



# Urban land rights and child nutritional status in Peru, 2004

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## Abstract

Advocates of land-titling programs in developing countries posit that these programs lead to a multitude of benefits, including health improvements. This paper presents the results of a child health survey of several Lima communities after various time exposures to Peru's urban land-titling program. The results provide suggestive evidence that improved property rights increase children's weight but not their height, which is consistent with previous work on the topic. However, titles also appear to raise children's risk of being overweight or obese, implying that the observed weight gain is not necessarily an improvement in nutritional status.

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## 1. Introduction

With slum dwellers now accounting for 43% of the urban population in developing countries (UN-HABITAT, 2003), slum growth confronts the current generation of development policy-makers with one of its greater challenges as it attempts to improve the lives of the urban poor. To this end, policy-makers have increasingly focused on urban property formalization, which is thought to improve credit access, real estate market dynamism, and residential tenure security, with the broader objective of increasing long run well-being among the poor (Deininger, 2003).

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de Soto (1989, 2000, 2003), a major proponent of this policy model, argues that property reform allows market-oriented development to become “a truly humanistic cause and an important contribution to the war on poverty” (2003, p. 185). Much at his urging, governments around Latin America – and, to a lesser extent, the world at large – have undertaken land titling projects in their efforts to alleviate urban poverty. However, the specific effects of these property interventions in promoting “humanism,” as well as alleviating the myriad deprivations associated with poverty, remain unclear.

This paper considers the effects of urban land formalization on children’s nutritional status, an important correlate and long-term determinant of well-being and poverty. Although formalization campaigns rarely cite nutrition as an explicit goal, it is closely related to their overall aims. Certainly, nutrition affects health status, an indicator of well-being; if titling affects children’s health, this would be important in itself. Moreover, nutritional deprivation in childhood may also lower lifetime productivity (Strauss and Thomas, 1998)—by impairing cognitive development, limiting educational attainment, decreasing adult body size, and heightening morbidity and mortality through the life course.<sup>1</sup> Land titling arguably remedies one market distortion – poorly defined property rights – but in considering its effectiveness as a panacea to poverty, one should also take into account its impact on childhood deprivation.

One might indeed expect land titling to have such an impact. Previous work has suggested that titles allow squatter households to increase labor force participation, primarily because they no longer need to keep an adult ‘guard’ at home to protect informal property rights (Field, 2002). Using data from a land-titling program in urban Peru, the same program analyzed in this paper, Field finds that titling leads to a 17% increase in weekly household labor hours and a 47% reduction in the likelihood that household members work at home. As time allocation incentives change, so too may the nature of child nutrition and care. The direction of this change depends on the balance of income and substitution effects. Increased labor force participation by any household member could improve child nutrition by boosting labor income. However, in the case of a child’s primary caregivers – in particular the mother – working could reduce the time spent caring for the child, the quality of that care, and the availability of mother-specific inputs such as breast milk. A large body of research has examined these topics, and the results indicate that, although maternal work often affects child well-being, the nature of the effect varies by context, wage rate, the nature of women’s work, and a variety of other factors (Glick, 2002).

Other potential pathways from property rights to child nutrition might involve investment incentives or credit access, but the existing evidence casts doubt on these as possibilities in Peru. Although the Peruvian program has led to an increase in housing investment (Field, 2005a), baseline plumbing investment rates among beneficiaries (1.5–3% annually) are probably too low to alter nutrition in the short run. Titling could also expand credit access, enabling credit-constrained households to better finance their children’s nutrition, but evidence for credit market expansion in Peru is either non-existent or extremely limited, depending on the author (Calderón Cockburn, 2003; Field and Torero, 2004).

To investigate the effects of land titling on child nutritional status, I analyze data from a survey of 27 Lima communities that have participated in Peru’s urban land-titling program, which past

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<sup>1</sup> On cognitive development and learning capacity, see Cravioto and Arrieta (1986). On educational attainment, see Glewwe and Jacoby (1995) and Alderman et al. (2001), but also note Behrman’s critique of this literature (1996). See Martorell and Ho (1984) and Fogel (1994) on morbidity and mortality, and Martorell (1999) on body size.

research (Field, 2002, 2003) has cast as a natural experiment. Over an 8-year period, the program distributed formal property titles to 1.4 million households, on parcels averaging roughly 200 m<sup>2</sup> (Apoyo, 2000).<sup>2</sup> Coupled with its impressive scale, the program's focus on urban areas makes it the first of its kind, a potential model for future urban development policy. With governments from numerous other countries following suit, the effects of the Peruvian program on poverty and well-being have implications for urban slum dwellers around the world. To gauge the nutritional effects of this exogenous change in tenure status, I conducted a cross-sectional health survey of 549 households, exploiting community-level variation in the timing of program intervention. The titling intervention ran from roughly 1996 to 2003/2004, the children were born from 1999 to 2004, and the data were collected in a single cross-section in 2004. Using households that had property titles before the program as a control group (following Field), I estimate the differential effects of titling by examining differences in outcomes between *ex ante* squatters and non-squatters across varying time exposures to the program. If the effects of titling build over time, differences will be smaller among households with longer program exposure.

To date, the recent paper by Galiani and Schargrodsky (2004) is the only analysis of child health and nutrition in the urban land rights literature. Using data from a small-scale expropriation in an Argentine locality, the authors find that land-titling is associated with increases in weight-for-height among children, but height-for-age remains unchanged. They conjecture that their results imply a change in short-term nutritional status, which is consistent with the accepted wisdom on child growth patterns (Falkner and Tanner, 1986), and conclude that titling enables investment in human capital, acknowledging that the effects are probably modest.

However, this conclusion does not consider that wasting, or thinness, is generally not a problem in Latin America, nor is it a problem in their sample. With the average child in their sample slightly above the median of the weight-for-height standards used internationally, it is unclear that an increase in weight represents a marked health improvement. In interpreting their results, one can no longer avoid the implications of rising overweight and obesity among urban youth, particularly in the Americas. In Brazil, for example, the prevalence of overweight and obesity in 6–17 year olds rose from 4% in 1974 to 14% in 1997; in Chile, prevalence roughly doubled, to 27%, in the 13 years leading to the year 2000.<sup>3</sup> To some extent, this trend stems from the diet and lifestyle changes that accompany urbanization, which Popkin (1993) calls the “nutrition transition.” Urbanites tend to be more sedentary than their rural counterparts, and they consume foods thought to be less healthy. For children, the consequent rise in excess weight is associated with elevated risk for type-2 diabetes and cardiovascular problems, as well as adult obesity and premature mortality (Dietz, 1998; Reilly et al., 2003).

Consistent with Galiani and Schargrodsky's results, I find title-related increases in weight but not height. Unfortunately, the data reveal some baseline differences between treatment and control groups, implying that the program may not have been a natural experiment. However, the results are robust to the inclusion of a wide array of exogenous controls, which suggests that the estimates are not primarily driven by *ex ante* heterogeneity. In addition, the analysis shows that the differential effects are largest when comparing groups with the strongest and weakest *ex ante*

<sup>2</sup> In the 1980s, de Soto (1989) placed the value of this land at roughly US\$ 8.5 billion (p. 56), although his estimates have drawn criticism for inaccuracy and political motivation (Rossini and Thomas, 1990; Woodruff, 2001).

<sup>3</sup> The prevalence estimates for Brazil (Wang et al., 2002) and Chile (Kain et al., 2002) are both presented in the review by Lobstein et al. (2004). Both estimates are based on the childhood obesity standards recommended by the International Obesity Task Force (Cole et al., 2000).

property rights, as one would expect if the effects of titling are strictly monotonic. The findings are compelling in their consistency with those of Galiani and Schargrodsky, but given the high prevalence of stunting (short stature) and the low prevalence of wasting in Peru, they set forth ambiguous implications for children's health and well-being.<sup>4</sup> Indeed, the results also indicate that titling is associated with increased risk of being overweight or obese.

The paper makes several contributions. First, by separating property rights into more than two groups – more, in other words, than a simple squatter/non-squatter framework – I take better advantage of the heterogeneity in *ex ante* property rights documentation, thus adding to the identification strategy in Field (2002). This improved identification yields evidence that the effects of titling are strongest among households with the weakest initial land rights, as one would expect in the absence of thresholds. Furthermore, the height-for-age and weight-for-height results here appear similar to those of Galiani and Schargrodsky (2004), making a case for the external validity of both their study and my own. However, the analysis of excess weight also raises the concern that these findings – valid or not – do not necessarily imply an improvement in human capital.

## 2. Urban land titling in Peru

During 1940–2002, Peru's population shifted from 35 to 72% urban. Coupled with government efforts to evict the poor from city centers, this generated clusters of informal communities on the urban periphery. In their efforts to mitigate the rise of informality, several past municipal and national governments undertook smaller-scale titling programs, in many cases seeking political clout, but these efforts have been deemed irregular, time-intensive, and costly (World Bank, 1998). In contrast, the current formalization campaign is touted for its swiftness, geographical breadth, and low-cost to beneficiaries.<sup>5</sup>

Peru's national government passed into law the Urban Property Rights Project in 1996. Proceeding in a series of 2-month campaigns, the newly created Commission for the Formalization of Informal Property (COFOPRI) distributed approximately 1.4 million titles to the residents of 8 major metropolitan areas over the following 8 years, with the most activity between 1998 and 2000. Each campaign formalized between 50 and 70 communities, encompassing 30,000–35,000 plots of land (Yi Yang, 1999). Upon entering a given community, COFOPRI and RPU first created a property registry, and then mapped the community and investigated any conflicts over property. After these steps, COFOPRI issued titles to households that met an array of eligibility criteria. First, to quell selective migration, the agency required at least one household member to have resided at the site since before 1995. Another leading eligibility condition, intended to appease private landowners, was that the parcel be on state-owned land.

Past research has argued that program intervention occurred quasi-randomly across beneficiary areas, producing a reliable natural experiment to evaluate the effects of property rights on various social and economic processes. For the most part, these analyses have used data from the COFOPRI baseline survey, a cross-section of past and future beneficiaries collected midway through title distribution. Field (2002) presents convincing evidence that the ordering of

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<sup>4</sup> According to data from the 2000 Demographic and Health Survey, 25% of Peruvian children under 5 were stunted, whereas 1% was wasted. For an easily accessible presentation of these data, see ORC Macro (2006).

<sup>5</sup> For an alternative overview of Peru's urban land titling program, see Field (2002).

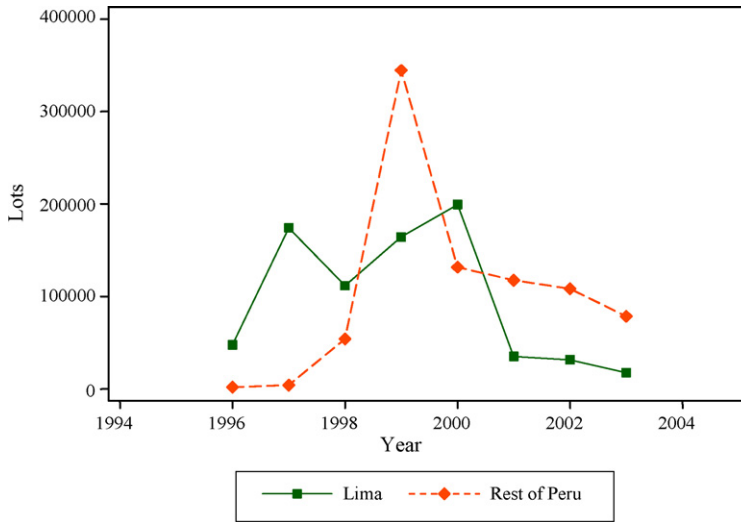


Fig. 1. Lots formalized in Lima and the rest of Peru. *Source:* COFOPRI (2004).

program intervention was not correlated with community development. To this end, she examines the relationship between the ordering of program intervention and district-level indices of poverty and infrastructural consolidation, finding no significant differences between ‘treated’ and ‘untreated’ areas across water, sanitation, roofing, electricity, schooling, literacy, residential crowding, malnutrition, and overall poverty. Additionally, she maps the phases of COFOPRI intervention against district-level poverty indicators in Lima, discerning no spatial association between the two.<sup>6</sup> The only available account of the community selection process states that the ordering of intervention was based on local geography, the demands of community members, and existing tenure-related documentation (Yi Yang, 1999 [quoted in Field, 2002, p. 16]). Although this description raises the possibility that the timing of program intervention was correlated with community development, the identification strategy described below attempts to mitigate any biases arising from endogeneity of this sort.

### 3. Data

#### 3.1. Data collection

The data for this study were collected in a multistage cluster survey over 13 days in July and August 2004. Given the reliance of the identification strategy on variation in program exposure, the sample frame consisted of two groups of Lima human settlements: (1) those formalized during COFOPRI’s first year of operation (from 15 July 1996 to 14 July 1997) and (2) those formalized from its fifth year onwards (from 15 July 2000 to 20 October 2003). This design was based on the assumption that one could better isolate the cumulative effects of titling by focusing on the two extremes of program exposure.

<sup>6</sup> Field (2002) also provides anecdotal evidence that households in untreated areas were unlikely to have changed their behavior in anticipation of the program.

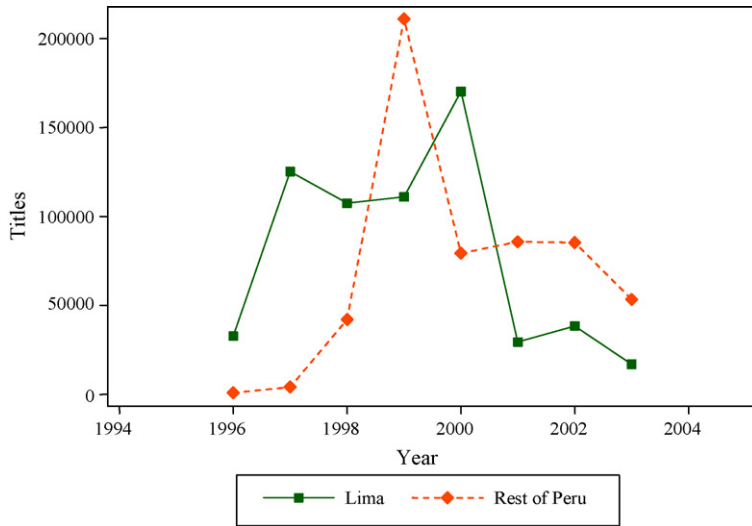


Fig. 2. Titles distributed in Lima and the rest of Peru. *Source:* COFOPRI (2004).

The survey focused on Lima because of its representativeness and its exposure to the titling program at all time points. Lima is home to approximately one third of the nation's population, and its residents make up a large proportion of COFOPRI beneficiaries, receiving 46% of the 1.4 million titles distributed through August 2004 (COFOPRI, 2004). As Figs. 1 and 2 show, Lima has dominated the rest of the country in lot formalization and title distribution, particularly during the early phases of program intervention.<sup>7</sup> Although the program targeted eight cities, intervention in most of these cities began only in 1998 or 1999. Only Lima and its smaller southern neighbor, Ica, contain communities that were formalized during both the early and late periods, as defined here. If the sample contained households from the other six program cities, the analysis would need to control for possible non-random city assignment, which Field (2002) does by including city fixed effects. Given the two periods of interest here, the six cities other than Lima and Ica would only have communities formalized in the later period, which would render their fixed effects parameters collinear with the early/late parameter. Ica's role in the first year (and overall) was minor, so the sample focused exclusively on Lima communities.

The first stage of sample selection drew from COFOPRI's register of Lima settlements that were formalized during the early period (1996–1997) and the late period (2000 onwards).<sup>8</sup> Communities were first stratified at the district level and then randomly selected within these strata, with added weight given to districts where more lots were formalized during the specified period. The final sample was drawn from 14 of Lima's 309 early communities and 13 of 70 late communities.

<sup>7</sup> COFOPRI formalized all lots in a given community at the same, whereas the timing of actual title distribution (within communities) varied considerably within communities.

<sup>8</sup> The register contained communities with 250 or more formalized lots. Over the 8-year period, 467 such communities were formalized in Lima.

The second stage would have ideally drawn on a detailed geographic sample frame (target population) to select individual households, but Peru last conducted a census in 1993, so up-to-date geographical data were unavailable. Furthermore, the finest spatial divisions in the 1993 census were at only the district (not community) level, thus lacking the depth necessary to fully exploit the program's timing variation. In the absence of an ideal sample frame, the survey relied on a randomized block design. Upon arrival at each community, interviewers mapped its city blocks and then randomly selected five for surveying. Beginning at the corner nearest the center of the community, they then circumnavigated each block clockwise until completing four interviews. A few irregularities in the field led to the under- or over-sampling of a few communities, but the survey maintained an average of 20.3 households per community (19.6 early, 21 late).<sup>9</sup> Five hundred and forty-nine households comprised the sample, with 294 in early communities and 255 in late.

Participating households needed to satisfy the following eligibility criteria. First, at least one household member needed to be a mother of a living child under the age of 6.<sup>10</sup> Second, to ensure that the household was eligible for COFOPRI intervention, the interviewee was required to have lived continuously at the site since before 1995. Finally, the property owner (*de jure* or *de facto*) needed to be a household member. If a block did not contain four households that met the filter requirements, surveyors rounded a neighboring block until completing the cumulative four households. They revisited empty households later in the day to minimize absentee bias.

A team of 10 experienced interviewers from Instituto Cuánto, a non-governmental survey group, carried out the survey. Interviews were directed at the mothers of children under the age of 6, with one such interviewee randomly selected per household. The questionnaire was largely based on the instruments of the COFOPRI baseline survey and the 2000 DHS, addressing issues related to land ownership, housing conditions, background characteristics and child health. To assess children's nutritional status, surveyors worked in teams of two to perform anthropometric measurements on the interviewee's children under the age of 6. Using a hanging balance to weigh the children, they recorded weight to the nearest 10th of a kilogram. Length or height was recorded to the nearest millimeter. In children under 2, surveyors measured recumbent length using a wooden platform and sliding footboard, while they measured older children standing. The Review Panel for Human Subjects at Princeton University approved the study protocol prior to fieldwork; mothers gave written consent for themselves and the children.

### 3.2. Variable definitions

The analysis uses several anthropometric indicators to gauge nutritional status. Sex-specific height-for-age and weight-for-height *z*-scores are used as continuous variables.<sup>11</sup> Binary variables for stunting and wasting, defined as being more than two standard deviations below the reference median in height-for-age and weight-for-height, respectively, are used to identify under-nutrition. For excess weight, the analysis uses the International Obesity Task Force's (IOTF) new set of international weight standards for children to create a combined

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<sup>9</sup> The foremost logistical problem was that, in some smaller communities, surveyors could not find 20 eligible households.

<sup>10</sup> Household members were defined by living and regularly sharing meals with the household head.

<sup>11</sup> The *z*-scores here are based on the 1978 WHO/NHCS reference population (WHO, 1986), as recommended by specialists at the Peruvian National Institute of Health.



Table 1  
Descriptive statistics, nutritional status

	Mean	S.D.	N
Height-for-age z-score	−0.85	1.13	618
Stunted	0.13	0.34	618
Weight-for-height z-score	0.77	0.93	618
Weight-for-age z-score	0.04	1.03	618
Wasted	0.003	0.06	618
Overweight	0.29	0.46	436
Obese	0.08	0.28	436

Notes: Samples for overweight or obese are restricted to children ages 21 months and up. The excess weight measure is constructed using the definitions of child overweight and obesity endorsed by the International Obesity Task Force (Cole et al., 2000). The other measures are defined using the 1978 WHO/NHCS reference population (WHO, 1986), as recommended by specialists at the Peruvian National Institute of Health.

indicator of overweight and obesity.<sup>12</sup> Developed by Cole et al. (2000) using data from six countries, the IOTF standards provide age- and sex-specific BMI cutoff points for children aged 2–18.<sup>13</sup>

Aside from the central independent variables, which are described alongside the empirical framework in Section 5, the regressions include a number of exogenous covariates to improve precision and to check the robustness of the results. The first controls relate to ownership status and tenure, including the mode of acquisition (invasion, payment, or inheritance) and the length of residential tenure. Ideally, the tenure variable would reflect the longest time any household member lived at the property. Due to time constraints, however, the survey did not collect residential histories for all household members, so the variable measures only the mother's residential tenure. Young Peruvian couples commonly move in with the woman's parents, so marriage-related migration should not bias the results.<sup>14</sup>

I also add controls for lot size, household size, and housing conditions. Pre-program household size is included because it may be correlated with *ex ante* socio-economic status, and also to account for Field's proposition that greater numbers of working-age household members attenuate the burden of informality—and, here, of childcare.<sup>15</sup> Because titling may alter incentives to invest in immobile household infrastructure, the analysis also controls for *ex ante*

<sup>12</sup> I also ran regressions using older definitions for overweight and obesity now considered obsolete (WHO, 1986), yielding slightly more robust, but generally similar, results.

<sup>13</sup> For male children, the BMI cutoff for overweight ranges from 18.4 at age 2 to 17.4 at age 5, and the cutoff for obesity ranges from 20.1 at age 2 to 19.3 at age 5. For female children, the same cutoffs range from 18.0 to 17.2 and from 19.8 to 19.2, respectively. The cutoffs are specified in half-year increments from age 2 to age 18, at which point they reach the generally accepted adult BMI cutoffs of 25 for overweight and 30 for obesity. In the present analysis, age is rounded to the nearest half year, so the overweight and obesity indicators are for children ages 21 months ( $\approx 2.0$  years) and up.

<sup>14</sup> Some women answered that they had lived at the site "all my life" or "always," which censors the household's residential tenure at the mother's age. For this reason, I use two indicators of residential tenure: a continuous variable for the number of years the mother has lived at the site, and a dummy variable indicating that she has lived there her whole life.

<sup>15</sup> In her tally of 'working-age' members, Field includes all household members between the ages of 5 and 70. In the present data, children born since 1996 introduce the possibility of endogenous fertility, so I define 'working-age' as between ages 9 and 70. Both definitions yield equivalent results, although neither rule out selective migration.



access to piped water and public sanitation. Other covariates include the household head's sex; the mother's education and age; and dummies for the child's sex and birth year.<sup>16</sup>

Table 1 shows descriptive statistics for nutritional status.<sup>17</sup> Mean height-for-age is  $-0.85$  z-scores, with a standard deviation of just over 1 z-score, and 13% of the sample is stunted. The average weight-for-age score lies very close to the median of the WHO/NHCS population, but this appears to be driven mostly by the sample's short stature; weight-for-height averages at 0.77 z-scores, with over a third of the sample overweight or obese. The sample shows no evidence of wasting.

#### 4. Method

Property rights varied considerably before Peru's land titling program, so that the program would be expected to affect households with weak *ex ante* rights more than their counterparts with stronger *ex ante* rights. Combined with plausibly exogenous variation in program exposure, this provides an opportunity for difference-in-difference estimation. To examine the impact of land titling on child nutritional status, I calculate differences in anthropometric outcomes across households with varying *ex ante* property rights, and then compare these differences in early and late communities. If the cumulative effects of the program grow over time, differences will be greater in communities with longer program exposure (*i.e.*, early communities). This could arise as a result of delays in title distribution or lengthy adjustment to new circumstances.<sup>18</sup>

As is common in analyzing the effects of social programs, a number of empirical problems arise. First, not all households in program areas received titles, and those that did often faced non-random delays. Of the households in the sample that did not have formal titles before the program, 50% remain without titles in late areas, compared to 10% in early areas.<sup>19</sup> All parcels of land in a given community were *formalized* at the same time, but not all received titles immediately. COFOPRI cites the following as reasons for delay (or outright exclusion from the program): insufficient evidence for pre-1995 residence; disputed ownership claims; cooperative association membership; or location on an archeological site, flood plane, mining site or private property. Anecdotal evidence also suggests that some COFOPRI assessors refused to confer titles if the dwelling in question exhibited severe structural flaws. Therefore, within communities, neither the receipt of a title nor its timing was exogenously determined. For these reasons, rather than measure the effects of actual titles, the analysis uses the timing of formalization at the community level.

Section 2 also cited evidence of quasi-random timing across districts, but other factors – political or otherwise – still may have influenced the sequence of program intervention, particularly at the community level. I circumvent this issue with the difference-in-difference estimator described above. Due to previous formalization attempts, a number of households in program areas already possessed titles before 1996. By using these households as an additional comparison group, the analysis can distinguish program effects even without the assumption of

<sup>16</sup> In preliminary results, parity effects were small and insignificant. Here, I omit the parity dummies in an effort to limit the number of parameters in the model. The omission does not substantively change my results.

<sup>17</sup> Observations with z-scores of absolute value greater than 5 were dropped as outliers. Three such observations were encountered in the main dataset; their deletion did not substantially alter the results.

<sup>18</sup> In her work on household time allocation (2002), Field finds evidence of lagged effects.

<sup>19</sup> Thirty three households reported receiving COFOPRI titles before the program entered their communities. Assuming that this reflected recall error, I set title year equal to the year of community formalization for these households.

totally random assignment. Although there are likely to be systematic differences in the nutritional status of children from households with varying *ex ante* rights, as long as these differences do not vary across early and late communities, the model will correctly identify average (linear) treatment effects.

Using such a framework, Field (2002) analyzes differences between two groups—‘squatters,’ who had no pre-program property titles whatsoever, and ‘non-squatters,’ who had some form of title before the program. Her main analyses therefore consider a binary representation of *ex ante* property rights. In a sense, however, title possession is the observable component of a latent variable that measures the strength of a household’s property rights. The underlying variable need not be binary. Research in sociology and anthropology (Peattie and Aldrete-Haas, 1981) has long argued that property rights lie along a spectrum in settings with imperfect tenure systems. Indeed, in Peru, some households not reached by previous government titling programs wrote their own, extra-legal documents—examples include ‘sales titles’ and ‘proofs of adjunction.’ Data from the COFOPRI baseline survey indicate that households possessing COFOPRI titles are the most likely to report feeling secure about their tenure arrangements, with those possessing municipal titles (the formal-sector predecessors of COFOPRI) slightly less so, and those possessing other documents still less likely. Households completely lacking documents (Field’s ‘squatters’) are the least likely to report a sense of tenure security (Apoyo, 2000).<sup>20</sup> One would expect the effects of titling to be strongest in the group with the weakest *ex ante* rights. Field does examine two sources of heterogeneity – the length of residential tenure and household size – but her analyses do not take advantage of this more basic variation in baseline property rights.

To fix ideas, consider the following reduced-form demand function for a child’s nutritional status,  $N$ :

$$N = N(\theta, P; C, H, \eta) \quad (1)$$

The demand for a child nutritional status depends on the strength of the household’s property rights,  $\theta$ ; a vector of prices for nutritional inputs and other household consumption goods,  $P$ ; a set of child-specific characteristics,  $C$ ; a set of other household-level characteristics (including wealth),  $H$ ; and an error term,  $\eta$ . Suppose, as in the Peruvian case, that the government distributes formal land titles to communities in a randomly chosen order, with the binary variable  $T$  indicating whether a community is exposed to treatment in an early phase (1) of program intervention or a late phase (0). The new titles confer property rights at least as strong as those that previously existed, such that  $\theta^T \geq \theta_j^I$  for all households  $j$ , with  $\theta^T$  denoting the property rights associated with treatment (assuming homogeneity for simplicity) and  $\theta_j^I$  denoting the household’s initial property rights. For child  $i$  in household  $j$  of community  $k$ , we can write the following difference estimator:

$$E[\Delta N_{ijk}] = \alpha(T_k \times [\theta^T - \theta_j^I]) \quad (2)$$

$\alpha$  is our parameter of interest, corresponding to the cumulative (linear) effect of property rights on child nutrition over the average time interval that separates early and late communities.

<sup>20</sup> The question was, “How secure do you feel about your property?” 94% of COFOPRI title-holders reported feeling “very secure” or “secure,” compared to 87% of municipal title-holders, 78% of sales title-holders, 71% of holders of other unofficial documents, and 47% of those lacking property documents.

Unfortunately, we have no cardinal measure of  $\theta$ , only some knowledge of its ordinal properties. Following Field, we can get a sense of  $\alpha$  by separating households into two observable groups, one with strong rights ( $S$ ) and one with weak ( $W$ ). Setting  $\bar{\theta}'_S$  and  $\bar{\theta}'_W$  to the average property right strength in each of these two groups, Eq. (2) can then be rewritten in group-level differences-in-differences:

$$E[\Delta N_{ijk}] = \alpha(T_k \times [\bar{\theta}'_S - \bar{\theta}'_j]) = \alpha[\bar{\theta}'_S - \bar{\theta}'_W] \left( \frac{T_k \times [\bar{\theta}'_S - \bar{\theta}'_j]}{[\bar{\theta}'_S - \bar{\theta}'_W]} \right) \quad (3)$$

In the right-most expression of Eq. (3),  $\alpha$  is scaled-up by the average difference in property right strength between the strong and weak groups. After scaling up  $\alpha$  in this way, the remaining term in the parentheses is the product of two dummy variables, one indicating treatment and the other indicating membership to the group with weaker *ex ante* rights.

With the current data, the scaled-up coefficient can be estimated using the following specification:

$$N_{ijk} = \beta_0 + \beta_1(\text{early}_k) + \beta_2(\text{weak}_j) + \beta_3(\text{early}_k * \text{weak}_j) + \mathbf{C}'_j \boldsymbol{\gamma} + \mathbf{H}'_j \boldsymbol{\psi} + \varepsilon_{ijk} \quad (4)$$

This is a standard difference-in-difference framework, with *early* capturing whether the child lives in an early (1) or late (0) community and *weak* indicating whether the child's household belongs to the group with weak (1) or strong (0) *ex ante* property rights.  $\mathbf{C}_i$  is a vector of child-specific characteristics, and  $\mathbf{H}_j$  contains exogenous household and maternal characteristics.  $\beta_3$  represents the reduced-form program effect, or  $\alpha(\theta'_S - \theta'_W)$ . This is very similar to Field's specification, but by framing it this way, we highlight an important, if unsurprising, prediction:  $\beta_3$  will be larger when groups  $S$  and  $W$  are further apart on the property rights 'spectrum.' To test this prediction, I separate households into three categories: those with formal titles before the program, those with informal documents before the program, and those with no documents at all. I designate these as households with 'full,' 'partial,' and 'absent' *ex ante* property rights. In order to allow the maximum flexibility for the parameters in (4), I analyze differences among these groups in a series of two-way comparisons: one between households with full property rights and those with partial or absent rights (taking advantage of the full sample); one between full rights and absent rights; and one between partial rights and absent rights. In each of these formulations, *weak* is an indicator for the group with weaker *ex ante* rights, while the definition of *early* remains constant throughout. Assuming there are no threshold effects, regressions comparing 'full' and 'absent' should yield the largest estimates of  $\beta_3$  (in absolute value).

For Eq. (4) to be properly identified, differences among the three groups in characteristics exogenous to the program should not vary across early and late communities. Table 2 tests this assumption for observables, presenting descriptive statistics for the variables in  $\mathbf{H}$ , which should be exogenous to the program. The first two columns include means and standard deviations for the full sample, and the remainder of the table summarizes the sample means for the three classifications of pre-program property rights. The last three columns show  $p$ -values for differences-in-differences in each of the comparison frameworks detailed in the previous paragraph. For the identifying assumption to hold, one would expect few significant differences-in-differences for the variables that appear in Table 2.

The first two comparisons, but not the third, violate the identifying assumption in several areas. These results contrast those of Field (2002), who finds no differences-in-differences in pre-program characteristics using data from the COFOPRI baseline survey. This difference appears

Table 2  
Sample means and differences, pre-program characteristics

	<i>Overall</i>		<i>By pre-program property rights</i>						<i>p-Values for differences- in-differences</i>		
	Mean	(S.D.)	<i>Full rights</i>		<i>Partial rights</i>		<i>Absent rights</i>		F vs. P/A	F vs. A	P vs. A
			Early	Late	Early	Late	Early	Late			
Mother's age	30.69	(6.85)	29.29	32.88	31.53	31.39	29.08	32.05	0.17	0.75	0.41
Mother's education	9.50	(3.15)	10.04	10.88	8.95	8.75	9.93	7.63	<b>0.00</b>	<b>0.00</b>	<b>0.01</b>
Female HH head	0.21	(0.41)	0.18	0.26	0.25	0.18	0.22	0.15	<b>0.07</b>	0.17	0.99
# Working-age mems	4.18	(1.86)	5.01	2.73	4.62	3.90	4.41	4.08	<b>0.00</b>	<b>0.00</b>	0.26
Mode of acquisition											
Invaded	0.48	(0.50)	0.46	0.40	0.60	0.59	0.33	0.37	0.61	0.67	0.71
Paid	0.46	(0.50)	0.45	0.58	0.34	0.41	0.52	0.56	0.46	0.76	0.91
Inherited	0.06	(0.23)	0.09	0.02	0.06	0.01	0.14	0.05	0.36	0.66	0.55
Tenure											
Years	17.72	(7.75)	23.43	11.40	20.64	14.35	19.42	13.78	<b>0.00</b>	<b>0.01</b>	0.74
Whole life	0.22	(0.41)	0.45	0.03	0.37	0.06	0.33	0.08	<b>0.02</b>	0.17	0.70
Lot size (m <sup>2</sup> )	130.07	(54.50)	145.09	131.86	134.69	111.07	128.26	132.90	0.82	0.36	0.14
Pub sanitation <1996	0.34	(0.47)	0.80	0.00	0.48	0.02	0.48	0.03	<b>0.00</b>	<b>0.03</b>	0.96
Piped water <1996	0.38	(0.49)	0.82	0.03	0.50	0.07	0.60	0.10	<b>0.01</b>	0.12	0.62
<i>N</i>	<i>618</i>		<i>141</i>	<i>98</i>	<i>106</i>	<i>145</i>	<i>90</i>	<i>40</i>			

Notes: The *p*-values correspond to tests of differences-in-differences within the each of the three comparison frameworks. F, P, and A stand for 'full,' 'partial,' and 'absent,' respectively. All tests adjust for community-level clustering. Differences-in-differences significant at the 10 % level are presented in boldface.

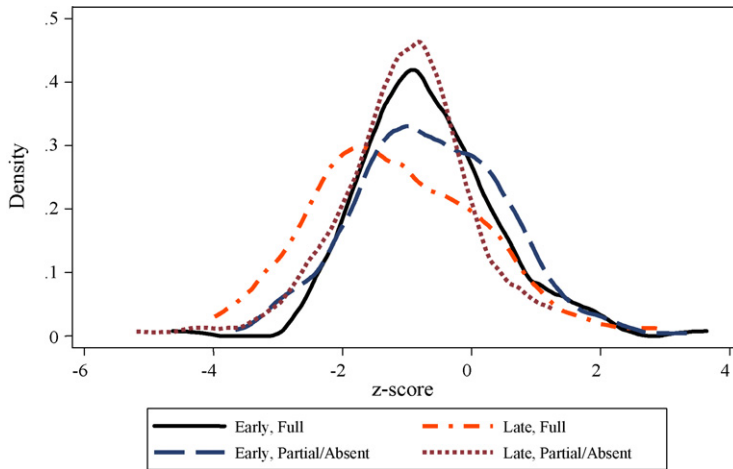


Fig. 3. Kernel density estimate: height-for-age z-score. *Notes:*  $N = 618$ . Densities were estimated using the Epanechnikov kernel, evaluated at 50 points in each of the four distributions.

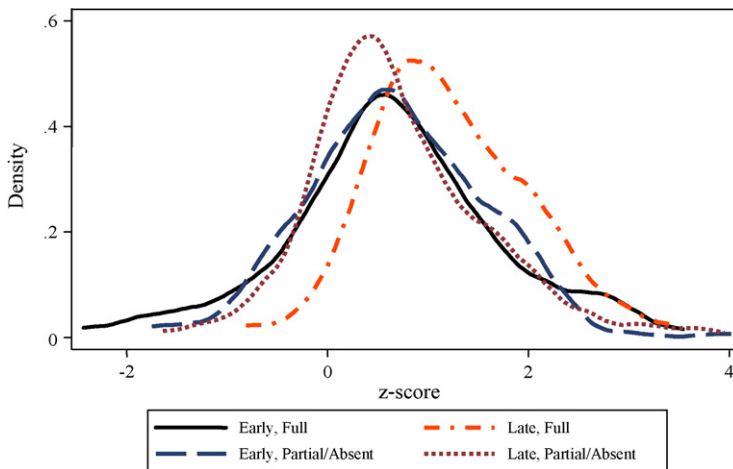


Fig. 4. Kernel density estimate: weight-for-height z-score. *Notes:*  $N = 618$ . Densities were estimated using the Epanechnikov kernel, evaluated at 50 points in each of the four distributions.

to be driven primarily by community age: late communities appear much younger than early communities, and they consequently have smaller differences in residential tenure.<sup>21</sup> For closely related reasons, differences in pre-program access to public sanitation and piped water are also

<sup>21</sup> With Peru's long history of rural–urban migration, it is possible that the current dwellers did not participate in the initial squatter invasion, so that the observed differences in residential tenure might not accurately reflect differences in community age. However, the differences in tenure are large enough to suggest that there are systematic differences in community age.

Table 3  
Effect of property rights on children's height

	<i>Full rights vs. partial/absent rights</i>		<i>Full rights vs. absent rights</i>		<i>Partial rights vs. absent rights</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Outcome: Height-for-age z-score</i>						
<i>Early*weak</i>	−0.100 (0.200)	0.092 (0.185)	−0.163 (0.294)	0.167 (0.287)	−0.103 (0.284)	−0.093 (0.267)
<i>Weak</i>	0.136 (0.147)	0.037 (0.174)	0.159 (0.244)	−0.108 (0.263)	0.029 (0.240)	0.016 (0.207)
<i>Early</i>	0.501 (0.149)***	0.096 (0.192)	0.501 (0.150)***	0.038 (0.204)	0.441 (0.145)***	0.221 (0.164)
Observations	618	618	369	369	379	379
<i>Outcome: Stunting</i>						
<i>Early*weak</i>	0.103 (0.077)	0.048 (0.066)	0.129 (0.106)	0.048 (0.104)	0.047 (0.088)	0.029 (0.088)
<i>Weak</i>	−0.083 (0.069)	−0.046 (0.066)	−0.085 (0.098)	−0.027 (0.105)	−0.002 (0.079)	0.002 (0.066)
<i>Early</i>	−0.157 (0.062)**	−0.062 (0.067)	−0.157 (0.062)**	−0.065 (0.073)	−0.075 (0.040)*	−0.026 (0.045)
Observations	618	618	369	369	379	379
Exogenous controls?	No	Yes	No	Yes	No	Yes

Notes: Ordinary least squares estimates, with standard errors in parentheses. Standard errors are adjusted for community-level clustering. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . In columns (1) and (2), *weak* = 1 for households with partial or absent pre-program property rights, 0 otherwise; in columns (3)–(6), *weak* = 1 only for absent pre-program rights. In all columns, *early* = 1 for households located in a community that was treated in the first phase of program implementation. Exogenous controls include the following household-level characteristics: # of working-age HH members and members<sup>2</sup>; mother's age and age<sup>2</sup>; mother's education and education<sup>2</sup>; lot size; whether HH-head is female; whether HH had public sanitation before 1996; whether HH had piped water before 1996; whether property owned by invasion, payment, or inheritance; and mother's residential tenure indicators. Child's birth year and sex are also included in the controls.

smaller in late communities; in fact, no late households in the sample had water or sanitation before 1994. Note, however, that the differences-in-differences in Table 2 do not, for the most part, extend to the comparison between households with partial rights and households with absent rights.

While I can mitigate differences in these variables by including them as covariates, I cannot rule out unobservable differences. To strengthen the case for internal validity, I perform two robustness checks. First, I present results for all three comparison frameworks, to see if the expected pattern emerges for  $\beta_3$ , the coefficient on *early\*weak*. If the nutritional effects of property rights are strictly monotonic, they should persist across all comparisons, with the largest difference in the comparison of full rights and absent rights. This might not occur in the presence of thresholds (*i.e.*, if the effects are not strictly monotonic), but the emergence of such a pattern would provide some indication of the results' validity. The exogenous controls serve as another robustness check; if their addition does not alter the program effects coefficient substantially, this would be another sign that the estimates likely correspond to causal effects.

## 5. Results

### 5.1. Non-parametric evidence

For a visual depiction of the comparisons in question, this section presents non-parametric density functions of height-for-age and weight-for-height by sub-group (Figs. 3 and 4). To keep matters uncomplicated, the section focuses on the results from the comparison that makes use of the full sample: full *ex ante* property rights against partial or absent *ex ante* rights. The corroborating numerical results from the full/absent and partial/absent comparisons appear in the next section.

The height-for-age plots in Fig. 3 are suggestive of some differences, particularly if one focuses on the left tail of each distribution. All children that fall to the left of  $-2$  are considered stunted. In early communities, stunting is more prevalent among children from ‘weak’ households than among their counterparts with stronger property rights, whereas in late communities, the opposite is the case. If this represents a causal relationship, it implies that titling increases the risk of stunting in squatter households. However, in light of the small sample size and the lack of controls, the relationship must be tested in a regression framework.

The weight-for-height density estimates in Fig. 4, in contrast, reveal large, systematic differences. In early areas, weight-for-height is distributed similarly in children from households with both strong and weak *ex ante* rights, suggesting either that titling has equalized the two groups or that the two groups were not especially different at the outset. Conversely, the two groups diverge impressively in late communities. The distribution of weight-for-height among children from households with full pre-program rights appears to fall everywhere to the right of the distribution among children with weaker pre-program rights. Naturally, Fig. 4 may also reflect baseline differences, a possibility I consider in the subsequent section. Nonetheless, these non-parametric curves suggest a clear association between property rights and children’s weight.

### 5.2. Regression results

Tables 3 and 4 present ordinary least squares estimates of Eq. (4).<sup>22</sup> All specifications adjust for clustering at the community level. In Table 3, none of the six specifications reveal significant effects of titling either on height-for-age *z*-score or on stunting. The coefficients are also very sensitive to the addition of exogenous covariates, implying that any differences we observe are likely spurious in any case. The coefficients for stunting are all positive, which is consistent with the observed patterns in the non-parametric density estimates. However, in none of the regressions is the coefficient on *early\*weak* significant. The lack of significance here cannot entirely rule out a small effect in this limited sample, but at the least, these findings suggest that land titling does not decrease children’s risk of stunting in the short run, as one might have expected.

The results for weight do, however, suggest significant effects, robust to the addition of controls in all three comparisons. The weight-for-height coefficients in the top panel of Table 4 indicate an overall effect of approximately 0.5 *z*-scores [columns (1) and (2)], which is equivalent to half a standard deviation in this sample. When households with partial *ex ante* rights are omitted, the program effects coefficient rises considerably – as expected – to 0.76 in the model with controls [column (4)]. Finally, columns (5) and (6) suggest a differential effect of about 0.35 *z*-scores between intermediate and full squatters: smaller, as expected, than the result from the

<sup>22</sup> For binary dependent variables (stunting and excess weight), probit estimation produced very similar results. These results are available on request from the author.



Table 4  
Effect of property rights on children's weight

	<i>Full rights vs. partial/absent rights</i>		<i>Full rights vs. absent rights</i>		<i>Partial rights vs. absent rights</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Outcome: Weight-for-height z-score</i>						
<i>Early*weak</i>	0.561 (0.145)***	0.478 (0.161)***	0.737 (0.159)***	0.759 (0.211)***	0.351 (0.154)**	0.366 (0.156)**
<i>Weak</i>	-0.520 (0.125)***	-0.415 (0.124)***	-0.483 (0.133)***	-0.476 (0.189)**	0.047 (0.126)	-0.043 (0.131)
<i>Early</i>	-0.564 (0.120)***	-0.544 (0.140)***	-0.564 (0.121)***	-0.622 (0.173)***	-0.178 (0.133)	-0.203 (0.127)
Observations	618	618	369	369	379	379
<i>Outcome: Overweight/obese</i>						
<i>Early*weak</i>	0.271 (0.097)***	0.237 (0.105)**	0.337 (0.105)***	0.334 (0.168)*	0.119 (0.076)	0.132 (0.083)
<i>Weak</i>	-0.301 (0.078)***	-0.279 (0.077)***	-0.309 (0.078)***	-0.318 (0.118)**	-0.010 (0.048)	-0.035 (0.054)
<i>Early</i>	-0.355 (0.075)***	-0.294 (0.079)***	-0.355 (0.075)***	-0.318 (0.110)***	-0.138 (0.063)**	-0.092 (0.074)
Observations	436	436	271	271	249	249
Exogenous controls?	No	Yes	No	Yes	No	Yes

Notes: Ordinary least squares estimates, with standard errors in parentheses. Standard errors are adjusted for community-level clustering. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . In columns (1) and (2), *weak* = 1 for households with partial or absent pre-program property rights, 0 otherwise; in columns (3)–(6), *weak* = 1 only for absent pre-program rights. In all columns, *early* = 1 for households located in a community that was treated in the first phase of program implementation. Exogenous controls are identical to those listed in the notes to Table 3.

full/absent comparison. Note that the coefficients do not change substantially with the addition of controls, implying that they do not simply reflect baseline differences-in-differences.

The lower panel of Table 4 contains estimates for excess weight, and the resulting coefficients are at least marginally significant in all regressions that include controls. The full-sample estimates indicate that titling increases the probability that a child is overweight or obese by about 25% points. With the overall prevalence of excess weight in the sample at 38%, this impact is striking. As one would expect, the effect escalates considerably in the framework contrasting households with full *ex ante* rights and households with completely absent *ex ante* rights. The estimates return to a smaller magnitude, as expected, in the partial/absent framework of columns (5) and (6). The coefficient in column (6) is almost marginally significant ( $p < 0.13$ ), but recall that the IOTF definitions of overweight and obesity are limited to children over 20 months of age, which decreases the sample size and, as a result, statistical power.

## 6. Conclusions

This study offers evidence on the nutritional consequences of land titling that gives reason for both optimism and reservation. Findings from a household survey in Lima suggest that the Peruvian land titling program produced increases in children's weight, but not their height. These

results concur with past work on the links between titling and child nutritional status but add evidence that the increases in child weight may be unhealthy—the full sample estimates imply a title-related increase in children’s risk of overweight or obesity by 23.7 [CI: 3.1–44.2] percentage points. The estimates also follow a pattern consistent with the framework suggested in Section 4, in which the effects of property rights are strictly monotonic.

The study suffered from a few irresolvable design flaws. Pre-program differences across tenure groups varied between early and late communities, suggesting that the ordering of program intervention may not have been random. As a result, the empirical strategy – which explored differences in outcomes between *ex ante* squatters and non-squatters across varying time exposures to the program – may not have been correctly specified. However, the results were resolutely robust to various definitions of tenure status, as well as the addition of exogenous controls. Because individuals in late communities may have already received benefits from titles, the differential effects might be considered lower bounds. This said, the estimated effects are large, perhaps implausibly so, but the small sample leads to wide confidence intervals, of which the lower bounds may be more realistic. For example, in the full sample results, the lower bound of the 95% confidence interval for the program impact on weight-for-height was 0.16 z-scores. The corresponding lower bound for excess weight was 3.1 percentage points.

In comparing the results with those of Galiani and Schargrodsky (2004), several methodological differences are worthy of note. Galiani and Schargrodsky analyze a clean, small-scale natural experiment, whereas this paper considers a program of nationwide scale (albeit with data from a single city), which both allows more room for politics to affect program ordering and helps in answering broader policy questions. In addition, Galiani and Schargrodsky use an instrumental variables strategy in which they instrument program timing for actual title possession, which is impossible with the Lima dataset because many households in late communities received titles before data collection. Because some effects of titling are likely to be lagged, instrumenting *early\*weak* for actual title possession would lead to biased estimates. Finally, whereas Galiani and Schargrodsky consider a binary representation of property rights, I take advantage of added variation in *ex ante* informal property rights to produce estimates over the property rights ‘spectrum.’ Despite these differences, the estimates presented here differ only marginally from those of Galiani and Schargrodsky, who find a weight-for-height impact of 0.30 z-scores in their full sample, compared with my main estimate of 0.48 z-scores. For a boy 75 cm tall, these effects correspond to increases of 0.25 and 0.40 kg for the Argentine and Lima samples, respectively. Given the wide confidence intervals and the relatively large variance of weight-for-height (0.93 in my sample), these differences are not striking. Furthermore, neither analysis finds any effect on children’s height.

In terms of evaluating the overall nutritional impact of titling, one would be hard pressed to find a clear indication in the evidence presented here. Less than 1% of the sample is wasted, while nearly 40% classifies as overweight or obese, so the increases in weight without corresponding increases in height do seem worthy of attention. However, among some children, the weight gain may be beneficial; Fig. 4 provides evidence of weight gain at all points in the distribution, not just the top. Additionally, the International Obesity Task Force standards for overweight and obesity (Cole et al., 2000) do not consider children below the age of 2, so the analysis of excess weight omits an important portion of the sample. Finally, the analysis may not have allowed sufficient lag time to detect improvements in linear growth. Perhaps the observed increases in weight will eventually be reflected in increased height, a surer indication of better health in a setting where stunting is prevalent. Even so, the evidence presented here should elicit concern over possible deleterious effects as well.

Given the immense impact of the Peruvian program on the labor market, time allocation and labor income are likely to underlie any effect of titling on child nutritional status.<sup>23</sup> The traditional development literature would suggest that the increases in household income would lead to improvements in child health (Strauss and Thomas, 1998), notwithstanding declines in childcare time. Although the evidence on maternal employment in developing economies has been inconclusive (Glick, 2002), with obesity and sedentarism on the rise, findings from industrialized settings may also be instructive. Using data from the United States, Anderson et al. (2003b) and Ruhm (2004) both find that mothers who work more hours are more likely to have obese children. In the Peruvian context, if titling leads working mothers to work more hours outside the home, an unintended consequence may be unhealthy increases in weight for their children.<sup>24</sup> Another possibility is that, as household income increases, the quality of food changes, especially in households with working mothers, which may be less attentive to children's health needs.

If titling does lead to unhealthy excesses in weight, one could contend that these have ambiguous ethical implications. In the traditional normative framework of economics, land titling has broadened the consumption set available to households, such that the increase in excess weight is merely an example of free agency. Arguably, this should not concern policy-makers or economists. However, as Anderson et al. (2003a) point out, this argument discounts several key factors. First, if households do not totally internalize the costs of excess weight, externalities arise. Furthermore, there may be 'internalities' – akin to addictions – that keep overweight or obese children from choosing rationally later in life; along these lines, evidence suggests that excess weight may, under normal circumstances, persist from childhood into adulthood (Lobstein et al., 2004). Finally, children are not free-choosing, well-informed agents. When parents – however informed or uninformed they are themselves – make decisions that compromise their children's health, the argument for free choice encounters serious problems.

While property interventions may have many beneficial effects, policy makers should heed, monitor, and perhaps mitigate their impact on children, whether positive or negative in the long run. In addition to targeting property institutions, policy efforts should address the various components of malnutrition, including both stunting and excess weight. More broadly, my findings highlight the need for greater attention in both policy-making and economic research to the coexistence of under- and over-nutrition (Lobstein et al., 2004). Globally, the challenge of malnutrition has taken on multiple dimensions, with important correlates across social and economic domains.

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<sup>23</sup> Field (2005b) also finds that short-run fertility has fallen among newly titled households, which may have led to differences in per capita investment in child health.

<sup>24</sup> In results not shown, the effects of land rights on weight-for-height and overweight/obesity were found to be considerably larger among children whose mothers participate in the labor market. Unfortunately, I do not have a measure of labor hours to examine this hypothesis. Results using a dummy for maternal labor force participation did not reveal an effect of titling on a mother's propensity to work. However, this crude measure of labor force participation does not allow for more subtle changes in hours worked, which appears to be the more important factor in industrialized settings (Anderson et al., 2003b).

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