

If so, then the positive effects suggested by the studies would overstate the real impact. The quantitative studies did little to dispel concerns about spillover effects, which could result in estimates that, again, overstate the gains from extending tenure recognition to more households.

As for the qualitative studies, in all cases respondents for individual interviews were randomly selected, although in some instances communities or groups within communities were selected specifically on the basis of their status as members of a vulnerable group (cf. Chilundo et al. 2005).

3.1. *Synthesis of quantitative evidence*

The quantitative evidence is mostly consistent with conventional economic theories of property rights. The evidence indicates clear benefits of formal tenure recognition measured in terms of productivity and consumption expenditure or income. The evidence suggests that increases in long-term investment and perceived tenure security are plausible channels through which tenure recognition may contribute to welfare. The credit channel finds no support, although the evidence base is very thin. Gains in productivity are significantly greater outside Africa and in wealthier settings, although the strong correlation between the two makes it impossible for us to determine whether this is a ‘wealth effect’ or an effect associated with regional differences in informal tenure conditions or infrastructure conditions. The quantitative evidence base provides little insight on consequences of formal tenure for social outcomes like displacement, conflict or gender equality (although the qualitative evidence is richer on these issues, as we discuss later). Also, we do not have quantitative evidence on whether such policies have implications for who has access to land and can actually benefit, and thus we cannot speak to the distributional effects of formal tenure recognition.

Table A.1 in the Appendix and Figure 4 show characteristics of the cases that our 20 quantitative studies covered. The studies cover rural areas in lower-income and lower-middle-income countries across Latin America, South Asia, East Asia and Africa. Population density is an indicator of stresses on land; the cases included in the quantitative synthesis are diverse on this score as visible in Figure 4. Representative institutions are presumed, in our theory of change, to help ensure that rural landholders benefit from tenure reform; again, the cases included in the synthesis are diverse with respect to the Polity measure of democratic governance. Income is an indicator for the size of markets as well as the quality of institutions. By construction, the study is limited to lower-income and lower-middle-income countries. But taking this into consideration, the cases are still quite diverse in their income levels.

effect estimates available from our set of studies (that is, effects on formal credit and perceptions of land expropriation), we simply plot the effect estimates, as these cases provide too little information to reliably characterise a treatment effect distribution.

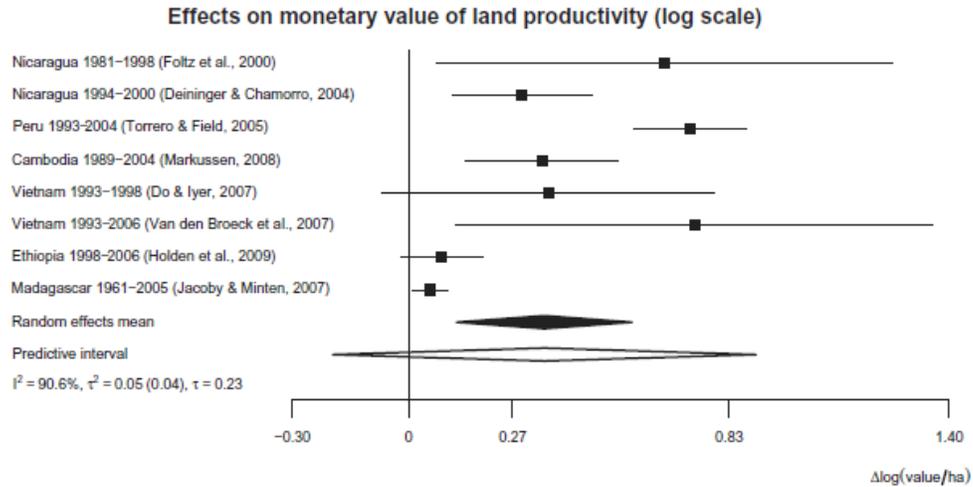


Figure 5. The forest plot shows estimates of the effect of de jure recognition of tenure on the monetary value of land productivity (log scale). Moves to the right on the x-axis indicate beneficial effects.

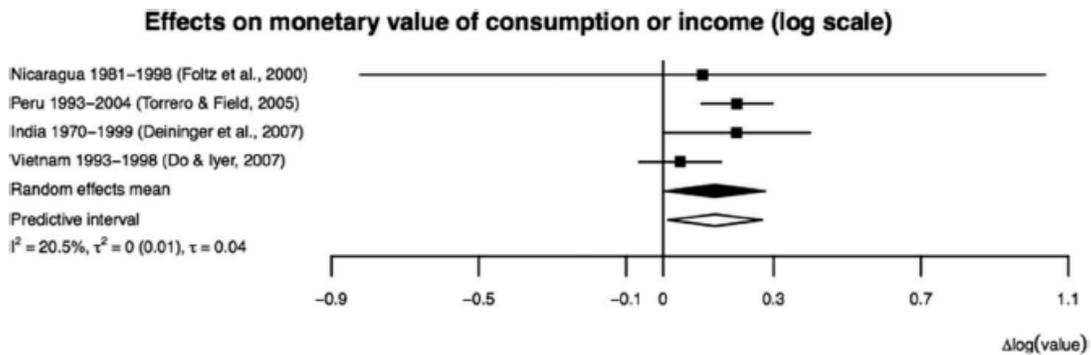


Figure 6. The forest plot shows estimates of the effect of de jure recognition of tenure on the monetary value of consumption or income (log scale). Moves to the right on the x-axis indicate beneficial effects.

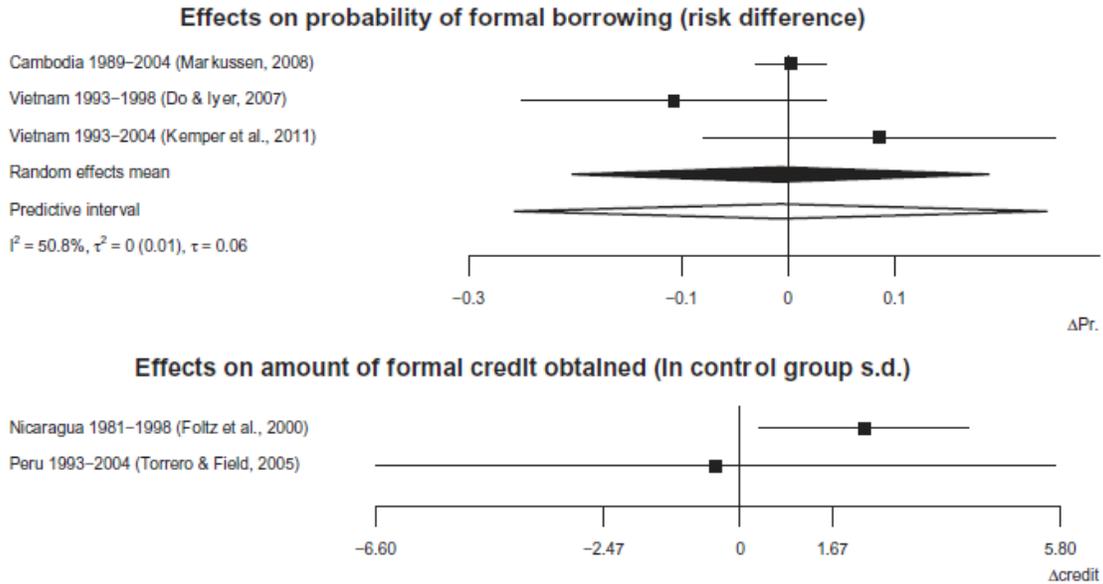


Figure 7. The two forest plots show estimates of the effect of de jure recognition of tenure on the probability of formal borrowing (top, risk difference scale) and the amount of credit received (bottom, measured in terms of control group standard deviations). Moves to the right on the x-axis indicate beneficial effects.

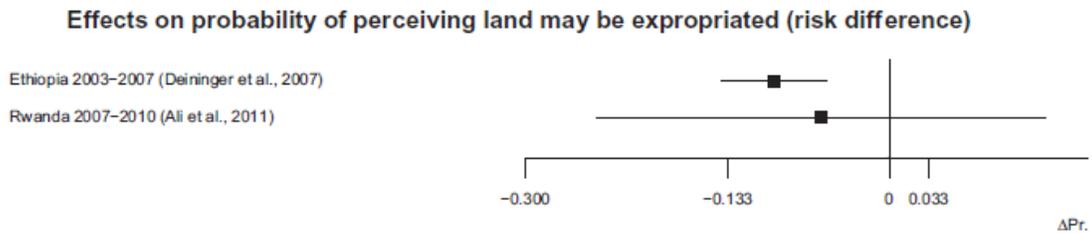


Figure 8. The forest plot shows estimates of the effect of de jure recognition of tenure on the farmers' perceptions that their land may be expropriated in the near future. Moves to the left on the x-axis indicate beneficial effects (a reduction in the perceived risk of expropriation).

Effects on probability of investment (risk difference)

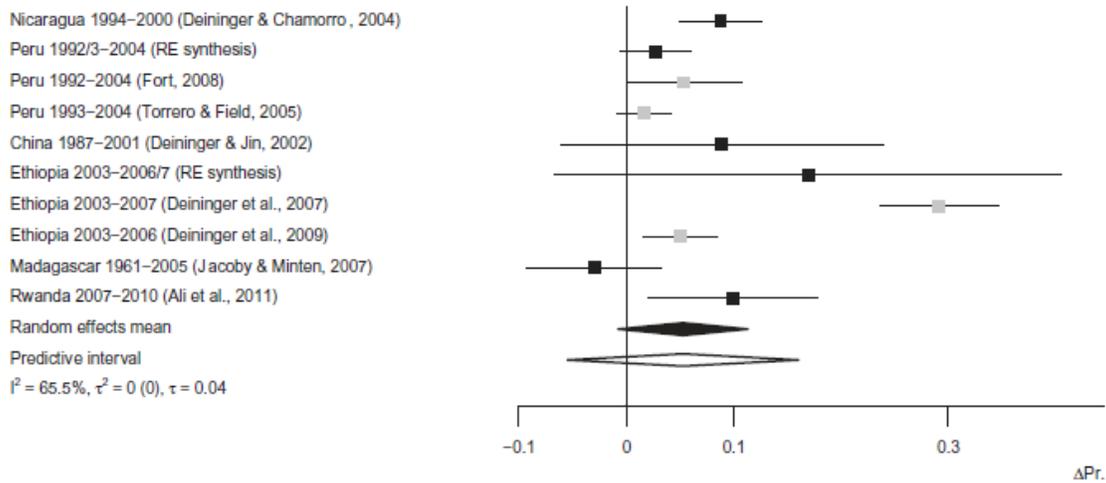


Figure 9. The forest plot shows estimates of the effect of de jure recognition of tenure on the probability of long-term investment (risk difference scale). Moves to the right on the x-axis indicate beneficial effects. The effect estimates that appear as grey squares are for sets of overlapping cases (for Peru 1992/3–2004 and Ethiopia 2003–2006/7) where the same intervention is evaluated in the same context but with different samples. The effects were first synthesised into a single, random-effects case-specific effect estimate (denoted as ‘RE synthesis’ in the labels to the left), and then these synthesised estimates were used to produce the random effects mean, predictive interval and I² heterogeneity measurement.

For ‘final’ outcomes, the evidence suggests substantially beneficial effects on average from de jure recognition of tenure. The available evidence suggests that de jure recognition of tenure boosts productivity (Figure 5), as measured in terms of the monetary value of land productivity, by around 40 per cent on average (*random effects mean* = 0.35, *s.e.* = 0.10, *exp(mean)* = 1.42). This is a substantively huge effect, although this estimate masks substantial heterogeneity between Latin American and Asia where the measured productivity effects were strong and sub-Saharan Africa where they were positive but much weaker. Figure 6 shows that the average effect on welfare, as measured by consumption or income, is about a 15 per cent increase (*random effects mean* = 0.14, *s.e.* = 0.04, *exp(mean)* = 1.15). In this case, the level of heterogeneity is quite low.

As far as mechanisms go, the available evidence provides little to indicate an operative causal pathway via the credit access effects, although there is some evidence to suggest an active pathway through tenure security and investment effects. Figure 7 shows that there exists no consistent evidence to indicate either a positive or negative effect of de jure recognition of tenure on either the probability of formal borrowing or the amount of formal credit obtained. On the other hand, the two studies that did assess perceptions of tenure security each found the de jure recognition reduced the probability that a farmer respondent believed that his/her land would be subject to expropriation in the near future (Figure 8; in this case, the negative estimates are indicative of a beneficial effect). The evidence for the long-term investment channel is displayed in Figure 9. On average, we find that de jure recognition of tenure increases the probability of long-term investment by about 5 percentage points (*random effects mean* = 0.05, *s.e.* = 0.02). None of these

mechanism effects appear to be strong enough to thoroughly explain the pronounced productivity or welfare effects.

Some of the outcomes listed in Table A.1 are not plotted in Figures 5–9. This is because those effects were defined or measured in ways that do not allow for direct comparisons. For example, as part of investment behaviour, three studies estimated effects on tree crop planting, an indicator of long-term productive investments, but they used very different measures. These papers were Bandiera (2007, Nicaragua), Do and Iyer (2007 Vietnam) and Holden, Deininger, and Ghebru (2009, Ethiopia). All three studies found significant increases in such investment, corroborating the positive investment effects shown in Figure 9. Two studies evaluate leasing out of land, another way that tenure recognition may boost land productivity. Deininger, Ali, and Alemu (2011, Ethiopia) find that households with land use certificates are significantly more likely (ca. 11 per cent) to lease out land. Kung (2006, China) finds a modestly positive association between a measure of land entitlement and likelihood of leasing out, although the effect is statistically insignificant. With respect to social conflict, only Markussen (2008, Cambodia) attempts to assess it, but in his case in Cambodia, reported incidence of conflict is too rare (ca. 1% of cases) to allow for reliable estimation of tenure security effects. With respect to effects on gender equality, Ali, Deininger, and Goldstein (2011, Rwanda) find no clear indication that recognition of ownership by women or share of land owned by women is increased, although for married women with a marriage certificate, there is a significant (ca. 9 per cent) boost in recognition of land ownership. Holden, Deininger, and Ghebru (2011, Ethiopia) find that the generally positive effects of certification on leasing out are significantly higher for women. None of the studies look at displacement outcomes.

We were able to conduct a formal moderator analysis for effects on productivity and long-term investment only; the other outcomes were too sparse for such an analysis. Recall that the moderator variables of interest include governance conditions, regions, income levels, population density, cash crop farming and years between intervention and assessment. We could not assess region-specific effects for all of the regions, as the number of observations per region was too small. Inspection of Figures 5–9 shows that within each region (the cases are grouped from top to bottom by region), there is substantial heterogeneity, although productivity and investment gains appear to be lower for cases in sub-Saharan Africa. Therefore, we include a sub-Saharan Africa indicator in the analysis.

Table 2 shows the results of our moderator analysis for effects of tenure recognition on productivity and long-term investment based on bivariate random effects meta-regressions. Productivity effects in sub-Saharan Africa are significantly lower, and productivity effects rise with income (log GDP per capita). Interpreting these coefficients is difficult, however, because of the high correlation between log GDP per capita and the sub-Saharan Africa indicator (Pearson's $\rho = -0.58$, $p < .001$). Therefore, we cannot say whether the coefficients that we see are appropriately interpreted as a 'wealth effect' or an effect owing to other regional differences in informal tenure institutions or infrastructure (or, whether there is some third variable inducing a spurious relationship all around).

3.2. Synthesis of qualitative evidence

Seven of the qualitative studies were carried out in sub-Saharan Africa, one study focused on Peru, and one on Vietnam. As with the quantitative studies, all cases were forms of recognition of individual land tenure and there was significant variation in the time span between the intervention itself and the research being undertaken to assess it, with the shortest period being one year and the longest 20 years. There were two cases of registration as part of land redistribution programmes (Chirwa 2008; Lesorogol 2005), six resulting from more standard land registration policies (Besteman 1990; Burgi 2008; Chilundo et al. 2005; Kerekes and Williamson 2010; Teklu 2005; World Bank 2008) and one unique case from Côte d'Ivoire where although its formal passage and implementation was prevented by the 2002 rebellion, the initial effects of the nonetheless informally implemented 1998 Rural Land Law were measured in advance of the actual intervention (Bassett 2009).

Table 2. Estimates from bivariate random-effects meta-regressions on each of the moderator variables.

Effects on:	Moderator:	Years transpired	Dem. gov. scores	Africa	Log (GDP/ capita)	Rural pop. density	Mixed agriculture	Cash crops
Land product-ivity	Moderator							
	coef.	-0.01	-0.02	-0.42*	0.37*	0.02	0.20	0.19
	(s.e.)	(0.01)	(0.03)	(0.12)	(0.11)	(0.04)	-0.19	(0.28)
	tau-sq.	0.05	0.06	0.02	0.02	0.06	0.05	0.06
	Residual het.							
	test <i>p</i>	0.00	0.00	0.01	0.00	0.00	0.00	0.00
	<i>N</i>	8	8	8	8	8	8	8
Prob. of invest-ment	Moderator							
	coef.	0.00	0.00	-0.06	0.04	0.02	0.01	
	(s.e.)	(0.00)	-0.01	(0.10)	(0.26)	(0.03)	(0.09)	
	tau-sq.	0.01	0.01	0.01	0.01	0.01	0.01	
	Residual het.							
	test <i>p</i>	0.00	0.00	0.00	0.00	0.00	0.00	
	<i>N</i>	8	8	8	8	8	8	

Bivariate random effects meta-regression estimates. Between study variance (tau-sq.) estimated via empirical Bayes. Regressions all include intercepts (omitted from display). Prob. of investment regressions include indicator variables that account for overlapping cases in Peru and Ethiopia (estimates omitted from display). Standard errors apply the Knapp and Hartung (2003) correction to account for uncertainty in the tau-sq. estimates.

* *p* <.05

Table 3 lists 23 findings from the qualitative synthesis, organised in nine groups identified during the extraction process, as well as an indicator of whether the finding represents a positive, negative or mixed effect. Five studies provided generally positive interpretation of the tenure recognition (Besteman 1990; Chilundo et al. 2005; Chirwa 2008; Lesorogol 2005; World Bank 2008); four presented mixed effects (Burgi 2008; Chilundo et al. 2005; Kerekes and Williamson 2010; Teklu 2005), and three presented primarily negative views (Bassett 2009; Besteman 1990; Kerekes and Williamson 2010). Among repeated qualitative findings, three reflected favourably on the intervention: credit and investment money from outside community made available (Besteman 1990; Chilundo et al. 2005); feelings of improved security and control over land (Lesorogol 2005; Chilundo et al. 2005); and a reduction of conflicts over land use, domestic decisions, inheritance (Lesorogol 2005; World Bank

2008). Two of the repeated findings reflected negatively: new land disputes and land grabs seen (Bassett 2009; Besteman 1990) and concerns over displacement and land unavailability (Besteman 1990; Bassett 2009; Kerekes and Williamson 2010). Social outcomes like displacement, perceived insecurity, social conflict or gender equality were part of the qualitative discussions of many of the studies considered here. Teklu (2005, Ethiopia) found that while land policies in the Amhara regional state of Ethiopia gave equal access to land for women, access did not equal control, and land registration efforts were unable to address important cultural norms prohibiting women from ploughing land, forcing them to pay men for this or enter into share-cropping arrangements with men. The World Bank (2008) (Vietnam), in investigating whether supporting the replacement of land titles and only naming the household head with titles or naming both husband and wife was worthwhile, found there was an increase in feelings of empowerment amongst female respondents who held joint husband–wife titles.

Besteman (1990) (Somalia) and Bassett (2009) (Côte d'Ivoire) also recorded concerns over displacement, with Besteman finding increased concern due to examples within the village of land-grabbing by outsiders. Kerekes and Williamson (2010) consider effects of the creation of Peru's two land registration bodies (Comisión de Formalización de la Propiedad Informal and Proyecto Especial de Titulación de Tierras y Catastro Rural [PETT]) and the commencement of their work on registered landowners' ability to access credit and defend their right via official channels. They found respondents lacked faith in government enforcement of the law and, in fact, feared displacement or eviction by the government, but not from individuals. Bassett (2009), who followed the effects of the incoming Rural Land Law on migratory pastoralists, found that the rising number of fields and others' herds on their traditional pastoral grazing lands was increasing over time, limiting the areas on which the FulBe pastoralists could graze their cattle.

Table 3. Qualitative findings organised by category of impact.

Perceived Impact: negative/unclear or mixed/positive (-/./+)	Summary of finding	Frequency %
	Productivity	
-	Productivity increased perversely due to tenure security anxiety (Besteman 1990)	11
-	Lowering quality and availability of pastoralist rangelands has decreased productivity (Bassett 2009)	11
	Investment	
+	External credit and investment money from made available (Besteman 1990) Chilundo et al. 2005	22
+	Increased income led to more money for farm investments (Chirwa 2008)	11
+	Joint husband–wife titles had a positive impact on credit access (World Bank 2008)	11
.	Registration did not improve access affordable credit (Kerekes and Williamson 2010)	11
	Long-term production	
+	Viewed land redistribution as positively impacting income and food availability (Lesorogol 2005)	11
	Leasing out land	
+	It was widely held that leasing was an advantage of land ownership (Lesorogol 2005)	11
	Consumption	
+	Respondents indicated they have more money for household needs (Chirwa 2008)	11
.	No perceived improvement in income or living standards (Chilundo et al. 2005)	11
	Gender equality	
+	Joint husband–wife land titles led to increased feelings of female empowerment (World Bank 2008)	11
.	Some improvement but not as much as policy would require (Teklu 2005)	11
	Perceived insecurity	
+	Feelings of improved security and control over land (Lesorogol 2005; Chilundo et al. 2005)	22
./+	Overall improved except for female-headed households (Teklu 2005)	11
./+	Titling may alleviate others claiming rights over one’s land, however, many preferred the lack of ‘restrictions’ on where one farms in customary tenure systems (Burgi 2008)	11
.	No faith in government titling as security, fear eviction by government. Private tenure security mechanisms resolve disputes more effectively (Kerekes and Williamson 2010)	11
-	Corruption within, and inaccessibility of, the registration process leads to insecurity (Besteman 1990)	11
	Social conflict	
+	Many indicated a reduction of conflicts over land use (Lesorogol 2005; World Bank)	22
-	New land disputes and land grabs seen (Bassett 2009; Besteman 1990)	22
	Displacement	
+	Owning land ensures one always has a home (Lesorogol 2005)	11
./+	Overall improved but anecdotal evidence of eviction of vulnerable females (Teklu 2005)	11
-	Concerns over displacement and land unavailability (Besteman 1990) (Bassett 2009; Kerekes and Williamson 2010)	33
-	Fear eviction from government, but not from individuals (Kerekes and Williamson 2010)	11

Findings on displacement were not all negative however, with positive results reported by Lesorogol (2005) (Kenya), where a group of traditional pastoralists had participated

in a land redistribution and registration programme as an effort to end a decades-old communal land dispute.

On perceived tenure insecurity, Lesorogol (2005) found positive views of increased security, as did Chilundo et al. (2005) (Mozambique), who examined household and community effects of land registration in two Mozambican provinces and found that increased security was perceived against attempts by outsiders to acquire local land. In contrast, however, Besteman (1990) found that farmers in Somalia felt that the threat of losing land to outsiders would increase over time due to corruption associated with, and inaccessibility of, nontransparent land registration processes. Burgi (2008) (Ghana) found that while some respondents indicated titling alleviates the possibility of others claiming rights over one's land, most preferred the lack of 'restrictions' on where one farms in customary tenure systems. As noted above, Teklu (2005) found cultural norms led to feelings of tenure insecurity in women in Ethiopia.

On social conflict, Lesorogol (2005) observed satisfaction in Kenya that the intervention had improved social cohesion in the area due to the registration process providing solutions to previous social conflicts. Likewise, the World Bank (2008) found female respondents in Vietnam overwhelmingly agreed that joint husband–wife titles offered them advantages in domestic disputes and decision-making. This contrasted Bassett's (2009) account in Côte d'Ivoire of numerous land disputes based on new attempts to legally demarcate traditional lands and Besteman's (1990) accounts in Somalia of numerous disputes over land grabs by outsiders.

We identified two examples of perverse productivity effects. Besteman (1990) found that productivity of individuals, not of parcels, increased in Somalia's Jubba valley for perverse reasons. Due to increased anxiety over growing tenure insecurity, villagers were clearing forested parts of their parcels to produce more while they could before presumed land grabs could take place. Bassett (2009) found that FulBe pastoralists faced lowering quality and availability of rangelands as farmer tenure claims increased, decreasing productivity of some respondents' herds.

On all remaining material outcomes, including investment, long-term production, leasing out land and consumption, the qualitative literature reflected almost exclusively positive experiences. On investment, although nonagricultural, Chilundo et al. (2005) found registration in Mozambique led to credit being given to a new carpenters association and to outside investments to build a local school. In the World Bank (2008) case of Vietnam, respondents felt joint husband–wife titles had a positive impact on their, and especially her, credit access. In the case of a Malawian redistribution programme for landless workers, respondents indicated they had more money for farm investments after the intervention (Chirwa 2008). One unclear result came from Besteman (1990), who found that government officials (the only group who had been able to register land in the area) lamented the lack of agricultural wage labourers for their registered farmlands following reforms. Another ambiguous result came from Peru, where Kerekes and

Williamson (2010) reported that respondents had not seen improvements in their ability to access affordable credit.

On long-term production and consumption, Lesorogol (2005) found that in Kenya, respondents largely viewed farming opportunities brought about by land redistribution as positive, whereas Chirwa's (2008) respondents indicated they had more money for household needs. An unclear result came from Chilundo et al. (2005), whose interviewees perceived no improvement in income or living standards, which the authors' attributed to lack of infrastructure to bring goods to market. On leasing out land, Lesorogol (2005) found widely held views that leasing land was an advantage of land ownership and a good opportunity for those with few livestock to improve their livelihoods.

4. Conclusion

The findings of this systematic review underscore the importance of tenure rights recognition in fostering productivity and increasing farm incomes. Such benefits appear to operate in part through increased perceptions of tenure security and investments, although the evidence suggests that these mechanisms do not explain all of the productivity and income gains. Neither do we find evidence for a credit effect. Tenure reform may also have negative social effects, including on women's access to land and on displacement of the poor or others facing social and financial barriers to participating in the reformed regime. Furthermore, productivity gains may take time to become apparent and the effects may vary substantially across settings. We stress, however, that available evidence provides a weak basis for establishing the general effectiveness of land tenure reform. None of the included studies were randomised control trials, and for only two out of the 20 studies reviewed (Ali, Deininger, and Goldstein 2011; Do and Iyer 2007) was there a concerted effort to address selection biases by explicitly accounting for processes of assigning tenure rights recognition. This leaves the studies vulnerable to biases that we conjecture would overestimate the benefits of de jure recognition.

The study results draw attention to significant gains in productivity in Latin American and Asian cases due to tenure formalisation, and the comparatively weak effects in African cases. Such regional differences may be based on the fact that most farms in sub-Saharan Africa are held under customary tenure arrangements that provide tenure security, although it should be noted that we did find that individual tenure recognition increased perceived tenure security in our African cases. Regional differences may also be explained by the low levels of wealth of African farming families and constraints on access to resources needed to translate tenure recognition into commercial activity. A third possibility is that tenure recognition reforms in the African cases were not coupled with investments in complementary 'public capital' (Reinikka and Svensson 2002) in a manner resembling the more holistic agrarian reform processes in Asia and Latin America. Future research should investigate these possibilities.

Only one study in Nicaragua found consistent evidence of a credit effect (Foltz, Larson, and Lopez 2000). A further study in Peru found mixed and statistically insignificant evidence

(Torero and Field 2005). None of the other studies found any significant effect on credit uptake. This is in contrast to predictions of neoclassical theories (de Soto 2000). Why might this be? First, in some cases (for example, Ethiopia or Vietnam), tenure rights recognition did not go so far as to include alienation rights. Even when it did, the character of the properties in question – smallholdings of the rural poor – may be unattractive to financial institutions as collateral. Finally, the bankability of the landholders themselves, and the transaction costs in extending credit through formal channels may be unaffected by a change in tenure status (Philips 2003). None of the studies considered gave systematic consideration as to whether the alternative to de jure recognition of freehold tenure was either customary or some other form of tenure. We failed to identify any studies that examined the effects of statutory recognition of customary tenure. It is unclear how recognition of individual tenure might compare with recognition of customary rights. Further research should examine this alternative, given its increasing prevalence.⁹

Finally, none of the studies made a distinction between the effects of tenure reform on prerecognition versus postrecognition inhabitants, with all studies estimating effects for postrecognition inhabitants only. To the extent that recognition occurs as part of displacement processes (as the current literature on ‘land grabs’ suggests), the generally positive benefits that we see in these studies may conceal social costs. Such possibilities ought to be taken up in further research.

Notes

1. The full report may be accessed online at <http://campbellcollaboration.org/lib/project/220/>.
2. That is, we applied the ‘PICOS’ framework (Waddington et al. 2014, p. 362).
3. We had initially intended to include also studies on statutory recognition and codification of customary or communal rural land rights, and registration of these rights in an official registry. However, our search did not turn up any otherwise eligible studies on such interventions.
4. See Lawry et al. (2014) for the full list of sources and the search terms.
5. The Appendix to Lawry et al. (2014) replicated the analyses for binary outcomes in terms of the natural logarithm of the risk ratio.
6. Our random effects meta-analyses were conducted using the empirical Bayes estimators from the ‘metafor’ package in Viechtbauer (2010). We apply the Knapp and Hartung (2003) adjustment to account for uncertainty in estimating interstudy heterogeneity, and we follow Higgins, Thompson, and Spiegelhalter (2009) in constructing predictive intervals.
7. Available from <http://www.systemicpeace.org/polity/polity4.htm>.
8. Available from <http://data.worldbank.org/data-catalog/world-development-indicators>.
9. Knight (2010) documents this in Botswana, Mozambique and Tanzania; the current Community Land Bill in Kenya and recognition of community lands in South Sudan are also indicative of this trend.

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Appendix

Table A.1. Characteristics of cases included in the quantitative synthesis and basic findings.

Region	Country	Setting	Study	Year policy or program initiated	Year of outcome assessment	Years between initiation & assessment	Comparisons used in the meta-analysis
Latin America	Nicaragua	Rural	Foltz, Larson, and Lopez (2000)	1981	1998	17	Households with full title versus no title.
	Nicaragua	Rural	Bandiera (2007)	1981	1998	17	Households who received land through land reform versus those who did not
	Nicaragua	Rural	Deining and Chamorro (2004)	1994	2000	6	Land parcels registered versus parcels informally occupied
	Peru	Rural	Fort (2008)	1992	2004	12	Land parcels covered by either medium or low tenure security and then either with or without title registration
South Asia	Peru	Rural	Torero and Field (2005)	1993	2004	11	Households with PETT title versus no title
	India	Rural	Deining, Jin, and Nagarajan (2007)	1970	1999	29	Households that were either subject to tenancy reform or not
East Asia	Cambodia	Rural	Markussen (2008)	1989	2004	15	Land parcels either with or without paper
	China	Rural	Deining and Jin (2002)	1987	2001	14	Household in Guizhou province where land reform was carried out vs. those outside Guizhou province
	China	Rural	Kung (2006)	1987	1999	12	Households either before, during or after the freezing of land allocations
	Vietnam	Rural	Do and Iyer (2007)	1993	1998	5	Households covered by new land-use rights laws vs. those not covered
	Vietnam	Rural	Van Den Broeck, Newman, and Tarp (2007)	1993	2006	13	Household members are listed in government documents as having title for plots or not
	Vietnam	Rural	Kempy, Klumpz, and Schumacher (2011)	1993	2004	11	Households either with or without land certificate
Africa	Ethiopia	Rural	Holden, Deining, and Ghebru (2009)	1998	2006	8	Land parcels either with or without certificate
	Ethiopia	Rural	Holden, Deining, and Ghebru (2011)	1998	2006	8	Households either with or without land certificate
	Ethiopia	Rural	Deining, Ali, and Alemu (2009)	2003	2007	4	Household issued a land certificate vs. those with no certificate
	Ethiopia	Rural	Deining et al. (2007)	2003	2006	3	Communities (kebeles) in which certificates were issued versus communities without certificates
Madagascar	Rural	Jacoby and Minten (2007)	1961	2005	44	Land parcels that are either titled or not	

(Continued)

