

Impact of property rights on poor households' investment decisions: a treatment evaluation of a titling programme in Peru

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Abstract

This paper examines the impact of a Peruvian large-scale titling programme on housing investment in urban slums. This experience was previously analyzed by Field (2005), who concluded that the resulting investment is limited only to small housing renovations, with no considerable impact on long-run investments. Using a unique data set from 2003 that contains panel information on several categories of housing investment, this study finds not only a positive relationship with short-run investments, but also with long-run ones. Results from a difference-in-difference propensity score matching indicate that the estimated average treatment effect implies an increase in the number of housing sizeable additions by 170-200 percent. Additional contributions of this paper are threefold: first, in the case of long-run investment, as income reduces, the title's impact tends to diminish, so that a significant effect obtains only for the top half of the distribution. These weaker results for poorer households suggest the existence of market failures, such as in the credit market. Second, results indicate that the title seems to be much more relevant in the decision as to whether to invest or not rather than in the level of investment. Third, the impact on long-run investment requires more than four years to be relevant.

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1. Introduction

Property rights are a key variable in economic development. Their relevance has long been considered in the economic literature, which judges them as an essential precondition for economic growth (North and Thomas, 1973; North, 1981; Johnson et al., 2002). The underlying argument is that fully secure property rights enhance investment incentives (Demsetz, 1967).

As Besley (1995) formally demonstrated in a theoretical model, the increasing incentive to invest resulting from tenure security can be explained by three different reasons. The most commonly cited link between investment and tenure security is that property rights protect individuals against eviction (Dercon et. al., 2005; Alchian and Demsetz, 1973). Additionally, a more secure tenure improves access to credit markets as properties with a title can be used as collateral (Feder et al., 1988; Carter and Olinto, 2002). Finally, investment is also promoted if individuals are able to trade the asset (Dercon and Krishnan, 2007).

Fragile property rights not only tend to reduce total investment but also have significant effects on its composition. Tenure insecurity hinders long-term investments in favour of short-term ones because the former are more expensive and therefore involve a greater risk. (Dercon et. al., 2005).¹

Nowadays, millions of people in urban areas of developing countries occupy dwellings without having a title. The lack of formal property rights represents a severe constraint for such people. For this reason, this topic has become essential on the policymakers' agenda as

¹ Although the majority of the literature on this topic is associated with an agricultural framework, their predictions can be expanded to housing investment in an urban context.

an instrument to reduce poverty and to promote investment in urban slums. In this context, many governments have started land-titling programmes in developing countries such as Colombia, Mexico, Honduras, Paraguay, Peru, Ghana, Egypt, Turkey and the Philippines (Galiani and Scharfrodsky, 2006).

The Peruvian experience is particularly interesting since it constitutes one of the largest government titling programmes targeted to urban areas in any developing country. Between 1996 and 2006, more than 1.5 million property titles were recorded by the Peruvian government, which benefited more than 7 million inhabitants of marginal communities (Cofopri, 2006).

Even though a considerable empirical literature explores the effects of property rights on investment, it has been principally focused on rural areas. Few researches have analyzed this issue in urban settlements.

The objective of this paper is to evaluate the impact of the Peruvian large-scale titling programme on housing investment in urban slums; and, in this manner, complement the few existing researches. The Peruvian experience was previously analyzed by Field (2005) and some of her findings contrast those of this paper.

In particular, Field (2005) employed cross-section data collected in 2000 with panel information on investment. Considering the titling programme similar to a “natural experiment”, she calculated an OLS dif-in-dif estimator and found a positive impact of title on the number of total investments. Additionally, she estimated probit models by using binary indicators of any short- and long-run investments. The results of her analysis indicated that the impact of titling is limited only to small housing renovations instead of long-run investments. These results are contrary to what could be expected, as economic theory has always put great emphasis on the relevance of property rights on long run investment.

The analysis proposed in this paper considers the methodology suggested by Field (2005) as a starting point using the number of investments as the dependent variable. Two different control groups are used in the analysis to provide more robustness to our results. The first one contains households in communities that were reached by the programme and that did not obtain a title. The second control group includes households that, according to requirements, were eligible to get a title, but did not get one because they lived in areas that were not treated by the programme. These households can be considered as potential future beneficiaries. We then extend the analysis and estimate the titling effect utilizing other econometric techniques and including a richer set of control variables.

The present study also employs a similar but more recent data base from 2003 that also contains panel data information on several categories of housing investment. OLS difference-in-difference estimates constitute the baseline results. However, and in order to deal properly with the specific characteristics of the dependent variable, discrete and count data models are also estimated as well as the dif-in-dif propensity score matching estimator. The usage of more strategies and the combination of them gives the estimates more precision and robustness. Among the variables included as controls, those associated with the selection of treated households and communities play an important role and help reduce any potential bias due to non-random treatment and construct the second control group more accurately.

The empirical results below indicate an important and positive relationship between title and total housing investment. However, in contrast to Field's findings, the titling impact on long-run investment appears highly significant and larger than that on short-run components. This is consistent with what we expect theoretically.

In addition to the measure of the total impact of the titling programme, other aspects of the effect of titling on investment are assessed that can also be considered as contributions

of this paper. First, analysing the relevance of title by different levels of income, we find that in the case of long-run investment, as income reduces, the title's impact tends to diminish until it disappears for the poorer quartiles of the distribution. The weaker results for poorer households suggest the existence of market failures, like in the credit market. Second, based on an ordered probit model, we estimate the differential marginal effects on each level of investment. Results suggest that title seems to be much more relevant in the decision of whether to invest or not rather than in the level of investment. This finding can be directly associated with the reduction of risk of expropriation. Third, we explore the dynamic response and find that impact on long-run investment requires more than four years to be relevant.

The aim of the analysis is to improve the understanding of the influence of tenure security on investment decisions in a poor urban settlement, and give policymakers additional perspectives on how this kind of programme can be evaluated.

The rest of this paper is organized as follows. The next section contains a brief description of the Peruvian titling programme and its selection criteria. Section 3 focuses on the data and on the empirical analysis. The baseline OLS results are presented in section 4. Section 5 reports results based on other econometric approaches, that, given the discrete nature of the dependent variable, are arguably better suited to the present application. The dynamic response of investment is considered in section 6. Finally, section 7 summarizes and provides concluding remarks and suggestions.

2. The Peruvian titling programme and its selection criteria

The Peruvian government has been developing property rights reforms since 1996, seeking to improve tenure security for urban settlements. The large informality in Peru's urban area produced in the last decades can be explained by the substantial urban-rural migration experienced in the second half of the last century. This was due not only to the

collapse of the rural economy, but also to the emergence of irregular armed groups in these areas. Bureaucratic procedures and high fees made it hard for any poor Peruvian household to obtain a title before the reforms (Field, 2003). As a result, estimates found more than three million informal properties in 1997 (World Bank, 2006).

In 1996, the public agency '*Committee for the Formalization of Private Property*' (Cofopri), supported by the World Bank, started an area-wide titling programme. Since then, this programme has constituted one of the largest governmental titling efforts targeted to urban areas in the world. Moreover, the reform has also introduced many legal, administrative and regulatory policies that have made the process of getting a title less cumbersome, and helped in the promotion of a formal property market. Thus, whereas the old process of obtaining a title was slow and expensive, the process after the reform became rapid and free-of-charge, as Cofopri incurred all the costs from the titling programme. To obtain a title, one only has to show residency in a public eligible area since before 1995² and have no other proper title (Field, 2003).

Cofopri established a massive titling procedure in the targeted neighbourhoods.³ In order to determine the cities that were treated in a staggered implementation, programme staff considered the following selection variables: city size, density of informality and length to commercial centres. Those variables were employed with the purpose of increasing the impact and reducing the costs of formalization (Field, 2003; Morris, 2004).

3 Empirical analysis

3.1. Data

² Archeological sites and flood planes are the most important ineligible areas (Field, 2003).

³ See Morris (2004) and Field (2003) for more details of the implementation of the titling programme.

The econometric analysis is based on a cross-section data set, commissioned by Cofopri, which was collected in June 2003 from five different regions reached by the programme. This data-base includes information of tenure status from 2331 properties with 836 of them having a title given by Cofopri. All the communities from the regions included in the data-base were meant to be considered in the programme, but by the time of the survey some had not yet been treated. Thus, the data-base contains information from communities that were reached by Cofopri (51 percent of the total sample) and from others that had not yet been treated but will be in the future (the remaining 49 percent). This will be very useful when it comes to evaluating the effects of the reform. Also, many household and community characteristics that can be used as control variables are provided in the survey.

As Field and Kremer (2005) have argued, ex-post cross-section data can be used to evaluate programmes if it incorporates retrospective questions about the intervention⁴, and if the data cover a long enough period to estimate the total benefits. Fortunately, our survey satisfies both requirements. In particular, it incorporates information about past housing investments in the last 10 years in eight different categories and also the year in which each one was developed. This offers enough data for the before- and after-programme time periods and plenty of time to measure the total effects.

The type of information available in the data-base enables us to define the investment variable after the programme as well as before. The former is given by the sum of the number of investments undertaken in the year prior to the implementation and the year in which the programme started in each region, while the latter is given by those investments completed in 2001 and 2002.⁵ In defining the baseline, it is possible we are picking data

⁴ To minimize any potential recall bias, it is recommendable to have an anchor question. The project itself is a very useful way of anchoring. Besides, respondents must give information of the key variable before and after the programme (Field and Kremer, 2005)

⁵ Like other authors in this area, we were faced with a difficult decision regarding the definition of the baseline period, i.e. the period 'before' the programme. Ideally, this baseline period should be set early enough so that the prospect of treatment does not affect investment in that period (otherwise it may not be a proper baseline). This suggests we should use a period long before the implementation of the programme.

points too close to the period of the reform. If indeed this is the case, however, notice that the bias of the average treatment effect is likely to be downward, as some of the effects of treatment may be reflected in the baseline investment data for the treated group⁶ (and so the growth in investment between the ‘before’ and ‘after’ periods amongst the treated is underestimated). In this case, one would expect the average treatment effects to be even larger than those reported in the next section. We experimented with alternative baseline periods, and the main findings of the empirical results did not vary much as a result of changing the starting year.

With the data available, it is also feasible to distinguish between short-run and long-run housing investment. Investments are considered short-run if they involved constructing walls, improving a roof, improving floors, improving walls, and painting walls; and long-run if they involved adding another story, constructing a bedroom or other rooms.

The investment variable has some specific characteristics. There is no information in the data about the amount of money involved in each investment. We only know how many investments of various types a household has carried out in a given year (this is also the case for Field’s data). Therefore the investment variable is discrete and, because it is quite common to not develop housing investments every year, its distribution is concentrated in a very few outcomes different from zero. More than 63 percent of the observations on total investments do not change over time in the data, mostly because there is no investment at all. For long-run investment, this proportion is as high as 93 percent. These characteristics should be taken into account when estimating the impact of titling on investment.

On the other hand, the further back in time we go, the worse is the quality of the investment data, since we rely on recall data here, which seems to be particularly important in this case (for instance, the number of long-run investments recorded in 1994 was six times lower than for 1995 – both periods are before the programme – and, none of the control groups reported any long-run investment in that year). The current choice of baseline appeared the most satisfactory, given these concerns.

⁶ Because the programme just started this year in each region with a few number of titles recorded –not necessarily those households in the survey–, the effects of titling tend to appear in the subsequent years, diminishing this potential bias problem.

3.2. Methodology of impact evaluation

The evaluation problem

Evaluating the impact of the titling programme on housing investment involves a comparison between outcomes among households who received a title and those potential outcomes that the same households would have had if they had not received the title. We only have data on actual outcomes, and do not observe potential outcomes. How to construct this counterfactual is, therefore, the main problem in any evaluation.

Let us formalize the impact effect as in Heckman et. al. (1997). Y_1 is the household's investment if it obtained a title by the programme and Y_0 if not. Also, D_i is a binary variable that equals one if a household is a beneficiary of the programme and zero otherwise. Finally, X are observable household and community characteristics. The average treatment effect on the treated can be written as follows.

$$ATT = E[(y_{1i,t} - y_{0i,t})|X, D = 1] = E[y_{1i,t}|X, D = 1] - E[y_{0i,t}|X, D = 1] \quad (1)$$

Since $E[y_{0i,t}|X, D = 1]$ is not observable, the analysis focuses on finding an approximation of it. Thus, this expression can be substituted by the observable outcome of non-treated households. In order to be able to do that, the conditional mean independence is implicitly assumed, according to which:

$$E[y_{0i,t}|X, D = 1] = E[y_{0i,t}|X, D = 0] \quad (2)$$

This expression supposes that, conditional on X , non-treated households have the same mean outcome as treated would have had in the absence of the programme.

Defining control groups and potential bias problems

Finding an approximation of the counterfactual is tackled by adding a control group, a similar group of households who were not exposed to the intervention. In order to satisfy the conditional mean independence, it is central to establish a credible control group, so that an accurate estimate of the counterfactual can be obtained. If this can be achieved, any systematic difference in investment between treated and non-treated households can be considered a consequence of the title given by Cofopri.

Two different control groups are used in the analysis to provide more robustness to our results. The first one contains households in communities that were reached by the programme and that did not obtain a title, either because they were already in possession of a registered title, or because they did not fulfil all the requirements. In this case, the selection is at the household level. The second control group includes households that, according to requirements, were eligible to get a title, but did not get one because they lived in areas that were not treated by Cofopri. Since these areas are also targeted by the programme, it is possible to consider those households as potential future beneficiaries. The selection in this case is, hence, at the area level.

Since non-treated households should be comparable to treated households, with respect to observable variables; it is important to investigate if this is supported by the data. Table 1 shows some descriptive statistics on the sample. We report not only the mean value but also the test for equality of means between each control group and treated one.

There are no large disparities among control groups and beneficiaries. However, whilst the first control group reports very similar values in almost all the variables, the second

control group presents some divergences, especially in household features. Households of this control group seem to be poorer on average than beneficiaries. This suggests controlling for these variables in the empirical analysis is important.

Table 1: Summary statistics^{1/}

	Benef.	Control group 1	P(T >t)	Control group 2	P(T >t)
<u>Household characteristics</u>					
Number members	4.82	4.75	0.57	5.21	0.00
Number children	1.42	1.35	0.43	1.59	0.02
Age head	46.05	45.56	0.56	47.10	0.19
Sex head (% of female)	25.5%	28.7%	0.26	33.5%	0.00
Education head (in years)	9.29	9.33	0.88	8.45	0.00
Monthly income (in current US\$)	185.73	185.34	0.97	168.90	0.09
Daily income (per person in US\$ PPP)	3.14	3.18	0.83	2.74	0.02
<u>Home characteristics</u>					
Lot size (m ²)	150.12	154.12	0.41	157.57	0.38
Residence (in years)	17.21	15.77	0.06	19.57	0.00
Walking distance to seven services (in minutes)	88.38	92.58	0.26	93.36	0.16
Water connection	65.0%	72.2%	0.01	43.7%	0.00
Electricity	99.8%	98.9%	0.05	99.5%	0.44
Phone	30.7%	34.6%	0.20	27.9%	0.32
Obtaining the property by intrusion	38.2%	42.7%	0.14	32.7%	0.07
<u>Housing investment</u>					
Short-run inv. (before prog)	0.237	0.256	0.70	0.223	0.75
Short-run inv. (after prog)	0.478	0.441	0.52	0.327	0.01
Long-run inv. (before prog)	0.024	0.034	0.50	0.023	0.95
Long-run inv. (after prog)	0.083	0.042	0.06	0.033	0.01
Obs.	836	356		391	

^{1/} The p-value of the mean test between beneficiaries and each control group is included (null hypothesis is that both means are equal)

These statistics also indicate that the programme has not focused systematically on the poorest families of the country, given the average daily income of each group. In order to analyze the impact of titling under severe poor conditions, we will especially examine the effect of the programme among the poorer households of these groups.

As to the investment behaviour between beneficiaries and control groups, Table 1 indicates how it differs considerably after the programme, especially for long-run investment. In that case, contrast the very similar investment ratio before titling with the large disparities after it. The question at this point is how much of this difference can be attributed only to the title.

However, even when the choice of the control group is based on all the observable information that is available, it is still possible that differences persist if there are some unobservable variables related to programme participation, like, for example, the demand for titling (Blundell and Costa Dias, 2000). Under this situation, the assumption of the conditional mean independence no longer holds and our estimation of the average treatment effect would be biased. In other words, the differences between treatment and control groups could be attributed not only to the impact of titling but also to pre-existing difference, or “selection bias” as commonly defined (Duflo and Kremer, 2005).

In the specific case of the present analysis, there may be potential selection bias in the first control group. In that case, some unobserved variables could explain why a specific household was treated by Cofopri while others were not. To deal with this problem, the analysis incorporated as controls the main variables identified by the programme as the requirements to obtain a title (residency time and non-possession of other proper title). Furthermore, the fact that the Peruvian experience provided, as Field (2003) has argued, a massive cost-free melioration in tenure security unrelated to demand, helps to reduce this potential endogeneity problem.

Additionally, the second control group gives us a chance of a comparison group that is not contaminated at all by this potential selection bias, since it only considers those households that would have been treated if the areas where they live had been reached by

the programme.⁷ As Dercon and Krishnan (2007) pointed out, it is possible to take advantage of the staggered implementation of the titling programme to evaluate properly its impact.

Nevertheless, there is another kind of potential bias due to the timing in which Cofopri reached each community. This problem is especially important if this timing is related to any unobservable variable that, at the same time, is correlated with investment.⁸ According to Morris (2004), the programme focused first on the easier to title lots, which is supported by the fact that the average cost of titling increased over time (from US\$ 53 in 1996 to US\$ 190 in 2003). This suggests that timing in the programme implementation was not exogenous. To reduce this potential problem, our analysis also includes the variables that were considered in the selection of the cities in each stage of the programme, such as the distance from commercial centres, the population density and the percentage of households that obtained property by invasion in each area as proxies for city size and concentration of informality, respectively.

The proposed analysis will calculate the average treatment effect on the treated by means of two approaches: a difference-in-difference estimation through regression and a dif-in-dif propensity score matching based on matching techniques.

Difference-in-difference estimation

The dif-in-dif estimator compares the difference in average housing investment before and after the programme for treated households with the before and after contrast for non-treated ones (Blundell and Costa Dias, 2000). The idea behind this approach is that the

⁷ If we consider in this control group all the households that live in these areas that were not treated yet, we would include future treated as well as future non-treated households. This does not isolate this control group from the selection bias.

⁸ In this case, this potential bias is in the area level and not in the household one as in the previous case.

investment behaviour of the control group is considered as the natural trend associated with other variables and not with the Cofopri title.

In the difference-in-difference estimator, the evaluation problem indicated in (1) is replaced by its difference, for which the available panel data information on housing investment is used. The average treatment effect, therefore, can be expressed as

$$ATT = E[(y_{1i,t} - y_{1i,t-1})|X, D = 1] - E[(y_{0i,t} - y_{0i,t-1})|X, D = 1] \quad (3)$$

where $t-1$ and t are time periods before and after the titling programme, respectively. Maintaining the conditional mean independence assumption, we are able to calculate the counterfactual of the second term of the expression.

In order to estimate this expression, we combine the panel dimension of investment variable with the cross-section nature of the control variables as follow. Let q_t be a time dummy that has a value of zero before the programme and one after it, while D_i and X_i are the treatment dummy and the vector of control variables, respectively. Thus, the coefficient on the interaction between time and beneficiary dummies (a_4 in equation (4) below) is the estimated programme impact, which is the conditional (on X_i) average effect of titling on treated households. Among the independent variables, we included those related to programme selection. The expression for the investment level is the following.

$$y_{it} = a_1 + a_2 D_i + a_3 q_t + a_4 D_i q_t + a_5 X_i q_t + e_{it} \quad (4)$$

which after taking first differences, becomes

$$\Delta y_{it} = a_3 + a_4 \Delta D_i q_t + a_5 \Delta X_i q_t + \Delta e_{it} \quad (5)$$

This strategy allows us to remove any bias produced by time-invariant unobserved heterogeneity as it cancels out upon subtraction. However, a dif-in-dif estimator does not control for time varying unobserved effects that influence participation in the programme (Blundell and Costa Dias, 2000).

The difference-in-difference estimator can be calculated by regression analysis using different techniques. Besides the baseline OLS, we also employ discrete as well as count data models, as these are probably better suited to the present application given the discrete nature of the dependent variable.

Ordered probit models capture very well the ranked discrete characteristic of the dependent variable, and so should be preferable to OLS. However, the investment variables in the data are not merely discrete and ordered, but, as already discussed, they come as counts. Presumably, count data on investment is more informative than discrete ordered data, and we will also consider the results from a count data model. In fact, depending if the variance is equal to (equidispersion) or exceed (overdispersion) the mean, we can employ the Poisson or the negative binomial model, which is a more general approach, to estimate the impact of titling.

There are some additional methodological concerns related to the difference-in-difference estimation by regression. The inclusion of observables variables in the regression tries to control for variation across households in order to compare properly treated and control groups. However, the results from the regression analysis may be misleading if treated households tend to have atypically high (or low) values of the control variables, compared to non-treated households. In such a case, there are few observations in the control group based on which the counterfactual can be estimated. Regression analysis, therefore, calculates the impact of the title extrapolating observations that are not the accurate counterfactuals. Additionally, a second weakness of regression analysis is that we

have to impose a particular specification, which is especially difficult in the case of investment.

Matching estimation

An alternative way of estimating the counterfactual outcome and dealing with the shortcomings associated with regression analysis is the matching method. Estimation based on that method involves the direct pairing of treated and non-treated households that have similar observable variables, and compares the difference in investment across such households. Provided the set of control variables is complete, this difference can then be attributed to the title.

However, exact matching based on many observable variables becomes quite difficult. For that reason, Rosenbaum and Rubin (1983) proposed the use of an index that sums up the observable variables of each individual. This index, known as the propensity score, is the probability of being titled conditional on observable variables.

$$p(X) = E[D|X] = \Pr[D = 1|X] \quad (6)$$

The likelihood of being titled can be modelled by using a binary choice model. As Heckman et. al. (1997) indicated, this method provides a low-bias result if we are able to incorporate in this participation regression the variables which explain the programme selection. For that reason, the propensity score in our analysis is estimated by a probit model, which includes not only variables that determine the participation in the titling programme (variables related to the eligibility of individuals for the first control group and of regions for the second one), but also variables that affect housing investment.

The propensity score matching creates the statistical comparison group – or counterfactual – by matching observations of titled households to those of non-titled

households with similar values of the propensity score (Gilligan and Hoddinott, 2007). The counterfactual is obtained as a weighted average of investment in non-treated households. Kernel matching is employed in the analysis as the technique to calculate the weight.⁹

Rather than calculating the average treatment in levels by matching techniques, we prefer to improve this estimator by combining this method with the previous dif-in-dif approach. Thus, as in Gilligan and Hoddinott (2007), we use the propensity score difference-in-difference estimator with the purpose of removing time-invariant unobserved heterogeneity between titled and non-titled households.¹⁰

Then, using the differenced average treatment effect on the treated of expression (3) and the kernel matching, we have the following expression for the estimator employed in the analysis (Gilligan and Hoddinott, 2007).

$$ATT^{DIDM} = \frac{1}{N_T} \sum_{i \in \{D=1\}} \left((y_{1i,t} - y_{1i,t-1}) - \sum_{j \in \{D=0\}} \frac{K(p(x)_j - p(x)_i)}{\sum_{j=1}^{N_{C,t}} K(p(x)_j - p(x)_i)} (y_{0i,t} - y_{0i,t-1}) \right) \quad (7)$$

The standard errors associated to the average treatment effect are calculated by means of bootstrapping.

This method is a non-parametric approach to the estimation of the treatment effect on outcomes. It is more general than regression in the sense that no functional form is required, which is especially attractive in this case¹¹ (Blundell and Costa Dias, 2000).

⁹ The kernel function is a symmetric density function that obtains its maximum when the propensity scores of two observations are the same and decreases when there is a great disparity between the propensity scores.

¹⁰ It helps to relax the strong assumption of conditional independence between the error term and treatment status in the determination of investment on which relies the conventional propensity score matching, since the combination of matching with dif-in-dif estimation permits an unobserved determinant of participation that can be considered as individual- and time-invariant components of the error term (Blundell and Costa Dias, 2000).

¹¹ The main disadvantage of the propensity score matching is that, because this method does not rely on extrapolation, there is a probability of being unable to find an accurate counterfactual based on the

4 Baseline results

Table 2 presents the dif-in-dif baseline results using OLS models, similar to the methodology employed by Field (2005). Results show a large and significant impact of Cofopri's title on total housing investment for both control groups. The estimated average treatment effect is 0.19 based on control group 1, and 0.31 based on control group 2. In other words, being treated implies that the expected number of investments increases by 0.20-0.30, depending on the control group. Compared to the evolution that the treated would have had if they would have not received the title, our estimated effects are actually rather large. In percentage terms, they imply that the number of investments rises by 60 percent on average as a result of receiving treatment.¹² These results are quantitatively similar to those of Field (2005), whose reported treatment effects at 68 percent.

Next, we disaggregate total investment and distinguish between short-run investment (i.e. the construction of walls, improvement of the roof, the floor, and the walls, including painting walls) and long-run investment (i.e. addition of another story, construction of bedroom and/or another room). We do this primarily in order to probe Field's (2005) surprising finding that long-run investment does not increase as a result of improved property rights. Estimated average treatment effects are reported in the second (short-run investment) and third (long-run investment) pair of columns in Table 2. These results are clearly very different from those reported by Field (and thus consistent with a large body of theoretical research in this area), in that the average treatment effect on long-run investment is about 0.08 and highly statistically significant. Again, this is a large effect, given the low baseline: an increase by 0.08 implies an increase by more than 200 percent.

propensity score for all the observations, which means that some observations are not included in the matched sample that is used to calculate the average treatment effect. That can imply a loss of information (Blundell and Costa Dias, 2000).

¹² This was calculated by using the average number of investments of the treated before the programme and adding the investment growth rate of the control groups as an approximation of the counterfactual evolution if they would not have received the title.

As for short-run investment, we obtain a large (0.23) and statistically significant effect for control group 2. For control group 1, the estimated average treatment effect is equal to 0.10 and not quite significant at conventional levels. Nevertheless, it is not significantly different from the estimated coefficient for control group 2, and so maybe the true effect lies somewhere in between these estimates. The average of the two estimates is 0.17, which implies that getting a title increases the number of investments by 40-50 percent. Although a large effect, it is nevertheless dwarfed by the long-run effect, as we have already seen (in percentage terms that is).

Table 2: OLS difference-in-difference models^{1/}

	Total investment		Short-run investment		Long-run investment	
	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas
ATT	0.1921** (0.096)	0.3091*** (0.089)	0.1044 (0.079)	0.2349*** (0.077)	0.0877*** (0.029)	0.0720*** (0.026)
R squared	0.0335	0.331	0.0283	0.0292	0.0293	0.0253
Obs	1090	1120	1090	1120	1090	1120
Impact by quartile of income						
ATT in quartile 1 (> P75)	0.6796* (0.397)	0.3166 (0.206)	0.3208 (0.286)	0.1835 (0.172)	0.3589** (0.149)	0.1331** (0.067)
ATT in quartile 2	0.2764 (0.202)	0.5057** (0.182)	0.1407 (0.156)	0.4316** (0.175)	0.1357** (0.068)	0.0740 (0.063)
ATT in quartile 3	0.0408 (0.156)	0.1695 (0.214)	0.0578 (0.135)	0.1062 (0.190)	-0.017 (0.046)	0.0633 (0.051)
ATT in quartile 4 (< P25)	0.0456 (0.136)	0.2920* (0.156)	0.0359 (0.131)	0.2690* (0.140)	0.0096 (0.030)	0.0231 (0.043)

Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level

^{1/} Including complete set of control variables (family size, number of children, possession of substitute title, monthly income, age head, sex head, education head, lot size, regional dummies, residential tenure, community has public light, walking distance to seven different community services, distance to commercial centre by car, density of population, if household obtain the property by intrusion and density of informality).

Furthermore, to see how sensitive the results are to the choice of the baseline year, we re-estimated the regressions moving back the baseline period by one year. Reassuringly, the

results (which are not reported, to conserve space) were very similar to those shown in Table 2.

As already noted, our results differ substantially from those obtained by Field (2005), who concluded that the effect of the programme on investment is limited to smaller housing renovations instead of long-run investments. There are (at least) two reasons for this. First, our regressions include a richer set of control variables than those considered by Field. Accordingly, among the variables that this analysis incorporates, those correlated with programme selection appear to be relevant. In this context, the inclusion of a substitute title also recognizes that people living under informal conditions have produced alternative titles with some properties of formal ones and, hence, their influence on investment decisions. In the same manner, other variables that control for community characteristics (e.g. city size, level of informality, access to some public services) appeared also to be relevant. Besides, among the family characteristics we have worked with the level of income instead of income shock, which is a more powerful source of information.

Second, and perhaps more importantly, our data span a longer period after titling than Field's data. This may be particularly important for the study of long-run investments, because individuals may need time to plan and accumulate funding for new (often expensive) long-run investment, and it is also possible it takes some time before individuals are fully convinced the new policies are not going to be reversed. Additionally, since the long-run component represents a major irreversible investment, the greater impact of Cofopri's title can be seen as evidence of the importance of titling for the reduction of the risk of expropriation among urban poor households. Tenure insecurity distorts investment decisions in favour of short-run investments, because they imply less risk.

However, this is only one side of the story. To explore the role of financial constraints, which are commonly faced by the poor, we also analyze the impact of the programme by level of income. To do so, we estimate regressions for each quartile of income. Our results,

shown in Table 2, indicate that as the level of income increases, the significance and the coefficient associated with the impact of titling also rises, especially in long-run investment. The explanatory capability of the model tends to increase with income as well. In the case of long-run investment, the programme effect among households at the lower end of the income distribution is extremely small and completely insignificant (with p-values in the range of 0.75 and 0.59). In contrast, for less poor households the effect is much larger and statistically significant.

These results suggest that other barriers exist, besides risk, which limit investment for the poorer households in the sample, and can be then attributed to persistent market failures. They are in line with the findings of Field and Torero (2006) that indicate the relationship between an improved ownership rights and greater access to the credit market is ambiguous in situations of poverty, because other restrictions also remain important. The policy implications of these results are crucial as the change in tenure status is necessary but not sufficient for the poorest among the poor to become investors. Therefore these types of programmes need to be complemented with other policy measures.

5. Robustness to functional form

Given the previously discussed characteristics of the dependent variable, in particular its discrete nature, OLS may not be an appropriate approach based on methodological grounds. Therefore, we test the robustness of its results by employing alternative econometric estimators which will illustrate the importance of the functional form.

5.1. Ordered Probit models

The goal of this section is to estimate the ordered probit model, derive the ATT from its estimates and compare the results to those from the previous OLS model. Since the investment variables takes integer values $0,1,2,\dots,J$, the expected value of investment

conditional on treatment status (before vs. after; treated vs. non-treated) based on the ordered probit is equal to

$$E[y|X, status] = \sum_{j=0}^J \Pr(y = j|X, status) \cdot j.$$

Thus, first we estimate the ordered probit and predict the probability associated with each level of investment given treatment status. The expected value of investment, for each of the four treatment status groups, is then calculated by multiplying the probabilities obtained in the previous stage, by their corresponding level of investment. Then, the average difference-in-differences for the treated individuals is computed using the same definition as above (see eq. 3), and bootstrapping is used to calculate the standard errors of the last expression.

Results from the ordered probit models are shown in Table 3. As can be seen, the results are significantly lower than those obtained by the previous model, which may indicate that, as argued before, the functional form is important. However, even if OLS tends to overestimate the impact of the title, the main conclusions remain.

An interesting additional analysis can be done by estimating the dif-in-dif probabilities associated with each level of investment. The results obtained will indicate the impact of titling, first, on the probability of changing the condition from non-investor to investor and second on how this probability is distributed among the different levels of investment. In that sense, getting the title leads to an increase in the likelihood of at least one investment by about 4.6 percentage points for the first control group and 11.2 percentage points for the second (values associated with zero investment). The increase in the likelihood of investment is distributed among the different levels of non-zero investment, with the major part concentrated in going from zero to one investment. This suggests the main effect of titling is in moving non investors to make one investment. The probability of exactly one investment increased by 2.2 and 5.1 percent for the first and second control group

respectively, while the one of exactly two investments more than halves. This indicates that the effect of the title tends to decrease as the level of investment increases.

Table 3: Ordered probit difference-in-difference models^{1/}

	Total investment		Short-run investment		Long-run investment	
	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas
ATT	0.1037 (0.069)	0.2595*** (0.061)	0.0546 (0.081)	0.1991*** (0.061)	0.0611*** (0.021)	0.0583*** (0.015)
DiD [Pr(Inv=0)]	-0.0462	-0.1118	-0.0275	-0.0966	-0.0398	-0.0392
DiD [Pr(Inv=1)]	0.0220	0.0513	0.0142	0.0478	0.0258	0.0266
DiD [Pr(Inv=2)]	0.0100	0.0220	0.0057	0.0188	0.0069	0.0060
DiD [Pr(Inv=3)]	0.0054	0.0155	0.0034	0.0140	0.0072	0.0066
DiD [Pr(Inv=4)]	0.0035	0.0094	0.0021	0.0078		
DiD [Pr(Inv=5)]	0.0030	0.0085	0.0021	0.0081		
R squared	0.0425	0.0351	0.0398	0.0313	0.0867	0.0783
Obs	1090	1120	1090	1120	1090	1120

Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level

^{1/} Including complete set of control variables.

5.2. Count data models

As seen above, the estimates of the average treatment effects can be sensitive to the choice of functional form. Treatment effects based on the ordered probit model are somewhat smaller than those based on OLS. We now consider the results from a count data model, which may be more appropriate than OLS and ordered probit, given that the investment variables in the data come as counts. Again, the goal is to estimate the ATT, following the same methodology described above. Thus, we estimate a count data model, obtain the predicted number of investments given treatment and no treatment, and then compute the average difference-in-differences for the treated individuals. As before bootstrapping is used to estimate the standard errors.

In count data models the behaviour of the variance is crucial. The basic Poisson regression restricts the variance-mean ratio to be unity. To test if this is supported by the data, we employ a likelihood ratio test for overdispersion. The outcome provides statistical evidence against the assumption that the variance is equal to the mean. Hence, it is preferable to employ the negative binomial maximum-likelihood regression model instead of the Poisson one.

Table 4 shows the results of this model. Similar to the ordered probit, these results tend to be lower than those obtained in the OLS model, with the OLS results being in between 0.19 and 0.31, and the ones in the present model are in between 0.17 and 0.26; a similar pattern can be observed in the remaining components. They also present a better explanatory power in the case of the long-run investment as well as a larger effect.

Table 4: Negative binomial difference-in-difference models^{1/ 2/}

	Total Investment		Short-run investment		Long-run investment	
	Control group 1: Programme areas only	Control group 2: Benef and non-benef in non-prog areas	Control group 1: Programme areas only	Control group 2: Benef and non-benef in non-prog areas	Control group 1: Programme areas only	Control group 2: Benef and non-benef in non-prog areas
ATT	0.1789* (0.098)	0.2643*** (0.078)	0.1172 (0.103)	0.2021*** (0.067)	0.0461*** (0.011)	0.0440*** (0.014)
R squared	0.0280	0.0258	0.0252	0.0226	0.0685	0.0624
Obs	1090	1120	1090	1120	1090	1120

Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level

^{1/} Including complete set of control variables; ^{2/} In the case of the long-run investment, marginal effects for treated are reported because bootstrapping failed for this specification.

5.3. Propensity score matching models

Up until now we have relied on fairly traditional econometric techniques to estimate the treatment effects of interest. There is some evidence that the simple OLS dif-in-dif estimator gives atypically high estimates of the treatment effects, compared to the ordered

probit and the count data models. We now tackle the issue of functional form in a slightly different way, using propensity score matching. This estimator is attractive, primarily because it is non-parametric in the second stage (at which point the average treatment effects are estimated), and so at that stage very little structure is imposed on the functional-form relationship between treatment and outcomes. Another advantage is that with this estimator there is no extrapolation. In order to ensure the results are comparable to those reported in previous sections, we combine propensity score matching and difference-in-differences, following Gilligan and Hoddinott (2007).

Due to the different characteristics of the two control groups, the probit models used to generate the propensity scores have been estimated separately. Thus, for the first control group the household's probability of being selected by the programme is modelled, while for the second control group we model the probability that the community gets selected. Probit results are shown in Annex 1. For both control groups, variables that according to the programme's guidelines explain the treatment selection are highly significant. In the probit based on the first control group, the number of months of tenure has a positive association with treatment, while households that also have a municipal title – the closest substitute of Cofopri's title among the other possible titles – are considerably less likely to be treated. In the probit based on the second control group, city size, informality density and the distance to commercial centres have a positive sign. This suggests that the programme staff, with the objective of increasing the impact of the project, chose firstly bigger but remote communities in which many households are without a title.

Moreover, these results can provide us more information about the scope and targeting of the programme. On the one hand, some relationships from the probit based on the first control group can suggest that the programme focused on relatively disadvantaged households. For instance, the probability of participating in the programme tends to decline when the head of household's education and the monthly income rise. Also, the number of children is positively associated with the likelihood of selection. On the other hand, some

results based on the second control group point in the opposite direction, for example the positive relationship between the treatment and the community's public light. This specific result could be associated more with the preference for those areas that are easier to formalize than with the impact objective.

Table 5 shows the estimated average treatment effects, based on propensity score matching. There are few observations – no more than 27 in any case – that did not have a common support and were dropped for the matched sample, indicating the control groups are appropriately configured. The results show a positive impact of titling in all the cases. Particularly, the estimated effects on long-run investment are clearly significant and large. They are close to 0.06, which is equivalent to an increase of 170-200 percent in the number of housing sizeable additions.

Table 5: Dif-in-dif propensity score matching models^{1/}

	Total Investment		Short-run investment		Long-run investment	
	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas
ATT	0.1297 (0.098)	0.1921** (0.110)	0.0685 (0.082)	0.1345 (0.098)	0.0612** (0.029)	0.0580** (0.025)
Obs	1090	1120	1090	1120	1090	1120

Bootstrap standard errors are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level

^{1/} Including complete set of control variables.

Summing up, even though alternative models employed to improve the baseline estimation lead to changes in the estimated treatment effects, the main conclusions remain, which is that Cofopri's titles have a positive impact over all the components of investment. Interestingly, and in contrast to the results reported by Field (2005), the treatment effects appear particularly significant regarding long-run housing additions.

6. Dynamic response

Since the programme was implemented in stages between the different regions, the analysis above does not shed any light on how the effects of titling evolve over time. In other words, although we do know that Cofopri's title impact positively on investment, we do not recognize if this impact tends to be immediate or if it takes time to be relevant. As far as we are aware, this issue has been ignored in the empirical literature, yet this is potentially important. For example, recall that Field (2005) fails to find any significant effect of titling on long-run investment. It may be that this is because there is not enough time in Field's data between the implementation of the programme and the realization of its full effects. Our data span a longer time period and so provide a better basis for looking into this issue. To do so, we modified slightly the analytical framework and, using information of housing investment in the subsequent periods after the programme, construct the temporary investment behaviour of each region. Considering as time zero the two years prior the treatment, we generate a binary variable of any investment in two-year periods and compare each of them with the pre-programme baseline.¹³ This analysis can provide some useful insights about the timing not only of the programme's impact but also of its correct evaluation.

Table 6 reports the results of the dynamic analysis. We distinguish between average treatment effects after the first, and the third, 2-year period after the programme (2-y Period 1, and 2-y Period 3, respectively).

¹³ Unfortunately that means that as time passes, those regions in which the programme started later do not have observations in final periods.

Table 6: Probit difference-in-difference dynamic models^{1/ 2/}

	Total Investment		Short-run investment		Long-run investment	
	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas	Control group 1: Programme areas only	Control group 2: Benef and non- benef in non- prog areas
ATT (2-y Period 1)	0.0711** (0.037)	0.0043 (0.033)	0.0688* (0.037)	0.0052 (0.033)	0.0086 (0.009)	-0.0019 (0.007)
ATT (2-y Period 3)	0.0196 (0.055)	0.1351*** (0.053)	0.0087 (0.053)	0.1195** (0.051)	0.0655** (0.045)	0.0479** (0.029)

Adjusted standard errors for intragroup correlation are reported in parenthesis; only significant results are presented; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level

^{1/} Including complete set of control variables; ^{2/} Marginal effects are reported in the table.

In the case of total and short-run investments, the impact of title on housing renovations is significant even in the following two years after the programme. On the contrary, title enhances the probability that a household makes a long-run investment by 6.6 or 3.2 percent, but only four years after of being treated. Thus, households appear not to react promptly to the incentive provided by the title due to the greater magnitude of this kind of investment. According to these results, a considerable horizon of time is required for it to be possible to measure the complete impact of a titling programme. These results can also explain partially the divergence with the findings of Field (2005) and those in the present paper.

7. Final remarks

Nowadays, many governments have started land-titling programmes as an instrument to enhance investment and to reduce poverty in urban slums. The Peruvian government has developed one of the largest titling programmes targeted at urban areas in the developing world, with more than 1.5 millions property titles given until now. Nevertheless, there is little empirical research on the impact of these programmes. Among these, Field (2005) analyzed the Peruvian case and found that the resulting investment is limited only to small housing renovations and that there is no considerable impact on long run investments. This

is a surprising result, given the emphasis that theoretical literature provides to the impact of property rights on long-term investment.

Using a more up-to-date data-base, as well as a more complete set of control variables, we also found a positive relationship between Cofopri's title and housing investment. However, whereas the impact of the programme on short-run renovations seems to be not significant in some cases, the title presents a highly significant and larger effect on long-run investment. The robustness of these results was tested using different econometric estimators, which are arguably more suitable than OLS given the discrete nature of the dependent variable. The results from the dif-in-dif propensity score matching indicate that the estimated average treatment effect implies an increase in the number of housing sizeable additions by 170-200 percent. These results contrast substantially to those obtained by Field (2005), but are in line with what we expected theoretically, that is that having a title reduces distortions that favours less risky short-run investments produced by tenure insecurity

It is also interesting to highlight that the impact of the programme is different depending on the level of income. The results indicate that as the household's income increases, the significance and the coefficient associated with the effect of titling also rises, particularly in long-run investment. This suggests that, besides risk, other barriers which limit investment among the poor exist and can then be attributed to persistent market failures.

Additionally, Cofopri's title seems to be more relevant to the household decision of whether to invest or not, rather than to the level of investment. Hence, the increase in the probability of becoming an investor is concentrated principally in going from zero to one investment. The effect of the title tends to decrease as the level of investment increases.

With respect to the timing of the impact of the titling programme, our dynamic analysis shows that while the effects on housing renovations can be significant even in the following

two years after the implementation, its impact on long-run investment requires more than four years. This result has serious implications for the evaluation of programmes of this kind, suggesting that, in order to measure its total impact, a considerable time-horizon is needed.

Finally, following the limitations of the available data on this particular issue, one might encourage similar researches to be – quantitatively and qualitatively –, ventured in a future. Collecting new panel data sets can allow further research to produce more accurate estimations. Also, more specific data-bases can provide information on how to split the total impact among its different components – reduction of risk of expropriation, access to the credit market and capability of trading the asset.

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Annex 1: Probit models for participation in titling programme

	Control group 1: Programme areas only	Control group 2: Benef and non-benef in non-prog areas
Family Size (number of members)	-0.0117 (0.027)	-0.0508* (0.026)
Number of children	0.0274 (0.039)	-0.0499 (0.042)
Head of household's age	-0.00005 (0.004)	0.0015 (0.004)
Head of household's sex +	-0.1308 (0.099)	-0.1617* (0.096)
Head of household's education	-0.0151 (0.025)	0.0285 (0.026)
Lot size	-0.0008 (0.001)	-0.0008** (0.0004)
Monthly income	-0.0001 (0.0001)	0.0001 (0.0001)
Household obtain the property by intrusion +	-0.1059 (0.094)	0.2312** (0.094)
Possession of substitute title (municipal title) +	-0.3914*** (0.089)	
Residential tenure	0.0009** (0.0004)	-0.0011*** (0.0004)
Community's public light +	0.5618*** (0.189)	1.0410*** (0.164)
Distance to seven different communal services	0.0002 (0.001)	-0.0019** (0.0008)
Density of population	0.00002 (0.00001)	0.000003 (0.00001)
Distance to commercial centre	0.0043 (0.003)	0.0099*** (0.003)
Informality	0.0915 (0.352)	1.0103 (0.371)
Regional dummies		
Lima north +	-0.3652** (0.171)	0.2039 (0.143)
Lima south +	-0.1719 (0.202)	0.0645 (0.158)
Lima east +	-0.2945* (0.166)	-0.2106 (0.133)
Arequipa +	(dropped)	0.9838*** (0.178)
Trujillo +	-1.0653*** (0.153)	(dropped)
Obs	1090	1120
R squared	0.0998	0.1103

Notes: 1. Dependent variable equals one if household receive a Cofopri's title. 2. Results are presented as the change in the probability of obtaining a title for an infinitesimal change in continuous control variables and as the discrete change for dummy variables (+). * indicates 10% significance level; ** indicates 5% sign. level; and *** indicates 1% sign. level