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# The Impact of Land Titling on Labor Allocation

## Evidence from Rural Peru

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## INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

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## ABSTRACT

This paper analyzes the relationship between land property rights and household labor allocation. It posits that land titling has two opposite effects on labor decisions. On one hand, enhancement of tenure security should lead to reductions in guarding requirements and to increases in the hours that households spend off their land (Field effect). On the other hand, decreases in the risk of expropriation should lead to higher parcel-attached investments and to higher labor productivity related to land (productivity effect). To investigate this hypothesis, a massive land titling program in rural Peru (the Special Program of Land Titling, or PETT) is analyzed. Propensity score matching estimations suggest that the productivity effect is much larger than the Field effect, leading to overall increases in household labor allocations to agricultural self-employed activities. These estimations are robust to different specifications within a cross-section and a four-round panel dataset.



**Keywords: property rights, land titling, labor allocation**

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# 1. INTRODUCTION

There is widespread consensus in the economic literature regarding the importance of property rights for economic development (for example, North 1981; De Long and Shleifer 1993; Acemoglu, Johnson, and Robinson 2001). The basic idea is simple: Property rights allow agents to reap the benefits of their investments, align their incentives, promote the allocation of resources to their most profitable uses, and spur economic growth. An unclear definition of ownership is regarded as one of the factors that hinder growth in the developing world and impede poverty reduction. An absence of property rights has an especially pervasive impact on the poor because these populations cannot afford to invest their scarce resources in capital that might be expropriated afterward and because such an absence prevents collateralization and transferability of their few assets. In this spirit, their *dead capital* cannot be used to finance any new investments or to cushion any shocks that reduce their income.

Existing economic literature on developing countries has focused primarily on the impact of ownership on credit access and land-attached investments. On one hand, the balance on the former is mixed: Although some studies claim a positive impact of titling (for example, Feder et al. 1988), an increasing number of papers find that the effect is negligible (for example, Pender and Kerr 1999; Place and Migot-Adholla 1998; Boucher, Barham, and Carter 2005; Field and Torero 2004; Galiani and Schargrodsky 2007). The general consensus seems to be that property rights are probably a necessary but insufficient condition for credit and that titling efforts in developing countries need to be accompanied by other policies in order to have an effect. On the other hand, the balance on property-specific investments, in contrast, seems to be less ambiguous. Besley (1995); Gavian and Fafchamps (1996); Carter and Yao (1999); Deininger and Chamorro (2002); Banerjee, Gertler, and Ghatak (2002); Antle et al. (2003); and Field (2005) find a theoretically predicted positive relationship between investment and property rights.

In the particular case of Peru, a few papers have dealt with these issues. For the purposes of this study, it is of particular relevance that, with the same dataset used here, Torero and Field (2005) document no changes in access to credit or increases in land-attached investments related to property titles. Moreover, this relationship in rural Peru has also been documented by Fort (2007, 2008[1]), Larson et al. (2000), Antle et al. (2003), and Barrantes and Trivelli (1994).[2]

However, the relationship between property rights on one side and investments and credit constraints on the other remains open: If households with property rights do make more land-attached investments and most of them remain credit constrained, how do they finance these investments? To my knowledge, the only paper dealing with this unexplored aspect is by Carter and Olinto (2003), who argue that small landholders might increase their fixed assets only at the expense of reducing their mobile capital, leaving their overall investment unaltered.

More recently, a few studies have analyzed the impact of property rights on household labor decisions. The most salient paper in this area is that of Field (2007). She analyzes the impact of a titling program for urban squatters in Peru and argues that squatter households devote time to protecting their dwellings. Her results suggest that granting ownership rights reduces the need for this duty and allows for more hours of work away from home.

While Field's paper focuses on housing titling programs for urban squatters, this analysis explores whether land titling in rural areas has a different impact. In this paper it is asserted that the main

[1] Using a different and independently collected dataset, Fort analyzes the Special Program of Land Titling (PETT), the same as Torero and Field (2005), and reaches the same conclusions as Torero and Field. In this vein, Fort (2008, 325) argues that "the results show that there is a positive effect of titling on the probability of making investments as well as on the value of investments… This effect could be almost entirely attributed to changes in farmer's willingness to invest and not to better access to credit."

[2] Larson et al. (2000), Antle et al. (2003), and Barrantes and Trivelli (1994) focus on particular areas of rural Peru. First, Larson et al. analyze the case of the valley of Huaral and find a positive relationship between titling and the construction of waterways and canals. Additionally, they find that titled farmers exhibit greater use of conservation techniques. Second, Antle et al. analyze the impact of titling on investment in terraces in the department of Cajamarca in northern Peru and find that the probability of investment in terraces increases by 6.6 percent with land registration. Finally, Barrantes and Trivelli focus on the valley of Cañete and find that titling does not have an impact on credit access.

difference between the two is that the latter grants property rights to a productive asset while the former does not. In this sense, a theoretical model is established. in which land ownership rights have two opposite effects on labor allocation. Following Field's logic, titling might increase the number of off-farm hours of work: landowners have a smaller need to guard their plots and thus reduce the time spent on agricultural activities on their land, and spend more time away from their plots. This paper also argues that the productive nature of land brings an additional effect in the form of a return to agricultural self-employed activities. As supported by both theoretical and empirical evidence, property rights seem to provide investment incentives that are specific to parcels. Then, if there are any complementarities between capital and labor in agricultural self-employed activities, the marginal return of working more hours on the land should increase relative to competing alternative activities. Thus, the *productivity effect* should lead—contrary to the *Field effect* —to increases in the allocation of labor to agricultural activities on the farmer's own land.

Additionally, a different potential source that households might use to finance increases in land-attached assets was preliminarily explored. This paper asserts—in the line of Carter and Olinto (2003)—that these increases might come at the expense of reduced accumulation of other types of capital. The main difference suggested is that this process may be driven by shifts in investments from nonagricultural to agricultural activities.

Thus, the contribution of the paper is twofold. First, it analyzes an alternative channel through which land titling might affect labor allocation in rural areas and empirically tests the relative weight of the Field effect vis-à-vis the productivity effect. Second, it builds upon the Carter and Olinto hypothesis and proposes an alternative channel by which titled households finance increases in land-attached capital.

For these purposes, the paper uses data from the *Programa Especial de Titulación de Tierras y Catastro Rural* (PETT, or Special Program of Land Titling), a massive land titling program in rural Peru. To date, the PETT has issued more than 800,000 new property certificates, offering them to previously untitled households with very few requirements and virtually for free. This type of assignment allows the problem of endogeneity between property rights and household choices, which has hampered many of the previous papers, to be addressed. Two pieces of information are analyzed: a cross-section survey and a four-round panel dataset of households. The analysis divides a household's labor structure into three categories: wage earning, nonagricultural self-employed, and agricultural self-employed. Propensity score matching methods in both datasets are used to disentangle the effect of property titles on the incomes and hours of work in these activities. Within this framework, labor reallocation and average returns per hour of work in each category are estimated, checking for any patterns and trade-offs.

The main results of this paper are twofold. First, it finds that property titles change the allocation of labor from nonagricultural to agricultural self-employed activities. In this sense, it provides some evidence about the prevalence of the productivity effect over the Field effect. Second, it suggests that these increases in agricultural self-employed hours are accompanied by more-than-proportional rises in income in this category. The evidence for the latter is weaker but would imply increases in productivity in this sector. Analogously, though in a smaller magnitude, it also finds reductions in the return per hour of nonagricultural self-employed activities. Even when these results may suggest some recomposition of capital between activities, lack of detailed data on alternative investments prevents stronger evidence about this possibility from being determined at this point.

Arguably due to controls for time-invariant unobservable characteristics, differences in magnitude arise from the cross-section and panel data estimates for the relation between titling and the composition of income and labor allocation. But, all in all, the direction of the results is robust to different specifications and samples.

The remainder of the paper is organized as follows. Section 2 presents a theoretical model for the effect of titling on labor allocation. Section 3 presents a description of the institutional context leading to the implementation of PETT. Section 4 discusses the data and the construction of the cross-section and panel samples and spells out the methodological approach. Section 5 presents the results. Section 6 presents some concluding remarks.

## 2. THEORETICAL MODEL

Two main approaches have been suggested to model the effect of property rights on households' productive decisions. The first, proposed by Besley (1995), explains why households with better-defined property rights invest more in parcel-attached capital. He argues that tenure insecurity acts as a random tax on agricultural activities. In this setting, land investments are risky because if the parcel is effectively expropriated, then the capital in it is also taken away. In his view, tenure security is a function of formal property rights; thus, a titling program should lead to an increase in agricultural self-employed investments. The second approach is that of Field (2007), who analyzed the impact of weak property rights for housing among urban squatters. Her argument is that tenure security is not a sole function of formal property rights but also of the effort that households exert in guarding their estates. According to her model, if formal property rights and guarding time are substitutes in providing tenure security, an enhancement of the former should lead to a reduction of the latter. The time released can subsequently be spent working outside the home, inducing an increase in labor supply.

The model proposed in this paper integrates both Besley's and Field's approaches in a single framework to model the impact of formal land property rights on rural households' labor allocations. On one side, it includes Besley's incentives of property rights for increases in land investments. His model mainly deals with capital incentives. However, a straightforward extension is that complementarity between labor and capital should also induce changes in agricultural self-employment. On the other side, the framework includes Field's notion of time allocation decisions as determinants of tenure security.

To incorporate this intuition, some additional assumptions are introduced. First, the model modifies the role of guarding labor in Field's model by including some differences due to the productive nature of land, as opposed to the unproductive nature of dwellings.[3] The model posits that agricultural self-employed hours—in addition to their effect as direct productive inputs—play an additional role by informally protecting property rights. In this setting, households may overspend time working on the land as a consequence of this additional effect. Second, it incorporates a restriction for credit access, which characterizes most rural households' contexts in developing countries, including Peru, as discussed in Section 1. This constraint is brought into the model to better reflect most of the findings in the recent literature. Third, it incorporates a nonagricultural self-employed sector. This addition is also in line with empirical evidence about the increasing importance of the nonfarm economy in rural areas (for example, Haggblade, Hazell, and Reardon 2007).

More formally, assume a rural household has three sources of income: (1) agricultural self-employment, (2) nonagricultural self-employment, and (3) wage-earning activities. For tractability, assume that the household has a constant utility of leisure and maximizes its expected income subject to time and investment constraints.[4] Denote $F(.)$, $H_f$, $K_f$, and $P$ as the output, hours of work, capital, and (normalized) price of agricultural self-employment and $G(.)$, $H_n$, and $K_n$ as the output, hours of work, and capital of nonagricultural self-employment. Assume constant elasticity of substitution (CES) production functions for both activities.

$$F(H_f, K_f) = \gamma\left[aH_f^{\alpha} + (1-a)K_f^{\alpha}\right]^{1/\alpha} \tag{1a}$$

$$G(H_n, K_n) = \eta\left[bH_n^{\beta} + (1-b)K_n^{\beta}\right]^{1/\beta} \tag{1b}$$

[3] Field's approach, based on the Agricultural Household Model, includes a tenure security function as a function of time exclusively used for guarding purposes. This tenure security function is solely part of the household's utility; its relationship with the production side relies on the time constraint (that is, guarding takes away time that could be used working).

[4] For tractability purposes, this assumes that the household is risk neutral.

where $0 \le a, b \le 1$. The household also receives an income from salaried employment, which is determined by $H_w$ and the prevailing wage in the market ($w$). Thus, the household income is determined by $PG(H_n, K_n) + F(H_f, K_f) + wH_w$.

There are two constraints in this problem. First, as suggested by previous findings in the empirical literature, it is credit constrained. In this sense, it holds a limited endowment $\overline{K}$ that it can allocate as capital between alternative self-employed activities (that is, $K_f + K_n \le \overline{K}$). Second, there is the usual time restriction, such that $H_n + H_f + H_w \le T$.

Furthermore, the household faces a risk of expropriation because its land—and, hence, its land-attached investments—might be taken away. This probability of expropriation is a function of formal property rights (characterized by the parameter $\theta$) and the time spent working on the land ($H_f$). In this line, $H_f$ has two effects on agricultural production. On one side, it has a standard effect on output via the agricultural production function. On the other side, it guards capital and reduces the probability of expropriation. This probability is modeled through the function $\phi(H_f, \theta)$, which is decreasing and convex in both arguments: $\phi_\theta, \phi_{H_f} \le 0$; $\phi_{\theta\theta}, \phi_{H_f H_f} \ge 0$. Additionally, assume that $\theta$ and $H_f$ are substitutes in the tenure security function (that is, $\phi_{\theta H_f} \ge 0$): As formal property rights arise, informal guarding through $H_f$ becomes marginally less important. For simplicity, assume that the household chooses the level of $K_f$ but faces the risk of expropriation after it has been used in the production process. If r is the unitary value of the capital (relative to the nonagricultural output), then the expected capital loss is $L(H_f, K_f \mid \theta) = r\phi(\theta, H_f)K_f$.

Then the household's problem is:

$$\textbf{MAX} \qquad PG(K_n, H_n) + F(K_f, H_f) + wH_w - L(H_f, K_f \mid \theta)$$
$$\text{subject to}: H_n + H_f + H_w \le T\,; K_n + K_f \le \overline{K} \tag{2}$$

Assume that both constraints are binding and that there exists an interior solution.[5] Then, the first-order conditions are as follows:

$$PaH_f^{\alpha-1}\gamma\left[aH_f^{\alpha} + (1-a)K_f^{\alpha}\right]^{\frac{1}{\alpha}-1} - rK_f\phi_{H_f} = w \tag{3a}$$

$$bH_n^{\beta-1}\eta\left[bH_n^{\beta} + (1-b)K_n^{\beta}\right]^{\frac{1}{\beta}-1} = w \tag{3b}$$

$$P(1-a)K_f^{\alpha-1}\gamma\left[aH_f^{\alpha} + (1-a)K_f^{\alpha}\right]^{\frac{1}{\alpha}-1} - r\phi = (1-b)(\overline{K} - K_f)^{\beta-1}\eta\left[bH_n^{\beta} + (1-b)K_n^{\beta}\right]^{\frac{1}{\beta}-1} \tag{3c}$$

Equations (3a)–(3c) simply state that the productivity of labor in both self-employed activities should be equal to the external wage and that the productivity of capital should be the same among its alternative uses. To analyze the impact of formal property rights enhancement, the comparative statics were calculated for $K_f$, $H_f$, and $H_n$[6]. Total differentiation of the system of first-order conditions yields:

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[5] The conditions for the solution to be a maximum rely on the determinant of the system:
$\frac{1}{H_f K_f [aH_f^{\alpha} + (1-a)K_f^{\alpha}]^2}\eta r b(1-b)(1-\beta)H_n^{\beta-2}(\overline{K} - K_f)^{\beta}[bH_n^{\beta} + (1-b)(\overline{K} - K_f)^{\beta}]^{\frac{1}{\beta}-2}[r\phi_{H_f}^2 H_f K_f (aH_f^{\alpha} + (1-a)K_f^{\alpha})^2 - Pa(1-a)(1-\alpha)H_f^{\alpha}K_f^{\alpha}(2\phi_{H_f} + H_f\phi_{H_f H_f})F_{(.)}]$.

To assure a maximum, the following restrictions can be imposed: and

$r\phi_{H_f}^2 H_f K_f [aH_f^{\alpha} + (1-a)K_f^{\alpha}]^2 - Pa(1-a)(1-\alpha)H_f^{\alpha}K_f^{\alpha}(2\phi_{H_f} + H_f\phi_{H_f H_f})F_{(.)} \le 0$.

[6] Comparative statics for the system were calculated using Mathematica.

$$\frac{\partial K_f}{\partial \theta} = \frac{1}{\Delta}\frac{K_f}{H_f}\left[(\tau + \lambda H_f \phi_{H_f H_f})\phi_\theta + H_f(\tau - \lambda \phi_{H_f})\phi_{\theta H_f}\right] \tag{4a}$$

$$\frac{\partial H_f}{\partial \theta} = \frac{1}{\Delta}\left[(\tau - \lambda \phi_{H_f})\phi_\theta + \tau \phi_{\theta H_f}\right] \tag{4b}$$

$$\frac{\partial H_n}{\partial \theta} = -\frac{1}{\Delta}\frac{H_n K_f}{[\overline{K} - K_f]H_f}\left[(\tau + \lambda H_f \phi_{H_f H_f})\phi_\theta + H_f(\tau - \lambda \phi_{H_f})\phi_{\theta H_f}\right] \tag{4c}$$

where:

$$\begin{aligned}
\Delta &= rH_f K_f \phi^2_{H_f}[aH_f^\alpha + (1-a)K_f^\alpha]^2 - Pa(1-a)(1-\alpha)H_f^\alpha K_f^\alpha(2\phi_{H_f} + H_f\phi_{H_f H_f})F_{(.)}] \le 0 \\
\tau &= Pa(1-a)(1-\alpha)H_f^\alpha K_f^\alpha F_{(.)} \ge 0 \\
\lambda &= rH_f K_f [aH_f^\alpha + (1-a)K_f^\alpha]^2 \ge 0
\end{aligned}$$

The signs of $\frac{\partial K_f}{\partial \theta}$, $\frac{\partial H_f}{\partial \theta}$, and $\frac{\partial H_n}{\partial \theta n}$ are ambiguous. In terms of the model, the Field effect would be represented by the size of $\phi_{\theta H_f}$. If $\phi_{\theta H_f}$ is large enough, there is a substitution in the tenure security function between formal property rights and agricultural labor for guarding purposes. In this case, the agricultural activity (being directly affected by the titling) decreases and there is a substitution toward nonagricultural activities (such that $\frac{\partial H_f}{\partial \theta} \le 0$, $\frac{\partial K_f}{\partial \theta} \le 0$, $\frac{\partial H_n}{\partial \theta} \ge 0$, $\frac{\partial K_n}{\partial \theta} \ge 0$).

There is also an effect by which agricultural capital is enhanced. This effect is represented by the size of $\phi_\theta$, which captures the direct decrease in land-attached expropriation risk derived from land titling. If $\phi_\theta$ is large relative to $\phi_{\theta H_f}$, we are able to capture the pure investment effect. In an extreme, consider a situation in which $\phi_\theta > 0$, $\phi_{\theta H_f} = 0$. In this case, due to the increase in the benefits of land assets, it can be shown that $\frac{\partial K_f}{\partial \theta} \ge 0$. However, the impact on labor allocation is somewhat more ambiguous and depends on the complementarity between labor and capital in the agricultural production function. To illustrate this point, take the two extreme cases: perfect complementarity between factors ( $\alpha \to -\infty$ ) and perfect substitutability ( $\alpha = 1$ ).

First, assume that $\alpha \to -\infty$. Replacing $\phi_{\theta H_f} = 0$ and taking limits of (4a)–(4c) leads to (5a)–(5c). In this case, $\frac{\partial K_f}{\partial \theta}$ and $\frac{\partial H_f}{\partial \theta}$ both experience a proportionate increase.

$$\left.\frac{\partial K_f}{\partial \theta}\right|_{\alpha \to -\infty} \to -\frac{K_f}{H_f}\frac{\phi_\theta}{(2\phi_{H_f} + H_f\phi_{H_f H_f})} \ge 0 \tag{5a}$$

$$\left.\frac{\partial H_f}{\partial \theta}\right|_{\alpha \to -\infty} \to -\frac{\phi_\theta}{(2\phi_{H_f} + H_f\phi_{H_f H_f})} \ge 0 \tag{5b}$$

$$\left.\frac{\partial H_n}{\partial \theta}\right|_{\alpha \to -\infty} \to \frac{K_f H_n}{H_f(\overline{K} - K_f)}\frac{\phi_\theta}{(2\phi_{H_f} + H_f\phi_{H_f H_f})} \le 0 \tag{5c}$$

On the contrary, assume that the factors in $F_{(.)}$ are perfect substitutes ($\alpha = 1$). To reduce the number of activities, assume that $\gamma a > w$ (so $H_n = 0$ ).[7] Then, the household problem reduces to $P\gamma\left[aH_f + (1-a)K_f\right] + \eta\left[b(T - H_f)^{\beta} + (1-b)(\overline{K} - K_f)^{\beta}\right]^{1/\beta} - r\phi_{(\theta, H_f)}K_f$, for which the following condition should hold in an interior solution:[8]

$$\left(K_f, H_f\right) \in \left\lfloor\frac{T-H_f}{\overline{K}-K_f}\right\rfloor^{1-\beta} \frac{\left\lfloor\gamma a - r\phi_{H_f}K_f\right\rfloor(1-b)}{b} = \gamma(1-a) - \phi r \tag{6}$$

If $\phi_{\theta H_f} = 0$, condition (6) implies that an increase in $\theta$ leads to higher , lower , or a combination of both.[9] In this way, when there is perfect substitutability between production factors, $\frac{\partial K_f}{\partial \theta} \geq 0$ and $\frac{\partial H_f}{\partial \theta} \leq 0$. Thus, the results of the model show the following:

1. If the Field effect is large, substitution of labor and formal property rights in the tenure security function can lead to a reduction in agricultural and an increase in nonagricultural labor. Intuitively, there is a shift away from farm labor as the guarding requirements of the plot are relieved.
2. If the productivity effect is large enough, the reduction in the risk of expropriation leads to higher investments in agricultural capital. However, the reallocation of labor depends on the complementarity or substitutability of labor and capital in the agricultural production function. If they are complementary factors, there will be an increase in agricultural and a reduction in nonagricultural labor. However, this is not the case if they are substitutes.

All in all, the impact of titling on labor allocation is ambiguous. The model is a good starting point to reason out the mechanisms through which property rights might affect households' income-generating strategies. However, both effects cannot be estimated separately. . First, this would require a structural model, which is a daunting task because of limited data. Second, it would require the assumption of a specific functional form for the expropriation function, of which little is known. Thus, this paper attempts only a reduced-form estimate of the net impact of the productivity vis-à-vis the Field effect. The parameters of the model cannot be estimated and do not determine the relative magnitudes of the two drivers. This paper attempts only to calculate the net effect of property rights on labor allocation.

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[7] It is straightforward to derive the results for $\gamma a < w$. We only need to substitute $\gamma a - r\phi_{H_f}K_f$ for . In both cases, these expressions are not affected by a change in $\theta$.

[8] Note that $G_{(.)} = \eta\left[b(T - H_f)^{\beta} + (1-b)(\overline{K} - K_f)^{\beta}\right]^{1/\beta}$. This rules out $H_f = T$ and $K_f = \overline{K}$ as possible solutions. The case where $H_f = 0$ and $K_f = 0$ is trivial because there is no agricultural production. The remaining corners are $(H_f = 0,\ K_f > 0)$ and $(H_f > 0,\ K_f = 0)$. In the former, $K_f \in \{P\gamma a - r\phi_{H_f}K_f - \eta(1-b)[bT^{\beta} + (1-b)(\overline{K} - K_f)^{\beta}]^{1/\beta - 1}T^{\beta-1} = 0\}$ and an increase in $\theta$ leads to an increase in . In the latter, $H_f \in \{P\gamma a - \gamma b[b(T - H_f)^{\beta} + (1-b)\overline{K}^{\beta}]^{1/\beta-1}(T - H_f)^{\beta-1} = 0\}$ and changes in $\theta$ do not affect the equilibrium. So, in the corners, the optima $\frac{\partial K_f}{\partial \theta} \geq 0$ and $\frac{\partial H_f}{\partial \theta} = 0$ hold.

[9] To understand this result, notice that the right-hand-side term of (6) depends positively on production factors $\frac{\partial K_f}{\partial \theta} \geq 0$ and $\frac{\partial H_f}{\partial \theta} \leq 0$.

# 3. INSTITUTIONAL CONTEXT

In the early 1990s, land property rights in Peru were highly informal. Poor property rights are the result of a long historical process that can be traced back to the late 1960s. In 1968, the Peruvian government implemented a large agrarian reform. As part of the reform, large estates (haciendas) were expropriated from their owners and redistributed among the workers. These workers were required to form cooperatives to administer these lands. During this process, it is estimated that 9.4 million hectares were expropriated and adjudicated to cooperatives (IADB 2001).

By the mid-1970s, most of these cooperatives were bankrupt; however, legislation prevented them from dissolving. The laws allowing for their dissolution were not passed until 1980.[10] As they dissolved, these cooperatives transferred their land to the individual members. However, this transfer did not include any official documents.

These chaotic land transfers delayed the formation of a true and accurate land property registry. Moreover, for many additional years, the country kept laws restricting the sale, rental, and collateralization of land and requirements for the maximum and minimum sizes of plots.[11] All these factors stimulated extralegal arrangements in the land market that led to a prevailingly high degree of informality. The 1994 Agricultural Census reveals that, out of a total of 5.7 million parcels, only 971,000 (17 percent) had registered property titles (Antle et al. 2003).

As a consequence, there has been a high degree of land tenure insecurity in Peru. Some hacienda owners have demanded to have their land returned in judiciary courts. This has been possible due to two circumstances. First, when the haciendas were expropriated, the government issued compensatory bonds (*Bonos de la Reforma Agraria*) to the landowners. However, even after four decades of agrarian reform, the Peruvian government has still not paid these bonds. Some argue that rather than an expropriation, this was actually a confiscation and thus should be void. Second, the absence of official documentation of land transfers between cooperatives and their members increase fears of legal retaliation among landholders. Without proof, poorer peasants are usually afraid of facing the richer former hacienda owners in court.[12]

During the early 1990s, a set of initiatives was passed to eliminate the cumbersome restrictions on land transactions. Nevertheless, this set of laws had little impact on formalization. Among other arguments, the lengthy and costly process of formalization of property is considered to be one of the obstacles for farmers.[13] In this context, the Peruvian government created the *Programa Especial de Titulación de Tierras y Catastro Rural* (Special Program of Land Titling, or PETT) in 1992. The objective of the project was to build an accurate cadastre of agricultural parcels and to provide property titles to informal landholders. Communities were selected by PETT officers, and surveyors then traveled to these communities to convey information about land ownership status.

When the landholders did not have a property title, surveyors offered a Certificate of Possession if they could prove "direct, continuous, peaceful, and public" occupation of the land for five or more years. Few requirements were imposed to demonstrate occupation. According to the PETT regulation, the following were acceptable proofs: (1) documents related agricultural loans; (2) municipal fee payments; (3) receipts of payments for irrigation water, inputs, or other factors where the plot can be identified; (4) registration in the community irrigation association; (5) sale receipts of agricultural/livestock output; (6) judiciary inspection of the plot; or (7) any other document "proving possession." In the latter case, it is enough to present a written declaration from all adjacent neighbors for confirmation. After the Certificate of Possession is issued, it is sent to the National

[10] Legislative Decree 85 of 1981 formally allowed the dissolution of agricultural cooperatives.

[11] For a detailed discussion of changes in the law regarding land ownership, see Eguren (2004).

[12] For example, the National Agrarian Confederation (which groups small landholders) during its September 2003 strike demanded that the government "enact a legal security decree to guarantee the property of the beneficiaries of the Agrarian Reform putting end, once and for all, to the land lawsuits supported by bad judges." The press release, published in many Peruvian newspapers, can be found here: http://www.rel-uita.org/old/sindicatos/cna.htm

[13] There are no specific measures of how lengthy or costly the process of land titling in Peru can be. De Soto (2000) provides a vivid description of the red tape and prohibitively expensive cost of formalizing urban dwellings in Lima in the 1990s. The difficulties of titling land in rural areas were probably even larger.

Superintendence of Public Registration (SUNARP). This agency notifies the community neighbors about the registration and sets a 30-day period in which complaints can be presented. If no complaint is filed, then SUNARP registers the property rights. The delivery of final property titles to the owners is usually carried out in a massive public ceremony. It is of high importance that this process is implemented at no cost to landholders.

PETT was launched slowly in 1993, but the bulk of the titling process started in 1996, when it was boosted by loans from international agencies (IADB 2001). The process was implemented through a "cadastre and titling of individual landholdings in pre-determined geographic zones, within pre-established boundaries and in systematic registration (sweeping)."[14] In other words, the project selected communities and *swept* them, offering titles to households—implying that there was no selection of potential recipients of titles within selected villages. The 2004 PETT survey (described in Section 4) indicates that around 89 percent of households in villages where any certificate was issued by PETT received property titles. Fort (2008) also finds that only a reduced number of households were not able to receive their property titles within villages in which PETT operated. Some anecdotal evidence also indicates that before moving to a different area, PETT waited for most of a community to be titled in order to hand over the titles in a public ceremony. Presumably, households that were not titled within selected communities were absent during the days in which PETT registrars visited the village or had problems with the required documentation. All this information is consistent with the sweeping strategy of the program.

PETT has been important for at least two reasons. First, it has had a very large scope. The program has titled and registered more than 1.5 million parcels, increasing the share of legally owned plots by 50 percent (Fort 2008). This makes it one of the largest land-titling programs in the world. Second, if the project aimed to reduce tenure insecurity, there is evidence that this goal has been accomplished. For example, Larson et al. (2000) interviewed a sample of farmers in the valley of Huaral (around 80 kilometers north of Lima) and found that among untitled households, 73 percent thought that titles would increase their tenure security.[15] Also, the PETT survey (described in the following section) shows that the program enhanced their tenure security. When households were asked about the most important gains of titling, 66.5 percent of them considered tenure security issues (that is, a reduced risk of land expropriation, a decreased likelihood of having their land squatted on, and a greater possibility of bequeath) to be the most important benefits.

Moreover, from a methodological standpoint, PETT allows some identification problems that hamper most of the previous literature to be addressed. The way in which PETT operated seems to indicate that there was little self-selection of beneficiaries at the household level. Unfortunately, there was no experimental selection of villages in which titles were offered, and the rules for the selection of communities were not clear. This poses some challenges for the identification strategy, which must rely, by assumption, on selection on observables (discussed in Section 4). Anecdotal evidence and interviews with PETT officers suggest that the communities chosen were the most accessible ones. While the loan contract with IADB imposed implicit goals for titled land, it did not establish criteria for the selection of villages. In a country like Peru, which is characterized by considerable geographic diversity and heterogeneous accessibility, the most likely option was to choose the communities that could be most readily accessed in order to more easily reach titling goals. Another potential bias might have come from program officers choosing communities that had the highest potential of reaping the benefits of titling (for example, more educated, with better land quality and climate, and so on). In this line, previous work analyzing the impact of the PETT project has also shown this concern (Torero and Field 2005; Fort 2008). This would be likely if the government were pursuing a favorable impact evaluation of the program by IADB.

[14] See Appendix A of the loan contract between the Inter-American Development Bank and the Republic of Peru in IADB (2001).

[15] Interviewed households could give more than one perceived benefit for titling. In addition to the increase in tenure security, other benefits of property titles were the following: enhancement of access to credit (44 percent), larger possibility of getting credit from formal sources (30 percent), sale of land (10 percent), and facilitation of inheritance (5.1 percent).

# 4. DATA AND EMPIRICAL STRATEGY

The data for the evaluation of PETT were collected in 2004 by the Group for the Analysis of Development (GRADE). One shortcoming was that the program did not gather baseline information prior to implementation. To address this problem, the evaluation team decided to follow up with households from the Peruvian Standard Living Measurement Surveys (LSMS) of 1994, 1997, and 2000. In addition to the usual LSMS information, the 2004 survey asked households if they had received a title by PETT and, if so, when they had received it. In this way, their baseline (pretreatment) information could be inferred from their reports in the previous surveys.

The sample was collected to resurvey most of the households in the 2000 LSMS. Unfortunately, it was not possible to obtain information from households that had moved from their 2000 locations. The attrition rate during this period was around 10 percent. To gain information from households that had been titled in previous periods, random samples from the 1994 and 1997 surveys were also resurveyed. A subsample of the 1994 and 1997 households was interviewed in the 2000 round because of a reduced panel component in the LSMS design. Additionally, to have enough evaluation cases, an oversampling of the areas in which PETT operated was included, for which no panel information is available. This oversampling was proportional to the sizes of the primary sampling units in the 2000 LSMS, preserving the representativeness of the sample at the domain level of the original survey.[16]

With this framework, two samples of information, a cross-section dataset and a panel of households, were constructed. Regarding the cross-sectional data, the variability of households' information in 2004 was exploited. After the exclusion of households that did not own any land or that had property titles not awarded by PETT, the final dataset includes 1,043 households, of which 44 percent have received the program intervention.

While the cross-sectional data provide a larger number of observations due to the oversampling, the panel allows for removal of any time-invariant unobservables at the cost of a smaller sample. Given the complexity of the panel dataset, a brief description is provided. While all households in the panel are required to be in the 2004 sample, they can appear in a biannual fashion (2004/1994, 2004/1997, or 2004/2000), a triannual fashion (2004/2000/1997, 2004/2000/1994, or 2004/1997/1994), or in all years (2004/2000/1997/1994). Because of the relatively small sample contained in each subpanel, all the observations are pooled together.

Thus, it should be noted that households may appear in more than two rounds of the survey.[17] In the case of titled households, when more than one pretreatment observation is available the observation from the closest round to the titling was used.[18] In an analogous way, when titled households have more than one post-treatment observation   the one from the closest round immediately after the titling was chosen. This procedure better proxies the criteria under which the beneficiaries were chosen by their pretreatment characteristics and reduces contamination of the effect due to other factors in the post-treatment measurement. However, in the case of untitled households, there is no reason to drop any available information. In this sense, the number of groups in the control group is increased by constructing more than one span. For example, if the household was untitled, three spans were constructed for a household surveyed in 2004, 2000, and 1997: 1997–2000, 2000–2004, and 1997–2004. The final panel dataset has 572 spans[19]: 118 treatment and 454 control spans.

[16] For a more detailed description of the sample, see Torero and Field (2005).

[17] For example, a household that was titled in 2002 may appear in the 1997, 2000, and 2004 rounds. Its observations for the 1997–2000 period would be included in the control group, while the data for the 2000–2004 span would be assigned to the treatment group.

[18] For example, a household might declare in the 2004 survey that it had received the title in 1998. If possible, the pretreatment characteristics are calculated with the 1997 wave. If the latter is not available, the information from the 1994 round was used. However, it is possible that only information for 2000 or none of the pre-2004 rounds were available. In such cases, there is no way to recover the pretreatment characteristics and, in this case, these households are dropped from the sample.

[19] These spans are constructed from 419 panel households: 301 untitled and 118 treated ones.

The analysis of the relationship between property rights and households' decisions is usually hampered by their endogenous relation. If households are required to exert effort or incur costs to receive a title, then only those perceiving net gains from this process would demand it. A considerable number of papers have accounted for this problem only to a limited extent by applying usual exclusion restrictions or by exploiting the variability of differences in the security given by alternative titles. The Peruvian context, and in particular the data presented here, proves especially favorable in addressing this problem. As mentioned, the program registrars were the ones visiting various villages and offering property titles to informal owners with few, if any, requirements. In this sense, titling came as a consequence of an assignment by PETT, irrespective of individual demand for it. As stated, based on how PETT operated, bias from households' demand was not expected. Nonetheless, biases arising from differences in the attributes of the settlements chosen by the program can still affect the analysis.

Following the impact evaluation literature, the impact of PETT on households' incomes and time allocations was estimated using matching methods (Angrist 1998; Angrist and Krueger 1999). Denote as the income and hours of work in wage-earning, agricultural self-employed, and nonagricultural self-employed activities of the household $i$. Next, consider the following two potential outcomes for each of these variables: if the household received a title and if it did not. Also define $D_i$ as an indicator variable for the titling, where $D_i = 1$ if the household was titled by PETT and $D_i = 0$ if the household was not. Then the average treatment on the treated (ATT) effect would be defined as:

$$\tau = E[Y_{1i} - Y_{0i} \mid D_i = 1]. \tag{7}$$

A simple comparison of sample means of titled and untitled households would yield a biased estimate of , given that:

$$E[Y_{1i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0] = E[Y_{1i} - Y_{0i} \mid D_i = 1] + \{E[Y_{0i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0]\}$$
$$\tau = E[Y_{1i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0] - \{E[Y_{0i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0]\}. \tag{8}$$

The term $E[Y_{0i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 0]$ in equation (8) represents the bias that would arise because the outcomes of the untitled households would not necessarily have been the same as those of the titled ones had they benefited by the program. Ideally, if the treatment were assigned randomly, the bias would disappear because $D_i$ would be independent of $Y_{1i}$ and $Y_{0i}$. Arguably, assignment of property titles was not random, so it was conditioned on a set of observables. As mentioned, there were no clear rules in the assignment of the titled villages. However, some anecdotal evidence highlights ease of access to the location and PETT officers' perceptions of potential impact as determinants of the treatment. This is the same approach that other papers analyzing PETT have followed (Torero and Field 2005; Fort 2007, 2008).

If there is selection on observables, it is possible to condition on this set of variables to calculate the ATT. In the case of the cross-sectional data, however, there are no pretreatment characteristics to control for the selection of the program. Furthermore, controlling for current household characteristics would impose an *over-control* bias because the post-treatment characteristics had probably been affected by the treatment and would therefore be endogenous.[20] To circumvent this problem, a set of proxies were proposed for pretreatment characteristics. On one hand, district characteristics from two censuses previous to the land titling program, the 1993 Population Census and the 1994 Agricultural Census, were

[20] Previous papers have dealt with the relationship of property titles to varied outcomes such as fertility (Field 2003), housing decisions (Galiani and Schargrodsky 2004), and education (Galiani and Schargrodsky 2007). Given that these were some of the controls I was planning to use, there is a need to look for pre-treatment proxies.

used.[21] These include controls for differences in population, land, agricultural activity characteristics, education, and household composition. Additionally, time-invariant geographic characteristics, measures of the accessibility of the village such as the distance to the provincial capital, slope, altitude, and regional dummies were used. The estimation assumes that this set of variables is close to the one that PETT authorities chose to operate the program and that the census variables are good proxies for them. Moreover, given that these proxies come from a period prior to the PETT operations, they might arguably have determined the treatment but were not subsequently affected by it.

The reasoning of the estimation is similar for the panel estimations. There are only three differences in this case. First, rather than using proxies, the probability of selection is calculated using household-specific pretreatment characteristics. Second, to remove time-invariant unobservable attributes, the outcome variables are the differences between the pre- and post-treatment values (that is, difference-in-differences estimation). Fourth, given the various nature of the spans for the control and the treatment groups, the control (or controls) for each treated household is forced to come from the same span.[22]

Under these assumptions—upon conditioning on observables $X_i$ (either proxies or pretreatment variables from surveys)—we can get valid counterfactuals for titled households because the untreated households would be a close representation of what would have happened to the treated ones had the program not taken place. Then, it would hold that:

$$E[Y_{0i} \mid X_i, D_i = 1] = E[Y_{0i} \mid X_i, D_i = 0] . \tag{9}$$

Thus, re-expressing (8) with the assumptions embedded in (9), can be estimated as:

$$\begin{aligned} \tau &= E\{E[Y_{1i} \mid X_i, D_i = 1] - E[Y_{0i} \mid X_i, D_i = 1] \mid D_i = 1\} \\ \tau &= E\{E[Y_{1i} \mid X_i, D_i = 1] - E[Y_{0i} \mid X_i, D_i = 0] \mid D_i = 1\} . \end{aligned} \tag{10}$$

To reduce the dimensionality problem of conditioning on the full set of variables in , a propensity score is estimated. As suggested by Dehejia and Wahba (1998), the probability of assignment to treatment, conditional on pretreatment variables, summarizes the preintervention variables and allows for comparison of the treatment and control groups. As suggested by the impact evaluation literature, the probability of receiving the titling can be approximated using a probit model. In the case of the panel data, however, households may appear more than once in the calculation of the propensity score and their observations would be correlated. In this line, a random-effects probit is estimated.[23]

$$P(D_i = 1 \mid X_i) = \Phi(X_i \beta) \tag{11}$$

Furthermore, to increase comparability between the observations in the treatment and control groups, all the estimations are performed within common support. This procedure excludes the observations in the treatment and control groups in which the propensity scores of the treatment and

[21] The Population Census was conducted on July 11, 1993, and the Agricultural Census was conducted between October 15 and November 14, 1994. It should be noted that these are the only two censuses available during the period of analysis. A district is the smallest political unit in the country and has, on average, around 2,650 households. These data are the only source of indicators that are representative at the district level.

[22] The logic for this is as follows. The Peruvian economy has experienced different trends in each of the periods in the panel. For example, 1994–1997 was a period of economic recovery after the crisis following the implementation of structural reforms in the early 1990s. The 1997–2000 span was a period of stagnation, and 2000–2004 was characterized by economic expansion again. Thus, it would be inappropriate to consider the differences in outcomes of a nontitled household during 1997–2000 as a suitable counterfactual for another one titled between 1997 and 2004.

[23] A fixed-effects probit cannot be estimated, because of the incidental parameter problem (see Lancaster 2000). As an alternative approach, a random-effects probit is proposed, even when this implies an assumption of orthogonality between the explanatory variables and the household-specific disturbance. These results are robust to the exclusion of the random effects.

control groups do not overlap (that is, the controls that are below the minimum propensity score in the treatment group and the treatments that have propensity scores that exceed the maximum of the propensity score in the control group).

Following (10) and (11), a propensity score matching ATT estimator can be stated as:

$$\tau = E\{E[Y_{1i} \mid \Phi(X_i\beta), D_i = 1] - E[Y_{0i} \mid \Phi(X_i\beta), D_i = 0] \mid D_i = 1\} \quad (12)$$

As a final procedure to estimate the ATT, alternative methods were chosen on which to condition the outcomes. Although several methods are available in the literature, the one-to-one, radial, and kernel density estimators were used. The standard errors and significance of the estimates are calculated with a thousand-iteration bootstrap of the matching procedure. Given that most of comes from census variables, the standard errors are clustered at the district level.

# 5. RESULTS

## Cross-Section

Table 5.1 shows the summary statistics for the labor time allocation and income-generating structure from wage-earning, nonagricultural self-employed, and agricultural self-employed activities. While it appears that titled households have higher incomes and work more hours in all the activity categories—as mentioned in the previous section—no inference about the effect of the treatment can be made from a simple mean comparison.

**Table 5.1—Cross-section (2004): Average household income and hours of work in treatment and control groups**

| | Control | Treatment | |
|---|---|---|---|
| Monthly income [1] | | | |
| Wage earning | 334.0 | 450.9 | *** |
| Nonagricultural self-employment | 142.2 | 186.0 | |
| Agricultural self-employment | 414.3 | 452.7 | |
| Total | 890.5 | 1,089.6 | *** |
| Weekly hours of work | | | |
| Wage earning | 54.3 | 62.6 | ** |
| Nonagricultural self-employment | 9.5 | 13.0 | ** |
| Agricultural self-employment | 38.4 | 40.4 | |
| Total | 102.1 | 116.0 | *** |
| Observations | 585 | 458 | |

Source: Author's calculations using the 2004 PETT survey.
Note: [1] Monthly income is reported in equivalent soles of May 2000 of metropolitan Lima.
Asterisks denote significance of t-tests, on the difference of means between the treatment and of means between the treatment and control groups: *** 1%, ** 5%, * 10%.

Summary statistics for the pretreatment characteristics in the titled and untitled groups, based on a set of pretreatment proxies, are presented in the first two columns of Table 5.2. The chosen variables at the district level were education, household demographics, employment composition, and infrastructure variables from the 1993 Population Census and landholding and quality of agricultural potential from the 1994 Agricultural Census.[24] Additionally, the collection of GPS geo-referenced household locations in the 2004 survey was exploited to construct household-specific distance to the provincial capital, slope, and altitude measures. The latter allowed the construction of household-specific propensity scores even within the same district.

[24] One of the problems with this method is that some of the households might have lived in a different location before 1993. Thus, attributing to these households the district averages of the 1993 Population Census and the 1994 Agricultural Census would be incorrect. However, only 5 percent of the sample is affected by this problem. The exclusion of these households does not affect the cross-sectional results in this section.

**Table 5.2—Cross-section (2004): Unmatched and matched summary statistics of treated and control group**

| | Unmatched | | | Matched[1] | | |
|---|---|---|---|---|---|---|
| | Control | Treatment | | Control | Treatment | |
| Population Census 1993 (district) | | | | | | |
| % of households (HHs) with pipeline water | 22.10% | 22.30% | | 22.20% | 22.60% | |
| % of HHs with electricity | 26.90% | 26.20% | | 25.80% | 26.00% | |
| % of HHs with dirt floors | 71.70% | 73.60% | | 73.90% | 75.20% | |
| % of HHs with sewerage | 13.00% | 14.40% | | 14.50% | 13.80% | |
| % of population speaking native language | 26.50% | 29.50% | | 29.00% | 34.10% | ** |
| % of population with secondary education | 21.70% | 20.20% | *** | 20.00% | 20.10% | |
| % of population with tertiary education | 5.70% | 6.60% | *** | 6.50% | 6.10% | |
| Average dependency ratio | 0.50 | 0.49 | | 0.49 | 0.50 | |
| % of population working in agriculture | 18.70% | 26.40% | *** | 26.90% | 27.40% | |
| Population density in district | 84.21 | 108.83 | ** | 110.48 | 189.63 | |
| Agricultural Census 1994 (district) | | | | | | |
| Average size of landholding (ha) | 3.41 | 3.69 | | 3.61 | 3.48 | |
| % of land with irrigation in district | 53.00% | 39.00% | *** | 37.60% | 35.40% | |
| Total agricultural land in district (ha) | 6,799.6 | 7,440.55 | | 7,641.75 | 7,046.60 | |
| Bioclimate score | 52.29 | 61.85 | *** | 64.3 | 66.92 | |
| Score of soil quality | 58.02 | 60.33 | *** | 60.66 | 59.61 | |
| Geographic variables (HH) | | | | | | |
| Distance to provincial capital (km) | 16.37 | 11.04 | *** | 10.72 | 10.38 | |
| Slope | 10.44 | 12.44 | *** | 12.81 | 13.8 | |
| Altitude (km above sea level) | 1.39 | 2.05 | *** | 2.10 | 2.15 | |
| Jungle[2] | 30.10% | 29.20% | | 29.10% | 27.60% | |
| Highlands[2] | 44.50% | 65.60% | *** | 67.00% | 70.50% | |
| Weather variables (district) | | | | | | |
| Average rainfall in district [3] (mm/month) | 67.44 | 61.33 | ** | 61.53 | 58.82 | |
| Average temperature (°C) | 16.87 | 13.87 | *** | 13.68 | 13.13 | |
| Observations | 585 | 458 | | 417 | 417 | |

Source: Author's calculations using the 2004 PETT survey,
Notes: [1] Means of treatment and control groups after a one-to-one propensity score matching on common support (with a caliper of 0.05) on all the variables included in this table.
[2] In the case of discrete variables, a t-test of proportions was applied.
[3] Average of the minimum and maximum monthly rainfalls in the district (in mm).
Asterisks denote significance of t-tests, on the difference of means between the treatment and of means between the treatment and control groups: *** 1%, ** 5%, * 10%.

An initial analysis of raw means (in the first two columns) confirms that titled and untitled households were arguably selected based on different pretreatment characteristics. More specifically, it suggests that titling was more intensive in areas with a higher population working in agricultural activities, better soil, and (albeit not significant) slightly larger agricultural holdings. Furthermore, these results confirm anecdotal evidence suggesting that the program more intensively targeted areas that were closer to provincial capitals, which are probably the easiest to reach. This suggests that the beneficiaries of PETT were on average 5 kilometers closer to capitals than households that did not receive titles.

To address these differences in preintervention characteristics, a propensity score matching estimation was performed based on the variables of Table 5.2 and applying the matching methods suggested in Section 4. The probit estimation for program selection is presented in Table A.1 in the appendix. To check for balancing in the variables of the score between the treatment and control groups, the third and fourth columns of Table 5.2 present the means of the matched sample based on a one-to-one matching (with a caliper of 0.05). Although the percentage of the 1993 population speaking a native language in the district remains statistically different between the titled and untitled groups, none of the rest of the pretreatment proxies are significantly different.[25] Even when not conclusive, this exercise suggests that the procedure would eliminate the bias (at least on observables) derived from the nonrandom selection of the program.

Table 5.3 provides the difference in the outcome variables between the treatment and control groups using one-to-one (unrestricted and with a 0.05 caliper[26]), radial (with radii of 0.05 and 0.01[27]), and kernel matching (with a 0.06 bandwidth[28]) techniques. Different matching techniques yield similar results, suggesting relative robustness in the effects. The calculated ATT effects indicate that titled households increase their monthly income from agricultural self-employed activities by around 160 soles (about 16 percent of the sample mean income). Additionally, any increase in the weekly hours of work in the same category is much smaller: about 3.5 hours (3 percent of sample mean). Furthermore, the results suggest that neither income nor hours of work changed by much and neither is statistically significant.

**Table 5. 3—Cross-section (2004): Average treatment on the treated (ATT) effect for household income and hours of work, by matching method**

| Dependent Variable | One-to-One: No Caliper | One-to-One: Caliper=0.05 | Radius: r=0.05 | Radius: r=0.01 | Kernel |
|---|---|---|---|---|---|
| Monthly income [1] | | | | | |
| Wage earning | 42.73 | 57.37 | 38.26 | 45.45 | 41.71 |
| | (89.15) | (83.00) | (67.32) | (70.80) | (65.50) |
| Nonagricultural self-employment | 6.91 | 9.57 | 16.47 | 11.30 | 12.57 |
| | (63.59) | (63.32) | (47.25) | (51.63) | (45.02) |
| Agricultural self-employment | 154.38 * | 158.69 * | 165.85 ** | 155.32 ** | 161.33 ** |
| | (94.81) | (91.85) | (70.34) | (74.56) | (71.71) |
| Total | 204.01 | 225.63 * | 220.58 ** | 212.08 * | 215.61 ** |
| | (129.56) | (121.22) | (93.09) | (104.34) | (92.74) |
| Weekly hours of work | | | | | |
| Wage earning | 4.77 | 4.75 | 3.66 | 4.05 | 3.72 |
| | (7.42) | (7.38) | (5.81) | (5.91) | (5.50) |
| Nonagricultural self-employment | -1.37 | -1.21 | -0.30 | -0.74 | -0.52 |
| | (4.09) | (3.97) | (3.03) | (3.37) | (2.99) |
| Agricultural self-employment | 3.34 | 3.71 | 3.81 | 3.19 | 3.64 |
| | (3.43) | (3.26) | (2.49) | (2.76) | (2.51) |
| Total | 6.75 | 7.25 | 7.17 | 6.49 | 6.83 |
| | (8.78) | (8.45) | (6.11) | (6.62) | (6.26) |

Source: Author's calculations using the 2004 PETT survey, the 1994 Peruvian National Agricultural Census and the 1993 Peruvian Population Census.
Note: Matching based on propensity score (in common support), estimated through a probit equation of the treatment dummy on the covariates included in Table 5.2. The probit estimation is reported in Table A.1 in the appendix.
[1] Monthly income is reported in equivalent soles of May 2000 of metropolitan Lima.
Standard errors in parentheses were corrected by bootstraps of 1,000 replications, with clusters at the district level. Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance level: *** 1%, ** 5%, *10%.

[25] Table A.2 (in the appendix) shows additional balancing of selection variables for one-to-one (unrestricted), radial (with radii of 0.05 and 0.1), and a 0.06 bandwidth kernel matching. The radial and kernel estimations seem to do a better job and achieve balancing of all the selection variables. However, the results are qualitatively the same.

[26] Alternative calipers (that is, 0.025, 0.01, 0.075, 0.1, 0.12, and 0.15) lead to very similar results.

[27] These results are also robust to alternative radii. Radii of 0.025, 0.01, 0.075, 0.1, 0.12, and 0.15 yield similar results.

[28] This 0.06 bandwidth was cross-validated following the leave-one-out procedure proposed by Frölich (2004) and Black and Smith (2004).

All in all, this information constitutes the first evidence that the productivity effect is larger than the Field effect. Also, given the much larger increase in income versus hours of work in agricultural self-employed activities, the returns per hour in this sector are increasing. Comparatively, though, changes in returns in the nonagricultural activities remained constant. Though not conclusive, this would be indicative of a shift in investments from nonagricultural to agricultural self-employed activities. Indeed, these results should be interpreted with caution due to various pitfalls: They use district-level proxies for household characteristics, exploit only geographic variation of a policy that was implemented in different moments of time, and so on. The following section attempts to address these shortcomings.

### Panel Data

Estimations using the panel dataset present several advantages with respect to those discussed in the previous section. First, they capture not only geographic dispersion of the treatment but also time variation. Second, they allow us to control for individual household pretreatment characteristics rather than relying on aggregate-level proxies. Better controls are important because, in essence, this paper relies on selection in observables to estimate the effect of PETT. Third, if there are omitted variables in the selection process, the panel estimation allows us to at least exclude time-invariant unobservables.

Table 5.4 includes the summary statistics of incomes and hours of work from wage-earning, nonagricultural self-employed, and agricultural self-employed activities in the panel data. Consistent with Table 5.1, households in the treatment groups experience larger increases in all income categories when compared to a control group. However, there are clearly differentiated patterns in labor allocation. Changes in hours of work spent on wage-earning activities are roughly the same in both groups, but titled households have larger decreases in their hours of work spent on nonagricultural activities and much larger increases in the hours of work spent on self-employed activities. As detailed in this section, these differences in labor allocation remain even after controlling for observables and time-invariant unobservables.

**Table 5.4—Panel: Changes in household income and hours of work in pre- and post-treatment periods**

| Dependent | Control | | | | Treatment | | | |
|---|---|---|---|---|---|---|---|---|
| Variable | Pre | Post | Diff | | Pre | Post | Diff | |
| Monthly income[1] | | | | | | | | |
| Wage earning | 154.7 | 309.6 | 154.8 | *** | 244.1 | 416.0 | 171.9 | *** |
| Nonagricultural self-employment | 72.5 | 100.3 | 27.7 | * | 134.9 | 185.3 | 50.4 | |
| Agricultural self-employment | 233.3 | 311.2 | 77.8 | *** | 262.6 | 421.6 | 159.0 | * |
| Total | 460.6 | 721.0 | 260.4 | *** | 641.5 | 1022.9 | 381.4 | *** |
| Weekly hours of work | | | | | | | | |
| Wage earning | 55.8 | 59.6 | 3.8 | | 63.5 | 67.1 | 3.6 | |
| Nonagricultural self-employment | 8.9 | 7.6 | -1.3 | | 17.0 | 12.1 | -4.8 | ** |
| Agricultural self-employment | 37.4 | 38.3 | 1.0 | | 31.1 | 43.5 | 12.4 | *** |
| Total | 102.1 | 105.6 | 3.5 | | 111.6 | 122.7 | 11.1 | * |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.

Note: [1]Monthly income is reported in equivalent soles of May 2000 of metropolitan Lima.

Asterisks denote significance of t-tests, on the difference of means between the treatment and of means between the treatment and control groups: *** 1%, ** 5%, * 10%.

Summary statistics for (unmatched) pretreatment characteristics of titled and untitled households are presented in the first two columns of Table5.5. They show that the pretitling features of the two groups were considerably different. For example, the distance to the capital is almost twice as large in the control compared to the treatment group. Some differences also arise in the characteristics of dwelling, age, and migration status of the head of household. To compare these dissimilar groups, a random-effects probit was estimated to calculate the probability of receiving a property title (see Table A.3 in the appendix). The explanatory variables considered are characteristics of the head of household, demographic composition, assets, and some district and geographic characteristics. Fortunately—given the large number of control households (around four controls per treated household)—matching methods achieve balance of all proposed characteristics in the common support, as suggested by the matched characteristics of households in the last two columns of Table 5.5.[29]

**Table 5.5—Panel: Pretreatment characteristics of the treatment and control group, before and after matching**

| Dependent Variable | Unmatched Control | Unmatched Treatment | | Matched[1] Control | Matched[1] Treatment |
|---|---|---|---|---|---|
| Household head | | | | | |
| Age | 45.51 | 48.31 | * | 46.74 | 46.12 |
| Years of education | 5.51 | 5.38 | | 4.74 | 5.88 |
| Female[2] | 8.10% | 11.00% | | 9.00% | 5.10% |
| Married[2] | 87.90% | 83.10% | | 87.20% | 89.70% |
| Speaks native language[2] | 33.90% | 34.70% | | 39.70% | 39.70% |
| Migrant[2] | 76.90% | 83.90% | * | 75.60% | 78.20% |
| Household composition | | | | | |
| Household size | 5.55 | 5.62 | | 5.73 | 5.67 |
| Dependency ratio | 1.10 | 1.07 | | 1.03 | 1.10 |
| Assets | | | | | |
| Own land | 3.90 | 3.64 | | 3.10 | 3.55 |
| Value of durable assets[3] | 439.24 | 652.45 | | 398.10 | 621.98 |
| Connected to electricity grid[2] | 19.80% | 28.00% | * | 19.20% | 28.20% |
| Dirt floor[2] | 79.50% | 86.40% | * | 91.00% | 88.50% |
| Cane/mud walls[2] | 21.60% | 13.60% | * | 15.40% | 15.40% |
| Mud roofs[2] | 28.40% | 16.90% | ** | 25.60% | 19.20% |
| District characteristics | | | | | |
| Bioclimate score | 53.50 | 63.56 | *** | 58.27 | 60.45 |
| Score of soil quality | 56.45 | 60.47 | ** | 57.76 | 57.50 |
| Average temperature (°C) | 16.83 | 13.43 | *** | 15.03 | 14.33 |
| Average rainfall (mm/month) [4] | 64.16 | 59.76 | | 68.59 | 65.38 |
| Population density | 103.32 | 92.40 | | 95.64 | 80.80 |
| Geographic characteristics | | | | | |
| Distance to provincial capital (km) | 18.12 | 9.44 | *** | 11.19 | 11.59 |
| Slope | 12.23 | 13.94 | | 13.62 | 14.13 |
| Altitude (km above sea level) | 1.49 | 2.14 | *** | 1.98 | 1.89 |
| Jungle[2] | 29.30% | 26.30% | | 33.30% | 34.90% |
| Highlands[2] | 48.50% | 68.60% | *** | 57.30% | 59.00% |
| N | 454 | 118 | | 102 | 102 |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.

Note: [1] Means of treatment and control groups after a one-to-one propensity score matching (with a caliper of 0.05), in the common support, on all the variables included in this table.

[2] In the case of discrete variables, a t-test of proportions was applied.

[3] Value of the following durable assets: kitchen, television, radio, phone, refrigerator, motorcycle, blender, boiler, car, and sewing machine. The value is expressed in equivalent soles of May 2000 of metropolitan Lima.

[4] Average of the minimum and maximum monthly rainfalls in the district (in mm).

Asterisks denote significance of t-tests, on the difference of means between the treatment and of means between the treatment and control groups: *** 1%, ** 5%, * 10%.

Although this probability is based on pretreatment characteristics, the outcomes are differences in incomes and hours of work. Each treated household's change in an outcome was matched with that of a

[29] This is also the case for other matching methods: unrestricted one-to-one and radial (with radii of 0.01 and 0.05) matchings, as shown in Table A.4 in the appendix.

control household (at least for those in the common support region), such that the difference in probabilities of treatment is minimized. Also, the time span in both pairs of observations was forced to be the same. If the changes in the outcomes of the titled household come from a certain time span (that is, 1997–2000, 2000–2004, or 1997–2004), it was compared to the changes in the outcomes of an observation in the control group in the same period. In this way, the estimation eliminates time effects due to macroeconomic policies or economic cycles, as well as time-invariant unobservables.

The ATT effects (difference-in-differences estimations) are provided in Table 5.6. Four different estimators applying one-to-one (unrestricted and with a 0.05 caliper) and radial (with radii of 0.01 and 0.05) matching methods are shown.[30] Aside from small differences, the estimates are the same across alternative matching methods. Results indicate, on one side, that there is a reallocation of labor from nonagricultural to agricultural self-employed activities. Although hours of work on nonagricultural activities decreased on average by around 2 hours, the estimate suggests that the decrease among the treated groups was more than double this number. For hours of work in agricultural activities, there was almost no change in the control group, while the treatment group experienced a considerable increase (between 10 and 11, depending on the estimate).

Though not statistically significant, the point estimates suggest large increases in agricultural earnings. On average, the increase among the titled households was around four to six times that experienced by those in the control group. In contrast, the income gains of titled households in the nonagricultural sector were roughly half those of their counterparts. Once again, changes in income relative to changes in hours in the treatment group suggest an increase in productivity in agricultural self-employment.

Thus, the panel results seem to confirm the results from the cross-section. There are some differences in the magnitudes, arguably arising from controlling for household time-invariant nonobservable attributes in the difference-in-differences estimation. Despite these small differences, however, the storyboard remains the same: The proposed Field effect is smaller than the productivity effect.

**Table 5.6—Panel: Average treatment on the treated effect - Ddifference-in-Differences (DID) estimation, by matching method**

| | One-to-One (No Caliper) | | | Caliper = 0.05 | | |
|---|---|---|---|---|---|---|
| | Control | Treatment | DID | Control | Treatment | DID |
| Monthly income [1] | | | | | | |
| Wage earning | 197.3 * | 185.5 *** | -11.9 | 205.1 * | 186.6 *** | -18.5 |
| | (140.8) | (67.3) | (159.5) | (125.2) | (74.6) | (149.2) |
| Nonagricultural | 68.5 | 28.8 | -39.7 | 68.8 | 28.8 | -40.0 |
| self-employment | (60.0) | (51.1) | (76.5) | (69.4) | (56.6) | (92.2) |
| Agricultural self- | 39.7 | 231.8 ** | 192.1 | 49.3 | 231.2 *** | 181.9 |
| employment | (84.7) | (114.2) | (140.7) | (97.7) | (114.0) | (146.0) |
| Total | 305.6 * | 446.1 *** | 140.5 | 323.2 * | 446.6 *** | 123.4 |
| | (187.3) | (145.6) | (235.0) | (184.9) | (144.6) | (232.7) |
| Weekly hours of work | | | | | | |
| Wage earning | 16.2 | 3.1 | -13.1 | 15.8 | 2.4 | -13.5 |
| | (14.9) | (7.5) | (16.3) | (14.8) | (8.4) | (16.9) |
| Nonagricultural | 1.4 | -4.1 | -5.5 | 1.2 | -4.1 | -5.4 |
| self-employment | (3.9) | (2.8) | (4.9) | (4.1) | (3.2) | (5.4) |
| Agricultural self- | 0.1 | 11.9 *** | 11.9 * | 0.2 | 11.3 *** | 11.1 * |
| employment | (5.8) | (3.6) | (6.7) | (5.5) | (3.7) | (6.4) |
| Total | 17.6 | 10.9 | -6.8 | 17.3 | 9.5 | -7.8 |
| | (16.5) | (7.1) | (17.3) | (16.1) | (8.2) | (18.0) |

[30] These results are fairly robust to the choice of different calipers and radii (not shown here)

**Table 5.6—Continued**

| | Radius = 0.05 | | | Radius = 0.01 | | |
|---|---|---|---|---|---|---|
| | Control | Treatment | DID | Control | Treatment | DID |
| Monthly income [1] | | | | | | |
| Wage earning | 178.38 | 183.52 | 5.14 | 179.46 | 193.94 | 14.48 |
| | (94.20)*** | (75.20)** | (125.10) | (90.40)*** | (80.70)*** | (122.00) |
| Nonagricultural self-employment | 56.66 | 32.00 | -24.66 | 51.57 | 34.96 | -16.61 |
| | (34.30)** | (56.70) | (67.50) | (36.20)* | (60.00) | (69.20) |
| Agricultural self-employment | 59.30 | 234.63 | 175.33 | 63.04 | 244.27 | 181.22 |
| | (41.30) | (129.50)** | (136.60) | (43.50) | (139.40)** | (145.80) |
| Total | 294.35 | 450.15 | 155.81 | 294.08 | 473.17 | 179.09 |
| | (116.70)*** | (158.40)*** | (201.20) | (120.90)*** | (172.70)*** | (212.70) |
| Weekly hours of work | | | | | | |
| Wage earning | 10.69 | 2.71 | -7.98 | 9.47 | 2.70 | -6.77 |
| | -(9.80) | -(8.90) | -(13.60) | -(9.70) | -(9.50) | -(13.30) |
| Nonagricultural self-employment | 0.59 | -4.04 | -4.64 | -0.02 | -3.35 | -3.33 |
| | (2.20) | (3.30) | (4.00) | (2.30) | (3.70) | (4.50) |
| Agricultural self-employment | 0.16 | 11.22 | 11.06 | -0.12 | 9.64* | 9.76 |
| | (3.50) | (3.90)*** | (5.30)** | (3.40) | (4.40)* | (5.40)* |
| Total | 11.45 | 9.88 | -1.56 | 9.33 | 8.98 | -0.34 |
| | (10.80) | (8.30) | (14.00) | (10.70) | (9.40) | (14.00) |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.
Note: Matching based on propensity score, in the common support, estimated through a probit equation of the treatment dummy on the covariates included in Table 5.5. Probit estimation is presented in Table A.3 in the appendix.
[1] Monthly income is reported in equivalent soles of May 2000 of metropolitan Lima.
Standard errors in parentheses were calculating with a bootstrap of 1,000 replications, with clusters at the district level.
Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance levels: *** 1%, ** 5%, * 10%.

### Check for Attrition in Sample

One concern is that attrition might be partly responsible for the results presented previously. The logic is as follows: If property titles enable households to sell their land and leave their communities, then the most likely to do so are those households with less potential for agricultural activities. These observations would then drop out of the sample, and only those with higher prospective agricultural income will remain in it.

To address this problem, the full-sample estimates of the previous subsection were compared with the ones from a restricted panel of the 2000–2004 rounds. When collecting information from the previous LSMS in 2004, the design of the survey targeted most of the rural households in the 2000 LSMS. As a result, the attrition rate for this subsample was considerably lower (around 10 percent in a four-year period). Table 5.7 shows the characteristics (in 2000) of households that owned land in 2000 and were reinterviewed in 2004 (Panel A) and of those that owned land in 2000 but dropped out of the 2004 sample (Panel B). Households in Panel B were more likely to speak a native language and had a smaller proportion of female heads. However, they are similar in all other household characteristics. Of more importance is that they are similar in terms of their land and their hours of work and income across alternative activities. This piece of information suggests that—at least in the most relevant dimensions—households that "moved out" or could not be reinterviewed were not significantly different from the ones that stayed.

**Table 5.7—Characteristics (in 2000) of attrited and nonattrited households (between the 2000 and 2004 rounds)**

| | Both in 2004 and 2000 | Only in 2000 |
|---|---|---|
| Female household head | 12.1% | 4.5% |
| Age of household head | 48.2 | 46.2 |
| Household head is married | 83.1% | 81.2% |
| HH head speaks native language | 44.9% | 51.9% |
| Years of education of household head | 5.8 | 6.1 |
| Household head is migrant | 78.3% | 75.1% |
| HH size | 5.1 | 5.0 |
| % of members 0–6 y.o. | 16.8% | 15.4% |
| % of members 7–14 y.o. | 19.4% | 16.9% |
| % of members 15–35 y.o. | 30.8% | 33.3% |
| % of members 36–60 y.o. | 21.0% | 20.0% |
| % of members >60 y.o. | 12.0% | 14.4% |
| Connection to sewerage | 24.4% | 17.4% |
| Connection to electricity grid | 42.4% | 41.6% |
| Own land (ha) | 2.4 | 3.1 |
| HH weekly hours of work | 109.7 | 108.9 |
| Nonagricultural self-employed | 13.4 | 12.4 |
| Agricultural self-employed | 35.0 | 35.6 |
| Wage earning | 61.4 | 60.9 |
| HH monthly income | 557.1 | 536.3 |
| Nonagricultural self-employed | 113.3 | 103.5 |
| Agricultural self-employed | 223.7 | 202.0 |
| Wage earning | 220.2 | 230.9 |
| N | 881 | 98 |

Source: Author's calculations using the 2004 PETT survey and the 2000 Peruvian LSMS.
Notes: Includes only the attrited and nonattrited households in the sample that had land in 2000.
Asterisks denote significance of t-tests, on the difference of means between groups: *** 1%, ** 5%, * 10%.

Table 5.8 presents the propensity score results for the 2000–2004 subsample. Standard errors of the ATT increase considerably—probably due to a quite smaller sample size—but, all in all, the estimates have the same direction as the ones from the full sample. These results suggest that attrition is not likely to be responsible for the results.

**Table 5.8—Average treatment on the treated effect (difference-in-differences) estimation (one-to-one matching)**

| | Sample | |
|---|---|---|
| | 2000–2004 | All[1] |
| Income (monthly) | | |
| Wage earning | 105.8 | -11.9 |
| | (309.8) | (159.5) |
| Nonagricultural self-employment | -23.5 | -39.7 |
| | (219.2) | (76.5) |
| Agricultural self-employment | 161.1 | 192.1 |
| | (192.2) | (140.7) |
| Total | 243.4 | 140.5 |
| | (257.0) | (235.0) |
| Hours of work (weekly) | | |
| Wage earning | -13.3 | -13.1 |
| | (27.1) | (16.3) |
| Nonagricultural self-employment | -19.2 | -5.5 |
| | (25.0) | (4.9) |
| Agricultural self-employment | 8.1 | 11.9 |
| | (11.3) | (6.7) * |
| Total | -24.3 | -6.8 |
| | (19.4) | (17.3) |
| N treated | 45 | 118 |
| N control | 118 | 454 |

Source: Author's calculations using the 2004 PETT survey and the 2000 Peruvian LSMS.
Notes :Estimation followed the same procedures as the estimations in Table 5.6 with a restricted sample for (nonattrited) households in the 2000–2004 panel.
[1]This column corresponds to the results presented in Table 5.6.
Standard errors in parentheses were calculated with a bootstrap of 1,000 replications, with clusters at the district level. Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance level: *** 1%, ** 5%, * 10%.

A possible explanation for this is that even when a proportion of households moved out, it was not because of the high prospects of selling their land. The Peruvian land market is relatively thin and might have hindered this kind of behavior.[31] This is also consistent with the findings of Larson et al. (2000) and Barrantes and Trivelli (1994), who argue that property titles in Peru do not increase the fluidity in the land market. Treated households might not have been able to sell their land even after a title was issued.

### The Role of Tenure Security

One of the main points of the model presented in Section 2 is that titles increase households' tenure security. If the households increase their perception of security ($\phi$), they shift their capital toward agricultural activities; thus, productivity in this sector increases and more labor is reallocated to it. In terms of the model, imagine that there are two types of households in the population, characterized by $\phi^1(H_f,\theta)$ and $\phi^2(H_f,\theta)$, where $\phi^1_\theta > \phi^2_\theta$ . If this is the case and and are complementary factors in the production function, a change in $\theta$ should have a larger effect on group 1, that is, ${dK_f^1}/{d\theta} > {dK_f^2}/{d\theta}$, ${dH_f^1}/{d\theta} > {dH_f^2}/{d\theta}$.

[31] Zegarra (1999) discusses the relative underdevelopment of Peruvian land markets and some of the causes of it.

Unfortunately, there are no questions regarding tenure security perceptions in the 1994, 1997, and 2000 LSMS rounds, but this question was included in the 2004 PETT survey. Titled and untitled households were asked to estimate the likelihood of having their land expropriated. They had to choose the most accurate among five options: (a) very sure expropriation will NOT take place, (b) relatively sure expropriation will NOT take place, (c) expropriation is quite a likely outcome, (d) expropriation is very likely, or (e) does not know. Those that responded (a), (b), and (c) were labeled as "sure" about their property rights, while those who answered (c) and (d) were deemed "unsure." A small proportion did not know (e) and were excluded from the sample.

When the sample was split, there were not many households that were titled and remained unsure about their property rights. In this respect, the titling process seemed to have been effective in increasing the perception of security. Despite the small number of observations in this group, a propensity score estimate was constructed and differentiated effects were calculated for "sure" and "unsure" titled households. This would be equivalent to a heterogeneous treatment effect. In a regression framework, this is similar to an estimate of $\beta_3$ in $Y = \beta_1 Titled + \beta_2 Sure + \beta_3 Titled\, x\, Sure$.

The results are presented in Table 5.9. There is a large increase in the standard errors, so the results are not statistically significant. However, they indicate that, if anything, there is a much larger increase in hours of work and income in agricultural self-employment among the "sure." In contrast, this group experiences the contrary effect for nonagricultural activities. This result suggests that, at least in part, tenure security is driving the effect of property titles on labor allocation.

**Table 5.9—Cross-section: Average treatment on the treated effect by tenure security perception**

| | Sure | | | Not Sure | | | (A)-(B) |
|---|---|---|---|---|---|---|---|
| | Treatment | Control | Diff (A) | Treatment | Control | Diff (B) | |
| Monthly income | | | | | | | |
| Wage | 440.6 | 402.4 | 38.2 | 573.0 | 338.9 | 234.1 | -195.9 |
| earning | (40.0) | (73.5) | (83.5) | (234.3) | (164.8) | (272.7) | (272.9) |
| Nonagricultural | | | | | | | |
| self- | 170.0 | 171.9 | -1.9 | 424.3 | 208.4 | 216.0 | -217.9 |
| employment | (36.0) | (59.9) | (61.6) | (213.2) | (130.7) | (247.6) | (243.1) |
| Agricultural self- | 463.2 | 304.2 | 158.9 | 273.1 | 249.8 | 23.3 | 135.6 |
| employment | (63.4) | (80.6) | (91.9) | (86.9) | (143.6) | (169.9) | (171.2) |
| Total | 1073.7 | 878.5 | 195.1 | 1270.4 | 797.0 | 473.4 | -278.2 |
| | (73.1) | (108.4) | (121.4) | (356.6) | (238.3) | (407.9) | (395.4) |
| Weekly hours of work | | | | | | | |
| Wage | 61.5 | 57.1 | 4.4 | 61.3 | 54.8 | 6.5 | -2.1 |
| earning | (4.1) | (6.5) | (7.2) | (13.6) | (15.4) | (19.8) | (20.1) |
| Nonagricultural | | | | | | | |
| self- | 12.3 | 14.1 | -1.8 | 22.9 | 15.2 | 7.8 | -9.5 |
| employment | (2.3) | (4.0) | (3.9) | (10.1) | (8.3) | (12.0) | (11.6) |
| Agricultural self- | 41.1 | 37.5 | 3.6 | 38.2 | 38.0 | 0.2 | 3.4 |
| employment | (1.9) | (3.3) | (3.6) | (3.7) | (6.0) | (7.0) | (7.6) |
| Total | 114.9 | 108.7 | 6.2 | 122.5 | 108.0 | 14.5 | -8.2 |
| | (4.2) | (8.1) | (8.1) | (18.3) | (18.4) | (23.7) | (23.6) |
| N | 401 | 336 | | 48 | 230 | | |

Source: Author's calculations using the 2004 PETT survey.
Notes: Estimation method: one-to-one matching with 0.05 caliper.
Households were asked if they considered expropriation as a likely outcome. There were five options: (a) very sure expropriation will NOT take place, (b) relatively sure expropriation will NOT take place, (c) expropriation is quite a likely outcome, (d) expropriation is very likely, or (e) does not know. (a) and (b) were considered to be "sure," while (c) and (d) were considered "unsure" about their tenure security. Those who responded (e) were excluded from the sample.
Standard errors in parentheses were calculated with a bootstrap of 1,000 replications, with clusters at the district level.
Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance level: *** 1%, ** 5%, * 10%.

### Substitution of Family and Hired Labor

The estimates presented so far are based on household labor allocation. Another possibility is that these estimates merely reflect the substitution of family labor for hired labor: The increase in household labor in agricultural activities might come at the expense of a reduction of hired labor, and the net effect of these changes can be ambiguous. Analogously, the reductions in nonagricultural household labor can be a mere substitution for an increase in hired workers. In terms of equation (2) in Section 2, family and nonfamily labor are indistinguishable (both grouped in $H_f$ and $H_n$).

To analyze this possibility, data for hired labor was used in the panel framework. Unfortunately, the surveys did not collect data for hours of hired agricultural labor and have no information (neither hours nor wages) about nonagricultural hired labor. However, there is information about household expenses for agricultural wages. Propensity score matching estimations with this data are presented in Table 5.10. The results indicate that, if anything, titled households have larger agricultural payroll expenses than their counterparts. This might indicate that titled households are not only allocating more family labor to agricultural self-employed activities, but are even hiring more external labor for these activities. In this line, the results are probably not driven by substitutions between family and nonfamily labor.[32] Thus, the general conclusions of the paper still seem to hold.

**Table 5.10—Panel: Changes in household (monthly) expenditures in agricultural wages in pre- and post-treatment periods, by matching method**

| | Control | | | Treatment | | | DID |
|---|---|---|---|---|---|---|---|
| | Pre | Post | Diff (a) | Pre | Post | Diff (b) | (a)-(b) |
| One-to-one | 28.1 | 30.5 | 2.3 | 20.2 | 28.3 | 8.0 | 5.7 |
| | (9.6) | (10.4) | (10.8) | (5.1) | (6.4) | (6.0) | (12.1) |
| Caliper=0.05 | 29.3 | 31.6 | 2.3 | 21.2 | 29.1 | 7.8 | 5.6 |
| | (10.9) | (10.8) | (12.0) | (6.5) | (8.0) | (7.3) | (13.6) |
| Caliper=0.01 | 30.0 | 34.0 | 4.1 | 23.2 | 30.3 | 7.1 | 3.0 |
| | (11.1) | (12.6) | (14.8) | (7.9) | (9.6) | (8.0) | (16.8) |
| Radius=0.05 | 27.8 | 29.3 | 1.5 | 21.2 | 29.5 | 8.2 | 6.7 |
| | (6.6) | (6.7) | (5.7) | (6.6) | (8.0) | (6.7) | (8.4) |
| Radius=0.01 | 28.9 | 30.8 | 1.9 | 22.9 | 30.1 | 7.2 | 5.3 |
| | (5.8) | (5.9) | (6.3) | (7.4) | (9.4) | (8.1) | (10.1) |
| Kernel | 28.9 | 30.5 | 1.7 | 22.7 | 29.5 | 6.8 | 5.1 |
| | (5.6) | (6.0) | (5.8) | (7.5) | (9.7) | (8.0) | (9.5) |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.
Notes: Expenses in agricultural wages are expressed in equivalent soles of May 2000 of metropolitan Lima.
Standard errors in parentheses were calculated with a bootstrap of 1,000 replications, with clusters at the district level.
Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance levels: *** 1%, ** 5%, * 10%.

### Robustness Check

A robustness check for the hypothesis of shifts between investment in nonagricultural and agricultural self-employed activities is presented. This robustness check is based on the following reasoning. If the reason for changes in productivity between these two sectors is investment, larger changes are expected in the capital stock in each to experience more pronounced variations with longer time exposure to the property title. If this is true, then the differential in productivity between agricultural and nonagricultural self-employment should be higher for households that received the PETT intervention earlier. It should be

[32] Indeed, one can argue that this reasoning is inaccurate because there can be differences in hourly wages between treated and untreated areas. If wages in treated areas are sufficiently high, titled households might actually be hiring fewer hired hours of work. Although I cannot rule out this possibility, it has been shown that propensity score matching techniques balance local observable characteristics of treated and untreated areas.

noted that no definite conclusions should be extracted from this check and that more detailed data on investment patterns in different activities is required for a thorough assessment of the proposed productivity channel.

First, the panel dataset and separate households based on time of exposure to the treatment was used to test for this possibility. They were separated t into two groups: (1) households whose post-treatment outcomes were measured four or fewer years after receiving the title and (2) those whose post-treatment outcomes were measured more than four years after receiving the title. For example, consider a household in the treatment group observed in the 1997–2004 span. If it received the title after 2000, then it would be in the first group. Otherwise, it would be in the second. Next, within these subsamples, propensity score matching was performed based on the same variables proposed above to check for differences in the ATT (difference-in-differences) effect. A caveat of this robustness check is that these results should not be interpreted as an independent estimation of the effect of the treatment in groups that have held the title for more or fewer years. When disaggregating by time of exposure, the small number of observations per individual period prevents me from estimating separate probit equations for each one. The estimates are based on the same probit equation, and the only difference is the group in which titled and untitled households are matched by their propensity score. Thus, these results should be interpreted more as decompositions of the ATT effect for the whole sample in different periods rather than as individual estimates.[33] For the sake of brevity, only the decomposition of the one-to-one unrestricted matching is presented, though the results are consistent regardless of the specific matching technique used.

The results of this check are presented in Table 5.11 and are comparable to those in the first three columns of Table 5.6. Two patterns emerge from this decomposition. First, it suggests that the process of labor reallocation is gradual over time. Increases in hours of agricultural work and reductions in hours of nonagricultural self-employed activities are larger in households that have been exposed to the treatment for longer periods of time. These results are consistent with how the productivity effect operates due to higher capital accumulation with respect to longer exposure to property rights. Second, it can be noted that the relative difference between changes in income and changes in hours of work in both agricultural and nonagricultural self-employed activities rises within households that have held property titles for longer. This would suggest that investment recomposition has some influence in the results.

[33] Nevertheless, the two approaches are the same, assuming that the weights given to different variables by the probit equation are constant in time, that is, if the selection criteria of the program remained unaltered in the different subperiods.

**Table 5.11—Decomposition of the average treatment on the treated effect by number of years of titling[1]**

| | More Than Four Years of Titling | | | Four or Fewer Years of Titling | | |
|---|---|---|---|---|---|---|
| | Control | Treatment | DID | Control | Treatment | DID |
| Monthly income | | | | | | |
| Wage earning | 182.4 | 218.3 | 35.9 | 96.9 | 150.9 | 54.0 |
| | (130.8) | (96.5)*** | (164.7) | (94.5) | (76.1)** | (125.5) |
| Nonagricultural self-employment | 92.6 | -33.7 | -126.2 | 84.3 | 72.8 | -11.5 |
| | (81.0) | (74.9) | (107.7) | (64.4) | (52.2) | (84.0) |
| Agricultural self-employment | 91.2 | 409.6 | 318.4 | 46.4 | 87.0 | 40.6 |
| | (105.8) | (241.1)*** | (268.2) | (90.1) | (73.9) | (105.4) |
| Total | 366.1 | 594.2 | 228.1 | 227.6 | 310.7 | 83.1 |
| | (187.4)** | (266.7)*** | (337.2) | (155.9) | (123.9)*** | (200.5) |
| Weekly hours of work | | | | | | |
| Wage earning | 26.0 | -2.2 | -28.2 | 7.3 | 4.4 | -2.9 |
| | (17.5) | (10.6) | (22.1) | (12.2) | (9.7) | (15.8) |
| Nonagricultural self-employment | 3.5 | -3.4 | -6.8 | 0.5 | -4.8 | -5.3 |
| | (4.5) | (3.5) | (5.9) | (4.0) | (3.9) | (5.7) |
| Agricultural self-employment | -4.0 | 8.7 | 12.7 | 1.2 | 13.0 | 11.8 |
| | (6.4) | (5.0)* | (8.4) | (4.7) | (4.4)*** | (6.3)** |
| Total | 25.7 | 2.9 | -22.8 | 9.0 | 12.7 | 3.7 |
| | (18.4) | (10.0) | (23.6) | (13.3) | (9.7) | (16.4) |
| N | 454 | 49 | | 454 | 69 | |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.
Notes:[1] Average treatment on the treated (ATT) effects are calculated following the same methodology as in Table 5.6. Probit regression was estimated using the full sample (1994–2004), but individuals are matched only within each sample.
Standard errors in parentheses were calculated with a bootstrap of 1,000 replications, with clusters at the district level. Significance of the estimate is reported following the percentiles of the distributions of the bootstrapped estimator.
Significance levels: *** 1%, ** 5%, * 10%.

# 6. CONCLUDING REMARKS

In this paper, evidence has been presented about the relatively unexplored relationship between labor allocation patterns and land property rights. The paper develops a model in which there are two effects by which land titling affects labor allocation. On one hand, titling releases time that was used for land-guarding purposes and should thus have a negative effect on on-farm hours of work (Field effect). On the other hand, property rights should increase tenure security, promote land-attached investments, increase productivity in agricultural activities, and increase the number of on-farm hours of work (productivity effect).

To empirically assess the net impact of these two opposite effects, the Special Program of Land Titling (PETT), a massive program in rural, Peru was analyzed. Two separate datasets, a cross-section based on a 2004 survey and a four-round (1994/1997/2000/2004) panel dataset, were constructed. Using proxies for pretreatment characteristics from censuses previous to the implementation of PETT, propensity score matching methods were used on the 2004 cross-section. The results showed that the productivity effect dominates the Field effect, such that land titling leads to a larger number of on-farm hours of work. Additionally, estimates of the effect of the titles in a smaller panel of households confirm the cross-sectional results. The findings are consistent with the theoretical model when land titles considerably decrease the possibility of expropriation and there is complementarity between agricultural capital and labor.

There is previous evidence of the positive relationship between property rights and investments in agricultural capital. In that spirit, this study suggests that these results are driven by increases in agricultural labor productivity. Nonetheless, the evidence of this investment-shift hypothesis presented here is far from conclusive. The objective of this paper has been to present a preliminary exploration of this effect. More work is required in this area to fully understand investment reallocation processes and should constitute a priority in a future research agenda.

## APPENDIX: PROBIT MODELS FOR PROBABILITIES OF TREATMENT

**Table A.1—Probit for propensity score: Cross-section (2004)**

| | Coefficient | S.E. |
|---|---|---|
| Population Census 1993 (district level) | | |
| % of households with pipeline water | 2.8791 | (0.6104)*** |
| % of households with electricity | -0.4445 | (0.4937) |
| % of households with dirt floors | -1.9335 | (0.3584)*** |
| % of households with sewerage | 4.4837 | (0.8702)*** |
| % of population speaking native language | -0.0805 | (0.2219) |
| % of population with secondary education | -4.2751 | (1.3289)*** |
| % of population with tertiary education | 4.2069 | (2.5265)* |
| Average dependency ratio | -5.8237 | (2.5447)** |
| % of population working in agriculture | 1.0125 | (0.3171)*** |
| Population density in district | 0.0009 | (0.0003)*** |
| Agricultural Census 1994 (district level) | | |
| Average size of landholding | 0.0498 | (0.0253)** |
| % of land with irrigation in district | 0.0062 | (0.2344) |
| Total agricultural land in district ('000s ha) | 0.0164 | (0.0069)*** |
| Bioclimate score | -0.0021 | (0.0035) |
| Score of soil quality | 0.0289 | (0.0053)*** |
| Geographic variables (household level) | | |
| Distance to provincial capital (km) | -0.0350 | (0.0050)*** |
| Slope | -0.0088 | (0.0046)* |
| Altitude (km above sea level) | 0.0003 | (0.0010) |
| Average temperature (°C) | -0.1004 | (0.0207)*** |
| Weather variables (district level) | | |
| Average rainfall in district [2] | -0.0088 | (0.0020)*** |
| Jungle | 1.7793 | (0.2692)*** |
| Highlands | 1.4868 | (0.2976)*** |
| Constant | 3.7232 | (1.5277)** |
| Observations | | 1,043 |
| LR test | | 326.9 |
| Prob > χ | | 0.0000 |
| Pseudo R | | 0.2285 |

Source: Author's calculations using the 2004 PETT survey, the 1994 Peruvian National Agricultural Census and the 1993 Peruvian Population Census.
Notes: Dependent variable: 1= Titled by PETT, 0=Not titled.
[1] Average of the minimum and maximum monthly rainfalls in the district (in mm).
Standard Error (SE) clustered at the district level.
Significance level: *** 1%, ** 5%, * 10%.

**Table A.2—Pretreatment proxies of the treatment and control groups, before and after matching**

| | No Caliper | | Radius (r=0.05) | | Radius (r=0.01) | | Kernel | |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment | Control | Treatment | Control | Treatment | Control | Treatment |
| Population Census 1993 (district) | | | | | | | | |
| % households (HHs) with pipeline water | 21.3% | 22.3% | 21.1% | 22.6% | 22.1% | 21.1% | 23.4% | 22.3% |
| % HHs with electricity | 24.0% | 26.0% | 24.8% | 26.0% | 25.9% | 24.8% | 28.0% | 26.0% |
| % HHs with dirt floors | 72.9% | 73.8% | 74.4% | 75.2% | 73.9% | 74.4% | 74.0% | 73.8% |
| % HHs with sewerage | 14.5% | 14.5% | 13.5% | 13.8% | 14.4% | 13.5% | 15.4% | 14.5% |
| % population speaking native language | 35.3% | 28.9% * | 33.6% | 34.1% | 29.1% | 31.6% | 31.1% | 28.9% |
| % population with secondary education | 19.8% | 20.0% | 19.8% | 20.1% | 20.0% | 19.8% | 20.4% | 20.0% |
| % population with tertiary education | 6.1% | 6.6% | 6.2% | 6.1% | 6.6% | 6.2% | 7.0% | 6.6% |
| Average dependency ratio | 49.8% | 49.4% | 49.8% | 50.0% | 49.4% | 49.8% | 49.5% | 49.4% |
| % population working in agriculture | 28.5% | 26.9% | 29.3% | 27.4% | 28.0% | 29.5% | 26.8% | 26.9% |
| Population density in district | 120.9 | 112.1 | 117.8 | 119.6 | 110.3 | 114.8 | 119.7 | 112.1 |
| Agricultural Census 1994 (district) | | | | | | | | |
| Average size of landholding (ha) | 3.539 | 3.603 | 3.765 | 3.480 | 3.575 | 3.765 | 3.571 | 3.603 |
| % irrigated land in district | 35.90% | 37.71% | 36.95% | 35.40% | 37.78% | 36.95% | 35.58% | 37.71% |
| Total agricultural land in district (ha) | 7700.5 | 7624.2 | 7642.9 | 7046.6 | 7629.9 | 7642.9 | 7340.8 | 7624.2 |
| Bioclimate score | 63.2 | 62.2 | 62.6 | 66.9 | 62.2 | 62.6 | 62.5 | 62.2 |
| Score of soil quality | 60.7 | 60.5 | 60.1 | 59.6 | 60.4 | 60.1 | 60.1 | 60.5 |
| Geographic variables (household) | | | | | | | | |
| Distance to provincial capital (km) | 10.4 | 10.6 | 11.0 | 10.4 | 10.7 | 11.0 | 11.2 | 10.6 |
| Slope | 12.4 | 12.8 | 13.2 | 13.8 | 12.8 | 13.2 | 12.9 | 12.8 |
| Altitude (km above sea level) | 2.1 | 2.1 | 2.1 | 2.2 | 2.1 | 2.1 | 2171.1 | 2.1 |
| Jungle[1] | 29.9% | 29.0% | 29.3% | 27.6% | 29.0% | 29.3% | 30.0% | 29.0% |
| Highlands[1] | 65.5% | 66.8% | 67.2% | 70.5% | 66.8% | 67.2% | 66.3% | 66.8% |
| Weather variables (district ) | | | | | | | | |
| Average rainfall [2] | 53.30 | 61.37 | 61.93 | 58.82 | 61.25 | 61.93 | 62.63 | 61.37 |
| Average temperature (°C) | 14.08 | 13.68 | 13.54 | 13.13 | 13.67 | 13.54 | 13.44 | 13.68 |

Source: Author's calculations using the 2004 PETT survey, the 1994 Peruvian National Agricultural Census and the 1993 Peruvian Population Census.
Notes: Balancing comparable to Table 5.2, using alternative matching methods. Standard errors clustered at the district level.
Significance level: *** 1%, ** 5%, * 10%.
[1] A test of differences of proportions was applied for discrete variables.
[2] Average of the minimum and maximum monthly rainfalls in the district (in mm).

**Table A.3—Random-effects probit for propensity score: Panel**

| | Coefficient | S.E.[1] |
|---|---|---|
| Household head | | |
| Female | 1.1193 | (1.8200) |
| Age | 0.0695 | (0.0348)** |
| Married | -1.2873 | (1.6881) |
| Speaks native language | -0.9402 | (0.9485) |
| Years of education | 0.0749 | (0.1177) |
| Migrant | -1.6771 | (1.0696) |
| Household composition | | |
| Household size | 0.1475 | (0.1514) |
| Dependency ratio | -0.1885 | (0.4486) |
| Assets | | |
| Own land | 0.1109 | (0.0845) |
| Dwelling connected to electricity grid | -0.5523 | (0.9458) |
| Value of durable assets[2] | 0.0005 | (0.0002)** |
| Dwelling has dirt floor | -0.4144 | (0.9335) |
| Dwelling has cane/mud roofs | 0.3653 | (0.9171) |
| Dwelling has mud walls | 0.5313 | (0.9107) |
| District characteristics | | |
| Average temperature (°C) | -0.3705 | (0.1435)*** |
| Average rainfall [3] | -0.0637 | (0.0193)*** |
| Bioclimate score | -0.0121 | (0.0260) |
| Score of soil quality | 0.1382 | (0.0405)*** |
| Population in 1993 (thousands) | 0.0142 | (0.0201) |
| Population density | 0.0005 | (0.0010) |
| Geographic characteristics | | |
| Distance to provincial capital (km) | -0.1734 | (0.0478)*** |
| Slope | -0.0491 | (0.0330) |
| Altitude (km above sea level) | -0.0017 | (0.0008)** |
| Jungle | 13.7559 | (2.2659)*** |
| Highlands | 13.224 | (2.3468)*** |
| Constant | -13.2631 | (4.2237)*** |
| Observations | | 572 |
| Wald $\chi^1(25)$ | | 74.97 |
| Prob>$\chi^1$ | | 0.0000 |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.
Notes: Dependent variable: 1=Titled by PETT, 0=Not titled.
1 Standard errors clustered at the district level. Significance level: *** 1%, ** 5%, * 10%.
2 Value of the following durable assets: kitchen, television, radio, phone, refrigerator, motorcycle, blender, boiler, car, and sewing machine. The value is expressed in equivalent soles of May 2000 of Metropolitan Lima.
3 Average of the minimum and maximum monthly rainfalls in the district (in mm).

**Table A.4—Pretreatment characteristics of treatment and control group, before and after matching: Panel**

| | One-to-One | | Radial (r=0.01) | | Radial (r=0.05) | |
|---|---|---|---|---|---|---|
| | Control | Treatment | Control | Treatment | Control | Treatment |
| Household head | | | | | | |
| Age | 47.7 | 49.0 | 48.8 | 47.3 | 46.3 | 46.1 |
| Years of education | 5.4 | 4.8 | 5.2 | 5.4 | 5.2 | 5.9 |
| Female | 12.0% | 8.0% | 8.9% | 11.3% | 11.3% | 5.1% |
| Married | 82.0% | 75.0% | 74.6% | 82.5% | 85.9% | 89.7% |
| Speaks native language | 36.0% | 38.0% | 43.0% | 35.1% | 33.2% | 39.7% |
| Migrant | 83.0% | 79.0% | 79.0% | 82.5% | 76.3% | 78.2% |
| Household composition | | | | | | |
| Household size | 5.6 | 5.6 | 5.6 | 5.6 | 6.0 | 5.7 |
| Dependency ratio | 1.0 | 0.9 | 1.0 | 1.1 | 1.1 | 1.1 |
| Assets | | | | | | |
| Own land | 3.9 | 3.8 | 4.2 | 4.0 | 4.5 | 3.6 |
| Value of durable assets[2] | 727.1 | 337.3 | 419.4 | 726.8 | 427.1 | 622.0 |
| Connected to electricity grid | 21.0% | 18.0% | 21.9% | 26.8% | 21.9% | 28.2% |
| Dirt floor | 86.0% | 93.0% | 84.3% | 87.6% | 81.4% | 88.5% |
| Cane/mud walls | 23.0% | 26.0% | 30.2% | 13.4% | 18.4% | 15.4% |
| Mud roofs | 16.0% | 22.0% | 25.6% | 16.5% | 29.0% | 19.2% |
| District characteristics | | | | | | |
| Bioclimate score | 61.0 | 62.3 | 58.4 | 61.3 | 54.7 | 60.5 |
| Score of soil quality | 60.2 | 62.9 | 62.4 | 59.3 | 57.4 | 57.5 |
| Average temperature (°C) | 13.7 | 13.3 | 17.2 | 13.8 | 16.0 | 14.3 |
| Average rainfall [2] | 70.5 | 74.8 | 73.9 | 61.2 | 66.1 | 65.4 |
| Population density | 119.4 | 106.3 | 112.4 | 91.3 | 92.2 | 80.8 |
| Geographic characteristics | | | | | | |
| Distance to provincial capital (km) | 10.5 | 9.8 | 9.8 | 11.5 | 11.2 | 11.6 |
| Slope | 13.3 | 13.1 | 13.4 | 12.2 | 13.6 | 14.1 |
| Altitude (km above sea level) | 17.3 | 20.9 | 20.7 | 16.5 | 19.8 | 18.9 |
| Jungle | 38.0% | 32.0% | 39.9% | 43.5% | 33.3% | 34.9% |
| Highlands | 50.0% | 53.0% | 54.9% | 52.4% | 60.3% | 59.0% |

Source: Author's calculations using the 2004 PETT survey and the 1994, 1997 and 2000 Peruvian LSMS.
Notes Balancing comparable to Table 5.5, using alternative matching methods. Standard errors clustered at the district level.
[1]Value of the following durable assets: kitchen, television, radio, phone, refrigerator, motorcycle, blender, boiler, car, and sewing machine. The value is expressed in equivalent soles of May 2000 of metropolitan Lima.
[2]Average of the minimum and maximum monthly rainfalls in the district (in mm).
Significance level: *** 1%, ** 5%, * 10%.

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