

Response to Replication Report for
“Property Rights for the Poor: Effects of Land Titling”
(Journal of Public Economics, 2010)

Replication Team: D. Cameron, E. Whitney and P. Winters

September 18, 2015

by

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This document provides a formal response to the replication analysis conducted by Cameron, Whitney and Winters (August, 2015), and funded by the International Initiative for Impact Evaluation (3ie), on our article “Property Rights for the Poor: Effects of Land Titling” (from now own GS) published in the *Journal of Public Economics*, 2010. Our paper is part of a series of published and current studies based on a natural experiment in Argentina (see also Galiani and Schargrodsky (2004), Di Tella, Galiani and Schargrodsky (2007), and Galiani and Schargrodsky (2015)).

The replication report includes two main parts: (i) a Pure Replication Analysis (PRA) in their section 3, and (ii) an Additional Analysis (AA), or “Replication Plus” study (terminology that the replication team used in the communication with us) in their section 4. Only the PRA contains a replication of our work, while the AA concerns “new research” conducted by the replication team based on the natural experiment we originally exploited and our data. Thus, the AA is not a replication but rather a collection of new findings that extend and build on our article.¹

Regarding the replication by Cameron et al. we were glad to read that the authors were able to replicate all our original results.² They also report that our results are robust to their alternative specifications and robustness checks conducted, indicating that “the original paper provided an accurate assessment of the impact of land titling on the poor”. In a nutshell, after conducting all their analyses (including their “Replication Plus” exercises), the replication team concludes that: “the robustness of our results [reported in the replication report] confirms the importance of GS’s contribution to the literature”.

Even though we disagree with the unnecessarily aggressive tone of some statements in the replication report (particularly in the abstract), we focus here only on those comments that could be misleading to readers.

First of all, the replicators dedicate a full section (section 2) in which, in order to motivate their “Replication Plus” analysis, they criticize our study for not being comprehensive enough, and instead analyzing only a limited subset of possible impacts. This general criticism can probably be made of any research in the social sciences. We find it, however, a bizarre comment for an exhaustive paper with 30 (thirty) dependent variables and 17 (seventeen) appendix tables, complemented by additional articles on the same natural experiment.³

¹ See Cattaneo et al. (2015) for an elaboration on these issues.

² In their report, Cameron et al. correct many times the decimal rounding of our estimates. This is just the result of them using a newer version of STATA than the one we used to estimate our results several years ago. It is not clear to us why they need to do that since our do files indicate the version used and, more importantly, it does not make any economic or statistical difference at all.

³ In their Table 1, the replication team presents as indicators not analyzed (unbolded) in our research, variables such as Transferability, Property Transactions, Household Entry and Exit, Fertility, Health

Second, the replication team claims that they did not have all the data for a complete replication, including the data for their “Replication Plus” analysis. We consider this is incorrect. Subjected to IRB regulations as well as our compromise with the NGO that conducted our surveys to preserve the address and other individual data that in small areas could identify families, we provided all the necessary data for the replication team to conduct their replication plan,⁴ following the profession’s standard. Our STATA files, also made available in our websites,⁵ include the raw data collected in 2003 and 2007 and notes with the survey questions.⁶

Third, in our original paper we present a balance of the parcel characteristics (available for the whole population of parcels in the main area of analysis) and the original squatter characteristics (available for the sample of surveyed households) showing the lack of differences between intention-to-treat and control parcels. Though the authors replicate our balance tables, they also report a table of balance of parcels characteristics using only the sample of surveyed parcels, and find that two variables that we reported balanced were not. However, this unbalance is just an artifact from the fact that, for optimizing statistical power given the costly data collection process, we oversampled the late treatment parcels and hence, the sample of parcels was clearly not representative of the population (since only one former owner constitutes the late treatment in our analysis). We judged that using all the available parcel data was the right analysis because we were not trying to verify, as it is common now in RCTs, randomization balance but to gauge instead the exogeneity of the intention to treatment. We recall that in our empirical analysis we not only show that all our results are extremely robust to controlling for all the pre-treatment observable variables, including the parcel and original squatter characteristics, but also that we conducted the analysis separating the early and late treatment effects.

In spite of these disagreements with the replication team, we thank them for a professional pure replication effort, which we believe was done with care. We believe that the results from this effort, reported in the first portion of the replication report (PRA), enhance the credibility of the results reported in our published paper “Property Rights for the Poor: Effects of Land Titling” (*Journal of Public Economics*, 2010).

We believe pure replication efforts are important, as they provide reassurance on results published in academic journals. We also believe that subsequent research based on (and

Attainment, and Consumption levels. Fertility is analyzed in our section 5.2 and our Galiani and Schargrodsy (2004) paper. Household Entry and Exit is evaluated in our attrition analysis. Indeed, our paper takes a lengthy consideration of the issue of household attrition. Child health is the topic of the Galiani and Schargrodsy (2004) paper. Durable consumption is studied in our Table A.5. Transferability and Property Transactions are analyzed in current research (Galiani and Schargrodsy, 2015).

⁴ See, in particular, their replication questions in p. 2 of the replication plan (Winters and Whitney, 2014).

⁵ See <http://econweb.umd.edu/~galiani/data.html> and http://www.utdt.edu/listado_contenidos.php?id_item_menu=4526.

⁶ The replication team presents an incomplete list of our raw variables in Table 2 of their report.

motivated by) published work could be important in advancing scientific knowledge. However, we do not regard the latter as part of a replication study, but rather as endeavors that build on and extend previous results and as such produce new academic research. By implication, we believe that the AA in the second part of the replication report should be evaluated by a peer review process (which would also gauge the suitability of our natural experiment and databases for evaluating their additional hypotheses), and thus we do not assess here the importance or correctness of the results reported.

References:

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