

Property rights and honest behavior

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October 22, 2024

Abstract

Do formalized property rights increase honest behavior? We ran a lab-in-the-field experiment with a standard dice-in-a-cup task in the same villages of rural Benin where, ten years before, a unique large-scale randomized control trial introducing the formalization of property rights was carried out. Results show that holding formal certificates of private property rights increases preferences for truth-telling. This finding supports the argument that laws and moral behavior work in synergy and that preferences are not exogenous but shaped by institutions (Bowles, 2016).

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Since Adam Smith, economists have praised how well-defined property rights foster economic development. Besley and Ghatak (2010) list four transmission channels: property rights allow individuals to internalize the fruits of their investment and efforts (Locke, 1980, Grossman and Hart, 1983); they decrease the unproductive costs of defence and protection and increase coordination (Hobbes, 1660, Hume, 2007, Smith and Price, 1973); they facilitate the gains from trade as assets can be transferred to those who can use them most productively (Coase, 1960); and finally they can be used as collateral to gain access to capital markets (Feder and Feeny, 1991, De Soto, 2000). One further channel may pass through the effect of property rights on social and moral preferences that are the lubricant of well-functioning markets and societies (Bowles, 2016). In fact, institutions may affect preferences (Bowles, 1998) and may thus impact behaviour well beyond the incentive effect, but it is by no means clear the direction of such effects (Frey and Jegen, 2001, Gneezy et al., 2011, Bowles and Polania-Reyes, 2012). The formalization of property rights and the establishment of a more certain rule of law may have ambiguous effects on the moral norms that govern individual’s preferences for truth-telling. On the one hand, it might be that the formalization of property rights induces more law-abiding behaviour, and this crowds-in the moral norm internalizing law abidance and truth-telling. On the other hand, it may well be the case that the formalization of property rights leads to cheaper third-party enforcement and thus it ends up crowding out intrinsic motivation to obey such moral norm.

The interplay between the formalization of property rights and the development of moral norms is the subject of the present paper. We study whether the introduction of formal land rights influenced individuals’ moral norms of truth-telling. In 2020 we ran a lab-in-the-field experiment, based on a variant of the dice-rolling task Jiang (2013), Fischbacher and Föllmi-Heusi (2013), in some of the same villages of rural Benin where, between 2009 and 2011, a previous randomized control trial was carried out under the auspices of the World Bank and the Millenium Change Corporation to study the impact of formalization of lands’ property rights on economic development (Goldstein et al., 2018).

1 Literature Review

The dice-rolling task used in our experiment has quickly become a workhorse design to study preferences for truth-telling, and a recent meta-study ((**alias?**)) has highlighted how the moral norm of truth-telling is very robust across many dimensions, including subjects’ gender and nationality, payoff levels and repeated playing. Several studies have also proven the external validity of the task by showing how high levels of truth-telling in the dice-rolling task positively correlate with positive school conduct of US students (Cohn and Maréchal, 2018), work ethics (no absenteeism) of Indian nurses (Hanna and Wang, 2017), responsible payment by passengers of French public transport services (Dai et al., 2018), good conduct of Swiss inmates from a maximum-security prison (Cohn et al., 2015); and negatively correlates with an index measuring the

prevalence of rule violations (PRV) constructed on country-level data of corruption, tax evasion, and fraudulent politics across 23 countries around the world (Gächter and Schulz, 2016).

2009-2011's property rights' RCT

In the last thirty years several developing agencies have promoted a number of projects aimed at the formalization of previously undocumented, informal or customary property rights, following the influential work of the World Bank (see inter alia Feder and Feeny 1991) and Hernando De Soto (2000). Their hypothesis was as powerful as simple: if land and housing assets informally owned by poor people were to be legalized and recorded, they could be used as collateral to gain access to capital markets and hence promote investment and development. The hypothesized "De Soto effect" (Besley et al., 2012) has met with mixed evidence of success: Jacoby and Minten (2007) found that formalization had no appreciable impact while Field (2005), Galiani and Schargrodsky (2010), Deininger and Feder (2009) found that formalization had a positive impact but it was not channeled through credit as the De Soto effect hypothesized. The only two Randomized Control Trials studies conducted so far found in one case (Goldstein et al., 2018) small average effects on only some measures of investment and in the other case (Huntington and Shenoy, 2018) no significant effect of any measured investment outcome. The first of the two RCTs is the baseline of our lab-in-the-field experiment.

To address this, the Beninese government implemented a land-tenure reform known as the Plan Foncier Rural (PFR) with the support of the Millennium Challenge Corporation. The PFR has involved socio-land surveys at the village level to identify rights holders and their rights and to demarcate parcel boundaries for registration in a public repository (Delville, 2006). While this process does not confer de jure legal ownership, it provides a presumption of ownership recognized by courts, enabling the sale or use of registered plots as collateral. The certificates registering possessory rights can be converted into land titles through a shorter, cheaper, and simplified procedure compared to the regular process for titling uncertified land. The PFR reform in Benin has significantly altered the institution of property rights over land, creating a system similar to formalized ownership (Fabbri and Dari-Mattiacci, 2020). This is the first case of a large-scale land reform implemented as a randomized control trial (RCT) process involving hundreds of rural villages. The objective of the PFR program was to formalize land rights in 300 rural villages across 40 communes. After a preliminary phase, 575 out of 2062 rural villages were identified. A subsample of 300 villages was then selected via public lottery for the implementation of the PFR. The villages not chosen for the PFR did not receive any intervention and continued to have customary land rights. For a detailed description of the reform's characteristics, implementation process, and evaluation of its effects on investments, please refer to Goldstein et al. (2018) and For the aims of this research, we combined an experimental design with the RCT implementation of the PFR. We randomly selected data from 32 villages in the north (Mono

and Couffou) or two provinces in the south (Alibori and Borgou) among those in the complete list of villages in the PFR. The research design compares participants' choices in randomly chosen villages to have the land tenure reform implemented (treated villages) against non-selected villages (control villages) that maintain customary land rights.

1.1 Main Hypothesis

Formalization of property rights and the establishment of a more certain rule of law may have ambiguous effects on the moral norms that govern individual's resistance against cheating. On one hand it might be that the formalization of property rights induces more law-abiding behaviour and this reinforces the moral norm that internalizes law abidance and thus reduces cheating. On the other hand it may well be the case that the formalization of property rights leads to an easier enforcement of such rights and thus crowds out intrinsic motivation to obey moral norms against cheating. Since theoretical reasoning provide no clear prediction we will apply two-sided tests of significance. Notice that we refer to villages where property rights have been previously formalized as *treated* villages and villages where no formalization has been implemented as *control* villages.

Hypothesis 1 *Preference for truth telling is the same in treated and control villages.*

1.1.1 Heterogeneity

We will study heterogeneity in the preference for truth telling in the treated and control villages by using data on the level of market integration. As a proxy for market integration, we will use a village distance from the closest paved road (below and above the median in the sample).

We will additionally test whether background data collected in the survey – including gender and income – generate differences.

2 Experimental Protocol

The research strategy leverages the RCT implementation of the PFR to compare moral preferences for truth-telling between treated and control villages. To measure these preferences, we conducted a lab-in-the-field incentivized experiment in a sample of villages included in the lottery pool. Participants were asked to privately roll a six-sided die ten times, as in (Jiang, 2013), and report their outcomes on a sheet collected by the experimenter. They received a payoff proportional to one randomly chosen report among the ten submitted. Since participants self-reported their dice outcomes, they had the potential to cheat. While the true outcomes were not verified by the experimenter, we could assess whether the group's reports were distributed truthfully.

The primary aim of the study is to determine if the land rights reform influenced the willingness to report dice outcomes truthfully. Based on previous

research, we considered the village community as the relevant reference group for our participants. Our main hypothesis is that formalizing land rights affects the degree of truth-telling, tested by comparing the average reported outcomes between the treated and control groups.

This hypothesis is tested by estimating the following regression equation:

$$t_i = \alpha + \alpha_F F_i + \delta_T T_i + X_i + \epsilon_i \quad (1)$$

where t_i is the reported outcome of the dice roll, T_i is a dummy equal to 1 for subjects in treated villages, and X_i is a vector of the individual characteristics specified in the post-experimental survey.

As outlined in the pre-analysis plan¹, we assess the impact of the reform on the preference for truth-telling across different treatments. Additionally, we explore potential variations in reported outcomes based on distance from paved roads (as a proxy for access to formal justice and market integration), gender, and income.

2.1 Fieldwork Procedures

Data collection occurred from January to March 2020, in 32 villages randomly chosen from the list of those included in the original RCT. In particular, the 32 villages were chosen from a subset of villages located in two areas: the first one in the southern provinces of Mono and Couffou and the other one in the northern provinces of Alibori and Borgou. Prior to each session, a research assistant visited the selected village and invited as many volunteers as possible to gather at a scheduled time and place for the research project. Participants had to be village residents over 18 years old, with only one participant per household allowed to take part.

On the day of the experiment, the research team randomly selected nine male and nine female participants from those who responded to the invitation. Those not selected received a show-up fee of CFA 500 (\$0.85) and were asked to leave. The team conducted 32 fieldwork sessions across different villages (16 treated), with a total of 575 individuals participating in the experiment².

Sessions were held in common spaces like school classrooms or public buildings. The experimenter read the instructions aloud, and then participants were called individually into a separate room to make their decisions privately. To minimize experimenter effects, a blind procedure was used for the subject’s decision. Participants rolled the dice inside a cup with a hole on the bottom that allowed the subject to privately read the outcome of the roll. Participants also engaged in an incentivized risk elicitation task, a socio-demographic survey, and additional unrelated fieldwork activities. Each session lasted about three hours, and participants earned an average of CFA 2800 (\$4.5), equivalent to approximately one and a half days’ wages for the median participant in the sample.

¹The experimental design, hypotheses to be tested, and regression model specifications were documented in a pre-analysis plan submitted to the American Economic Association’s RCT Registry (ID: AEARCTR-0005324).

²One participant had to leave due to illness

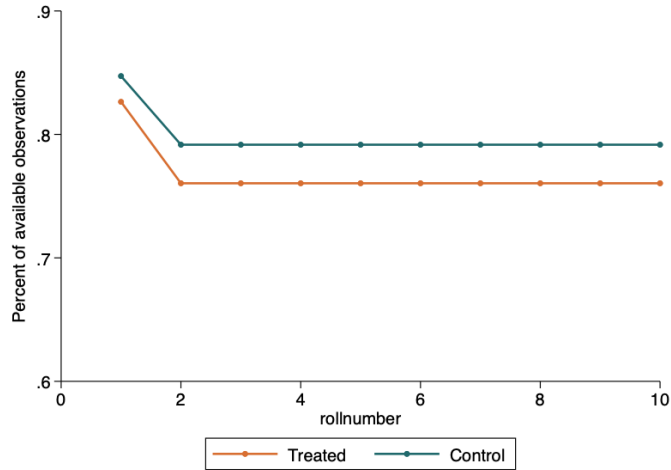


Figure 1: Number of observations for every die roll by treatment

3 Results

Descriptive statistics and Sample Balance

The experiment involved 576 subjects which were supposed to report ten dice rolls for a total of 5760 observations. Unfortunately, some participants only reported the outcome of their first attempt. Without supervision during the activity that was necessary to preserve the anonymity of the protocol, it was impossible to instruct them on the proper procedure or request a repetition if they misunderstood the instructions. In rarer instances, it became challenging to discern the results following the first attempt³. Graph 1 illustrates the number of observations available.

The internal validity of our study depends on two successful sample randomizations. The first one concerns the original RCT. Indeed a comprehensive impact evaluation by the World Bank’s Gender Innovation Lab confirmed the success of the lottery-based randomization. Using extensive pre- and post-treatment survey data collected by a national agency as well as administrative data from the Millennium Challenge Corporation, Benin, the evaluation demonstrated a pre-intervention balance between treatment groups. This cross-evaluation dispelled concerns about the randomization process (Omondi, 2019, Goldstein et al., 2018) Regarding the correct randomization of our sample of villages, we gathered data from residents of the 32 randomly selected villages from the RCT pool. Appendix A, Table ??, provides descriptive statistics for the pre-registered socio-demographic characteristics of our participants. Although the sample is well-balanced for most variables such as social status income and wealth levels, participants in

³A significant number of participants had never used a pen before; they would mark the initial figure, rendering subsequent results incomprehensible.

	expPFR		Total
	0	1	
Control	209	35	244
Treated	72	166	238
Total	281	201	

Table 1: Subjects for which observations on the die-roll are at least partly available in treated and control villages and that have owns land under PFR

the treated group tend to be older, more likely to be married and to manage household finances, and have a slightly higher literacy rate compared to the control group. To address these imbalances, we include these characteristics as controls in our analysis.

Another important check concerns the potential spillovers between treatments. Various types of contamination might occur, such as villagers moving between control and treated villages after the RCT or villagers in control villages owning land in treated villages, and vice versa. To investigate these effects, we collected data on participants' origins, migration status, reasons for migration, and years of residence in the village. Another type of spillover may arise if individuals in control villages, after ten years of intervention, learned about the PFR and its benefits and decided to use it regardless. In our sample, only 27% of participants were aware of the reform, raising some concerns.

In addition to the variable $\textit{treated}$, which indicates whether a subject resides in a treated village, we coded an additional variable $\textit{expPFR}=1$ if the participant's household has a land parcel treated by PFR. Table [\ref{tab&expPFR}](#) shows that the two groups only partially overlap. How is this possible? On the one hand, 78 subjects in treated villages have not been exposed to the PFR. This could be because they do not own any land, or their plot was not included in the program for various reasons (e.g., lack of agreement with neighbors, absence during the survey, etc.). On the other hand, 41 subjects in control villages have been exposed to the PFR. This could occur if a respondent is a resident of a control village but owns land in a treated village or if they are one of the 0.2% of Beninese who independently acquired a PFR property certificate despite residing in a control village, thus paying for it themselves. We will illustrate results both with respect to *treated* and to *expPFR*.

4 Truthful reporting

We first illustrate the data graphically

4.0.1 Non parametric tests

To be added

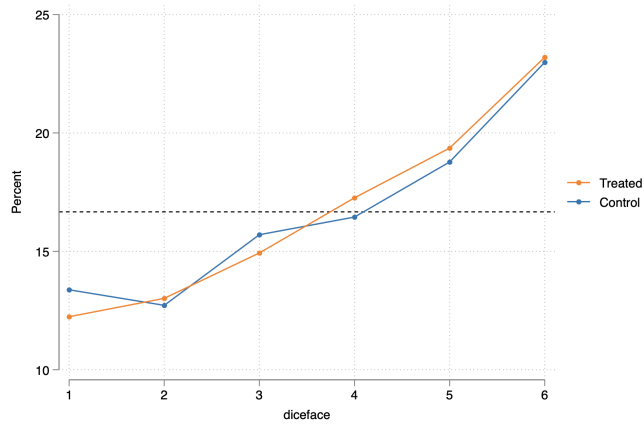


Figure 2: Truthful reporting by treated village

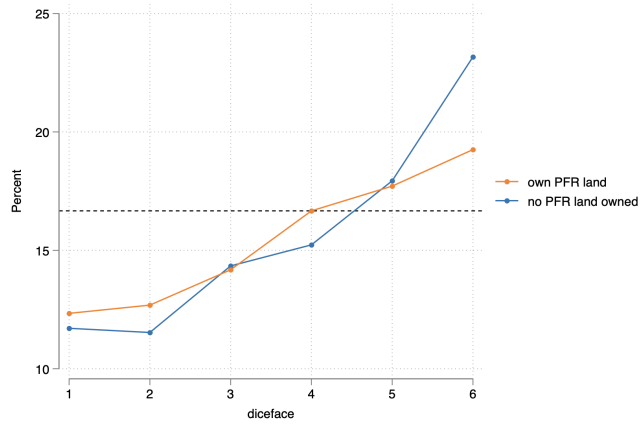


Figure 3: Truthful reporting by expPFR

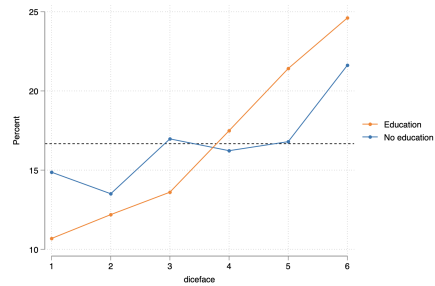


Figure 4: Truthful reporting by the level of education

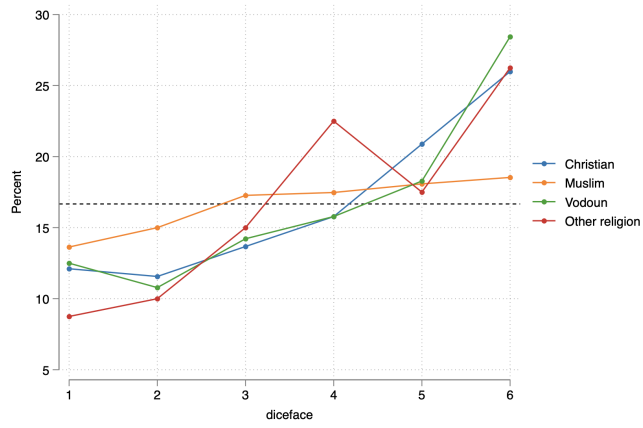


Figure 5: Truthful reporting by religion

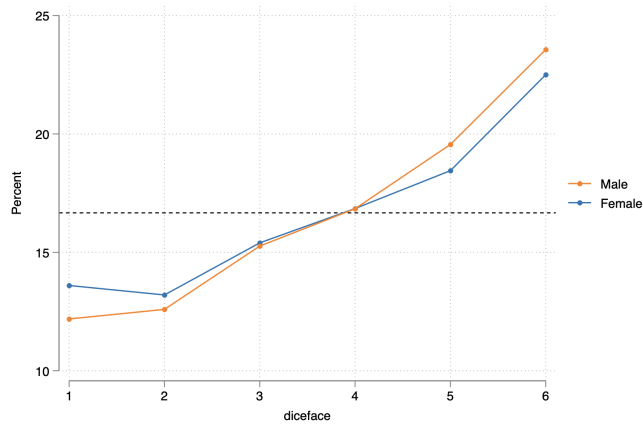


Figure 6: Truthful reporting by gender

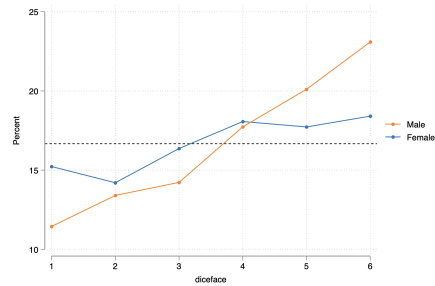


Figure 7: Truthful reporting by treatment and female

4.0.2 mixed-effect ordinal regression for multilevel data

Appendix A: Supplementary Analysis

labelformat=AppendixTables

5 Appendix B: Instructions

Instructions are adapted from Ariely et al. (2019).

In this task, you are asked to throw a 6-faces dice 20 times. Every time, before you roll the dice, you will be asked to choose one side of the dice in your mind: top or bottom. Be sure to make your choice before you roll the dice. Then, after rolling the dice, please enter the outcome of your roll, i.e., the number shown on the chosen side of the dice (top or bottom) on each line of this sheet. Bear in mind that there are two possible sides to each roll: top or bottom. Here, the figure shows the different outcome combinations.

Please return your record sheet to the experimenter after you throw the dice 20 times. The experimenter will then randomly draw a number from 1 to 20 by rolling a 20-faces dice. This dice determines which of your rolls is relevant for your payment. Once determined the relevant roll, you will be paid a number of tokens equal to the number you reported for that particular line of the sheet. For example, assume the experimenter rolls the 20 faces dice and obtains the number 3, then the number you reported on the third line determines your payment. The experimenter will check your record sheet and pay you a number of tokens equal to the number you reported on the third line.

Please discuss any questions with the experimenter before starting the task!

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	(1)	(2)	(3)	(4)	(5)
dicereport					
treated	0.079 (0.140)				
Control		0.000 (.)			
Treated		-0.013 (0.167)			
male=0		0.000 (.)		0.000 (.)	0.000 (.)
male=1		-0.016 (0.117)		-0.071 (0.110)	-0.169 (0.113)
Control × male=0		0.000 (.)			
Control × male=1		0.000 (.)			
Treated × male=0		0.000 (.)			
Treated × male=1		0.163 (0.167)			
expPFR			-0.203** (0.099)		
expPFR=0				0.000 (.)	0.000 (.)
expPFR=1				-0.353*** (0.133)	-0.340*** (0.129)
expPFR=0 × male=0				0.000 (.)	0.000 (.)
expPFR=0 × male=1				0.000 (.)	0.000 (.)
expPFR=1 × male=0				0.000 (.)	0.000 (.)
expPFR=1 × male=1				0.296* (0.172)	0.368** (0.172)
age					-0.001 (0.004)
school					-0.247 (0.173)
education					0.156*** (0.052)
married					0.017 (0.140)
polygam					-0.021 (0.092)
christian					0.264** (0.105)
vodoun					0.333** (0.143)
otherreligion					0.146 (0.263)
incomeweek					0.000 (0.000)
/					
cut1	-2.068*** (0.106)	-2.076*** (0.123)	-2.192*** (0.091)	-2.228*** (0.110)	-2.010*** (0.214)

Table 1: Sample Balance, All Observations Treatment vs Control)

	(1)		
	mu_1	mu_2	b
male	.5069444	.4930556	.0138889
age	36.82292	40.09028	-3.267361***
muslim	.4131944	.4583333	-.0451389
vodoun	.1875	.1840278	.0034722
christian	.4027778	.3298611	.0729167*
married	.8333333	.8958333	-.0625**
familymembers	10.07639	9.847222	.2291667
managemoney	.9652778	.9930556	-.0277778**
literate	.3333333	.4027778	-.0694444*
bornvillage	.7222222	.6909722	.03125
yearsvillage	30.87153	32.35764	-1.486111
incomeweek	8468.229	9026.736	-558.5069
wealth_land_ind	5.102431	5.479167	-.3767361
wealth_1_1	.59375	.6423611	-.0486111
wealth_1_2	.3680556	.3680556	0
wealth_1_3	.1805556	.2673611	-.0868056**
wealth_2_1	.6319444	.6319444	0
wealth_2_2	.0659722	.0902778	-.0243056
wealth_2_3	.7708333	.7847222	-.0138889
wealth_2_4	.2743056	.3333333	-.0590278
sec	4.364583	4.451389	-.0868056
bornvillage	.7222222	.6909722	.03125
population	2748.125	2934.375	-186.25
Observations	576		

Table 2: Sample Balance, Only Non-missing Observations Treatment vs Control

	(1)		
	mu_1	mu_2	b
male	.5327869	.5546218	-.021835
age	35.94672	39.40336	-3.45664***
muslim	.4057377	.4831933	-.0774556*
vodoun	.1721311	.1764706	-.0043394
christian	.4303279	.3151261	.1152018***
married	.8278689	.8991597	-.0712908**
familymembers	10.16803	9.970588	.1974446
managemoney	.9631148	.9915966	-.0284819**
literate	.3770492	.4705882	-.0935391**
bornvillage	.7172131	.7184874	-.0012743
years_village	29.89754	32.31513	-2.417585*
incomeweek	8763.73	9509.244	-745.5142
wealth_land_ind	5.260246	6.12395	-.8637037
wealth_1_1	.5942623	.6428571	-.0485948
wealth_1_2	.3893443	.3823529	.0069913
wealth_1_3	.1885246	.2731092	-.0845847**
wealth_2_1	.6352459	.6428571	-.0076112
wealth_2_2	.0737705	.0882353	-.0144648
wealth_2_3	.7827869	.8067227	-.0239358
wealth_2_4	.2868852	.3319328	-.0450475
sec	4.393443	4.466387	-.0729439
bornvillage	.7172131	.7184874	-.0012743
population	2923.77	3129.832	-206.0614
Observations	482		

Table 3: Sample Balance, ALL Observations expPFR

	mu_1	mu_2	b
male	.5384615	.4501992	.0882623**
age	37.10462	40.20717	-3.102556***
muslim	.4184615	.4581673	-.0397058
vodoun	.1692308	.2071713	-.0379405
christian	.4092308	.310757	.0984738**
married	.8553846	.876494	-.0211094
familymembers	9.984615	9.932271	.0523445
managemoney	.9753846	.9840637	-.0086791
literate	.3692308	.3665339	.0026969
bornvillage	.7323077	.6733068	.0590009
yearsvillage	31.19385	32.15936	-.9655164
incomeweek	9154.923	8219.92	935.0028
wealth_land_ind	5.36	5.201195	.1588048
wealth_1_1	.5938462	.6494024	-.0555562
wealth_1_2	.3692308	.3665339	.0026969
wealth_1_3	.1907692	.2669323	-.076163**
wealth_2_1	.6061538	.6653386	-.0591848
wealth_2_2	.0646154	.0956175	-.0310021
wealth_2_3	.7569231	.8047809	-.0478578
wealth_2_4	.2369231	.3904382	-.1535152***
sec	4.273846	4.581673	-.3078272**
bornvillage	.7323077	.6733068	.0590009
population	2803.569	2890.04	-86.47061
Observations	576		

Table 4: Sample Balance, ALL Observations expPFR

	mu_1	mu_2	b
male	.5658363	.5124378	.0533985
age	36.29181	39.55721	-3.265399***
muslim	.4092527	.4925373	-.0832846*
vodoun	.1672598	.1840796	-.0168198
christian	.4234875	.3034826	.120005***
married	.8469751	.8855721	-.0385971
familymembers	9.989324	10.18408	-.1947558
managemoney	.9715302	.9850746	-.0135444
literate	.4163701	.4328358	-.0164657
bornvillage	.7224199	.7114428	.0109771
yearsvillage	30.11744	32.45274	-2.335299*
incomeweek	9316.192	8874.129	442.0628
wealth_land_ind	5.487544	5.965174	-.4776296
wealth_1.1	.594306	.6517413	-.0574352
wealth_1.2	.3950178	.3731343	.0218835
wealth_1.3	.202847	.2686567	-.0658097*
wealth_2.1	.6085409	.681592	-.0730511
wealth_2.2	.0747331	.0895522	-.0148191
wealth_2.3	.7615658	.840796	-.0792302**
wealth_2.4	.2419929	.4029851	-.1609922***
sec	4.327402	4.572139	-.2447372
bornvillage	.7224199	.7114428	.0109771
population	2961.922	3114.428	-152.5062
Observations	482		

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