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Fernando M Aragon,
Oswaldo Molina, &
Ingo W. Outes-Leon

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Can public policies change risk preferences?

The effect of property titling on risk aversion *

Fernando M. Aragón† Oswaldo Molina‡ Ingo W. Outes-León§

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Abstract

Evidence suggests that major events, like war or natural disasters, affect risk preferences. This paper shows that similar effects can also be caused by public policies. Using the case of a large titling program in Peru, we find that this policy reduced risk aversion. The effects are sizeable and seem to be driven by the reduction in background risk associated with improved security of tenure. Our results highlight the potential of public policies to affect human behavior not only by shaping the economic environment, but also by changing individual preferences.

1 Introduction

A growing literature finds that risk preferences are not fixed, but can change due to extreme events such as economic shocks, war, or natural disasters. The results are, however, mixed. Some studies find that after a negative shock, risk aversion may increase, decrease, or remain unchanged.[1] There is also no consensus on the mechanism linking these events to changes in preferences. Some studies suggest this may be due to increase background risk (Cameron

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†Department of Economics, Simon Fraser University, Burnaby, British Columbia, V5A 1S6, Canada; Tel: +1 778 782 9107; Email: faragons@sfu.ca

‡Department of Economics, Universidad del Pacifico, Av. Salaverry 2020, Lima 11, Peru; Tel: +511 2190100 Email: o.molinac@up.edu.pe

§Blavatnik School of Government, Oxford University, Radcliffe Observatory Quarter, Woodstock Road, Oxford OX2 6GG, UK; Tel: +44 (0) 1865 614 343 Email: ingo.outes-leon@bsg.ox.ac.uk

[1]For instance, Cameron and Shah (2015) and Samphantharak and Chantarat (2015) find that risk aversion increases after a flood, while Eckel et al. (2009), Hanaoka et al. (2017), and Voor et al. (2012) document reduction in risk aversion after major negative shocks such as Hurricane Katrina, the Great East Japan Earthquake, or Burundi's civil war. See Chuang and Schechter (2015) for a recent survey of this literature.

and Shah, 2015), change in reference point after a loss, as predicted by prospect theory, (Page et al., 2014) or even an emotional response to a traumatic or stressful experience (Eckel et al., 2009; Hanaoka et al., 2017)

This paper studies the effect of a titling program on risk aversion. To the best of our knowledge, this is the first study of the effect of public policies on risk preferences. Our main contributions are to show that a public policy can reduce risk aversion, and point out to changes in background risk as a possible mechanism.

Studying the effect of public policies on risk aversion is important for at least two reasons. First, most evidence on factors affecting risk preferences comes from dramatic, negative, shocks such as financial crises, wars, or natural disasters. These shocks affect the economic environment in several dimensions. Thus, their effect on preferences can be confounded by changes in income, migration, and psychological scars, among others. By focusing on a narrower policy reform, we can better identify the effects on preferences and understand the mechanism driving the results. Second, the impact of public policies is usually studied in terms of their effect on external determinants of economic behavior such as prices, risk of expropriation, or rates of return. However, as we show in this paper, they may also affect deeper determinants of human behavior, such as risk preferences. This side-effect of public policies has been neglected in the academic and policy debate.

We use the case of the Peruvian urban titling program. This program issued over 1.2 million property titles to urban squatters, and is considered one of the largest programs of this kind in the developing world. The impact of this titling program has been studied before. Previous studies find significant effects on labor supply and residential investment, among other outcomes (Field, 2005; Field, 2007). Most of these effects seem to be driven by increased security of tenure, i.e., reduction in expropriation risk rather than better access to credit.

We use a novel household dataset with questions designed to elicit risk preferences. These questions follow the methodology developed by Barsky et al. (1997). Our identification strategy is a difference-in-difference approach that exploits the timing in program implementation. In particular, we compare the difference in risk aversion between eligible and non-eligible households in areas reached by the program relative to areas yet to be reached. Our empirical strategy is similar to Field (2007) and, although we use a different sample, we can actually replicate most of her results on the effect of titling on labor supply.

We find evidence that the titling program *reduced* risk aversion. The magnitude is economically significant: the program reduced the proportion of individuals with high risk aversion (around 73% of the sample) by almost 10 percentage points. This represents an increase in risk tolerance of 13% relative to the mean. These results are robust to alternative specifications, such as adding a rich set of control variables and district fixed effects, controlling for individuals' expectations to receive a title, or using an alternative identification strategy comparing only individuals with and without a title. Similar to Hanaoka et al. (2017) we also find that most of the change occurs among men, although we find an opposite effect.

We examine the mechanism driving our findings in two ways. First, we include as additional controls proxies of hours worked, access to credit and perceived increase in property prices. These variables control for possible changes in income and wealth which . are potential indirect channels for the titling program to affect risk preferences. However, after adding these proxies we still find a similar effect of the program on risk tolerance. Second, we examine whether the effect is stronger among individuals for whom the reduction in background risk would have been greater. We find that the effect occurs mainly among individuals who report improved security of tenure as a main benefit of a formal title, and among individuals who were victims of a crime in their neighborhoods. These groups are likely the ones that experienced a relatively larger reduction in background risk after the program. We interpret these findings as evidence that the effect of titling on risk aversion is driven by changes in background risk.

This paper relates to two different strands of the literature. First, it contributes to a growing literature on the effect of shocks on preferences. As previously mentioned, this literature finds that shocks, like wars and natural disasters, affect risk and time preferences (Callen et al., 2014; Callen, 2015; Cameron and Shah, 2015; Eckel et al., 2009; Hanaoka et al., 2017; Voors et al., 2012). There is, however, not yet a consensus on the sign of the effect, nor on the mechanisms driving these changes (Chuang and Schechter, 2015).

Second, it complements a literature on the effect of titling on economic outcomes. This literature has documented effects on labor supply, and residential and human capital investment, among other outcomes (Field, 2007; Field and Torero, 2006; Galiani and Schargrodsky, 2010). Our paper is closely related to DiTella et al. (2007). Using a natural experiment in Argentina, they find that individuals that received a property title report more pro-market beliefs. They hypothesize that titling may also change preferences, but are unable to examine it due to data limitations. Our paper provides evidence supportive of this hypothesis.

The rest of the paper is organized as follows. Section 2 provides some background on the Peruvian titling program and discusses possible mechanisms to affect risk preferences. Section 3 explains the data and our measure of risk aversion, as well as the empirical strategy. Section 4 presents the main results and examine the role of changes in background risk as a possible mechanism. Section 5 concludes.

2 Background

2.1 Property titling in Peru

Starting in 1996, Peru engaged in one of the largest urban titling programs in the developing world (Field, 2007). The program granted property titles to previously informal urban squatters. Obtaining a title became almost free and fast. In order to be eligible, homeowners were required to have no formal title and verify residence prior to 1995. Before this program, obtaining a title was a slow and costly process. Not surprisingly, the rates of informality were very high: more than a quarter of Peru's urban population had no formal title of their property (World Bank, 1992).

The program was carried on by a public agency COFOPRI (Committee for the Formalization of Private Property). Implementation involved a staggered area-wide titling: teams entered one neighborghood at a time, moving contiguously within cities (World Bank, 1998). This implementation feature has been used in previous studies of the impact of this program.

These studies find evidence of an increase in number of hours worked, as well as on residential investment (Field, 2005; Field, 2007). The effect seems to be driven mostly by increase in security of tenure, not so much by increase in income or access to credit (Field and Torero, 2006). Similar lack of effects on income and access to credit has been documented in other urban titling programs (Galiani and Schargrodsky, 2010; Galiani and Schargrodsky, 2011).

2.2 Why would property titling affect risk aversion?

A growing literature suggests that time and risk preferences are not fixed, but can be altered by shocks, such as wars, financial crises, or natural disasters (Cameron and Shah, 2015; Samphantharak and Chantarat, 2015; Eckel et al., 2009; Hanaoka et al., 2017; Voors et al., 2012) A possible explanation for this phenomenon is changes in background risk. The key idea is that risk aversion is affected by the level of risk in the environment. This idea was formally developed by Gollier and Pratt (1996) and Eeckhoudt et al. (1996). They show that, under mild conditions on utility functions, there is a positive relation between background risk and risk aversion: more background risk increases individual's risk aversion. They called this property "risk vulnerability". In this view, a natural disaster, or other shocks, could increase risk aversion by increasing the perceived background risk. Some empirical studies, both observational and experimental, support the importance of background risk and risk vulnerability.[2]

In our context, we interpret the property titling program as a *reduction* in background risk. This interpretation is based on previous studies which suggest that an important effect of the program has been an increase in security of tenure, i.e., reduction in expropriation risk. We corroborate this potential effect in our data: around 80% of individuals mention security of tenure as a main benefit of having a property title.[3]

If the relation between background risk and risk aversion is positive, as suggested by Gollier and Pratt (1996) and Eeckhoudt et al. (1996), then the titling program would *reduce* risk aversion. However, theoretically, the relation between background risk and individual risk aversion might be negative. For instance, using generalized expected utility preferences, Quiggin (2003) shows that more background risk could decrease risk aversion. This prediction is consistent with psychological evidence on diminishing sensitivity to risk. Thus, it is possible that the titling program would actually increase risk aversion.

There are other explanations of why risk aversion change. For instance, individuals may become more risk seeking after a loss, as predicted by prospect theory (Page et al., 2014), or

[2]For instance, Heaton and Lucas (2000) find that higher levels of background risk reduce stock market participation in U.S. **?** find that U.S. consumers more likely to face income uncertainty or to be liquidity constrained exhibit a higher degree of absolut risk aversion. Lin (2009) documents similar finding using data from Taiwan. In an laboratory setting, Lusk and Coble (2008) find that individuals exposed to more background risk exhibit more risk aversion. Cameron and Shah (2015) find that in Indonesia risk aversion is higher in areas that suffered a recent flood, and interpret their evidence as consistent with Gollier and Pratt (1996)'s hypothesis.

[3]In contrast, other potential benefits such as using property as collaterals (56% of respondents), access to public services (20%) or facilitating house sales (16%) are mentioned much less.

their attitude toward risk may change as an emotional response to a traumatic or stressful event (Lerner and Keltner, 2001; Eckel et al., 2009; Hanaoka et al., 2017) These explanations are less relevant in our case given that the policy reform did not caused major losses but instead was perceived as a positive shock. A more compelling alternative explanation is that the titling program changed expected income or wealth. This may happen, for instance, by increasing employment, facilitating access to credit, or increasing property values. In the empirical analysis (Section 4), we examine the effect of the titling program on risk aversion and the role of background risk as a possible mechanism.

3 Methods

3.1 Data

Our analysis uses data from a household survey commissioned by COFOPRI. The survey was collected in June 2003 in five Peruvian cities (Lima, Trujillo, Arequipa, Cuzco, and Huaraz). These five cities concentrate 36.7% of the Peruvian population and 60% of land titling beneficiaries. The neighbourhoods included in the sample contained some areas already reached by the titling program, and areas in which the program was expected to be implemented in the future. Using administrative records, we checked that the titling program eventually did reach all the neighborhoods included in the survey sample.

The survey contains information on individual, household, and plot characteristics. We use this information to construct indicators of residing in a program area (i.e., a neighborhood reached by the program) and being eligible to receive a title from the program. We define eligibility in the following way. In areas not reached by the program, eligible individuals are homewoners both living prior to 1995 and lacking a formal property title. In areas already reached by the program, we consider as eligible to all homeowners that have lived there before 1995 regardless of having or not a formal title. Thus, this latter group includes people who already obtained a COFOPRI title as well as households who did not obtained it.

Table 1 displays some mean values for key variables for the whole sample, as well as for program and non-program areas. Our final sample consists of 2,300 respondents located in 36 districts and 149 neighborhoods.[4] The survey respondent is the household head.[5]

Table 1: Mean values of key variables

Variable	Whole sample (1)	Program area: No (2)	Program area: Yes (3)
Program area	0.401	-	-
Eligible	0.472	0.349	0.655
Household size	4.7	4.7	4.7
Age	43.6	43.0	44.5
Sex	0.287	0.320	0.237
Completed secondary	0.704	0.694	0.718
Monthly expenditure (PEN)	565.0	534.1	611.7
Has property title	0.512	0.303	0.842
Lot size (m^2)	157.9	161.8	152.2
Was invasion	0.340	0.325	0.363
Residential tenure (years)	13.8	12.9	15.2
Distance to urban amenities (minutes)	14.6	15.5	13.1
Imputed risk tolerance (θ)	0.229	0.235	0.219
Has low risk tolerance	0.734	0.717	0.759
No. observations	2371	951	1420

3.2 Measuring risk aversion

They survey measures risk aversion following the methodology developed by Barsky et al. (1997). This methodology asks respondents two questions about their choice over lotteries with different payoffs.[6] These questions separate the respondents into four risk preferences categories. These categories can be ranked by risk aversion without having to assume a particular functional form

[4]The initial sample includes 3,000 observations but we loss several observations due to missing values, mostly on indicators of program eligibility.

[5]The household head is defined as individual who make most important decisions in the household.

[6]The first question is: "I would like to know which decision you would take in this situation. Suppose that your employer offers you an opportunity to change your salary. You would continue to to the same job, but with a different salary. This is the deal: we toss a coin, if it is heads you earn twice your current salary, but if it is tails then you would earn a third of your current salary. Please think for a moment about your possible new salary: double or a third. Would you participate in the deal?".

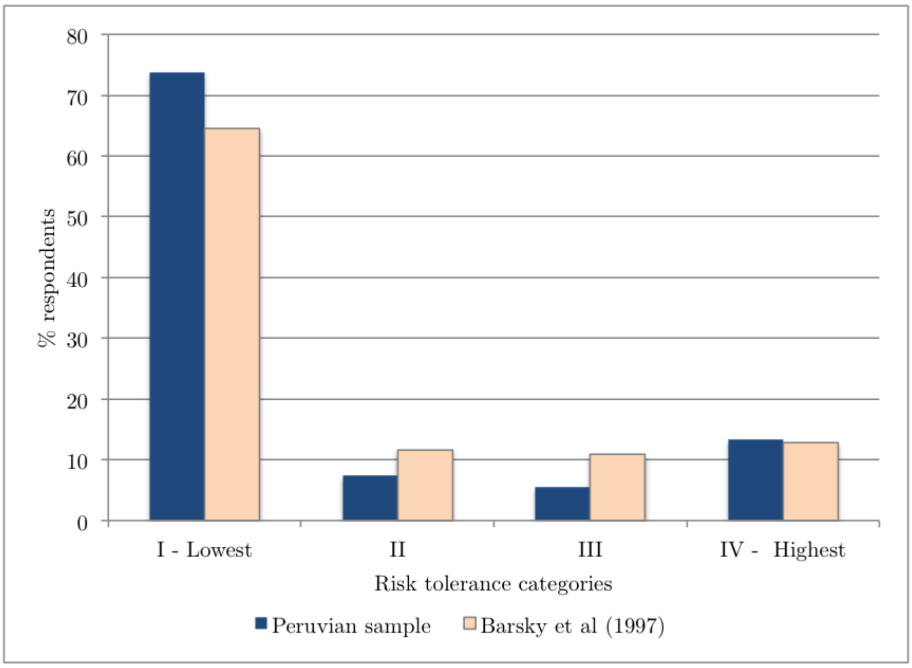
If the answer is "yes," the interviewer continues: "Suppose that instead of the previous deal, you are offered another with these two earning possibilities: earn double or half of your current salary. Would you participate in this alternative deal?".

If the answer to the first question is "no," the interviewer continues: "Suppose that instead of the previous deal, you are offered another with these two earning possibilities: earn double or earn 20 percent less of your current salary. Would you participate in this alternative deal?".

for the utility function. Following Barsky et al. (1997), for most of the analysis we work with the reciprocal of relative risk aversion, called “relative risk tolerance”.

Figure 1 shows the proportion of respondents in each of these fours risk categories. Category I corresponds to low risk tolerance (i.e. high risk aversion), while category IV corresponds to high risk tolerance (low risk aversion). We also include the distribution of answers reported by Barsky et al. (1997). Both distributions are similar, with a large proportion of respondents having low risk tolerance. Our sample is slightly skewed to the right, which implies a lower risk tolerance on average. This is expected given that our sample is poorer than Barsky et al (1997)’s.

Figure 1: Distribution of risk tolerance



These four categories define an ordinal measure of risk aversion. Barsky et al. (1997) transform these categories into a cardinal proxy of risk tolerance (θ).[7]

We use the values of θ estimated by Barsky et al. (1997) and assign them to each risk tolerance category. This “imputed” measure of risk tolerance is our preferred measure of risk preferences. We also present results using a binary measure of risk aversion: an indicator of having low risk aversion (i.e, being in category I), as well as using our own estimate of θ. To obtain this estimate, we follow the procedure described in Kimball et al. (2008).

[7]Note that risk aversion is defined as $\frac{1}{\theta}$.

We consider this measure of risk aversion a valid one for at least two reasons. First, several studies suggest that survey measures of risk aversion based on non-incentivized hypothetical questions (like the one we use) are as good as predicting individual choices as experimental measures. Moreover, they tend to be more stable over time (Dohmen et al., 2011; Chuang and Schechter, 2015).[8]

Second, our measure of risk tolerance captures a meaningful dimension of individuals' risk preferences. To see this, we regress our two measures of risk tolerance (binary measure and imputed risk tolerance) on several indicators of risky financial decisions, such as running a business at home, borrowing money to invest in a business, or whether an individual has invested or plan to invest on home improvements.[9] The results, shown in Table 2, present a positive correlation between our measures of risk tolerance and risky behavior: more risk averse individuals are less likely to run a business or invest.

3.3 Identification strategy

The main empirical problem is one of selection in unobservables: individuals with a property title may be systematically different than individuals without one. To address this concern, our empirical strategy exploits neighborhood-level variation to implement a difference-in-differences (DiD) approach. In particular, we compare the difference in outcomes of eligible and non-eligible households in areas reached by the program (program areas) relative to areas not yet reached by it (non-program areas). In the DiD terminology, program areas are treated, non-program areas are untreated, while eligible and non-eligible households are the treatment and control groups, respectively. To the extent that the difference in risk aversion between eligible and non-eligible households would have been similar in both areas, our approach identifies the effect of the titling program.

Our identification strategy is similar to the one used by Field (2007) in her study of the effect of titling on labor supply. As a check of our approach, we successfully replicate her main results on hours worked as well as heterogeneous effects by household size and gender (see Table A.2 in the Appendix). Note that, similar to Field (2007) and other related works, by focusing on eligibility but not actually on receiving a property title treatment, our approach produces

[8]The experimental measures are obtained in real-stake games.

[9]We use only the sample of individuals in non-program areas to avoid confounding the estimates with the program effects. The results are similar, however, when we use the whole sample.

Table 2: Measured risk tolerance and financial decisions

	Run business at home (1)	Has borrowed money for business (2)	Plan to invest in home improvements (3)	Has invested in home improv. in last 3 years (4)
A. Using cardinal measure of risk tolerance				
Imputed risk tolerance (θ)	0.170**	0.073*	0.278***	0.082*
	(0.071)	(0.041)	(0.084)	(0.047)
No. obs.	1,741	1,758	1,530	1,741
R-squared	0.028	0.008	0.045	0.011
B. Using binary measure of risk tolerance				
Has low risk tolerance	-0.049**	-0.030**	-0.097***	-0.033**
	(0.023)	(0.015)	(0.031)	(0.015)
No. obs.	1,741	1,758	1,530	1,741
R-squared	0.026	0.009	0.045	0.011
Mean outcome var.	0.202	0.063	0.457	0.099

Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions are estimated using OLS, use the sample of individuals in non-program areas, an include the following covariates: age, sex, indicator of complete secondary, household size, length of tenure, and average distance (in minutes) to urban amenities.

intention-to-treat (ITT) estimates, not average treatment effects (ATE).

The selection of treatment areas was driven by factors such as city size, density of informality and distance from commercial centers (Morris et al., 2004; Field, 2007). This feature raises concerns of potential endogeneous timing of the program. We address this concern by including district fixed effects as well as controlling for the average distance (in minutes) to urban amenities such as paved road, market, police station, health center, and school.[10]

We estimate the following regression:

$$risk_{ijd} = \alpha(eligible_i \times program_j) + \beta eligible_i + \gamma program_j + \delta \mathbf{X}_{ij} + \eta_d + \epsilon_{ijd}, \tag{1}$$

where the unit of observation is individual i, in neighborhood or area j, in district d. $program_j$ and $eligible_i$ are indicators or the area being reached by the program and the household being eligible for a title. The parameter of interest is α: it captures the DiD estimate and, if the identification assumption is valid, the effect of the titling program on risk preferences. We include individual controls $\mathbf{X}_{ij}$ such as household size, age, sex, education, length of tenure, and distance to urban amenities, as well as district fixed effects, η_d.

The main outcome, $risk_{ijd}$, is the imputed risk tolerance (θ). As explained above, this is a cardinal measure of risk tolerance. We check the robustness of our results to using alternative measures such as a binary indicator of having low risk aversion or the risk categories from the survey. We estimate the model using ordinary least squares (OLS) except when using the categorical outcome. In that case, we use ordered probit. Given that the source of variation is at the neighborhood level and to take into account possible spatial spillovers, we cluster the standard errors at neighborhood level (n=149).

4 Results

4.1 Effect on risk preferences

Table 3 presents our main results. Our preferred specification uses the imputed risk tolerance derived from Barsky et al. (1997) (column 1). However, the results are robust to using alternative measures of risk preferences including a binary indicator of having risk aversion (column 2), risk

[10]Field (2007) addresses this concern by including city fixed effects. Districts are the smallest administrative units in Peru. Cities are usually composed by several districts.

preference categories (column 3), and our own estimates of risk tolerance (column 4).

All the results suggest that the titling program is associated with an increase in risk tolerance, i.e., a *reduction* in risk aversion. This result echoes other studies which find that risk preferences change after individuals suffer a negative shock, such as natural disasters or war (Cameron and Shah, 2015; Hanaoka et al., 2017; Voors et al., 2012). However, to the best of our knowledge, this is the first study documenting change in risk preferences due to a policy reform.[11]

The magnitude of the effect is economically significant. Column 1 suggests that the titling program increased risk tolerance by 0.033. This represents an increase of 13% relative to the average value. The magnitude of this effect is comparable to estimates documented in other studies of the effect of natural disasters on risk aversion. For instance, Hanaoka et al. (2017) find a reduction of 8.1 percent in their measure of risk aversion after the Great Japan earthquake, while Samphantharak and Chantarat (2015) document an increase of almost 10 percent in risk aversion after the Thai 2011 mega flood.

Table 3: Effect of titling program on risk tolerance

	Imputed risk tolerance (θ) (1)	Has high risk aversion (2)	Risk tolerance category (I-IV) (3)	Own estimate of risk tolerance (4)
Eligible × program area	0.033** (0.014)	-0.098** (0.040)	0.344*** (0.133)	0.016** (0.007)
Estimation method	OLS	OLS	ordered probit	OLS
No. obs	2,371	2,371	2,371	2,371
R-squared	0.055	0.062		0.056
Mean outcome var.	0.229	0.734	1.597	0.069

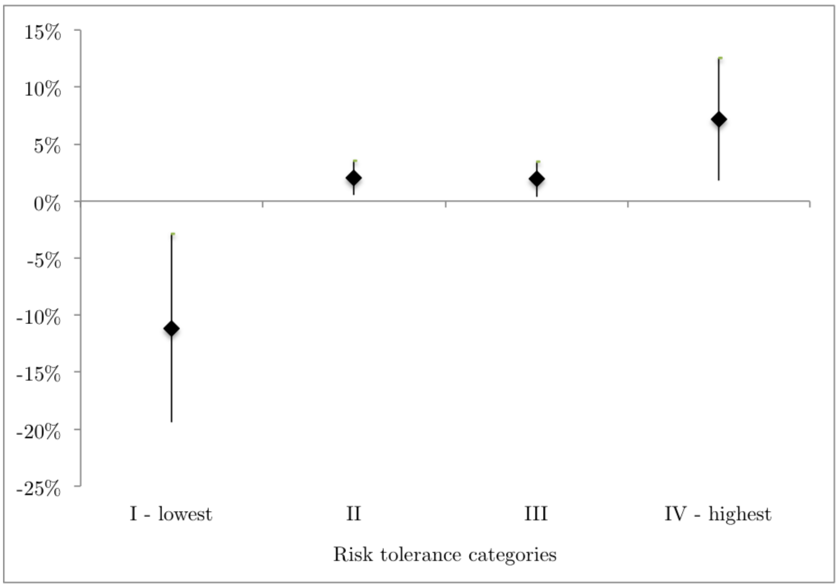
Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include district fixed effects and the following covariates: age, sex, indicator of complete secondary, household size, length of tenure, and average distance (in minutes) to urban amenities.

This reduction in risk aversion is driven by a change in distribution of risk preferences from

[11] We also examine heterogeneity of the effects by splitting the sample by gender, age, and education of the respondent (see Table A.1 in the Appendix). This analysis is motivated by previous findings that the effect of negative shocks (such as a natural disaster) on risk aversion tends to be disproportionally concentrated among men (Hanaoka et al., 2017). Our results suggest that the change in risk tolerance happens mainly among relatively young and educated men.

low to high risk tolerance. Figure2 depicts the marginal effects at the mean, estimated from the ordered probit model. It shows that the program reduced the probability of having high risk aversion (the most common category) by almost 10 percent, and increased the probability of having high risk tolerance by around 7 percent. We obtain similar result when using a binary mesure of risk aversion (Table 3 , column 2).

Figure 2: Marginal effects of program on risk tolerance



Notes: Figure depicts marginal effects of (eligible × program area) evaluated at the mean. Estimates obtained from ordered probit model (Table 3, column 2). Diamons are point estimates, while vertical lines represent the 95% confidence interval.

We check the robustness of our results to alternative model specifications (see Table 4). Results are similar clustering errors at district (n=36), instead of neighborhood level (column 1), using a more parsimonious specification without individual controls (column 2), as well as adding a rich set of district-by-eligible fixed effects (column 3). These fixed effects control for differences between eligible and non-eligible households within a district.

The program was anticipated in many untreated, non-program, areas. This may raise concerns of possible contamination: individuals in untreated areas may start changing their beliefs and risk preferences before being reached by the program. This contamination may attenuate our estimates. We examine this issue by adding as additional covariate an indicator of individuals believing they would receive a title in the near future (column 4). However, the expectation of treatment does not seem to affect risk tolerance nor affect our main results.

Finally, we use an alternative identification strategy. We restrict the sample to eligible households, and regress the measure of risk tolerance on an indicator of having a property title (column 5). To the extent that the control variables and fixed effects eliminate systematic differences between individuals with and without a title, this approach would produce valid estimates of the effect of the property title on risk aversion. Despite using a different approach, we obtain results similar to our baseline specification.

Table 4: Robustness checks

	Outcome = Imputed risk tolerance (θ)				
	(1)	(2)	(3)	(4)	(5)
Eligible × program area	0.033*	0.027*	0.037**	0.033**	
	(0.016)	(0.014)	(0.015)	(0.014)	
Expects to receive title in future				0.006	
				(0.007)	
Has property title					0.020*
					(0.011)
Check	Cluster S.E by district	No indiv. controls	District-by-eligible FE	Control for expectations	Sample of eligible HH
No. obs	2,371	2,371	2,371	2,367	1,118
R-squared	0.055	0.048	0.070	0.056	0.068

Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level except in column 1 in which they are clustered at district level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include district fixed effects. Columns 3-5 also include the same individual controls as baseline regression in Table 3. Column 2 adds district-by-eligible fixed effects while column 4 adds an indicator of the individual expecting to receive a title in the near future.

4.2 Exploring the mechanism

Previous studies find that titling programs increase labor supply and investments in residential improvements and human capital (Field, 2005; Molina and Soderbom, 2011; Galiani and Schargrodsky, 2010; Vogl, 2007). These effects seem to be driven mostly by increased security of tenure, i.e, reduction in expropriation risk. A related paper by DiTella et al. (2007) also document a change in beliefs: individuals who received property titles report more pro-market beliefs. However, no study has shown effects of titling programs on risk preferences.

A relevant question is: why would property titling reduce risk aversion? As discussed in

Section 2, a possible explanation is that the titling program reduced background risk. Under general conditions, formalized by Gollier and Pratt (1996) and Eeckhoudt et al. (1996), this decrease in background risk would reduce relative risk aversion.

We examine this explanation in two ways. First, we rule out some alternative explanations. The most compelling one is that the titling program affected levels of income or wealth. This could happen, for example, if households are able to work more hours (which could increase employment income), if their houses appreciate after having a formal title, or if they have (or expect to have) access to credit. To do so, we add to our baseline regression measures of self-reported monthly expenditure, hours worked, and perceived change in house prices after obtaining a formal title. We also add an indicator of respondents having a formal loan (from a bank or commercial house) as well as an indicator of believing they could obtain credit. These variables may capture actual and expected changes in permanent income and wealth due to the titling program.

Second, we examine whether the effect is stronger among individuals for whom the reduction in background risk would have been greater. One of the survey questions asks about the main benefits of having a formal title. We use this question to split the sample between individuals who report increase security of tenure as a benefit or not. We also split the sample between individuals who report being victims of a personal or property crime in their neighborhood (such as burglary, mugging, or kidnapping) or not. In both cases, we would expect a greater perceived reduction in background risk among the first group of individuals.

Table 5 displays the results. Columns 1-5 show that our baseline results remain unchanged when we add variables controlling for proxies of changes in permanent income or wealth. Columns 6-9 suggest that the effect of titling on risk tolerance only occurs among individuals who report more security of tenure as a benefit of having a formal title, or who had been victims of a crime. We interpret these findings as suggestive evidence that the observed effects on risk preference are not driven by changes in permanent income or wealth, but instead may be a consequence of a reduction in background risk.

Table 5: Examining role of background risk

	Outcome = Imputed risk tolerance (θ)								
	Consumption and employment		Property prices	Access to credit		Think security of tenure is benefit		Victim of crime	
						Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Elegible (×) program area	0.038***	0.033**	0.069***	0.034**	0.030**	0.034**	0.011	0.056***	0.011
	(0.014)	(0.014)	(0.017)	(0.014)	(0.014)	(0.016)	(0.025)	(0.019)	(0.019)
ln(monthly expenditure)	-0.015*								
	(0.008)								
Hours worked		-0.000							
		(0.000)							
Perceived change in house prices			0.000						
			(0.000)						
Has formal loan				-0.000					
				(0.007)					
Believes can obtain credit					0.019**				
					(0.008)				
No. obs	2,194	2,142	1,350	2,371	2,256	1,893	472	1,195	1,155
R-squared	0.060	0.059	0.112	0.055	0.062	0.062	0.126	0.080	0.060

Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include district fixed effects and the same individual controls as baseline regression in Table 3.

5 Conclusion

Recent evidence suggests that risk preferences are not fixed, but can change due to shocks, like war or natural disasters. Using the case of a large titling program in Peru, we show that change on risk preferences can also be caused by public policies. The effects are sizeable and robust to alternative specifications and identification strategies. Our evidence suggests that these effects on risk preferences might be driven by the reduction in background risk associated with improved security of tenure.

Our results highlight an interesting channel for policies to affect economic outcomes. This effect is usually analyzed as mediated by changes in the economic environment. However, we show that some public policies can also affect deeper determinants of human behavior, such as preferences. This potential channel is usually neglected in the academic and policy debate.

Due to data limitations, we only observe individuals few years after the program is implemented. Thus, we are unable to study whether the effect on risk preferences persists over time. Similarly, we cannot examine whether the change in preferences affected long-run economic decisions. Examining these issues warrant further research.

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A Additional tables

Table A.1: Heterogeneous effects by gender, age and education

	Outcome = Imputed risk tolerance (θ)					
	Women	Men	Age<37	Age≥37	No complete high school	Complete high school
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × program area	-0.002	0.046***	0.046***	0.009	0.013	0.057***
	(0.024)	(0.015)	(0.016)	(0.021)	(0.022)	(0.017)
No. obs.	680	1,691	992	1,379	1,136	1,235
R-squared	0.118	0.063	0.082	0.073	0.069	0.067
Mean outcome var.	0.225	0.230	0.227	0.229	0.236	0.222

Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include district fixed effects and the same individual controls as baseline regression in Table 3.

Table A.2: Replication of Field (2007)'s results on labor supply

	Hours worked			
	All	All	Women	Men
	(1)	(2)	(3)	(4)
Elegible × program area	4.487**	13.655**	1.333	5.254*
	(2.235)	(5.554)	(5.270)	(2.694)
Elegible x prog. area × HH size		-2.078*		
		(1.180)		
No. obs	2,205	2,205	663	1,542
R-squared	0.201	0.203	0.165	0.172
Mean outcome var.	37.0	37.0	23.2	42.9

Notes: Robust standard errors in parentheses. Standard errors are clustered at neighborhood level. * denotes significant at 10%, ** significant at 5% and *** significant at 1%. All regressions include district fixed effects and the same individual controls as baseline regression in Table 3.