Supplementary Information for

From Recognition to Integration: Indigenous Autonomy, State Authority, and National Identity in the Philippines

Contents

1 Statistics for Observational Analysis

Table A.1 compares *barangays* titled as of 2018 to those that are untitled but are part of the eligible universe designated by the National Commission on Indigenous Peoples (NCIP). The list of "identified" areas with no approved CADT or on-process application was compiled manually by the author based on submissions from each NCIP regional office. Table A.2 shows pre-treatment balance between *barangays* titled prior to 2010 (column 1) and three reference groups: *barangays* in the eligible subset that were not titled prior to 2010 (column 2), *barangays* titled after 2010, and *barangays* in the matched control group.

Table A.3 presents summary statistics for the two outcome variables and titling status for the universe of *barangays* included in the study. This includes all rural *barangays* except for those in the Cordillera Administrative Region, which was granted a degree of autonomy as a majority indigenous region.

Table A.1: Pre-Treatment Covariate Balance Among Eligible Barangays (Titled in 2018 vs. Untitled in 2018)

	Mean Titled	Mean Untitled	T p-val
Indigenous Prop. 2000	0.342	0.167	0.000
Birth Registration 2000	0.775	0.854	0.000
Ethnic Frac. 2000	0.601	0.739	0.000
Area (sq. km)	31.127	11.550	0.000
Elevation Mean	113.883	133.724	0.000
Elevation St. Dev	28.190	40.685	0.000
Slope Mean	3.672	4.880	0.000
Soil Quality Index	2.080	2.123	0.000
Mineral Deposits	0.015	0.007	0.012
Log. NCIP Dist.	10.469	10.400	0.280
Log. Coast Dist.	9.581	8.871	0.000
Log. Road Dist.	3.986	3.469	0.000
Catholic Prop. 2000	0.632	0.745	0.000
Street Pattern 2000	0.351	0.381	0.030
Highway Access 2000	0.684	0.739	0.000
Church 2000	0.906	0.888	0.033
Market 2000	0.242	0.247	0.688
Elementary 2000	0.894	0.798	0.000
Bgy. Health Ctr 2000	0.723	0.652	0.000
Water System 2000	0.492	0.501	0.531
Legibility Δ 1990-2000	-2.462	-2.827	0.640
HS Grad. Δ 1990-2000	0.040	0.065	0.000
Elem Grad. Δ 1990-2000	-0.143	-0.128	0.000
Log Pop Δ 1990-2000	0.184	0.177	0.609
Housing Quality Δ 1990-2000	0.234	0.328	0.000

Table A.2: Pre-Treatment Covariate Balance Among Barangays Titled pre-2010 and Comparison Groups

	Titled pre 2010	Eligible Untitled pre 2010	Titled post 2010	Matched Controls	T p-val: Titled pre vs Eligible	T p-val: Titled post vs Titled pre	T p-val: Titled pre vs. Matched
Indigenous Prop. 2000	0.335	0.178	0.360	0.344	0.000	0.263	0.577
Birth Registration 2000	0.786	0.847	0.740	0.781	0.000	0.004	0.604
Ethnic Frac. 2000	0.598	0.731	0.610	0.596	0.000	0.394	0.867
Area (sq. km)	32.916	12.414	25.951	29.610	0.000	0.004	0.134
Elevation Mean	118.694	131.670	99.460	116.561	0.033	0.027	0.781
Elevation St. Dev	29.060	39.775	25.563	28.958	0.000	0.220	0.964
Slope Mean	3.759	4.791	3.407	3.818	0.000	0.163	0.755
Soil Quality Index	2.066	2.123	2.121	2.062	0.000	0.003	0.775
Mineral Deposits	0.015	0.007	0.016	0.020	0.049	0.843	0.407
Log. NCIP Dist.	10.426	10.412	10.593	10.451	0.833	0.234	0.801
Log. Coast Dist.	9.557	8.918	9.654	9.552	0.000	0.190	0.919
Log. Road Dist.	3.991	3.499	3.961	4.035	0.000	0.901	0.795
Catholic Prop. 2000	0.631	0.739	0.637	0.627	0.000	0.706	0.764
Street Pattern 2000	0.351	0.379	0.349	0.352	0.072	0.948	0.964
Highway Access 2000	0.678	0.737	0.698	0.669	0.000	0.485	0.645
Church 2000	0.903	0.889	0.918	0.897	0.183	0.383	0.666
Market 2000	0.238	0.247	0.253	0.248	0.501	0.564	0.579
Elementary 2000	0.900	0.803	0.876	0.901	0.000	0.230	0.942
Bgy. Health Ctr 2000	0.720	0.657	0.728	0.717	0.000	0.776	0.885
Water System 2000	0.490	0.501	0.495	0.485	0.531	0.887	0.795
Legibility Δ 1990-2000	-2.800	-2.745	-1.173	-2.806	0.951	0.316	0.996
HS Grad. Δ 1990-2000	0.039	0.063	0.043	0.044	0.000	0.405	0.117
Elem Grad. Δ 1990-2000	-0.151	-0.128	-0.118	-0.154	0.000	0.000	0.591
Log Pop Δ 1990-2000	0.189	0.176	0.168	0.198	0.467	0.445	0.662
Housing Quality Δ 1990-2000	0.227	0.324	0.257	0.234	0.000	0.102	0.613

Table A.3: Summary Statistics (Study Barangays)

	Num. Vals	Num. NA	Min	Max	Median	Mean	Std. Dev
Titled Proportion 2010	34312	0	0	1	0.00	0.03	0.14
Titled Binary 2010	34312	0	0	1	0.00	0.03	0.17
Titled Binary 2018	34312	0	0	1	0.00	0.04	0.19
Eligible Universe	34312	0	0	1	0.00	0.21	0.40
Indigenous Pop. 2000	34272	40	0	1	0.00	0.08	0.23
Indigenous Pop. 2010	34309	3	0	1	0.00	0.11	0.26
Birth Registration 2000	34272	40	0	1	0.95	0.83	0.27
Birth Registration 2007	34227	85	0	1	0.98	0.88	0.22
Birth Registration 2010	34309	3	0	1	0.99	0.89	0.22
Birth Registration 2015	34307	5	0	1	0.98	0.92	0.17

2 Identity Robustness Checks

This section presents robustness checks for the indigenous identity results, presented in Table 1 in the main manuscript. Table A.6 replicates the main analyses using the binary land titling measure. Figure A.1 shows results from a bandwidth analysis, repeating the main two-way fixed effects specification (Equation 1 in the manuscript) and successively restricting the analysis to include subsets of eventually titled *barangays* that received titles within shorter windows around the 2010 census, ranging from 2 to 8 years. Within shorter periods of time around the census, the timing of titling is more plausibly independent of community characteristics. The point estimates remain positive and mostly stable across this range, providing support for the idea that there is a causal effect of titling on indigenous self-identification, and that the estimated effects in the main analysis do not simply reflect differences in the timing of when communities applied.

Table A.4 shows results from an alternative design more directly analogous to a regression discontinuity design using the timing of titling relative to the post-treatment census as the the running variable. Specifically, I estimate the following specification:

$$Y_{\Delta i} = \alpha + \tau Titled_i + \gamma CensusDistance_i + \lambda Titled_i * CensusDistance_i + \epsilon_i \quad (1)$$

where $Y_{\Delta i}$ is the change between the pre- and post-periods in the outcome variable for *barangay* i (in this case, the proportion of the population identifying as indigenous), $Titled_i$ is a binary indicator denoting whether *barangay* i received a title prior to the post-treatment census, and $CensusDistance_i$ is the running variable, constructed by subtracting the post-period census year from the titled year (for example, if *barangay* i was titled in 2012, its value for $CensusDistance$ is 2 when the 2010 census is used as the post-period). The

estimand in this specification (τ) is the effect of receiving a title in the same year as the census (i.e. when $CensusDistance_i = 0$) on the change from 2000 in either indigenous identification or birth registration.[1] Columns 1 and 2 show results using two different bandwidths for *CensusDistance*: 4 years and 8 years. For the identity outcome, which is observed only in 2000 and 2010, the estimated coefficients on the *Titled* indicator are positive for both bandwidths, although only statistically significant at the $\alpha = 0.05$ level using the 8-year bandwidth.

In Table A.5, I directly incorporate data on the date of CADT application filing, within the subset of CADTs for which this information is available. Of the 202 CADTs represented in the analysis (excluding CADTs in the CAR region from the total 223) approximately 21% (42 CADTs) are missing filing dates. Unfortunately, data on application filing dates are only available for CADTs that were approved as of 2018 (i.e. successful CADT applications). Within this subset, I estimate first-difference models, regressing change in indigenous self-identification between 2000 and 2010 on titling status in 2010 and controlling for the filing year, both as a continuous variable and as a factor variable (the latter restricting comparisons to CADTs filed within the same year). The results are consistent with the main findings in the paper, with point estimates similar to those in the titled subset.

Tables A.7 and A.8 show results for models estimating the effect of land titling on two different placebo outcomes: total (logged) population and migration (operationalized using the percentage of individuals in a *barangay* who have resided in the same municipality for the past five years). These analyses are meant to address the concern that the change in indigenous identity is due to changes in population composition. While titling does appear to have significant effects in some subsets of the data, they do not point in a consistent direction, suggesting they are unlikely to fully explain the main effects. Table A.9 shows estimates from first-differences specifications including province (columns 1-4) and region (columns 5-8) fixed effects.

Table A.10 replicates the analysis using Conley-type Heteroskedasticity-Autocorrelation-Robust (HAC) standard errors that account for spatial and temporal autocorrelation. With the exception of the titled subset, all coefficients remain statistically significant. Table A.11 restricts the analysis to *barangays* for which there is agreement between two data sources on titling: a map and a list both provided by the National Commission on Indigenous Peoples (NCIP). Within the eligible universe and the titled subset, I include only *barangays* that are considered untitled according to both sources (i.e. that have zero overlap on the map and do not appear on the list) and *barangays* that are coded as titled. In columns 1, 2, 4, and 5,

[1] Note that this estimand may not capture the effects of titling if these effects take time to kick in. In addition, the measurement of the running variable is course relative to the typical study employing a regression discontinuity design (I cannot tell, for example, whether *barangays* titled in 2010 received their title before or after census data were collected).

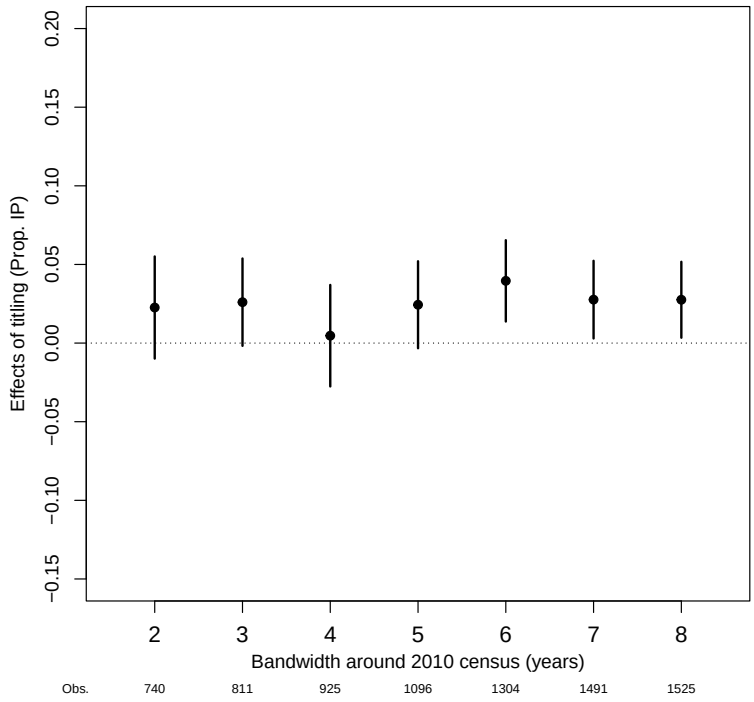


Figure A.1: Land titling and indigenous identification, bandwidth analysis. Figure shows estimated effects of titling on indigenous self-identification within a range of bandwidths around 2010, when the census was conducted. Coefficients come from a two-way fixed effects model, regressing indigenous self-identification on land titling (operationalized as the percentage of a *barangay* covered by a CADT in a given year) with period and unit (*barangay*) fixed effects. Line segments depict 95% confidence intervals with standard errors clustered at the *barangay* level. The bottom axis indicates the number of observations included for each bandwidth.

Table A.4: Land Titling and Indigenous Identity (Quasi-RDD)

	Dependent variable:	
	Indigenous Prop. Δ 2000-2010	
	(1)	(2)
Titled	0.219^{*}	0.089^{**}
	(0.121)	(0.043)
Census Distance	0.065^{*}	0.013^{*}
	(0.035)	(0.007)
Census Distance x Titled	-0.082^{**}	-0.018^{**}
	(0.036)	(0.008)
Constant	-0.150	0.0002
	(0.121)	(0.042)
Bandwidth	4	8
Observations	925	1,525
R^2	0.015	0.009
Adjusted R^2	0.012	0.007
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A.5: Land Titling and Indigenous Identification (First-difference with filing year)

	Dependent variable:			
	Indigenous Prop. Δ 2000-2010			
	(1)	(2)	(3)	(4)
Titled Prop.	0.025^{**}	0.019		
	(0.012)	(0.013)		
Titled (Binary)			0.036^{***}	0.028^{**}
			(0.012)	(0.013)
Filed Year	-0.003		-0.002	
	(0.002)		(0.002)	
Filed Year FE	N	Y	N	Y
Observations	1,442	1,442	1,442	1,442
R^2	0.006	0.034	0.009	0.036
Adjusted R^2	0.004	0.022	0.007	0.024
Note:	*p<0.1; **p<0.05; ***p<0.01			

I code titled *barangays* as those that have more than 50% overlap on the map and that appear on the list. Columns 3 and 6 drop partially titled *barangays* and only include as "treated" *barangays* that have 100% overlap and appear on the list. The results remain similar using these subsets. Finally, Table A.12 shows the main results excluding *barangays* in the former Autonomous Region of Muslim Mindanao (ARMM). The results remain similar to the main results in the paper.

Table A.6: Land Titling (Binary) and Indigenous Identification

	Dependent variable:							
	Indigenous Prop.							
	All Rural	All Rural	Eligible	Eligible	Matched	Matched	Titled	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Binary)	0.080***	0.079***	0.040***	0.040***	0.047***	0.047***	0.031***	0.030***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.011)	(0.012)
Same Municipality		−0.003		−0.049		−0.001		−0.055
		(0.013)		(0.042)		(0.091)		(0.063)
Log. Population		0.013***		0.001		0.004		−0.001
		(0.004)		(0.010)		(0.021)		(0.023)
Mean Age		0.003***		0.003*		−0.001		−0.007*
		(0.0005)		(0.001)		(0.004)		(0.004)
Mean HH Size		−0.001**		0.007		0.003		−0.011
		(0.001)		(0.004)		(0.011)		(0.012)
Observations	34269	34269	8022	8022	2138	2138	1526	1526
R^2	0.914	0.914	0.905	0.905	0.924	0.924	0.926	0.926
Adjusted R^2	0.829	0.829	0.809	0.809	0.849	0.848	0.852	0.852
CRSE at bgy level								

*p<0.1; **p<0.05; ***p<0.01

Table A.7: Land Titling and Population

	Dependent variable:			
	Logged Population			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled Prop.	0.027***	0.021**	−0.005	−0.065***
	(0.009)	(0.010)	(0.013)	(0.016)
Observations	34269	8022	2138	1526
R^2	0.980	0.984	0.981	0.979
Adjusted R^2	0.959	0.968	0.962	0.957
CRSE at bgy level				

*p<0.1; **p<0.05; ***p<0.01

Table A.8: Land Titling and Recent Migration

	Dependent variable:			
	Same Municipality			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled Prop.	0.002	−0.001	−0.004	−0.008**
	(0.002)	(0.002)	(0.003)	(0.004)
Observations	34269	8022	2138	1526
R^2	0.575	0.590	0.602	0.593
Adjusted R^2	0.150	0.180	0.204	0.186
CRSE at bgy level				*p<0.1; **p<0.05; ***p<0.01

Table A.9: Land Titling and Indigenous Identification (First-difference with Province and Region Fixed Effects)

	Dependent variable:							
	Indigenous Prop. Δ 2000-2010							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled Prop.	0.085***	0.065***	0.053***	0.014	0.065***	0.057***	0.052***	0.013
	(0.006)	(0.009)	(0.011)	(0.012)	(0.006)	(0.008)	(0.010)	(0.012)
Prov FE	Y	Y	Y	Y	N	N	N	N
Reg FE	N	N	N	N	Y	Y	Y	Y
Observations	34,269	8,022	2,138	1,526	34,269	8,022	2,138	1,526
Adjusted R^2	0.154	0.197	0.208	0.293	0.051	0.099	0.107	0.124
Note:								*p<0.1; **p<0.05; ***p<0.01

Table A.10: Land Titling and Indigenous Identification (Conley SE)

	Dependent variable:			
	Indigenous Prop.			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled Prop.	0.097***	0.050***	0.048***	0.028
	(0.017)	(0.010)	(0.010)	(0.019)
Observations	34269	8022	2138	1526
R^2	0.914	0.905	0.924	0.926
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A.11: Land Titling and Indigenous Identification - Source Agreement

	Dependent variable:					
	Indigenous Prop.					
	Eligible	Eligible	Eligible	Titled	Titled	Titled
	(1)	(2)	(3)	(4)	(5)	(6)
Titled (Prop.)	0.051***			0.047***		
	(0.009)			(0.015)		
Titled (Binary)		0.042***	0.026**		0.031**	0.012
		(0.007)	(0.010)		(0.014)	(0.018)
Observations	7086	7086	6484	1019	1019	417
Excludes partially titled	N	N	Y	N	N	Y
R^2	0.903	0.903	0.900	0.931	0.930	0.948
Adjusted R^2	0.807	0.807	0.799	0.861	0.861	0.896
CRSE at bgy level						*p<0.1; **p<0.05; ***p<0.01

Table A.12: Land Titling and Indigenous Identification - Excluding ARMM

	Dependent variable:							
	Indigenous Prop.							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Prop.)	0.095***	0.045***	0.048***	0.028**				
	(0.008)	(0.008)	(0.011)	(0.012)				
Titled (Binary)					0.078***	0.036***	0.047***	0.031***
					(0.006)	(0.006)	(0.008)	(0.011)
Observations	31889	7501	2138	1522	31889	7501	2138	1522
R^2	0.910	0.902	0.924	0.926	0.910	0.902	0.924	0.926
Adjusted R^2	0.821	0.803	0.848	0.852	0.821	0.803	0.849	0.852
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

3 Birth Registration Robustness Checks

This section presents robustness checks and additional analyses for the birth registration results, presented in Table 2 in the main manuscript. Table A.13 implements the main results using the binary land titling indicator. While the coefficient remains positive, it shrinks in size and drops below statistical significance in the matched and titled subsets. This may be due in part to the fact that birth registration is measured as an aggregate outcome at the *barangay* level. If increases in birth registration are driven by changes in behavior among indigenous residents living in an Ancestral Domain, effects may not be as apparent in areas where the Ancestral Domain covers a relatively small part of the *barangay*. Another possibility is ceiling effects, given that birth registration cannot increase beyond 100% and is high at baseline in many areas. I replicate the main analyses eliminating all *barangays* with baseline birth registration levels greater than 90%. As shown in Table A.14, the coefficients on the binary version of the titling indicator become significant in the matched and titled subsets when *barangays* with greater than 90% registration at baseline are excluded.

Table A.15 estimates the effects of titling on birth registration in the previous period. This tests whether titling in one period predicts birth registration rates in the previous period, which would suggest a violation of the parallel trends assumption. While I do observe a lagged effect in the full rural sample, I do not see any such effect in the other three subsets. Figure A.2 shows results from the same bandwidth analysis conducted for the indigenous identification variable, comparing change over time in birth registration between 2000 and 2007 for *barangays* that received titles within different bandwidths around the 2007 census (left-hand panel) and change over time between the 2000 and 2010 census for *barangays* titled within different bandwidths around the 2010 census (right-hand panel).[2] While the coefficients fall below statistical significance for some bandwidths using 2010, they remain consistently positive and similar in size.

Table A.16 shows results from the same quasi-regression discontinuity analysis used for the indigenous identity variable, using the timing of titling relative to the post-period census as the running variable. I estimate the RDD specification using both 2007 (column 1) and 2010 (columns 2 and 3) as the post-period census. For the specification using 2010 as the post-period I show results using two different bandwidths for *CensusDistance*: 4 years and 8 years. The coefficients on the titling indicator are statistically significant for both census years using the 4-year bandwidth, although only at the $\alpha = 0.1$ level when 2010 is used as the post-period. However, the coefficient using the 8-year bandwidth is small and statistically indistinguishable from zero. Overall upward trends in birth registration, combined with ceiling effects, may make it difficult

[2]Recall that birth registration is measured in four census years —2000, 2007, 2010, and 2015 —which ethnic identification is only measured in 2000 and 2010. Note also that the first CADTs were issued in 2002, 5 years prior to the 2007. This is the reason the bandwidth range is restricted to 5 years in the analysis using 2007 as the post-period (the left-hand panel).

to detect the effects of titling over a longer period of time (indeed, the coefficient using the 8-year bandwidth is substantially larger when *barangays* with baseline registration rates above 90% are excluded ($\hat{\tau} = 0.049$), although it is not statistically significant, possibly due in part to the resulting decrease in sample size).

Table A.17 shows results from a first-differences specification using change between 2000 and 2010 and controlling for filing year, among the subset of CADTs for which filing year is available. Table A.18 shows estimates from first-difference specifications, regressing change in birth registration between 2000 and 2010 on titling status including province and region fixed effects.

As with the identity results, I re-estimate the effects of titling on birth registration using Conley HAC standard errors. As shown in table A.19, results for all four subsets of the data remain statistically significant. I additionally generate standard errors clustering on both the time and unit dimensions. While Bertrand, Duflo and Mullainathan (2004) generally recommend clustering at the unit level to account for serial correlation in both outcomes and treatment over time, Cameron, Gelbach and Miller (2011) note that when there is geographic-based correlation, within-year cross-unit errors may also be correlated (even when accounting for common-year shocks with year fixed effects), warranting clustering on both time and unit dimensions. Given the small number of years in the dataset, I use a wild cluster bootstrap, following the procedure recommended by MacKinnon, Nielsen and Webb (2019) for the two-dimensional clustering case. Bootstrapped p-values are obtained by comparing a test statistic with (two-way) cluster-robust standard errors to a distribution of test statistics generated by re-sampling from a data generating process imposing the null hypothesis, where the re-sampling maintains the correlation structure in two dimensions. Table A.20 compares p-values obtained using this procedure to p-values from the original analysis for each of the four subsets (these correspond to columns 1, 3, 5, and 7 in Table 2). With the exception of the rural subset, all p-values remain below 0.05.

Finally, Table A.21 shows results restricting the analysis to *barangays* for which there is agreement between the two NCIP data sources and Table A.22 shows the birth registration results excluding *barangays* in the former ARMM. The results are similar to those in the main analysis.

4 Pre-treatment Parallel Trends

Tables A.24, A.25, and A.26 show results from an analysis of differential pre-treatment trends on a number of demographic measures constructed from data collected in the 1990 census: legibility (Whipple Index capturing age heaping), logged population, land tenure status (the percentage of residents in a *barangay*

Table A.13: Land Titling (Binary) and Birth Registration (Two-Way FE)

	Dependent variable:							
	Birth Registration							
	All Rural	All Rural	Eligible	Eligible	Matched	Matched	Titled	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Binary)	0.023***	0.023***	0.017***	0.017***	0.007	0.006	0.004	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Bgy. Health Center		0.003**		0.007***		0.017***		0.021***
		(0.001)		(0.002)		(0.005)		(0.006)
Street Pattern		−0.003***		0.005**		0.003		−0.001
		(0.001)		(0.002)		(0.003)		(0.004)
Highway Access		0.004***		0.001		0.013***		0.008
		(0.001)		(0.002)		(0.004)		(0.005)
Log Population		0.008**		−0.001		−0.015		−0.010
		(0.004)		(0.008)		(0.015)		(0.017)
Mean Age		−0.008***		−0.005***		−0.003		−0.007***
		(0.001)		(0.001)		(0.002)		(0.003)
Mean HH Size		−0.002*		0.013***		0.006		0.003
		(0.001)		(0.002)		(0.004)		(0.004)
Observations	34182	33581	7838	7756	2083	2083	1496	1474
R^2	0.824	0.825	0.818	0.820	0.764	0.766	0.765	0.765
Adjusted R^2	0.765	0.767	0.758	0.760	0.685	0.687	0.686	0.686
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

Table A.14: Land Titling and Birth Registration (Two-Way FE) - Excluding 90% Baseline

	Dependent variable:							
	Birth Registration							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Prop.)	−0.002	0.026***	0.031***	0.029***				
	(0.007)	(0.007)	(0.009)	(0.010)				
Titled (Binary)					−0.004	0.011**	0.017**	0.015*
					(0.006)	(0.005)	(0.007)	(0.008)
Observations	11375	3563	1118	824	11375	3563	1118	824
R^2	0.806	0.814	0.770	0.774	0.806	0.814	0.769	0.774
Adjusted R^2	0.741	0.752	0.693	0.698	0.741	0.752	0.692	0.698
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

Table A.15: Land Titling (Continuous) and Lagged Birth Registration (Two-Way FE)

	Dependent variable:			
	Lagged Birth Registration			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled Prop.	0.025***	0.008	−0.006	−0.010
	(0.007)	(0.007)	(0.009)	(0.009)
Observations	34186	7838	2083	1496
R^2	0.862	0.853	0.795	0.805
Adjusted R^2	0.793	0.780	0.693	0.707
CRSE at bgy level			*p<0.1; **p<0.05; ***p<0.01	

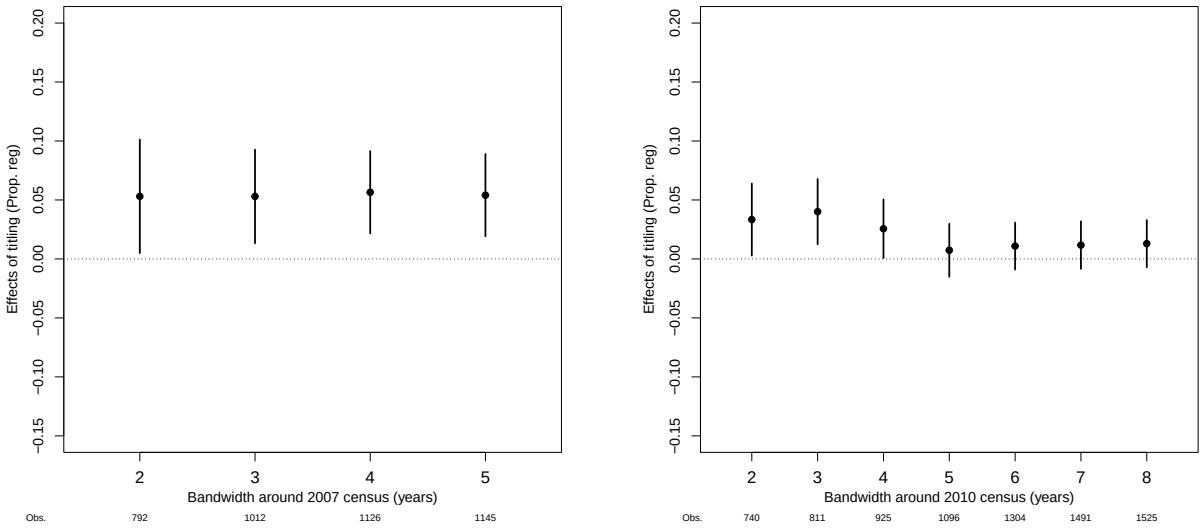


Figure A.2: Land titling and birth registration, bandwidth analysis. The left-hand panel shows estimated effects of titling on indigenous self-identification within a range of bandwidths around 2007, when the first post-treatment census was conducted. The right-hand panel shows results from the same analysis using the 2010 census as the post-treatment period. Coefficients come from a two-way fixed effects model, regressing birth registration on land titling (operationalized as the percentage of a *barangay* covered by a CADT in a given year) with period and unit (*barangay*) fixed effects. Line segments depict 95% confidence intervals with standard errors clustered at the *barangay* level. The bottom axis indicates the number of observations included for each bandwidth.

Table A.16: Land Titling and Birth Registration (Quasi-RDD)

	Dependent variable:		
	Prop. Registered Δ 2000-2007	Prop. Registered Δ 2000-2010	
	(1)	(2)	(3)
Titled (2007)	0.049*		
	(0.026)		
Census Distance (2007)	0.004		
	(0.009)		
Titled x Census Dist. (2007)	−0.001		
	(0.011)		
Titled (2010)		0.172*	0.004
		(0.101)	(0.035)
Census Distance (2010)		0.048*	0.007
		(0.029)	(0.006)
Titled x Census Dist. (2010)		−0.048	−0.013**
		(0.029)	(0.006)
Constant	0.063***	−0.093	0.064*
	(0.017)	(0.100)	(0.034)
Bandwidth	4	4	8
Observations	1,114	925	1,525
R^2	0.008	0.003	0.008
Adjusted R^2	0.005	−0.0001	0.006
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.17: Land Titling and Birth Registration (First-difference with filing year)

	Dependent variable:			
	Registered Prop. Δ 2000-2010			
	(1)	(2)	(3)	(4)
Titled Prop.	0.039***	0.039***		
	(0.009)	(0.010)		
Titled (Binary)			0.019**	0.020**
			(0.009)	(0.010)
Filed Year	−0.003***		−0.003***	
	(0.001)		(0.001)	
Filed Year FE	N	Y	N	Y
Observations	1,442	1,442	1,442	1,442
R^2	0.020	0.081	0.011	0.073
Adjusted R^2	0.018	0.070	0.010	0.062
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A.18: Land Titling and Birth Registration (First-difference with Province and Region Fixed Effects)

	Dependent variable:							
	Birth Reg. Δ 2000-2010							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled Prop.	0.047***	0.041***	0.024**	0.020*	0.048***	0.040***	0.026***	0.027***
	(0.006)	(0.007)	(0.010)	(0.011)	(0.006)	(0.007)	(0.009)	(0.010)
Prov FE	Y	Y	Y	Y	N	N	N	N
Reg FE	N	N	N	N	Y	Y	Y	Y
Observations	34,269	8,022	2,138	1,526	34,269	8,022	2,138	1,526
Adjusted R^2	0.106	0.062	0.077	0.126	0.055	0.026	0.037	0.042
Note:								*p<0.1; **p<0.05; ***p<0.01

Table A.19: Land Titling and Birth Registration (Conley SE)

	Dependent variable:			
	Birth Registration			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled Prop.	0.034**	0.034***	0.023**	0.021**
	(0.015)	(0.011)	(0.010)	(0.009)
Observations	34182	7838	2083	1496
R^2	0.824	0.818	0.765	0.765
Adjusted R^2	0.765	0.758	0.686	0.687
Note:				*p<0.1; **p<0.05; ***p<0.01

	Two-Way Clustering (Bootstrapped)	One-Way Clustering
All Rural	0.0910	0.0000
Eligible	0.0000	0.0000
Matched	0.0110	0.0002
Titled	0.0010	0.0017

Table A.20: P-Values for Birth Registration Analysis

Table A.21: Land Titling and Birth Registration - Source Agreement

	Dependent variable:					
	Birth Registration					
	Eligible	Eligible	Eligible	Titled	Titled	Titled
	(1)	(2)	(3)	(4)	(5)	(6)
Titled (Prop.)	0.033***			0.017**		
	(0.005)			(0.008)		
Titled (Binary)		0.028***	0.036***		0.011	0.010
		(0.005)	(0.008)		(0.007)	(0.011)
Observations	6937	6937	6352	1001	1001	416
Excludes partially titled	N	N	Y	N	N	Y
R^2	0.823	0.823	0.828	0.762	0.762	0.767
Adjusted R^2	0.764	0.764	0.770	0.682	0.682	0.688
CRSE at bgy level						*p<0.1; **p<0.05; ***p<0.01

Table A.22: Land Titling and Birth Registration - Excluding ARMM

	Dependent variable:							
	Birth Registration							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Prop.)	0.042***	0.043***	0.022***	0.019***				
	(0.005)	(0.005)	(0.006)	(0.006)				
Titled (Binary)					0.030***	0.025***	0.007	0.001
					(0.004)	(0.004)	(0.005)	(0.005)
Observations	31899.75	7477.75	2124.25	1518.25	31899.75	7477.75	2124.25	1518.25
R^2	0.760	0.755	0.765	0.765	0.759	0.754	0.764	0.764
Adjusted R^2	0.679	0.672	0.686	0.686	0.679	0.671	0.685	0.685
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

who own the lot they live on), high school graduation rates, and housing material quality (an additive index of the use of strong housing materials for the roof and walls of the house). For each variable, I regress change between 1990 and 2000 on an indicator for titling in 2010. Within the eligible universe, I do find some evidence of differential pre-trends in the quality of housing materials and high school graduation rates. However, I do not observe these differences in the matched or eligible subsets, for which the main results still hold.

As another indirect test of the parallel trends assumption, I compare birth registration rates by age in 2000, prior to the issuance of any Ancestral Domain titles for various subsets of the data. If the practice of registering newborn births varied over time, this should be reflected in differential rates of registration by age. In the absence of data measured pre-treatment this provides a rough proxy for registration over time. For example, if an area was "integrating" more rapidly over time, we might expect to see a steeper slope in the relationship between age and registration in that area relative to others. I test for differential trends by regressing age-specific birth-registration rates (i.e. the proportion of individuals born in a particular year in a *barangay* whose births are registered) on age in 2000 and an interaction between age and *barangay* titling status in 2010. As shown in Table A.23, I do not observe significant interactions between age and 2010 titling status in the matched, eligible, or titled subsets.

Table A.23: Pre-Treatment Birth Registration Rates by Age

	Dependent variable:		
	Birth Registration in 2000		
	Eligible	Matched	Titled
	(1)	(2)	(3)
Age	-0.001^{***}	-0.001^{***}	-0.001^{***}
	(0.00004)	(0.0001)	(0.0002)
Titled 2010 (Binary)	-0.031^{***}	-0.002	0.027^{*}
	(0.008)	(0.011)	(0.016)
Age x Titled	-0.0001	-0.0001	0.0003
	(0.0001)	(0.0002)	(0.0002)
Constant	0.853^{***}	0.826^{***}	0.795^{***}
	(0.003)	(0.007)	(0.014)
Observations (barangays)	5859	1665	1186
CRSE at bgy level		*p<0.1; **p<0.05; ***p<0.01	

5 Additional Alternative Explanations

Tables A.27 shows results from an analysis of heterogenous effects of titling by land value, proxied by the presence of mineral deposits (measured using data from the US Geological Survey) and soil quality. I do not find evidence that the effects of titling on indigenous identity are greater in areas with more valuable land, weighing against the idea that the increase in indigenous self-identification reflects individuals in titled areas

Table A.24: Land Titling and 1990-2000 Demographic Trends (Eligible Universe)

	Dependent variable:				
	Legibility	Logged Pop.	Land Tenure	HS Grad	Housing Quality
	(1)	(2)	(3)	(4)	(5)
Titled in 2010	−1.030	0.0004	−0.005	−0.020***	−0.108***
	(0.925)	(0.012)	(0.009)	(0.003)	(0.010)
Constant	−1.634***	0.181***	0.100***	0.060***	0.340***
	(0.349)	(0.005)	(0.003)	(0.001)	(0.004)
Observations	7,963	7,963	7,958	7,962	7,958
R^2	0.0002	0.00000	0.00004	0.007	0.014
Adjusted R^2	0.00003	−0.0001	−0.0001	0.007	0.014
Note:					*p<0.1; **p<0.05; ***p<0.01

Table A.25: Land Titling and 1990-2000 Demographic Trends (Matched)

	Dependent variable:				
	Legibility	Logged Pop.	Land Tenure	HS Grad	Housing Quality
	(1)	(2)	(3)	(4)	(5)
Titled in 2010	0.006	−0.009	0.013	−0.005	−0.007
	(1.167)	(0.020)	(0.012)	(0.003)	(0.015)
Constant	−2.806***	0.198***	0.078***	0.044***	0.234***
	(0.825)	(0.014)	(0.009)	(0.002)	(0.010)
Observations	2,138	2,138	2,138	2,138	2,138
R^2	0.000	0.0001	0.0005	0.001	0.0001
Adjusted R^2	−0.0005	−0.0004	0.00002	0.001	−0.0003
Note:					*p<0.1; **p<0.05; ***p<0.01

Table A.26: Land Titling and 1990-2000 Demographic Trends (Titled)

	Dependent variable:				
	Legibility	Logged Pop.	Land Tenure	HS Grad	Housing Quality
	(1)	(2)	(3)	(4)	(5)
Titled in 2010	−1.154	0.015	−0.012	−0.003	−0.025
	(1.624)	(0.031)	(0.017)	(0.004)	(0.020)
Constant	−1.510	0.167***	0.106***	0.044***	0.257***
	(1.409)	(0.027)	(0.014)	(0.004)	(0.017)
Observations	1,509	1,509	1,509	1,509	1,509
R^2	0.0003	0.0002	0.0003	0.0004	0.001
Adjusted R^2	−0.0003	−0.001	−0.0003	−0.0002	0.0004
Note:					*p<0.1; **p<0.05; ***p<0.01

identifying as indigenous for the purpose of obtaining private benefits associated with the communal title.

Figure A.3 shows estimated effects of titling on the presence of various *barangay*-level facilities measured in the census: *barangay* health centers, street patterns, highway access roads, elementary schools, water systems, and an index of these five facility types. Results from these analyses using the matched, eligible, and titled subsets are depicted in the plots in Figure A.3. While there do not appear to be any effects of the CADT on most of these, there is some evidence that receiving a CADT increases the probability of having a highway access road to the *barangay* and of having a *barangay* health center (note however that the effects are not statistically significant in the titled subset, for which the main results do hold). I conduct two additional analyses to understand the extent to which these increases drive the observed effects on birth registration. First, I examine the effects of titling on the registration of *new* births in a given year (Table A.28) to understand whether the effect can be explained by greater access to pre-natal or neo-natal healthcare leading to more births in government hospitals. More specifically, I construct a "pseudo panel" from the 2015 census data, estimating the percentage of registered births among individuals born in a given year within a *barangay* as a function of the *barangay*'s titling status in that year. If the increase in birth registration is driven entirely by new births in newly-available health facilities (or births in other government-run health facilities following pre-natal care in newly-available local health facilities), we would expect a positive effect. I do not find evidence that titling is associated with a differential increase in birth registration among newborns, suggesting that this is not the case. Second, to understand whether the effect is driven by a simultaneous extension of the state into titled areas, I restrict the analysis to *barangays* that

were fully serviced in terms of highway access and health services pre-treatment (Table A.29). The main results hold in this subset.

Table A.30 shows results from an analysis of the effects of titling on the influence of the New People's Army (NPA), the armed wing of the Communist Party of the Philippines. The NPA operates throughout the country, primarily in remote areas. Influence is measured using data from military intelligence reports produced by the Armed Forces of the Philippines (AFP) (Rubin 2020). This is a binary measure which takes a "1" if a *barangay* is considered to be "influenced" by the NPA, meaning that there is some degree of participation in organizations affiliated with the Communist Party of the Philippines (CPP) and the local NPA-affiliated militia is able to conduct planned attacks from the area. Data on this outcome is only available for four years (2011-2014), a period during which only 76 *barangays* changed titling status. The results should therefore be interpreted with caution. I do not find any evidence that titling leads to a reduction in NPA influence.

Table A.27: Land Titling (Continuous) and Indigenous Identification - Land Value Interactions

	Dependent variable:							
	Indigenous Prop.							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled Prop. x Mineral Dep	0.012	0.005	−0.017	0.005				
	(0.075)	(0.062)	(0.064)	(0.063)				
Titled Prop. x Soil Quality					0.021	0.002	0.017	0.004
					(0.033)	(0.032)	(0.034)	(0.033)
Titled Prop.	0.099***	0.051***	0.048***	0.032**	0.056	0.047	0.014	0.025
	(0.008)	(0.009)	(0.011)	(0.013)	(0.068)	(0.068)	(0.071)	(0.070)
Observations	32945	7794	2138	1497	32945	7794	2138	1497
R^2	0.909	0.900	0.924	0.925	0.909	0.900	0.924	0.925
Adjusted R^2	0.819	0.800	0.848	0.850	0.819	0.800	0.848	0.850
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

6 Legibility Outcome

Legibility is a measure of census data quality originally proposed by Lee and Zhang (2016). This measure builds explicitly on the insight of Scott (1998), that information gathering and the standardization of information about the population represent core activities of states. Specifically, it captures the accuracy of age reporting data and the extent of "heaping" around ages ending in 0 and 5. Since the actual distribution of ages in the population is unlikely to spike at these points, deviations from a smooth distribution can be

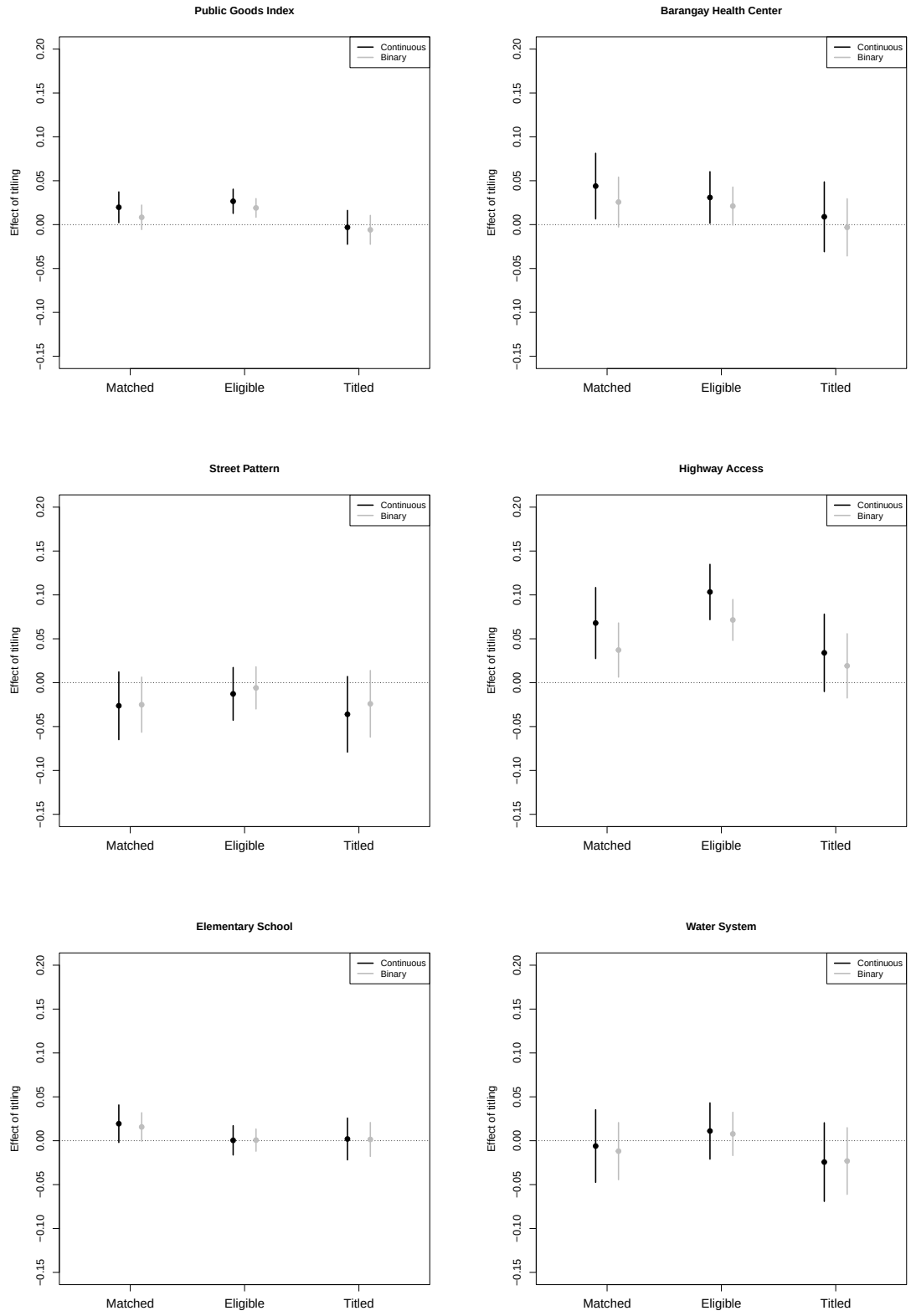


Figure A.3: Estimated effects of titling on *barangay*-level public facilities. Plots show coefficients from a two-way fixed effects model. Outcomes are binary indicators capturing the presence of a facility in a *barangay* and are measured for four census waves: 2000, 2007, 2010, and 2015. The upper left panel shows results using an equally-weighted mean index of the five facility types.

Table A.28: Land Titling and Birth Registration by Birth Year (Pseudo-panel)

	Dependent variable:			
	Birth Registration by Birth Year			
	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)
Titled (Prop.)	−0.020***	−0.009***	−0.002	−0.0004
	(0.002)	(0.002)	(0.003)	(0.003)
Observations	33879	7953	2125	1521
R^2	0.875	0.864	0.814	0.809
Adjusted R^2	0.866	0.855	0.802	0.797
CRSE at bgy level				*p<0.1; **p<0.05; ***p<0.01

Table A.29: Land Titling and Birth Registration Among Accessible Barangays, 2007-2015

	Dependent variable:	
	Birth Registration	
	(1)	(2)
Titled (Prop.)	0.030***	
	(0.009)	
Titled (Binary)		0.019***
		(0.006)
Observations	7,833	7,833
R^2	0.839	0.839
Adjusted R^2	0.758	0.758
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A.30: Land Titling and NPA Influence

	Dependent variable:							
	NPA Influence							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Prop.)	0.043	0.039	0.048	0.041**				
	(0.029)	(0.030)	(0.032)	(0.019)				
Titled (Binary)					0.015	0.003	0.002	0.007
					(0.028)	(0.022)	(0.010)	(0.022)
Observations	34312	8051	2138	1531	34312	8051	2138	1531
R^2	0.719	0.743	0.779	0.791	0.719	0.743	0.779	0.791
Adjusted R^2	0.625	0.657	0.706	0.722	0.625	0.657	0.706	0.721
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

interpreted as errors in data collection. This inaccuracy could occur for two reasons: 1) a lack of awareness among the population about their exact ages and 2) the inability of census enumerators to reach or gather data from the population. Both scenarios represent a lack of interaction between the state and society, and potentially a willingness to be measured and provide information to the state.[3] I operationalize *legibility* at the *barangay* level using a Whipple Index, calculating the percentage of the population with recorded ages ending in 5 or 0 and determining how much this deviates from the expected 20%. The index ranges from 0, which represents no heaping, to 500, which means that all reported ages end in 5 or 0. I calculate this using data from five census waves: 1990, 2000, 2007, 2010, and 2015.

Table A.31 shows the results from this analysis using both CADT measures. The coefficients are negative and, with one exception, statistically significant, indicating that titling is associated with greater legibility (i.e. less distortion in census data). As shown in Tables A.24, A.25, and A.26 above, pre-treatment trends in legibility do not differ significantly between titled and untitled areas in the eligible, matched, or titled subsets.

Table A.31: Land Titling and Legibility (Two-Way FE)

	Dependent variable:							
	Legibility (Whipple Index)							
	All Rural	Eligible	Matched	Titled	All Rural	Eligible	Matched	Titled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Titled (Prop.)	−6.609***	−5.221***	−2.841***	−3.925***				
	(0.664)	(0.714)	(0.818)	(0.962)				
Titled (Binary)					−4.649***	−2.918***	−0.874	−1.679**
					(0.542)	(0.525)	(0.631)	(0.755)
Observations	33846	7779	2083	1479	33846	7779	2083	1479
R^2	0.513	0.518	0.511	0.526	0.513	0.518	0.510	0.525
Adjusted R^2	0.391	0.398	0.388	0.407	0.391	0.397	0.388	0.405
CRSE at bgy level								*p<0.1; **p<0.05; ***p<0.01

7 Survey Information

7.1 Ethics approval and informed consent

The survey was conducted in partnership with Legal Network for Truthful Elections (LENTE), a Philippines-based NGO, between March and October 2018. The study received exempt approval from the Committee on the Use of Humans as Experimental Subjects (COUHES) at the Massachusetts Institute of Technology

[3] Lee and Zhang (2016) demonstrate that this measure of accurately captures a state's "presence on the ground" and is associated with other measures of state capabilities, such as tax contributions and public goods provision.

under Protocol # 1709098195. In addition, the research team sought permission from the regional offices of the National Commission on Indigenous Peoples. At the community level, locally-recruited enumerators met with village leaders to seek permission to conduct the survey. Individual respondents were selected through a random-walk procedure in which enumerators began in a central location in the village and walked in different directions, skipping a specified number of houses. In order to be eligible for participation in the survey, respondents had to self-identify as a member of an indigenous community, be over the age of 18, and could not live in the same household as a member of tribal leadership (leaders were administered a separate survey).

Respondents were asked to give verbal consent after the enumerator read a prepared script and offered to answer questions. The decision to require verbal rather than written consent was made in consultation with LENTE and local community contacts. This was deemed to be necessary given the particular experience of indigenous communities in the Philippines. Past experiences with extractive corporations and other actors, in which community members have been asked to sign paperwork they did not fully understand and ended up signing over legal ownership of their land, have rightly made people in many indigenous communities suspicious of signing documents. In addition, a non-trivial number of subjects were illiterate or partially literate, which means they would have to trust that the enumerator is accurately conveying the information on the document they are being asked to sign. Requiring documentation of consent in this case will make it much more difficult for surveyors to enter communities and successfully administer the survey. This waiver of written consent was approved by MIT COUHES. Upon completion of the survey, enumerators left respondents with information about the study, written in Tagalog, including contact information should they have any questions. Respondents were paid 50 Philippine pesos (approximately 1 USD) as a token of their participation at the end of the survey. They then had the option to contribute a portion of these funds to a community-level public good, as part of another component of the study not included in this paper. Respondents were not informed ahead of time that they would be compensated, to avoid coercing participation. The compensation amount and decision not to inform respondents about compensation ahead of time were determined in consultation with LENTE and other local collaborators, and also approved by MIT COUHES. Surveys were conducted face-to-face on tablets using SurveyCTO software. To ensure confidentiality of subjects during survey data collection, names, phone numbers, and village names will be encrypted on the tablets, using the encryption capabilities of SurveyCTO's software.

7.2 Survey experiment details and additional results

Figure A.4 shows an English translation of the flyer used in the priming treatment. Figure A.5 shows the showcards used to measure the outcome variable in the survey experiment. Enumerators were instructed to place the four cards down in a randomly-assigned order and ask the respondent to rearrange them in order of importance to the respondent as parts of his or her personal identity.

Table A.32 shows results from a balance test, regressing an indicator for the priming treatment on several individual-level covariates. In addition, per Lin and Green (2016), I conduct a permutation test for covariate imbalance, comparing a heteroskedasticity-robust Wald statistic from this regression to a distribution generated through simulated re-assignment of treatment and fail to reject the null hypothesis of no covariate imbalance across treatment arms. Figure A.6 shows estimated effects of the priming treatment on a battery of questions about attitudes toward *barangay* and municipal government officials, as well as an index of these questions. Table A.33 shows heterogeneous effects of the priming treatment by actual titling status within the survey sample.

Table A.32: Priming Experiment Covariate Balance

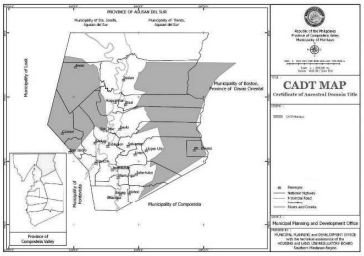
	Dependent variable:
	IPRA Prime
Female	0.040
	(0.048)
Completed Elementary	−0.066
	(0.056)
Completed HS	0.056
	(0.075)
Catholic	−0.085
	(0.053)
Evangelical	−0.125**
	(0.060)
Born in Bgy	−0.017
	(0.055)
Age	−0.002
	(0.001)
Constant	0.652***
	(0.087)
Observations	476
R^2	0.021
Adjusted R^2	0.006
Note:	*p<0.1; **p<0.05; ***p<0.01

THE IPRA LAW: RECOGNIZING THE RIGHTS OF INDIGENOUS PEOPLES TO PRESERVE THEIR DISTINCTIVE CULTURES

Republic Act No. 8371 or the Indigenous Peoples Rights Act (IPRA) of 1997 is a law enacted by the government of the Philippines to recognize, protect, and promote the rights of indigenous cultural communities/indigenous peoples (IPs/ICCs).

The IPRA Law recognizes several distinctive rights for IPs/ICCs, including:

- **Cultural Integrity**
- **Rights to Ancestral Domains/Ancestral Land**



The IPRA allows IPs/ICCs to apply for a title to their ancestral lands, known as a **Certificate of Ancestral Domain Title (CADT)**. Getting a CADT helps protect IP communities' rights to maintain their distinctive customs, cultural traditions, and identity without interference.

Ifugao people celebrating the end of the rice harvest

The IPRA law defines IPs/ICCs in part as groups of people who "…[share] common bonds of language, customs, traditions and other **distinctive cultural traits** or who have, through **resistance to political, social and cultural inroads of colonization**, non-indigenous religions and cultures, become **historically differentiated** from the majority of Filipinos…."

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Indigenous youth from the B'laan tribe performing a traditional dance in Sarangi

Figure A.4: Recognition Prime (English translation). Flyers were introduced using the following language read by survey enumerators: "This is not just a survey, but we are also trying to education the community about the basic concepts of the IPRA law and the legal rights of IPs. Here is some information about the IPRA Law. I don't have enough printed flyers to leave one with everyone, but we hope you will share this information with others in the community. I am going to ask you a few questions about it later to make sure you remember the information, so please pay attention."

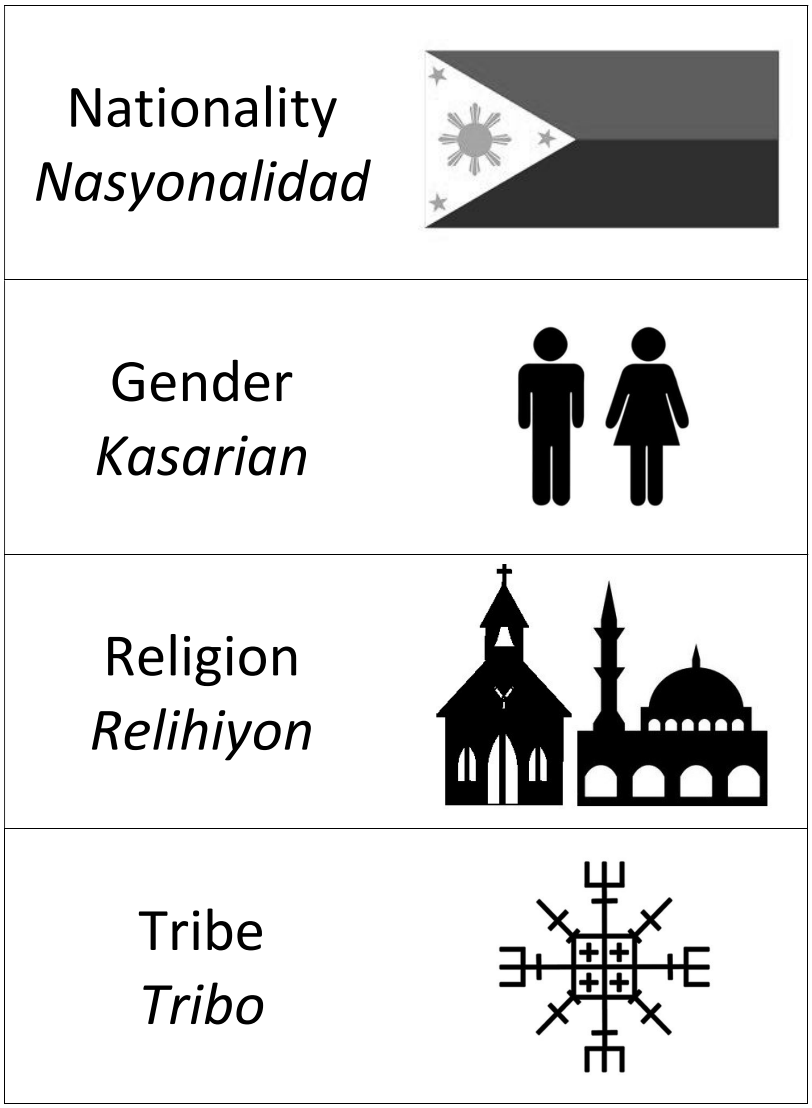

Figure A.5: Identity showcards. Showcards were introduced by enumerators as follows: "People may think of themselves as part of many different groups. Based on your answers you have given so far, here are some groups in which you might consider yourself to be a member." Then the groups were listed in the assigned order with reference to the respondent's previous answers. For example, if the respondent indicated earlier in the survey that they were a member of an Evangelical church, the enumerator would say "Your religion: Evangelical" while placing the religion card in front of the respondent. After placing all four cards, the enumerator asked "Of these four groups, which do you consider the most important to you? Which of these do you consider the second most important?," etc.

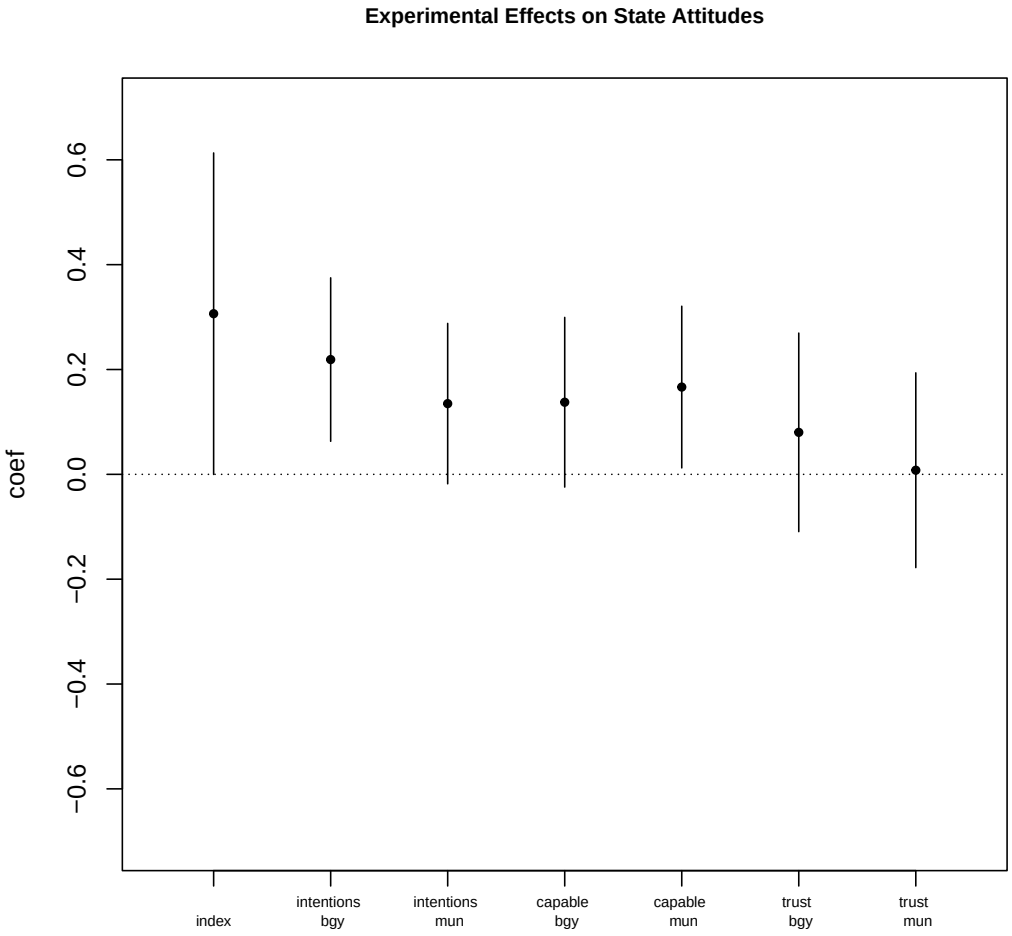

Figure A.6: Effects of priming on individual components of state attitudes index (intentions, capability, and trustworthiness of *barangay* and municipal government).

Table A.33: Recognition Prime and Tribal and National Identity, by Titling Status

	Dependent variable:	
	Tribe Top	Nationality Top
	(1)	(2)
IPRA Prime	−0.076	0.070
	(0.056)	(0.056)
Titled (Binary)	0.056	−0.112
	(0.068)	(0.068)
IPRA Prime x Titled	−0.014	0.063
	(0.095)	(0.095)
Covariate Adjustment	Y	Y
Observations	476	476
R^2	0.027	0.031
Adjusted R^2	0.008	0.013
Note:	*p<0.1; **p<0.05; ***p<0.01	

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