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Heterogeneous Effects of Property Rights on Housing Investment in Urban Peru

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Abstract

Empirical results reported by Field (2005) indicate that improved property rights tend to raise average housing investment among poor urban households in Peru. We investigate if this effect varies across households with differing incomes, how it evolves over time, and whether heterogeneous expectations about future tenure security matter for the estimated effects. The results indicate that the investment response among the poorest households represented by our sample is weak and not significant. Among households with higher incomes, the response is quantitatively large and statistically highly significant. The results further indicate that it may take several years until the response of long-run investment to reformed property rights can be found in the data. Finally, even though expectation of treatment affects the behaviour of non-treated households, the treatment effect changes only slightly when we take into account expectations in our previous estimations, indicating that our results are robust to this problem.

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1 Introduction

There is consensus among economists that sound property rights are crucial for economic growth and development in poor countries (e.g.Demsetz (1967), North and Thomas (1973), North (1981), Johnson et al. (2002)). One mechanism through which property rights drive growth is investment (Besley, 1995). While there is a large empirical literature investigating the effects of tenure security on agricultural investment in poor rural areas, little is still known about the impact of property rights on the investment decisions of households in urban areas.[1] Given that millions of individuals in poor urban areas of developing countries occupy dwellings without having a title, and that numerous titling programmes have been implemented recently across a wide range of developing countries, documenting the impact of property rights on residential investment in urban areas is an important task.[2] Field (2005) makes a significant contribution, documenting the positive and sometimes large average effects of a nation-wide titling program on housing investment among urban squatters in Peru. Galiani and Schargrodsky (2010) carry out an analysis in the same vein based on data on urban squatters in the Buenos Aires area, and find a positive average effect of titling on housing investment. Property titling thus appears to be an effective policy instrument for spurring residential investment and raising the standard of living among the urban poor.

The research by Field (2005) and Galiani and Schargrodsky (2010) has been very valuable in documenting the average effect of titling programmes on housing investment in urban areas. These papers do not shed light on whether the effects of better property rights on investment vary across households or over time, however. Intuitively, it seems quite plausible that the effects of more secure property rights on investment may be heterogeneous, depending on characteristics such as household composition or income. Adding a room to an existing house, for example, is a lumpy investment that may be difficult for poor households to make, due to a lack of savings and poor access to credit. It is also possible there is heterogeneity over time in the effects of titling on investment. Indeed, Field's (2005) results suggest that improved property rights

[1]The paper by Besley (1995) has been very influential. For an overview of the impact of property rights on economic development, see Besley and Ghatak (2009). For potential problems in the susteinability of titling programs, see Gutierrez and Molina (2016).

[2]Titling programs have been considered as one of the most effective governmental instruments for reducing poverty and promoting investment (Baharoglu, 2008; Field, 2007). In this context, there have been urban titling programmes in Angola, Egypt, Ghana, Malawi, Senegal, South Africa, Turkey, Afghanistan, Cambodia, India, Indonesia, Laos, Philippines, Argentina, Brazil, Bolivia, Colombia, Ecuador, Mexico, Honduras, Paraguay and Peru (Galiani and Schargrodsky, 2010; Durand-Lasserve et al., 2007).

impact more on short-run investment (e.g., painting a wall) than on long-run investment (e.g., building an extension). At first glance, this appears to be a puzzling result, since it is primarily long-run investment that theory predicts should respond strongly to better property rights. However, the post-reform period in Field's data is less than four years long. It is conceivable that the response of major household investment to the reforms involves lags.[3]

In this paper we use data on urban squatters in Peru to investigate if the effect of tenure security on housing investment varies across households with differing incomes, how it evolves over time, and whether heterogeneous expectations about future tenure security matter for the estimated effects. The government titling programme in Peru is one of the largest in any developing country targeted to urban areas. Between 1996 and 2016, more than 2.5 million property titles were recorded by the Peruvian government, which benefit at least 8 million inhabitants of marginal communities (Cofopri, 2018). Moreover, as is clear from our data, the urban poor in Peru are a very heterogeneous group. For example, average monthly household income per person is 45 dollars; for about 28% of households, income is less than half that, and for 9%, it is more than twice as high. Our data set spans a longer post-reform period than that covered by Field's (2005) data; hence, we have a better chance of discovering the effects on long-term investment if, as suggested above, households responded to the reform slowly.

Based on our empirical analysis we find evidence that titling effects on housing investment are stronger among households with relatively high incomes than among the poorest individuals. In fact, in most cases we cannot reject the hypothesis that titling has no effect on investment among those with the lowest incomes. A similar result holds for education: the effects on investment are small and statistically insignificant among individuals with little formal education, and large and statistically significant among those who have completed at least primary education. The data thus indicate that those most disadvantaged from a socio-economic point of view have gained the least (possibly nothing) from the titling program in Peru. This is a troubling finding for policy makers concerned with the effects of improved property rights on household investment among the poorest households in the urban population. We also find that significant lags are involved in the response of long-term investment to the reform. Contrary to Field (2005), we find that large long-term investments do respond to improved property rights, but only after about four

[3]For example, it likely takes time before poor households can accumulate the cash necessary to finance such an investment and perhaps the confidence to go ahead with it. If so, that is one possible explanation as to why Field's results indicate that the effect of titling on long-term investment is weak.

years after the reform. We conclude that Field's estimates are best interpreted as measuring the immediate (short-run) impact of the programme and that they likely underestimate the total effect of the titling program on long-run investment.

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 presents the data and discusses the identification strategy for the empirical analysis. Results are presented in section 4. The role of expectations is considered in section 5. Section 6 contains our conclusions.

2 The Peruvian titling programme and its selection criteria

The Peruvian government has been developing property rights reform since 1996, seeking to improve tenure security for urban settlements. The large informality in Peru's urban areas 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, 2007). As a result, estimates found more than three million informal properties in 1997 (World Bank, 2006).

In 1996, the 'Committee for the Formalization of Private Property' Cofopri (2018), a government agency 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 of the costs from the titling programme. To obtain a title, one only has to show that he has resided in an elegible public area since before 1995[4] and that he has no other proper title (Field, 2007).

Cofopri established a massive titling procedure in the targeted neighbourhoods[5]. In order to determine which cities were to be treated in a staggered implementation, programme staff

[4]Archeological sites and flood plains are the most important ineligible areas (Field, 2007).
[5]See Morris (2004) and Field (2007) for more details on the implementation of the titling programme.

considered the following selection variables: city size, density of informality and distance to commercial centres. Those variables were employed with the purpose of increasing the impact and reducing the costs of formalization (Field, 2007; Morris, 2004).

3 Empirical analysis

3.1 Data

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 on the tenure status of 2331 properties, 836 of which have a title given by Cofopri. All of the communities in the regions included in the data base were meant to be considered for 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 (2008) have argued, ex-post cross-section data can be used to evaluate programmes if those data incorporate retrospective questions about the intervention[6] 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 over 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

[6] To minimize any potential recall bias, having an anchor question is recommended. The project itself is a very useful way of anchoring. In addition, respondents must give information on the key variable before and after the programme (Field and Kremer, 2008).

2001 and 2002[7].

In defining the baseline, it is possible that we are picking data points too close to the period of the reform. If this is indeed 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 group[8] (and so the growth in investment between the 'before' and 'after' periods among 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 they are long-run if they involved adding another story or constructing a bedroom or other rooms[9].

3.2 Identification strategy

The main purpose of this empirical analysis is to identify the effects of titling on housing investment. Identifying these effects requires controlling for any systematic heterogeneity between beneficiaries and comparison groups that can affect investment behaviour but not due to the programme. As mentioned, Cofopri first targeted the areas to be treated and then implemented a massive titling programme in these areas, granting a title to all households that fulfilled the requirements. The fact that the programme provided, as Field (2007) has argued, a massive

[7] 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 that we should use a period long before the implementation of the programme. 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.

[8] Because the programme just started this year in each region with only a few 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.

[9] 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. Therefore the investment variable is discrete and, because is quite common not to develop housing investments every year, its distribution is concentrated in few outcomes different from zero. Given these characteristics, we have also employed count data models in order to check the robustness of the functional form. In all cases, the results are similar.

cost-free amelioration in tenure security unrelated to demand helps to reduce any potential endogeneity problem. However, there are still some factors that can complicate efforts to estimate causal effects. In particular, it is possible that there could be unobserved heterogeneity correlated with eligibility or with location (due to the different timing in which Cofopri reached each community) that influences housing investment. This is particularly important since the intervention does not seem to have been performed randomly. In the case of the implementation among beneficiary areas, 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 53 in 1996 to 190 US dollars in 2003). Our identification strategy tries to mitigate potential biases produced by these endogeneity problems.

Our treatment group differs from the comparison group in more than one dimension. To remove these differences and to address other potentially confounding factors, we adopt a difference-in-difference-in-difference (DDD) strategy that allows us to control for persistent characteristics of treated areas, eligible households and time. In this strategy, we compare titled households to control households that also lived in treated areas and measure the change in outcomes in these areas relative to the difference in non-treated areas. By doing this, we are able to allow for an effect of being eligible as distinct from being actually treated, as well as the particular effect of living in treated areas. Failure to do DDD (e.g., if we do difference-in-difference instead) could thus result in omitted variables bias, since we would not be controlling for the possibility that eligible households (or treated areas) have investment patterns that are different from those of non-eligible households (non-treated areas). In fact, we suspect that this could be the case. Since one of the requirements for being eligible is length of residency, it is more likely that non-eligible households need to invest more in housing. Similarly, the programme first focused on more consolidated communities, which also could be associated with lower natural investment trends.

Also, in order to verify that our results are not driven by community-specific characteristics, we include in our estimations city dummies to control for heterogeneity among them. Additionally, we include as covariates those selection variables that can explain eligibility to obtain a title (length of residency and non-possession of other proper title), as well as the variables that were considered in the selection of the cities in each stage of the programme (distance from commercial centres, and 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). However, our identification is not able to control for time-varying effects in the covariates due to the characteristics of our data.

In our empirical analysis, we exploit the panel dimension of the investment variable with the cross-section nature of the control variables to account for unobserved differences between households for being eligible, regions for different timing of being treated and time by estimating a DDD model. Let θ_t be a time dummy that has a value of zero before the programme and one after it, while E_i and A_i are a household elegibility dummy and a dummy for treated areas, respectively. X_i is a vector of observable characteristics, which include household demographic variables, community characteristics, city dummies and variables related to programme selection (at the household and area levels). The expression for the investment level is the following:

$$\begin{aligned} y_{ijt} = \alpha_1 + \alpha_2 E_i + \alpha_3 \theta_t + \alpha_4 A_j + \alpha_5 X_i \theta_t + \alpha_6 E_i \theta_t \\ + \alpha_7 A_j \theta_t + \alpha_8 E_j A_j + \alpha_9 X_i A_j + \alpha_{10} E_i A_j \theta_t + \mu_{ijt} \end{aligned} \tag{1}$$

This expression also includes the second-level interaction between dummies and a differentiated behaviour of control variables according to location (α_9 in the expression above). The coefficient on the interaction between time, eligibility and location dummies (α_{10}) is the estimated programme impact, which is the conditional average effect of titling on treated households. The expression finally estimated is the first difference of the equation above.

Additionally, we employ a difference-in-difference (DD) estimation in order to compare our results with those obtained previously by other authors. For more robustness, two different control groups are used. 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 fulfill all of 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.

The employment of two control groups can be useful, since both potentially suffer from different biases. To reduce this problem, we incorporate as controls those variables associated with programme selection at the household and area levels. In particular, 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 for obtaining a title. Additionally, the second control group gives us a chance to have a comparison group that is not contaminated by this potential selection bias, since it considers only those households that would have been treated if the areas in which they live had been reached by the programme. Nevertheless, this control group suffers from a programme timing bias due to the non-random implementation of the programme. To reduce this potential problem, our analysis also includes the variables that were considered in the selection of the cities by the programme (e.g., distance, city size and concentration of informality). However, none of these estimations allow us to control for differences between eligibility and location at the same time, as the DDD strategy does.

Since control 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 of the sample. There are no large disparities among groups. However, while households in treated areas report very similar values for almost all of the variables, those households in non-treated areas present some divergence. In particular, these households seem to be poorer and less educated on average than beneficiaries. This suggests that controlling for these variables in the empirical analysis is important.

As to the investment behaviour between beneficiaries and control groups, Table 1 indicates how it differs 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. The table also reports the differentiated investment pattern between eligible and non-eligible households, as well as between treated and non-treated areas. This reinforces the importance of using a DDD analysis in this particular case.

4 Results

Table 2 presents the DDD results. They show a large and significant impact of Cofopri's title on total housing investment. The estimated average treatment effect is 0.51, which means that being treated implies that the expected number of investments increases by 0.50 approximately. Compared to the outcome 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 more than 200 percent on average as a result of receiving treatment[10].

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., the addition of another story, the construction of bedroom and/or another room). We do this primarily in order to probe Field's (2005) 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) column 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 0.15 and highly statistically significant. Again, this is a large effect, given the low baseline: an increase of 0.15 implies an increase of more than 350 percent. As for short-run investment, we also obtain a large and statistically significant effect of 0.34, which means that getting a title increases the number of investments by 200 percent. Although a large effect, it is nevertheless dwarfed by the long-run effect, as we have already seen (in percentage terms that is).

Also, we analyse the impact of titling in housing investment by using dif-in-dif models and two control groups, in order to be able to compare our results with those obtained previously by other authors. In particular, results reported in Table 3 are quantitatively similar to those of Field (2005) for total housing investment. In this case, the average treatment effect is 0.20 based on control group 1 and 0.33 based on control group 2. Results also suggest a story similar to the one presented above: Cofopri's titles have a positive impact on all the components of

[10]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.

investment and, in contrast to Field (2005), it appears to be particularly large and significant regarding long-run housing additions. However, the coefficients associated with the treatment effect tend to be lower in these estimations than those in DDD models. Not controlling for different investment patterns between eligible and non-eligible households, as we are able to control in the DDD framework, seems to generate lower coefficients. This can be explained by the fact that being eligible to receive a title is associated with length of residency. Consequently, the dwellings of non-eligible households have been inhabited for less time and, therefore, they are more likely to need more investment.

The greater impact of Cofopri's title on sizeable investments found in our results can be explained by different mechanisms. We can expect that property rights enhance this particular type of investment by increasing not only the incentives to invest, but also the ability to do it (via a greater access to the credit market or more wealth). In the former case, since the long-run component represents a major irreversible investment, the greater impact of Cofopri's title might be seen as evidence of the importance of titling for reducing the risk of expropriation amog urban poor households. In fact, this seems to be an important issue: evidence suggests that untitled households feel much more insecure about the possibility of eviction and conflict than their titled counterparts[11]. As a result, tenure insecurity distorts investment decisions in favour of small investments, because they imply less risk. In a similar vein, recent evidence suggests that titling might reduce risk aversion due to less background risk (Aragon et al., 2017). In the latter case, large investments would be precisely more benefitted from the increasing wealth associated with titling[12] and the greater access to credit market[13].

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. Probably the most important reason for this is that 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

[11]According to a similar dataset collected by COFOPRI, the perception about the possibility of not been evicted and not have been experienced a conflict about the property is statistically significant larger in treated than untreated households (93% and 96% vs. 56% and 84%, respectively).

[12]The wealth effect can be attributed to many other positive impacts of titling that have been reported in the literature, such as the increase of household's labour supply, family size, education, health (Field, 2007; Galiani and Schargrodsky, 2010; Vogl, 2007). More wealth, of course, would imply a greater investment on housing, since, as Galiani and Schargrodsky (2010) emphasized, it is a normal good.

[13]Due to data limitations, we are not able to disentangle the mechanisms through which property rights affect housing investment.

funding for new (often expensive) long-run investment, and it is also possible it takes some time before individuals are fully convinced that 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 reducing the risk of expropriation among urban poor households. Tenure insecurity distorts investment decisions in favour of short-run investments, because they imply less risk.

4.1 Heterogeneous effects by level of income

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 included interactions for each quartile of income in the DDD models[14]. Our results, shown in Table 4, 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 of 0.79). In contrast, for less poor households the effect is much larger and statistically significant.

These results suggest that other barriers, besides risk, exist, and those barriers 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 that the relationship between 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.

4.2 Heterogeneous effects: dynamic response

Since the programme was implemented in stages in 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 know that Cofopri's title impact positively on investment, we do not recognize if this impact

[14]Similar results can be found by using the DD models.

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, we construct the temporary investment behaviour of each region. Considering the two years prior to the treatment as time zero, we generate a variable of the number of investments in two-year periods and compare each of them with the pre-programme baseline.[15] This analysis can provide some useful insights about the timing not only of the programme's impact but also of its correct evaluation.

Table 5 reports the results of the dynamic analysis. 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 12.6 percent, but only four years after 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 time horizon is required before it would be possible to measure the complete impact of a titling programme. These results can also partially explain the divergence of the findings in the present paper with the findings of Field (2005).

5 The role of expectations

As with this evaluation, the staggered implementation of programmes is commonly used as an opportunity to construct correct control groups (Ravallion, 2009). This relies on the assumption that there is no contamination effect produced by the anticipation of joining the programme of non-treated units. If individuals in the comparison groups think that they may participate in the programme in the future, they might alter their current behaviour in anticipation of doing so. If that were to be the case, we expect that estimations of treatment effects could be downward

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

biased.[16] Researchers are aware of this potential problem, and it is increasingly recounted in the literature (Malani and Reif, 2015; Banerjee and Duflo, 2008; Attanasio et al., 2012). However, it is particularly difficult to test empirically because expectations are commonly unobservable.[17]

Our data set allows us to verify the magnitude of this problem and revise the robustness of our previous results, since it provides specific information about expectations of non-treated households. In particular, households without a title (which exclude those who previously had a title[18]) were asked how long they tough it would be before they received a title. Given that information, we construct a binary variable for the expectation of treatment that absorbs the differential behaviour of those who expect to benefit immediately (who answered in less than one year) from those who believe that they would be treated later or never (i.e., in more than one year, never or do not know). In that sense, we believe that it is the former group that could presumably change its current behaviour due to the anticipation of joining the programme in the near future.

In order to test whether households that expect the treatment soon have modified their current decisions, we employ a strategy similar to that in a treatment evaluation. We estimate the DD model to explain investment among non-treated households, using the expectation of treatment instead of the treatment itself.[19] Thus, we are able to examine whether this expectation really influences the investment decisions of those households. This problem could be particularly important because this programme has been extensively advertised by the government.

Table 6 reports these estimations. Results show that the expectation of treatment does affect the behaviour of non-treated households.[20] In particular, we found that this effect is not only limited to the group that includes future beneficiaries but also to the group of households that were not treated when the programme reached their communities.[21] However, the impact

[16] In fact, since individuals in the control group change their behaviour in the same direction as those in treatment group, we could expect a downward bias. Even though this potential issue does not threat the validity of my previous results, it could lead to an underestimation of the impact of titling on housing investment.

[17] Attanasio et al. (2012), in the context of a structural model, provide evidence that the so-called announcement effect in the Progresa control villages is important.

[18] This excludes not only those that did not receive the title from the programme but also those that had previously had a proper title.

[19] We consider in these estimations those households that expect the treatment as treated units and those that do not expect it as comparison ones.

[20] We find similar results by using the DDD estimations.

[21] Since the programme has not finished yet, some households from the first control group could still expect to be treated in the future, even though their communities have been previously reached (which means that they were not eligible at the time of the intervention). Nevertheless, it is not necessarily true that households

differs between both groups. The expectation of treatment encourages future beneficiaries to undertake sizeable housing additions that involve a considerable amount of money, a decision that requires a strongly credible expectation. On the contrary, non-treated households in areas reached by the programme decide to embark only on short-run investments (i.e., minor housing renovations).

Given our findings about the importance of the expectation of treatment in the behaviour of some households in the control groups, we are interested in re-calculating the previous results and, therefore, measuring if there is any bias produced by this phenomenon. Thus, we re-estimate the previous models including as a control a variable that absorbs the differential behaviour of those particular households that expect the treatment. Table 7 presents these results. Regardless of the relevance of expectations in non-treated households behaviour, the treatment effect changes only slightly when we incorporate expectations into our DDD estimations, indicating that they are robust to this problem.

In the case of the DD estimations, we find that the coefficients associated with the treatment effect consistently tend to increase, both in magnitude and significance, in line with the sign of the expected downward bias. However, the magnitude of this change is not enough to be considered a serious concern vis-a-vis our previous results.

Since it is particularly difficult to measure expectations of being treated, there is very little evidence about the effect of expectations on treatment evaluation. Our results provide some empirical evidence about this phenomenon. Although the inclusion of a variable that quantifies expectancy implies a change in the coefficient associated with treatment, this does not seem to be very important in magnitude. This implies that anticipation bias is not enough to be considered a threat, at least in this context. Nevertheless, this is still an ongoing debate. The fact that we found an effect of expectations on the investment behaviour of non-treated households suggests that we should take into account this issue when we plan an evaluation. In that sense, it is important to check carefully the choice of the regions used as control groups or even consider the addition of questions associated with the expectation of treatment in the surveys, when we expect a general anticipation of the programme.

completely understand the rules followed in the intervention. However, we can expect that the effect on these households tends to be lower than that in those households that really are 'future' beneficiaries.

6 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 to date. 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 the theoretical literature places on the impact of property rights on long-term investment.

Using a more up-to-date data base, we also found a positive relationship between Cofopri's title and housing investment. The impact of titling is large and highly significant not only in the case of short-run investment but also in the long-run case. In particular, results from the DDD strategy indicate that the estimated average treatment effect implies an increase in the number of sizeable housing additions by 350 percent. These results contrast substantially with those obtained by Field (2005), but they are in line with what we expected theoretically, that is, that having a title reduces distortions that favour 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 households' income increases, the significance and the coefficient associated with the effect of titling also rises, particularly in long-run investment. This suggests that, in addition to risk, other barriers that limit investment among the poor exist and they can be attributed to persistent market failures.

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 two years following the implementation of the programme, its impact on long-run investment takes 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, we found that the expectation of treatment affects the behaviour of non-treated households. However, when we take this effect into account in our estimations, the treatment effect changes only slightly, indicating that our results are robust to this problem.

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Statistical Appendix

Table 1: Summary statistics

	Treated Areas		Non Treated Areas	
	Benef.	**Non Elegible**	**Elegible**	**Non Elegible**
Households Characteristics				
Number members	4.82	4.75	5.21	4.39
	(1.98)	(1.89)	(2.11)	(1.84)
Number children	1.42	1.35	1.59	1.53
	(1.29)	(1.43)	(1.38)	(1.17)
Age Head	46.05	45.56	47.10	39.66
	(13.29)	(13.39)	(13.18)	(12.55)
Sex head (% of female)	25.5%	28.7%	33.5%	35.4%
	(0.44)	(0.45)	(0.47)	(0.48)
Education head (in years)	9.29	9.33	8.45	8.79
	(4.33)	(4.32)	(4.14)	(3.91)
Monthly income (in current US$)	185.73	185.34	168.90	143.01
	(180.0)	(151.78)	(121.46)	(118)
Home Characteristics				
Lot size (m^2)	150.12	154.12	157.57	161.25
	(68.01)	(94.16)	(222.84)	(97.0)
Residence (in years)	17.21	15.77	19.57	6.87
	(11.64)	(11.78)	(11.54)	(7.88)
Water conection	65.0%	72.2%	43.7%	28.7%
	(0.48)	(0.45)	(0.49)	(0.45)
Electricity	99.8%	98.9%	99.5%	98.8%
	(0.05)	(0.11)	(0.07)	(0.42)
Obtaining the property by intrusion	38.2%	42.7%	32.7%	36.5%
	(0.49)	(0.49)	(0.46)	(0.48)
Housing Investment				
Short-run inv. (before prog.)	0.237	0.256	0.223	*0.75*
	(0.76)	(0.80)	(0.63)	(0.47)
Short-run inv. (after prog.)	0.478	0.441	0.327	*0.01*
	(0.98)	(0.73)	(0.71)	(0.98)
Long-run inv. (before prog.)	0.024	0.034	0.023	*0.95*
	(0.22)	(0.23)	(0.19)	(0.15)
Long-run inv. (after prog.)	0.083	0.042	0.033	*0.01*
	(0.38)	(0.25)	(0.18)	(0.28)
Maximum Sample Size	836	356	391	748

NOTE: Standard errors are reported in parenthesis.

Table 2: DDD models

Impact of titling on housing investment

	Total Investment	**Short-run investment**	**Long-run investment**
ATT	0.5068***	0.3394***	0.1452***
	(0.139)	*(0.104)*	*(0.039)*
Obs	1975	1975	1975
R^2	0.0378	0.0437	0.0289

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level.
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 3: DD models

Impact of titling on housing investment

	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	0.2017**	0.3346***	0.1345*	0.1954***	0.1062***	0.0848***
	(0.099)	*(0.093)*	*(0.071)*	*(0.069)*	*(0.031)*	*(0.026)*
Obs	1019	1045	1019	1045	1019	1045
R^2	0.0372	0.0348	0.0264	0.0243	0.0322	0.0290

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level.
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 4: DDD models

Impact of tilting on housing investment by level of income

	Total Investment	**Short-run investment**	**Long-run investment**
ATT in quartile 1	0.4265	0.2794	0.2260***
($> P75$)	*(0.263)*	*(0.194)*	*(0.084)*
	p=0.106	*p=0.150*	*p=0.007*
ATT in quartile 2	0.211	0.1782	0.0907*
	(0.183)	*(0.140)*	*(0.053)*
	p=0.231	*p=0.203*	*p=0.090*
ATT in quartile 3	0.0126	0.032	-0.0106
	(0.175)	*(0.148)*	*(0.040)*
	p=0.943	*p=0.828*	*p=0.791*

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level.
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 5: DDD dynamic models

Dynamic response of the impact of tilting on housing investment

	Total Investment	**Short-run investment**	**Long-run investment**
ATT (2-y Period 1)	0.2148*	0.2407**	-0.0064
	(0.129)	*(0.104)*	*(0.034)*
ATT (2-y Period 2)	0.0934	0.1692	0.0024
	(0.167)	*(0.125)*	*(0.048)*
ATT (2-y Period 3)	0.3674**	0.2167*	0.1259***
	(0.155)	*(0.113)*	*(0.044)*

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level.
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 6: DD models

Impact of expectation of treatment on housing investment

	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
Expectations	0.5769**	0.1278	0.4318**	0.0355	0.0011	0.0798**
	(0.261)	*(0.142)*	*(0.189)*	*(0.115)*	*(0.083)*	*(0.034)*
Obs	112	288	112	288	112	288
R^2	0.2193	0.1186	0.2544	0.0806	0.1098	0.0949

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level. The number of observations corresponding to the estimations with the first control group has reduced because it originally included households who did not receive the treatment because they had a proper title (and these households were not asked about their expectations).
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 7: DDD models

Impact of titling on housing investment (including treatment expectations)

	Total Investment	**Short-run investment**	**Long-run investment**
ATT without expectations	0.5068***	0.3394***	0.1452***
ATT with expectations	0.5010***	0.3434***	0.1455**
	(0.142)	*(0.106)*	*(0.039)*
Expectations	0.0378	0.0438	0.0289
Obs	1975	1975	1975

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level.
Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.

Table 8: DD model

Impact of titling on housing investment (including treatment expectations)

	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 without expectations	0.2017**	0.3346***	0.1345*	0.1954***	0.1062***	0.0848***
ATT with expectations	0.2174** *(0.106)*	0.3882*** *(0.102)*	0.1671** *(0.075)*	0.2108*** *(0.074)*	0.1004*** *(0.034)*	0.1104*** *(0.027)*
Expectations	0.0876	0.1902	0.1818	0.0547	-0.0324	0.0909***
Obs	1019	1045	1019	1045	1019	1045
R^2	0.0373	0.0359	0.0276	0.0245	0.0325	0.0324

NOTE: Adjusted standard errors for intragroup correlation are reported in parenthesis; * indicates 10% significance level; ** indicates 5% significance level; and *** indicates 1% significance level. Controls: (*i*) HH Head Characteristics: Education, Age, Sex; (*ii*) HH Characteristics: Family size, Number of children, Possession of substitute title, Household's monthly income; (*iii*) Plot Characteristics: Plot size, Residential tenure, Obtaining the property by intrusion; (*iv*) Area Characteristics: Public light, Walking distance to seven services, Distance to commercial centre, Population density and informality level; (*v*) Regional dummies.