Comments

Cooperative Property Rights and Development: Evidence from Land Reform in El Salvador: A Comment

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Montero (2022) uses a land reform in El Salvador to investigate the long-run causal effects of ownership structure on agricultural production and worker outcomes. The paper presents two main empirical findings. (1) Relative to outside-owned haciendas operated by contract workers, the productivity of worker-owned cooperatives is higher for staple crops and lower for cash crops. (2) The cooperative workers have higher incomes and more compressed wage distributions than those working in the haciendas.

In this comment, we show that the second set of results rests on two mistakes in the analysis: three-quarters of the observations are duplicates,

The issues discussed in this comment were identified during the Oslo Replication Games organized by the Institute for Replication. We thank Eduardo Montero for making the replication data and code freely available and for his admirable response in support of research integrity in his personal communications with our team. We further thank Abel Brodeur, Emir Kamenica, Kalle Moene, Sahar Parsa, Oddbjørn Raaum, and Rony Rodríguez-Ramírez for insightful comments. This comment was edited by Emir Kamenica.

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and income inequality is calculated over too few workers to be meaningful. When corrected, the data sources and research design provide no credible evidence regarding the causal effects of ownership structure on income levels and inequality.

The identified issues concern a central finding of the paper: the causal effects on income levels and inequality are highlighted in both the abstract and the conclusion, as well as in the author's own popularization of the work (see Montero 2017, 2019). The result has also been referenced by others as clear evidence that land reforms lead to "more equitable rural economies based on staple crops and higher incomes" (Boberg-Fazlić et al. 2022, 2) and that cooperative agriculture facilitates "a more equitable distribution of agricultural revenue" (Rao and Shenoy 2023, 710). Although these empirical claims are intuitively plausible and may well be true, they nonetheless lack empirical support of the kind claimed in Montero (2022).

We stress that the errors are confined to the analyses of income levels and distributions and that the research design employed in Montero (2022) is theoretically appropriate, but the specification described is not the one implemented, and the actual data available are insufficient for the analysis. Below, we start by describing the original results in more detail. We then describe the errors in turn and provide updated estimates, before we conclude.

I. Reproducing the Original Results

Montero (2022) estimates the long-run causal effects of a property rights reform in El Salvador on workers' income and inequality. The reform had been developed in secret, and the military moved swiftly to implement the first phase in 1980: seizing agricultural land from owners whose total agricultural land holdings across all plots exceeded 500 hectares (ha) and transferring ownership to agricultural cooperatives run by permanent laborers working the land.

Three separate analyses are used to assess the effects on worker inequality and income. Two of these use a regression discontinuity (RD) design to assess how inequality and income levels differ for properties on different sides of the 500-ha discontinuity threshold, while the last uses a quantile regression to assess differences in the income distribution of haciendas and cooperatives.

The analyses use data from a repeated household survey, *Encuesta de Hogares de Propósitos Multiples*, covering the period 2000–2013. These data include geographical identifiers for each participant at the levels of department, municipality, and canton.[1] The survey data also include

[1] Cantons roughly correspond to villages, according to n. 11 in Montero (2022).

information on whether an individual works in agriculture as a cooperative member or as a hacienda laborer. Montero (2022) uses this information to link the household survey data with the other property data, restricting the matching to cantons with only one cooperative/large hacienda (60).

Inequality is measured with the interquartile range (IQR) of real per capita household income, defined as the value for the 75th percentile minus the value for the 25th percentile. In the replication code, this calculation is done at the level of property-year. Collapsing the data to this level, the specification used to estimate the effect on inequality can be written as follows:

$$\text{IQR}_{po,t} = \gamma \text{Above500}_o + f(\text{holdings}_o) + \sigma_t + \epsilon_{po,t}, \quad (1)$$

where $\text{IQR}_{po,t}$ is the IQR in year t among workers at plot p owned by owner o before the reform, Above500_o is an indicator variable for whether owner o had over 500 ha in cumulative landholdings before the reform, and $f(\text{holdings}_o)$ is a linear control for the forcing variable (total landholdings of prereform property owner).

In columns 1 and 2 of table 1, we reproduce the main results on worker equity, using this specification. Column 1 displays estimates when limiting the sample to properties within 300 ha of the reform threshold, while column 2 displays estimates when limiting the sample to properties within 150 ha of the reform threshold. The results suggest that the income distribution in cooperatives is more compressed than that in haciendas.

The same survey data are used to study the effect of cooperatives on workers' income level, using the following specification:

$$\text{IncomePC}_{ipo,t} = \gamma \text{Above500}_o + f(\text{holdings}_o) + \sigma_t + \epsilon_{ipo,t}, \quad (2)$$

where $\text{IncomePC}_{ipo,t}$ is real income per capita (last month) for the household of worker i. We reproduce these estimates in columns 1 and 2 of table 3, using the broad and narrow bandwidths, respectively. The results suggest that workers at cooperatives have higher incomes on average. Finally, Montero (2022) uses the household survey data to estimate a quantile regression, which investigates how the income premium for cooperative workers varies across the income distribution. We reproduce this in the left-hand panel of figure 1. The figure suggests that the cooperative ownership disproportionately boosts the earnings of the lowest quantiles.

II. Identified Issues with the Analysis

In this section, we first describe a mistake in the data construction: three-quarters of the worker-level data consist of duplicate observations. We then show that the implemented analysis differs from the description

TABLE 1
Household Income per Capita, IQR

	Original results		Corrected data		Sample restriction		Corrected data and sample restriction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above500	−42.97**	−51.79**	−72.24***	−79.78***	−50.91	−90.86	−65.81	. . .
	(17.82)	(24.79)	(17.37)	(22.67)	(43.09)	(84.53)	(45.36)	
Observations	327	118	324	116	128	45	70	26
Clusters	98	36	98	36	54	21	31	12
#Treated prop.	30	21	27	19	3	2	1	0
#Workers	4,770	1,583	1,146	420	4,377	1,434	675	254
Bandwidth (ha)	300	150	300	150	300	150	300	150

Note.—The table shows estimates of the effect of the land reform on inequality. Columns 1 and 2 reproduce cols. 3 and 4 of table 5 in Montero (2022). The estimates in cols. 3 and 4 are based on the corrected data (excluding duplicates). The estimates in cols. 5 and 6 impose the sample restriction described in Montero (2022) to the original estimation data (including duplicates). The estimates in cols. 7 and 8 impose the sample restriction to the corrected data. "Above500" is a binary variable equal to 1 if the former owner had over 500 ha in cumulative landholdings in 1980; "observations" displays the number of property-year units; "clusters" displays the number of clusters (former land owners); "#treated prop." displays the number of property-year units above the RD threshold; "#Workers" displays the number of workers included in IQR calculation; while "bandwidth" shows the RD bandwidth in hectares.

** $p < .05$.

*** $p < .01$.

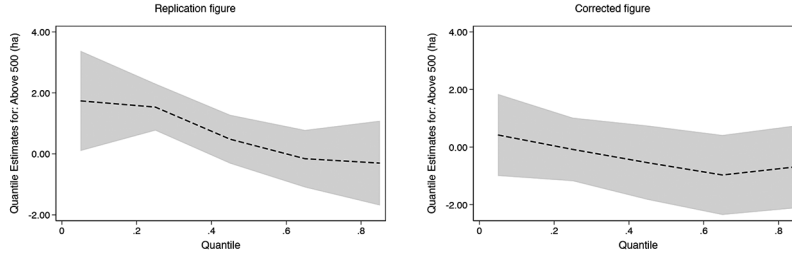


FIG. 1.—Estimated quantile RD coefficients, using a bandwidth of 150 ha from the RD threshold. The independent variable is the log of real per capita household income. The regression includes year fixed effects and a linear control for the forcing variable (total landholdings of the prereform property owner), estimated separately on each side of the RD threshold. Standard errors are clustered at the former-owner level, and the gray areas display 95% confidence intervals. *Left*, reproduced figure 6 in Montero (2022). *Right*, quantile regression following the description in Montero (2022): it does not impose any sample restrictions; it is based on the corrected data; and it includes controls for age and age squared of each worker.

in the paper and that it measures income inequality on the basis of too few workers to be meaningful. Correcting these issues, we conclude that the reported results on income levels and distributions do not hold.

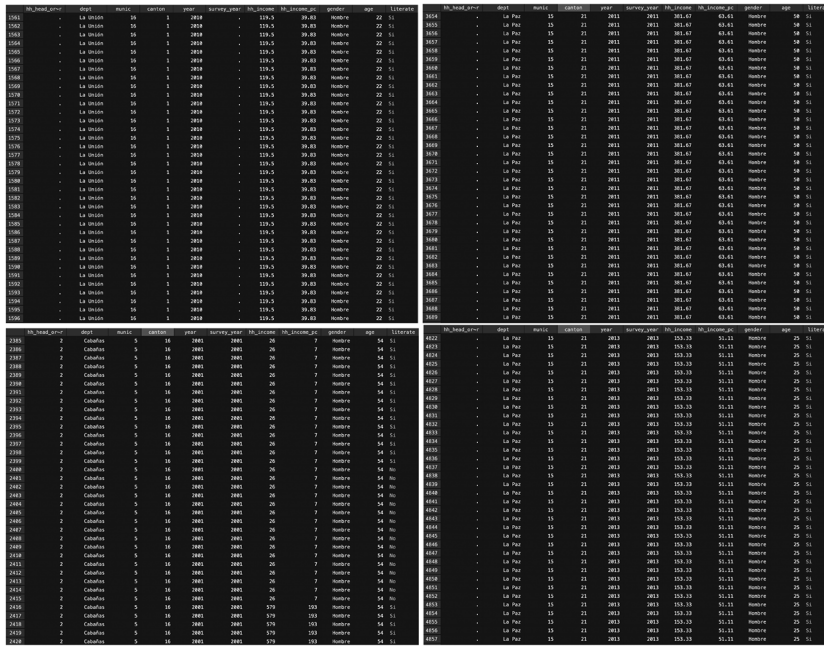
A. Duplicate Observations

A visual inspection of the replication data indicates a large number of duplicates, as illustrated by the screenshots from the data editor in figure 2.

We use the raw data from the survey to remove duplicates from the author-submitted analysis data file. We first restrict the survey sample to observations with nonmissing "property type," as this variable was used by Montero (2022) to match the survey data to the property data. Next, we identify a set of survey variables that are both retained in the author's analysis data and sufficient to uniquely identify individuals in the raw data.[2] This allows us to remove all duplicates by collapsing the author's estimation data to unique combinations of the identifying variables.[3] For the broad-bandwidth sample, this reduces the number of workers by three-quarters, from 4,770 to 1,146.

[2] The following variables are sufficient to uniquely identify individuals within cantons: age, household size, household income, landholding type, and the number of other workers at the property. The only exception is two nonunique observations in the survey for 2011. These observations are located in cantons that are not in the sample of Montero (2022).

[3] We manually checked households with more than one remaining observation against the raw data and removed individuals from those households that appear only in the estimation data. We removed 43 observations in this way. It is unclear to us how these observations appeared.



hh_head_ori~r	dept	munic	canton	year	survey_year	hh_income	hh_income_pc	gender	age	literate
1561	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1562	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1563	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1564	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1565	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1566	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1567	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1568	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1569	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1570	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1571	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1572	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1573	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1574	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1575	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1576	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1577	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1578	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1579	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1580	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1581	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1582	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1583	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1584	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1585	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1586	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1587	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1588	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1589	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1590	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1591	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1592	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1593	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1594	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1595	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si
1596	La Unión	16	1	2010	.	119.5	39.83	Hombre	22	Si

hh_head_ori~r	dept	munic	canton	year	survey_year	hh_income	hh_income_pc	gender	age	literate
3634	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3635	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3636	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3637	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3638	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3639	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3640	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3641	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3642	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3643	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3644	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3645	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3646	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3647	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3648	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3649	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3670	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3671	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3672	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3673	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3674	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3675	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3676	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3677	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3678	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3679	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3680	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3681	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3682	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3683	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3684	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3685	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3686	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3687	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3688	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si
3689	La Paz	15	21	2011	2011	301.67	63.61	Hombre	50	Si

hh_head_ori~r	dept	munic	canton	year	survey_year	hh_income	hh_income_pc	gender	age	literate
2385	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2386	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2387	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2388	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2389	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2390	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2391	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2392	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2393	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2394	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2395	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2396	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2397	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2398	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2399	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	Si
2400	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2401	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2402	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2403	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2404	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2405	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2406	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2407	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2408	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2409	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2410	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2411	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2412	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2413	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2414	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2415	2	Cabañas	5	16	2001	2001	26	7	Hombre	54	No
2416	2	Cabañas	5	16	2001	2001	579	193	Hombre	54	Si
2417	2	Cabañas	5	16	2001	2001	579	193	Hombre	54	Si
2418	2	Cabañas	5	16	2001	2001	579	193	Hombre	54	Si
2419	2	Cabañas	5	16	2001	2001	579	193	Hombre	54	Si
2420	2	Cabañas	5	16	2001	2001	579	193	Hombre	54	Si

hh_head_ori~r	dept	munic	canton	year	survey_year	hh_income	hh_income_pc	gender	age	literate
4022	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4023	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4024	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4025	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4026	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4027	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4028	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4029	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4030	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4031	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4032	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4033	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4034	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4035	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4036	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4037	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4038	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4039	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4040	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4041	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4042	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4043	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4044	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4045	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4046	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4047	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4048	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4049	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4050	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4051	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4052	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4053	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4054	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4055	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4056	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si
4057	La Paz	15	21	2013	2013	153.33	51.11	Hombre	25	Si

FIG. 2.—Four screenshots from the estimation data, labeled "ehpm_incomemodule _wreform.dta" in the replication package of Montero (2022). This data file is used to study the effect of the land reform on income levels and distributions.

The duplication issue affects 129 of the 327 property-year observations but leaves the number of aggregate property-year observations largely unchanged: as long as the duplicated observations of an individual are assigned to the same property, there will be at least one individual-level observation remaining to inform the property-year observation. This is largely the case, so while the duplicates reduce the individual-level sample to a quarter of its original size, the number of property-year observations is reduced by only three.[4]

[4] Six unique individuals were found to have duplicated observations allocated to *different* properties. All observations for these individuals were discarded (as we do not know the correct property assignment). Three property-year observations were based on a single observation from one of these six individuals.

A second data error concerns the variable for real household income, used in the computation of the outcome variables. By reviewing the computer code, we infer that Montero (2022) used a consumer price index (CPI) published by the World Bank to deflate nominal income from different survey years. However, the computer code does not include the year 2010, and observations from this year are assigned a value of real income equal to zero. This implies that the IQR is calculated to be zero for all properties in 2010. In our corrected dataset, we handle this by including 2010 in the computer code, using the CPI value for this year. The correction does not affect the estimates much.

B. Measuring Inequality with Too Few Observations

Inequality is measured at the property-year level as the IQR of the individual-level survey observations assigned to this property-year cell. As noted in Montero (2022), this measure requires a certain number of observations to be meaningful: "To construct measures of the income distributions, I limit the sample to cooperatives and haciendas for which there are at least five members represented in the household surveys" (Montero 2022, 75). The sample restriction means that the estimation sample will be more strongly affected by the duplicate issue than the "unrestricted" property-year sample, since the removal of duplicates strongly reduces the number of individual-level observations informing the property-year observations.

In addition, a review of the paper's replication code revealed that the sample restriction described in the main text was not actually implemented in the code for the RD regression on income inequality. Even without removal of the duplicates, some 29% of the property-year units in the original broad-bandwidth sample contain only one observation. The IQR of a single observation is by necessity zero, and these property-year observations cannot inform analyses of property-level inequality. An additional 32% of the property-year units in the author-submitted data contain two to four observations. In total, 61% of the property-year units in the paper's own estimation sample should therefore have been excluded, according to the criteria defined in the paper.

C. Updated Results

In columns 3 and 4 of table 1, we show estimates of the effects of property rights systems on the IQR once we correct the household data as described in section II.A. The data corrections alone do not affect the estimates much. As mentioned, even though the number of workers in the sample falls by three-quarters, the number of observations in the regressions barely changes as the sample is collapsed on property-year.

We next impose the sample restriction described in Montero (2022) and remove property-year units with fewer than five observations. We first impose the restriction to the original estimation data (which includes the duplicates). Estimates are shown in columns 5 and 6 of table 1. We are left with 128 property-year observations in the broad-bandwidth sample, only three of which are above the RD threshold. In the narrow-bandwidth sample, we are left with 45 observations, only two of which are above the RD threshold. None of the results are statistically significant (p-values of .243 and .295, respectively).

We then impose the sample restriction to the corrected dataset (which excludes the duplicates). Estimates are shown in columns 7 and 8 of table 1. The remaining dataset is insufficient to perform the inequality

analyses. The sample with the broad bandwidth has 70 remaining property-year units but only one observation above the RD threshold; the narrow-bandwidth sample has 26 property-year units below the threshold but no observations above the threshold. We thus conclude that the data at hand are insufficient to conduct the analysis as described in the paper.

D. Results with Alternative Sample Restrictions

The issues we have identified are clear errors: duplicate observations and a failure to implement an explicitly stated (and methodologically important) sample restriction in the actual code.

That being said, the number of individual-level observations required to compute a meaningful interquartile income range is not obvious. We find the paper's choice of a five-observation minimum reasonable, but it is worth examining how sensitive the results are to alternative thresholds. We use the corrected (nonduplicate) data.

Estimates are shown in table 2. We first restrict the sample to property-years with more than three observations (cols. 1 and 2) and then to property-years with more than two observations (cols. 3 and 4). The sample sizes increase somewhat for these specifications, but there are still only a few property-year units above the RD threshold. As a consequence, the estimated coefficients vary widely, from +182 in column 1 to −28 in column 2.

These results show that there are insufficient data to implement an RD analysis on observations at the property-year level, since many property-year cells have sparse counts and do not allow us to meaningfully compute

TABLE 2
ALTERNATIVE SAMPLE RESTRICTIONS AND SPECIFICATIONS, IQR

	PROPERTY-YEAR $n>3$		PROPERTY-YEAR $n>2$	
	(1)	(2)	(3)	(4)
Above500	182.20***	−28.35	157.08***	−3.53
	(21.52)	(49.07)	(21.82)	(42.03)
Observations	98	37	136	49
Clusters	46	16	62	20
#Treated prop.	4	3	6	4
#Workers	787	298	901	334
Bandwidth (ha)	300	150	300	150

NOTE.—The table shows estimates of the effect of the land reform on inequality, using alternative sample restrictions. All estimates are based on the corrected data (excluding duplicates). The sample used in cols. 1 and 2 is restricted to property-year units with more than three observations, while the sample in cols. 3 and 4 is restricted to property-year units with more than two observations. "Observations" displays the number of property-year units; "clusters" displays the number of clusters (former land owners); "#treated prop." displays the number of property-year units above the RD threshold; "#workers" displays the number of workers included in IQR calculation.

*** $p < .01$.

within-cell income inequality. We thus conclude that the original results on property rights systems on inequality do not hold for any reasonable sample restriction.

E. Results on Income Levels and Quantile Regression

Our main focus in this comment is on the inequality analysis, as this was a main outcome of interest in Montero (2022). The household survey data are, however, also used to estimate the effects of ownership structure on income levels and to run a quantile regression to identify the income effect at different points of the income distribution.

Starting with the RD analysis of income levels, we find that the estimates are no longer statistically significant in the corrected data (excluding the duplicates). As shown in columns 3 and 4 of table 3, the point estimate drops by three-quarters in the broad-bandwidth sample (p-value of .684) and switches sign in the narrow-bandwidth sample (p-value of .566).

Turning to the quantile regression, our code review found that this analysis implements the sample restriction described for the inequality analysis. The restriction is not warranted in the present context, as the quantile regression uses individual-level observations to compare the overall population of cooperative workers to the overall population of hacienda workers. Also, the sample restriction is not mentioned or discussed in the context of the quantile regression. In the right-hand panel of figure 1, we present the results from the quantile regression when we disregard the sample restriction and use the corrected data.[5] The figure is much less informative than the published figure, and it no longer suggests that the "earnings policies within reform cooperatives . . . help workers at the bottom of the income distribution" (Montero 2022, 76).

III. Conclusion

In "Cooperative Property Rights and Development: Evidence from Land Reform in El Salvador," it is claimed that workers of cooperatives have less income inequality and higher average incomes than workers of haciendas. The paper provides empirical evidence on this by combining household survey data with other data on cooperatives and haciendas.

In this comment, we have shown that these conclusions are wrong—in the sense that they are not supported by the data once duplicate observations are removed and the analyses are implemented as described in the

[5] We also add controls for age and age squared for each worker, as described in Montero (2022) but not implemented in the submitted computer code. The inclusion of these controls barely affects the estimated coefficients. The paper states that the quantile regression includes a control for gender as well. We are not able to add this control, as gender varies too little over the income distribution (93% of the sample are men).

TABLE 3
HOUSEHOLD EARNINGS PER CAPITA (PREVIOUS MONTH), LEVELS

	ORIGINAL RESULTS		CORRECTED DATA	
	(1)	(2)	(3)	(4)
Above500	60.49*	52.79	13.21	−24.24
	(31.03)	(40.61)	(32.35)	(41.90)
Observations	4,770	1,583	1,146	420
Clusters	98	36	98	36
#Treated	88	48	51	31
Bandwidth (ha)	300	150	300	150

NOTE.—The table shows estimates of the effect of the land reform on income levels. Columns 1 and 2 reproduce cols. 3 and 4 of table 5 in Montero (2022). The estimates in cols. 3 and 4 are based on the corrected data (excluding duplicates). "Observations" displays the number of workers; "clusters" displays the number of clusters (former land owners); "#treated" displays the number of workers above the RD threshold.

* $p < .1$.

paper. While the mistakes are confined to the results on worker income and inequality, these results are a key finding highlighted in the abstract and the conclusion of the paper. Recent papers citing Montero (2022) also highlight these results.

We fully agree with Montero (2022) when he argues that "property rights institutions are of central importance to understanding economic development" and that there is "limited causal empirical evidence on the impacts of different property rights systems" (87). Unfortunately, we still lack robust and credible empirical evidence regarding the effects of different property rights system on worker equity. We hope that more research will be conducted in this area.

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