Appendix to

Environmental Effects of Development Programs: Experimental Evidence from West African Dryland Forests

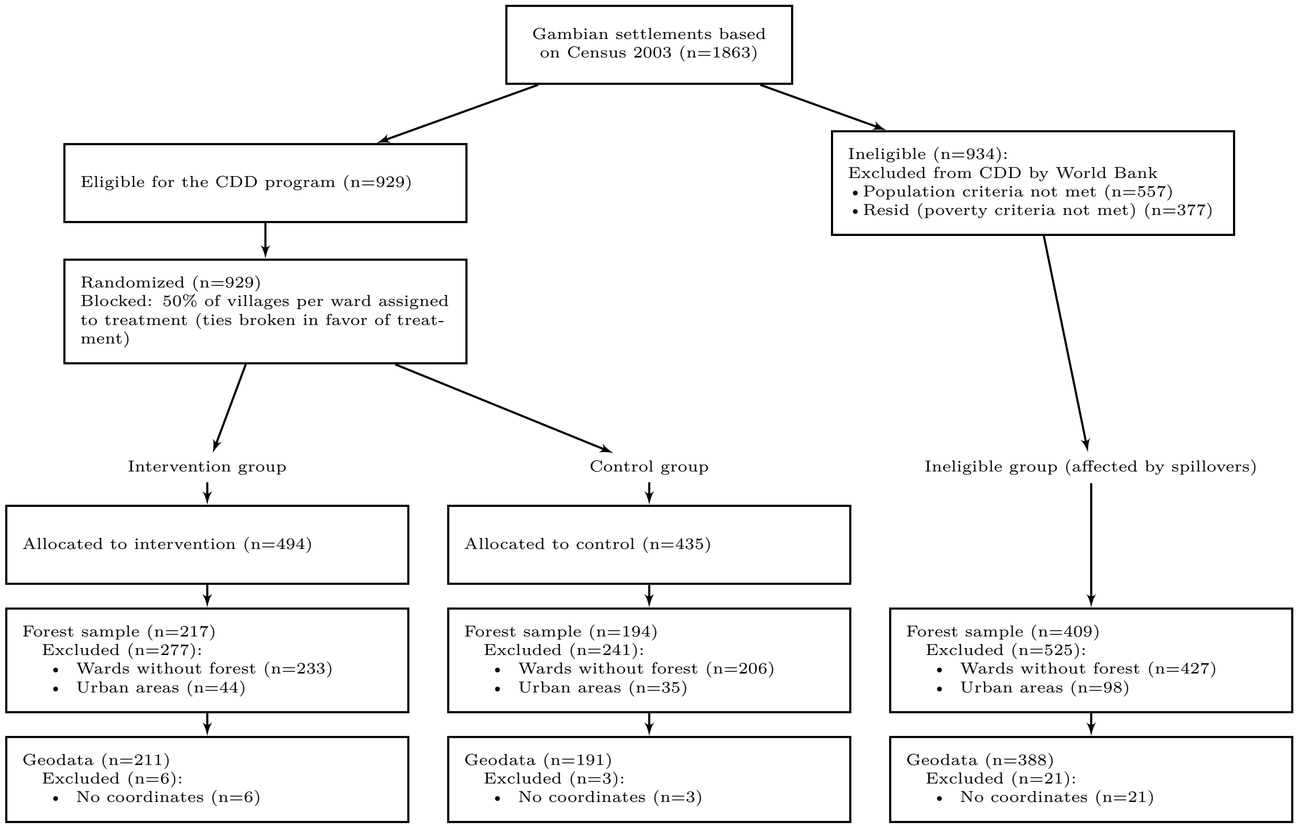
Simon Heß[a], Dany Jaimovich[b], and Matthias Schündeln[a]

[a]Goethe University Frankfurt
[b]Universidad de Talca

Full paper at: https://doi.org/10.1016/j.jdeveco.2021.102737

A Additional Figures and Tables

Figure A.1: Sample Composition Flow Chart



Notes: The original treatment randomization was blocked at the ward-level, and so is the forest sample restriction.

Table A.1: Robustness: Difference-in-Differences – Main Results Estimated with OLS

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.092**	**0.164**	**0.052**
	(0.09)*	(0.01)**	(0.44)
	[0.08]*	[0.00]***	[0.53]
implementation (2008-10) × treatment	-0.003	0.024	-0.009
	(0.95)	(0.70)	(0.89)
	[0.94]	[0.63]	[0.88]
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	7236	7236	7236
villages	402	402	402
post-program (2011-18) control mean annual loss (ha.)	0.208	6.350	0.526

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level (villages are the unit of treatment assignment). Standard errors underlying the p-values in square brackets allow for clustering at the ward-level (wards were the strata for the treatment randomization). Our sample comprises 36 wards. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. Ward-year and village fixed effects are included in all specifications.

Table A.2: Difference-in-Differences – Estimate in an Extended Sample, Including Wards with Baseline Forest Cover above the 75^{th} percentile

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.125**	**0.192**	**0.151**
	(0.14)	(0.02)**	(0.13)
implementation (2008-10) × treatment	-0.035	0.011	0.041
	(0.67)	(0.84)	(0.64)
observations	3672	3672	3672
villages	204	204	204
implementation-phase (2008-10) control mean annual loss (ha.)	0.623	11.05	1.086
post-program (2011-18) control mean annual loss (ha.)	0.21	6.35	0.53

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated to each project class. Project subclasses are built as listed in Table C.13. Ward-year and village fixed effects are included in all specifications. A small group of projects (3% of the total budget in all sample villages) are omitted, because they could not be matched to the administrative record on which base the classification.

Table A.3: Difference-in-Differences – Estimate in an Extended Sample, Including Wards with Baseline Forest Cover above the 25th percentile

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.043**	**0.077**	**0.011**
	(0.27)	(0.10)	(0.83)
implementation (2008-10) × treatment	-0.034	0.010	-0.019
	(0.41)	(0.82)	(0.70)
observations	11052	11052	11052
villages	614	614	614
implementation-phase (2008-10) control mean annual loss (ha.)	0.623	11.05	1.086
post-program (2011-18) control mean annual loss (ha.)	0.21	6.35	0.53

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated to each project class. Project subclasses are built as listed in Table C.13. Ward-year and village fixed effects are included in all specifications. A small group of projects (3% of the total budget in all sample villages) are omitted, because they could not be matched to the administrative record on which base the classification.

Table A.4: Difference-in-Differences – Estimate in an Extended Sample, Including All Wards

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.027**	**0.062**	**0.002**
	(0.37)	(0.10)	(0.95)
implementation (2008-10) × treatment	-0.036	-0.010	-0.027
	(0.25)	(0.79)	(0.48)
observations	14760	14760	14760
villages	820	820	820
implementation-phase (2008-10) control mean annual loss (ha.)	0.623	11.05	1.086
post-program (2011-18) control mean annual loss (ha.)	0.21	6.35	0.53

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated to each project class. Project subclasses are built as listed in Table C.13. Ward-year and village fixed effects are included in all specifications. A small group of projects (3% of the total budget in all sample villages) are omitted, because they could not be matched to the administrative record on which base the classification.

Table A.5: Difference-in-Differences: Forest Loss Using the Inverse Hyperbolic Sine Transformation Instead of the Logarithms

	(1) $\sinh^{-1}(\text{loss}^{1km})$	(2) $\sinh^{-1}(\text{loss}^{5km})$	(3) $\sinh^{-1}(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.047**	**0.119**	**0.043**
	(0.05)*	(0.01)**	(0.23)
implementation (2008-10) × treatment	-0.011	0.018	0.006
	(0.68)	(0.66)	(0.86)
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	7236	7236	7236
villages	402	402	402
post-program (2011-18) control mean annual loss (ha.)	0.208	6.350	0.526

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the inverse hyperbolic sine of the area of forest loss per year. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A.6: Difference-in-Differences: Forest Loss Using Levels Instead of Logarithms

	(1) loss^{1km}	(2) loss^{5km}	(3) loss^{poly}
post–program (2011-18) × treatment	**0.169**	**1.684**	**0.288**
	(0.03)**	(0.02)**	(0.08)*
implementation (2008-10) × treatment	0.002	0.610	0.166
	(0.98)	(0.40)	(0.35)
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	7236	7236	7236
villages	402	402	402
post-program (2011-18) control mean annual loss (ha.)	0.208	6.350	0.526

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the area of forest loss in hectares per year. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A.7: Difference-in-Differences: Alternative Inference Methods for Coefficient Estimates from Equation (1) and Table A.1

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
post–program (2011-18) × treatment	**0.092**	**0.164**	**0.052**
	p^{vill}=0.09*	p^{vill}=0.01**	p^{vill}=0.44
	p^{ward}=0.08*	p^{ward}=0.00***	p^{ward}=0.53
	p^{ri}=0.10	p^{ri}=0.01**	p^{ri}=0.44
	$p^{\text{Conley}_{10\,\text{km}}}$=0.04**	$p^{\text{Conley}_{10\,\text{km}}}$=0.00***	$p^{\text{Conley}_{10\,\text{km}}}$=0.38
	p^{bs-v}=0.01**	p^{bs-v}=0.01**	p^{bs-v}=0.36
	p^{bs-w}=0.08*	p^{bs-w}=0.00***	p^{bs-w}=0.53
	p^{wbs-w}=0.09*	p^{wbs-w}=0.00***	p^{wbs-w}=0.55
implementation (2008-10) × treatment	-0.003	0.024	-0.009
	p^{vill}=0.95	p^{vill}=0.70	p^{vill}=0.89
	p^{ward}=0.94	p^{ward}=0.63	p^{ward}=0.88
	p^{ri}=0.95	p^{ri}=0.71	p^{ri}=0.89
	$p^{\text{Conley}_{10\,\text{km}}}$=0.95	$p^{\text{Conley}_{10\,\text{km}}}$=0.68	$p^{\text{Conley}_{10\,\text{km}}}$=0.89
	p^{bs-v}=0.95	p^{bs-v}=0.72	p^{bs-v}=0.89
	p^{bs-w}=0.94	p^{bs-w}=0.62	p^{bs-w}=0.88
	p^{wbs-w}=0.94	p^{wbs-w}=0.63	p^{wbs-w}=0.89
observations	7236	7236	7236
villages	402	402	402

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table shows p-values based on several alternative inference methods. p^{vill} is based on village-level cluster robust standard errors. Unless specifically indicated otherwise, this is our method of choice. Among all alternative methods this empirically yields the most conservative p-values, as can be seen from the results above. p^{ward} is based on ward-level cluster robust standard errors (our sample comprises 36 wards). p^{ri} is based on randomization inference using the treatment effect estimate as test-statistic, as described in Heß (2017). $p^{\text{Conley}_{10\,\text{km}}}$ is based on Conley inference allowing for spatial and temporal correlation of the model error. In particular we allow for spatial correlation within 10 km and impose no restriction on temporal auto-correlation of the error term. p^{bs-v} is based on standard cluster-bootstrap, resampling villages, stratified by wards. p^{bs-w} is based on standard cluster-bootstrap, resampling entire wards. p^{wbs-w} is based on the wild bootstrap, resampling wards. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A.8: Spillover Effects: Restricting the Sample to Eligible Villages

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
post–program (2011-18) × treatment	**0.114**	**0.198**	**0.082**
	(0.02)**	(0.00)***	(0.18)
	[0.07]*	[0.01]**	[0.27]
post–program (2011-18) × $N^{\mathbf{2\,km}}_{\mathbf{Treat}}$	**0.066**	**0.121**	**0.053**
	(0.18)	(0.06)*	(0.34)
	[0.28]	[0.15]	[0.45]
post–program (2011-18) × $N^{\mathbf{2\,km-5\,km}}_{\mathbf{Treat}}$	**0.080**	**0.124**	**0.113**
	(0.01)**	(0.01)**	(0.00)***
	[0.05]*	[0.03]**	[0.01]**
implementation (2008-10) × treatment	0.018	0.025	-0.002
	(0.77)	(0.70)	(0.97)
	[0.79]	[0.77]	[0.98]
implementation (2008-10) × $N^{2\,\text{km}}_{\text{Treat}}$	0.081	-0.010	0.093
	(0.16)	(0.90)	(0.09)*
	[0.28]	[0.94]	[0.22]
implementation (2008-10) × $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat}}$	0.078	0.019	0.028
	(0.03)**	(0.73)	(0.47)
	[0.06]*	[0.80]	[0.53]
observations	7236	7236	7236
villages	402	402	402
mean $N^{2\,\text{km}}_{\text{Treat}}$	0.852	0.852	0.852
mean $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat}}$	2.691	2.691	2.691
total post-program loss in all villages (ha.)	785.4	21327.3	1774.9
estimated total post-program loss (ha.) due to CDD	247.7	9424.4	481.6

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parenthesis are based on standard error estimates allowing for spatial correlation and auto-correlation of the model error. We implement Conley inference, taking 10 km as spatial cutoff, while leaving the temporal autocorrelation unrestricted. We chose 10 km because two villages that are farther apart than 10 km cannot have a common third village within their 5 km perimeter, which implies that neither of the three variables relating to treatment can be spatially correlated beyond this distance. p-values in square brackets are based on randomization inference as described in Footnote 9 and in Heß (2017). The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Estimated coefficients are based on Equation (2). Ward-year and village fixed effects are included in all specifications.

Table A.9: Heterogeneous Effects by Pre-Treatment Variables for 5 km Buffers

village-level split:	distance to road	population	poverty	ELF
	(1)	(2)	(3)	(4)
	log(loss^{5km})	log(loss^{5km})	log(loss^{5km})	log(loss^{5km})
low × **post–program (2011-18)** × **treatment**	**0.063**	**0.098**	**0.174**	**0.089**
	(0.41)	(0.22)	(0.06)*	(0.32)
high × **post–program (2011-18)** × **treatment**	**0.264**	**0.215**	**0.157**	**0.232**
	(0.01)**	(0.02)**	(0.07)*	(0.01)**
low × implementation (2008-10) × treatment	-0.015	-0.073	-0.032	-0.035
	(0.87)	(0.48)	(0.64)	(0.71)
high × implementation (2008-10) × treatment	0.077	0.124	0.085	0.074
	(0.38)	(0.10)	(0.42)	(0.38)
split indicator×period	✓	✓	✓	✓
observations	7236	7236	7236	7236
villages	402	402	402	402
p-value $\beta^{\text{high}} = \beta^{\text{low}}$ (post-program coefficients identical)	0.018	0.031	0.027	0.017

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the difference-in-differences interaction terms with binary indicators dividing the sample according to the median of each pre-treatment village-level variable. The coefficients can thus be interpreted as treatment effect estimates for the sub-group defined by the sample split. Ward-year and village fixed effects are included in all specifications.

Table A.10: Heterogeneous Effects by Pre-Treatment Variables for Polygons

village-level split:	distance to road	population	poverty	ELF
	(1)	(2)	(3)	(4)
	log(loss^{poly})	log(loss^{poly})	log(loss^{poly})	log(loss^{poly})
low × **post–program (2011-18)** × **treatment**	**-0.111**	**0.038**	**-0.041**	**-0.097**
	(0.23)	(0.68)	(0.67)	(0.30)
high × **post–program (2011-18)** × **treatment**	**0.212**	**0.063**	**0.147**	**0.201**
	(0.04)**	(0.54)	(0.12)	(0.05)*
low × implementation (2008-10) × treatment	-0.067	-0.022	-0.137	-0.093
	(0.48)	(0.83)	(0.14)	(0.34)
high × implementation (2008-10) × treatment	0.055	0.007	0.122	0.067
	(0.55)	(0.94)	(0.19)	(0.46)
split indicator×period	✓	✓	✓	✓
observations	7236	7236	7236	7236
villages	402	402	402	402
p-value $\beta^{\text{high}} = \beta^{\text{low}}$ (post-program coefficients identical)	0.068	0.753	0.280	0.083

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the difference-in-differences interaction terms with binary indicators dividing the sample according to the median of each pre-treatment village-level variable. The coefficients can thus be interpreted as treatment effect estimates for the sub-group defined by the sample split. Ward-year and village fixed effects are included in all specifications.

Table A.11: Heterogeneous Effects by Distance to Road, Controlling for Population and Baseline Forest

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
post–program (2011-18) × treatment	**-0.074**	**-0.012**	**-0.156**
	(0.48)	(0.92)	(0.22)
post–program (2011-18) × treatment × high distance to road	**0.260**	**0.183**	**0.318**
	(0.02)**	(0.14)	(0.03)**
post–program (2011-18) × treatment × high population	**0.024**	**0.098**	**0.023**
	(0.83)	(0.43)	(0.87)
post–program (2011-18) × treatment × high baseline forest	**0.046**	**0.054**	**0.057**
	(0.68)	(0.66)	(0.68)
implementation (2008-10) × treatment	-0.017	-0.158	-0.166
	(0.89)	(0.27)	(0.24)
implementation (2008-10) × treatment × high distance to road	0.034	0.074	0.124
	(0.77)	(0.58)	(0.37)
implementation (2008-10) × treatment × high population	0.029	0.167	0.026
	(0.80)	(0.19)	(0.85)
implementation (2008-10) × treatment × high baseline forest	-0.018	0.134	0.170
	(0.88)	(0.28)	(0.22)
split indicators×period	✓	✓	✓
observations	7236	7236	7236
villages	402	402	402

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\mathrm{m} \times 30\,\mathrm{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interactions of the difference-in-differences interaction terms with binary indicators dividing the sample according to the median of each pre-treatment village-level variable. To simplify exposition, this specification does not estimate separate treatment effect for the two sub-group defined by the sample split, unlike the specifications in Tables A.9 to A.10. Instead, the first coefficient can be interpreted as the treatment effect in villages with low road access, low population, and low baseline forest cover. The coefficients of the interaction terms can be interpreted as the differences in treatment effects for the respective subgroup relative to that. Ward-year and village fixed effects are included in all specifications.

Table A.12: Difference-in-Differences Estimates by Village-Level Project Classification

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
post–program (2011-18) × share non-agricultural	0.286	0.024	0.084
	(0.02)**	(0.85)	(0.60)
post–program (2011-18) × share agricultural	0.031	0.216	0.041
	(0.58)	(0.00)***	(0.58)
implementation (2008-10) × share non-agricultural	0.056	-0.150	-0.153
	(0.57)	(0.16)	(0.25)
implementation (2008-10) × share agricultural	-0.033	0.088	0.019
	(0.61)	(0.21)	(0.80)
observations	7236	7236	7236
villages	402	402	402
control mean annual loss (ha.) post-program	0.28	6.89	0.58
p-value $\beta^{\text{agric}} = \beta^{\text{non-agric}}$ (post-program coefficients identical)	0.074	0.010	0.751

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2018. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated to each project class. Project subclasses are built as listed in Table C.13. Ward-year and village fixed effects are included in all specifications. A small group of projects (3% of the total budget in all sample villages) are omitted, because they could not be matched to the administrative record on which base the classification.

B Estimating the Aggregate Treatment Effect in Hectares

To calculate the total program-induced forest loss in hectares we need to take into account that the estimation is based on a log-level model specification. We implement an approach that accounts for the transformed dependent variable and produces an estimate of the total area of forest loss attributable to the CDD.[1] Our estimates using the 1 km buffers indicate that in total over the years 2011-2018, about 26.3 hectares were deforested as the result of the CDD program (Table 1, column 1). As discussed in Appendix C.2, some of the 1 km buffers are overlapping and thus this number cannot be interpreted in isolation but has to be put in relation to the total amount of lost forest summed across these buffers. This comparison suggests $\frac{26.3}{467.0} \approx 5.6\%$ of forest loss within the 1 km buffers of treatment villages in high forest wards is due to the CDD program.

In a level-level specification one would simply multiply the average treatment effect estimate with the number of observed years times the number of treated villages. In a log-linear specification, as used for Table 1, this is not possible due to two reasons: First, because the sum of two logarithms does not equal the logarithm of the sum. This could be solved by computing the loss in hectares for each village separately and summing them up in a second step. The second, more important problem is that the logarithm of the

[1] Another benefit of this method is that it straightforwardly extends model specifications that are non-linear in the covariates, such as the spillover effect specification which we explore in Table 1, columns 5-6, where treatment does not only affect the treated observation itself but also its neighbors.

expected value is not identical to the expected value of the logarithm. Formally, the specification estimates $E[\log(\text{loss}_{vwt})|\text{treatment}_v, v, w, t]$, while the total program-induced forest loss is given by:

$$\sum_v \sum_t \left(\mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v, v, w, t\right] - \mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v = 0, v, w, t\right]\right), \tag{1}$$

and there is no direct correspondence between these two expressions. More precisely, it follows from Jensen's inequality that $\log(\mathbb{E}[\text{loss}]) \geq \mathbb{E}[\log(\text{loss})]$, which implies that plugging in $\exp\left(\mathbb{E}\left[\log(\text{loss}_{vwt})|\text{treatment}_v, v, w, t\right]\right)$ for $\mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v, v, w, t\right]$ would produce an incorrect estimate of the program-induced forest loss. If this bias is stronger for the first expectation in the expression than for the second, the overall effect will be an underestimation of the program-induced forest loss. We thus use the following method to calculate the total program-induced forest loss based on the estimated log-linear specification:

1. For each village-year, we use the estimated model coefficients to obtain fitted values for the logarithmized forest loss, once for the actual treatment assignment and once assuming no village was treated. For each of these fitted values we draw 100 realizations by adding random regression error, bootstrapped from the fitted model. In this bootstrap procedure we draw model residuals for all 18 years grouped by village, so that potential temporal auto-correlation is accounted for, i.e., in each draw we take a series of the 18 regression residuals from one village and add it to the fitted value for another village.

2. These draws of logarithmized forest loss are transformed via the exponential function (or, $\sinh(\cdot)$ when the inverse hyperbolic sine was used instead of the logarithm) to obtain measures for the forest loss in hectares at the village-year level and then averaged separately for the 'actual treatment' variant and the 'no treatment' variant. This yields estimates for the two expected values in Equation (1).

3. These means are summed up over all years 2011-2018, and all villages. Finally, the number reported in Table 1 is the difference between the 'actual treatment' and the 'no treatment' result, as indicated in Equation (1).

For a linear model specification without a transformed dependent variable this procedure yields the same estimate as multiplying the average treatment effect estimate with the number of observed years times the number of treated villages (see Table A.6). For a log-linear model specification this procedure yields estimates that are comparable to making use of the relationship $\mathbb{E}\left[\text{loss}_{vwt}|\cdot\right] = \exp(X'_{vwt}\beta)\exp(\sigma^2/2)$.

C Additional information

While the recipient villages were informed about their treatment status in 2008, the disbursements were made later. The administrative records do not report the exact disbursement dates, but they do report a village's project appraisal date for 90% of treatment

Table C.13: List of Village-Level CDD Projects, Classifications and Descriptive Statistics

project description	classification	subclassification	frequency	median budget share
farm implements & inputs: unspec.	agric	agritool	20	70%
farm implements: planting equip..	"	agritool	39	28%
" : animals	"	agritool	31	47%
" : tools	"	agritool	28	53%
" : tools & animals	"	agritool	29	75%
" : tools & planting equip.	"	agritool	4	75%
" : animals & planting equip.	"	agritool	7	100%
" : tools & power tiller	"	agritool	1	100%
" : tractor	"	tractor	18	100%
" : power tiller	"	tractor	3	78%
ram fattening	"	animals	2	19%
cattle fattening	"	animals	3	28%
small ruminants	"	animals	3	28%
seed store/cereal banking	"	cerbank	17	37%
vegetable gardens	"	garden	17	50%
orchards	"	garden	1	48%
milling machine: coos	"	milmach	37	44%
" : unspec.	"	milmach	15	39%
" : rice	"	milmach	6	52%
" : multipurpose	"	milmach	2	50%
rice cultivation	"	other (agric)	3	68%
access road to rice field	"	other (agric)	1	18%
solar electrification	nonagric	infrastructure	17	97%
schools	"	infrastructure	3	52%
latrines	"	infrastructure	3	24%
feeder road rehab./construction	"	infrastructure	2	22%
consumer shops	"	infrastructure	2	40%
bio-gas	"	infrastructure	1	5%
PHC centre	"	infrastructure	1	98%
waiting sheds	"	infrastructure	2	20%
salt processing center	"	infrastructure	2	50%
market stalls	"	infrastructure	1	68%
video hall	"	infrastructure	1	100%
hand pump wells	"	water	32	61%
stand pipes	"	water	5	87%
repair of borehole	"	water	1	16%
open wells	"	water	3	61%
fishing equip.	"	other (non-agric)	2	48%
horse cart ambulance	"	other (non-agric)	1	27%
metal boats	"	other (non-agric)	1	25%
vehicle	"	other (non-agric)	2	64%
speed boat	"	other (non-agric)	1	22%

Project descriptions in column 1 are taken from the official CDD program records. Classification and sub-classification in columns 2 and 3 was done by the authors. Frequencies of projects are listed in column 4. The median budget share measures, among villages that implemented such a project, how much of the budget was used for this type of project. For example, tractors tend to require the whole budget and are thus usually the sole project a village implements, while ram fattening only requires a fraction of the budget and leaves room to implement further, different projects. The median budgets for each project type are computed based on the total project budget, including potential village contributions.

villages. Project appraisals were carried out by the CDD program's administrative staff after the village collectively picked a shortlist of projects but *before* disbursements were made. All recorded appraisal dates fall after the onset of the rainy season in 2008. About half indicate a date after the onset of the rainy season 2009. The earliest effects for the majority of villages should thus be expected from 2010 onwards. Our analysis below thus treats the period 2008-2010 separately, as it is neither pre-treatment, nor a period during which treatment effects could already have materialized.

C.1 Consistency of the GFDC data in the area of study

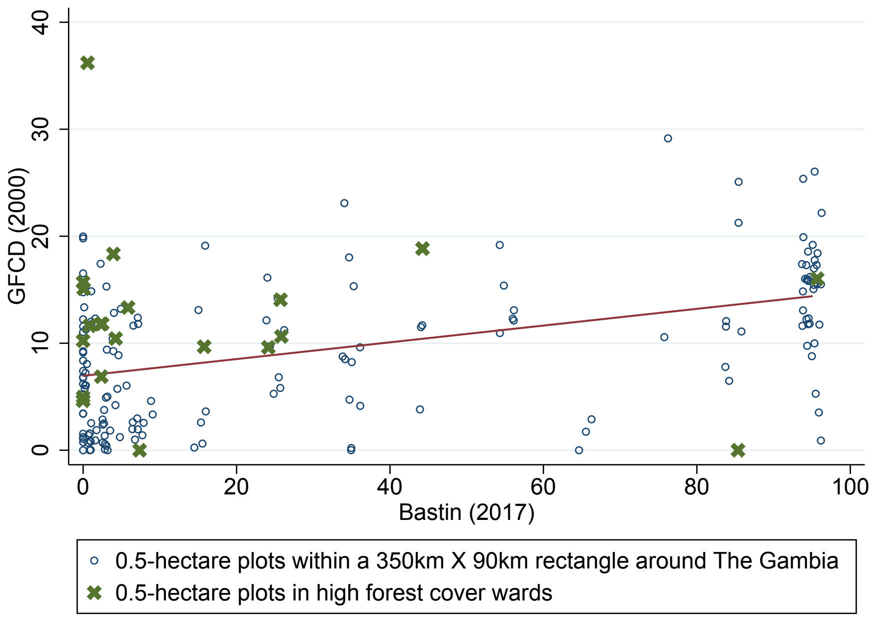
GFCD has been recently criticized by Bastin et al. (2017) for underestimating forest cover in dryland biomes. In order to check the accuracy of the GFCD in the area of study, we compare these data with tree cover densities estimates from Bastin et al. (2017) that were manually coded. Bastin et al. (2017) used satellite imagery for a large number of sampled 0.5-hectare plots in dryland biomes spanning the whole globe and manually (i.e., with the help of human collaborators) marked tree cover in each of these plots, to obtain a measure for tree cover density. We plot tree cover densities from these data against the measures obtained from the GFCD, to confirm that the GFCD-based measures qualitatively captures actual variation in the our study region. To this end, we extract data on all 188 0.5-hectare plots within a 350 km $\times$ 90 km rectangle centered on The Gambia from the Bastin et al. (2017) data set and plot these tree cover measures against the forest density recorded by the GFCD for those locations (see Figure C.2). There are good reasons to expect some differences between the forest cover assessments in these two data sets.

First, the GFCD uses satellite imagery from 15 years before the images underlying the data by Bastin et al. (2017). Second, there is spatial uncertainty, as we could only match the plots from Bastin et al. (2017) to the GFCD-pixels with an accuracy of about 50-100 m. Nonetheless, the baseline forest data from the GFCD correlates reasonably well with the manually coded canopy densities from Bastin et al. (2017). Additionally, this manually coded plot-level data contains a binary classification into forest and non-forest land use. In the 106 sample plots that Bastin et al. (2017) coded as non-forest, the median forest density according to the GFCD is 5.57%, while in the 82 plots that are coded as forest the median forest density in the GFCD is 11.76%. This is clear evidence that the GFCD data captures relevant aspects of the forest density variation we intend to measure. However, the GFCD systematically records much lower densities. Therefore, we consider the 2000 forest cover reported by GFCD as a lower bound of the true forest cover.

In order to further investigate the accuracy of the GFCD data, we also verified some of the data in the field, by visiting a small number of villages where recent forest loss has been recorded.[2] Our interviews with villagers tended to confirm the accuracy of GFCD

[2] For these field tests we visited four villages in the West Coast Region and the Lower River Division in October 2016. We identified prominent features from the GFCD in these villages, such as large areas of recent forest loss, and inquired about them with villagers knowledgeable about the local forests (such as the village chief or representatives from local forestry groups). We first asked villagers to identify

Figure C.2: Comparison of the Percentage of Forest Cover in the GFCD Data for the Year 2000 and the Bastin et al. (2017) Data for the Year 2015



Based on 188 0.5-hectare plots of the data from Bastin et al. (2017) that are within a 350 km×90 km rectangle centered on The Gambia. The 22 points that fall into the areas corresponding to our high forest cover sample are marked by an X. Among those there are two obvious outliers. The outlier at the bottom right can be identified as peritidal mudflats on the banks of a tributary of the Gambia River and seems to be misclassified in the Bastin et al. (2017) data (13.440444, -16.196639; goo.gl/maps/ZsjtuMhkvD42). The outlier at the top left corner seems to be fallow land, around 2 km from the nearest village, which might have been cleared after 2000 (13.216167, -16.322389; goo.gl/maps/Z4vskABFdn12). In both cases visual inspection based on Google Earth's historical satellite imagery from February 2004 suggests that the GCFD data is accurate.

data.[3]

Another critique of the GFCD is that the data do not distinguish between native forest and tree plantations, and therefore deforestation may be underestimated in places where the former is substituted by the latter (Tropek et al., 2014). Nevertheless, this will not be a concern for these data in The Gambia, as there are few tree plantations in the country.[4] This is reflected by the fact that there are no forest gains registered in the GFCD for The Gambia. In countries with large tree plantations, the GFCD often indicates forest gains

significant forest losses before revealing our data. All significant losses identified by the villagers also appear in GFCD, though the exact timing of events was not always clear. In almost all cases such losses were attributable to bushland and loose forest being cleared for cultivation.

[3]This is not the case for an alternative data source, the *GlobeLand30* dataset (Jun et al., 2014), which we initially considered as potentially useful for the empirical analysis, but later disregarded given that our field tests showed that the data was inaccurate, as the land cover classifications were incorrect and inconsistent.

[4]A similar argument has been made by Abman and Carney (2020) for the case of Malawi.

(Tropek et al., 2014; Heilmayr et al., 2016).

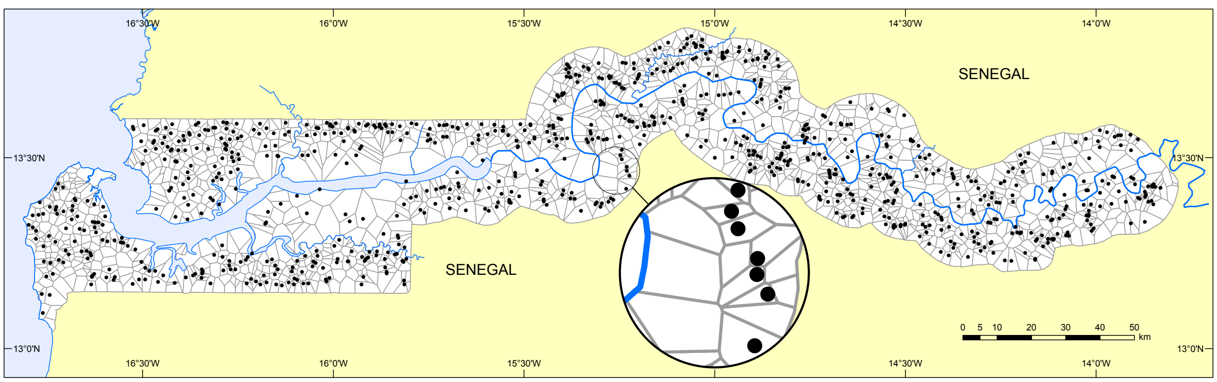
C.2 Data description

C.2.1 Units of observation

As Gambian villages do not have clearly designated demarcations to facilitate a partitioning of the country into a village-level dataset, we rely on two alternative ways to obtain village-level proxies for local deforestation. First, we aggregate pixels in buffers of 1 km and 5 km radii around each village to obtain village-level forest and deforestation measures. As a second method, we use the villages' Thiessen polygons as the unit of aggregation (Figure 1a and Figure C.3a).

Figure C.3: Aggregation Levels of Forest Cover and Forest Change Used in the Empirical Analysis

(a) 1 km buffers



Notes: Dots represent settlements that were eligible for the CDD program.

An advantage of using buffers is that they provide a village-level proxy of deforestation for an area that is constant across villages. A disadvantage, however, is that buffers can overlap, which complicates estimating aggregate effects. Due to the overlaps, estimated magnitudes based on the buffers are not comparable to nation-wide estimates of deforestation from other sources, as overlapping areas will be counted for multiple villages (i.e., the sum of all buffer areas does not correspond to the size of the country). It is thus important to always relate estimated effect sizes to the magnitude of deforestation within the same unit of aggregation for the control group.

Larger and smaller buffers have different advantages. The inhabited areas of villages (covered in buildings and village infrastructure) is typically very small, so that even the 1 km buffers will contain a lot of uninhabited land. For a random subsample of villages and households, sampled in the IHS 2015 (for more information on this survey, see Section 4.4), we have detailed information on each household's location. This allows us to obtain estimates of the spatial extent of villages. In this sample of 2,098 households with location data belonging to 121 of the 402 villages from our sample, 50% of households live within 170 meters of the village centroid. In 60% of the villages *all* sampled households live within the innermost 25 % of the 1 km buffer (i.e., within 500 meters of the centroid). Thus, the smaller 1 km buffers already cover substantial shares of unused land or land that is used agriculturally, which is usually found in a circular area around the villages. When using 1 km buffers, there is little spatial overlap with other villages compared to the 5 km buffers, which simplifies the interpretation of results. Though, in rural, remote villages, the 1 km buffers may not be large enough to capture the entire range of influence of a village. Especially areas that are hitherto unexplored may be located further away. Thus, deforestation resulting from more extensive agriculture is best captured in the 5 km buffer. Thiessen polygons (also called Voronoi tessallation) partition the map into regions of varying size, assigning each point on the map to the nearest village centroid. This process achieves a complete partitioning of the map, where polygons regions with more villages are smaller and polygons in regions where villagers are further apart are comparatively large. Thiessen polygons do not overlap, but have the disadvantage of being highly heterogeneous in size in The Gambia. The distribution of Thiessen polygon sizes in our high forest cover sample varies from 44 ha. to 5674 ha., with the 25^{th} percentile being 302 ha. and the 75^{th} percentile being 857 ha. Estimates for aggregate deforestation based on Thiessen polygons count each pixel once (for the nearest village) and are thus comparable to other measures of aggregate forest loss. Yet, the heterogeneity in polygon area makes the average treatment effect interpretation in our main specification less intuitive.

In our analysis we thus always present results for both buffer sizes and the Thiessen polygons.

C.2.2 GFDC Data

The mean value of the baseline forest cover (i.e., for the year 2000) in the 1 km and 5 km buffers and the Thiessen polygons are all very similar, at around 9% to 10%. The 1 km buffers contain very similar shares of forest cover as the 5 km buffers, suggesting relatively small shares of area covered by the settlements in our sample.[5]

Table C.14 also shows the average forest loss during the pre-CDD program period (2001-2007) recorded by the GFCD. Given the skewness of the forest loss data, we use a logarithmized dependent variable specification for our empirical analysis. To calculate the logarithm in spite of some observations with zero forest loss, we add a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel). In Appendix A we show that our main results do not depend on this transformation and remain qualitatively comparable when other alternatives are used, such as the inverse hyperbolic sine transformation or the untransformed loss. As indicated in Table C.14, in control villages during the seven years preceding the program, logarithmized forest loss in the 1 km buffers is on average 0.53 (corresponding to $\exp(0.53) \approx 1.7$ hectares, i.e., 0.5% of the buffer area). This number rises to 0.96 (2.6 hectares $\approx$ 0.4% of the average polygon area) for the polygons and to

[5]We do not have precise information about the area covered by the village settlements. In a subsample of 60 villages where we were able to measure it, the median area is $0.88\,\text{km}^2$ (Jaimovich, 2015), which is 28% of the area within the 1 km buffer.

3.97 (53 hectares ≈ 0.7% of the buffer area) for the 5 km buffers, which are larger and thus tend to include more previously uncultivated areas. The pre-treatment difference in forest loss between treatment and control villages is never statistically significant.

C.2.3 Additional Data

We have extensive information about the implementation of the CDD program. In addition to the treatment status of all program villages, we have information about the village-level projects implemented by each treatment village, including the types of projects, year and amount of the related disbursement and the contribution from the villagers. We were able to corroborate these data in the field for around 10% of the program villages and found that the administrative records are highly accurate.[6]

In order to match the data related to the implementation of the CDD program, as well as other data relevant for the analysis, with the data from GFCD, we put together a geo-referenced dataset of all settlements in The Gambia. Our main source for village centroid coordinates is a dataset collected by JICA (2003). These data contain the coordinates of all villages registered in the Gambian Census 2003, which we were able to match to the CDD data using village names and districts. In ambiguous cases we additionally relied on GPS coordinates taken during various surveys to complement our database, including our own fieldwork and the Integrated Household Survey 2015 (IHS 2015 henceforth). Through this process we reliably identified coordinates of 95% of villages listed in the Gambian Census 2003. Among villages which were eligible for the CDD program this rate is 97%.[7]

From these village-level geodata we derive further geographic characteristics of the Gambian villages for our analysis (described in Table C.14, Panel B). Control villages in the sample used for the empirical analysis are located on average 10.2 km from the Gambia River. We also calculated the number of nearby villages for each village. While within the 1 km buffer there are on average 0.35 neighboring villages eligible for the CDD program, this increases to 6.74 eligible villages within the 5 km buffers. This variable has a large dispersion, as some villages are relatively isolated while others are clustered together. This is reflected in the large variation in the size of the Thiessen polygons, which have an average area of 633 hectares and a standard deviation of 595 hectares, with some polygons being as large as 5,674 hectares. Treatment and control villages have no significant difference in means for any of these variables (Table C.14, column 5).

The main source for additional pre-CDD program village-level data is the Gambia National Census 2003. This was also the source used by the program staff to identify eligible villages for the program and to implement the randomization of treatment. Table C.14, Panel D describes variables from this source for the main estimation sample, with above-median forest cover. Despite the fact that only the poorest villages in each ward were eligible for the program, we still observe large heterogeneity in the poverty in-

[6]For a related project (Heß et al., 2021), we visited around 80 treatment and control communities in 2014, for data collection and piloting of questionnaires.

[7]In some cases we found discrepancies between the information from different sources. 12% of the centroids reported by JICA (2003) where located at more than 1 km from the centroids calculated using the IHS 2015 data.

Table C.14: Balance of Pre-Treatment Characteristics of Treatment and Control Villages

	mean		difference		
	(1) control	(2) treated	(3) raw	(4) cond.	(5) p-value
Panel A: Forest Characteristics					
% forest cover in 2000 within 1 km	9.21	9.08	−0.135	−0.199	0.37
% forest cover in 2000 within 5 km	10.31	10.13	−0.177	−0.263	0.10
% forest cover within polygons	10.18	9.86	−0.326	−0.398	0.14
log(forest loss (ha.) in 2001-2007 within 1 km)	0.53	0.39	−0.139	−0.130	0.23
log(forest loss (ha.) in 2001-2007 within 5 km)	3.97	3.91	−0.058	−0.050	0.44
log(forest loss (ha.) in 2001-2007 in polygon)	0.96	0.85	−0.114	−0.112	0.39
Panel B: Geographic Characteristics					
distance to river (km)	10.18	10.03	−0.150	−0.147	0.61
villages within 1 km	0.82	0.85	0.031	0.044	0.65
CDD eligible villages within 1 km	0.35	0.43	0.076	0.084	0.20
villages within 5 km	13.80	13.59	−0.208	−0.121	0.80
CDD eligible villages within 5 km	6.74	6.62	−0.127	−0.039	0.89
Thiessen-Polygon area (ha.)	681.25	671.70	−9.554	−22.708	0.66
Panel C: Village median split indicators used for Specification 3					
distance to road	0.49	0.51	0.025	0.023	0.62
population	0.48	0.52	0.045	0.039	0.41
poverty	0.50	0.50	0.005	0.011	0.80
ethno-linguistic fractionalization	0.48	0.52	0.035	0.047	0.30
Panel D: Census 2003 Characteristics					
population	337.93	356.27	18.338	14.619	0.61
poverty index (access to water, electricity, sanitation and literacy)	0.66	0.67	0.006	0.007	0.47
ELF index	0.24	0.27	0.031	0.036	0.06
share Fula	0.21	0.25	0.045	0.039	0.20
share Mandinka	0.51	0.43	−0.081	−0.074	0.03
share Wollof	0.07	0.07	−0.004	−0.004	0.82
share Jola	0.12	0.15	0.026	0.020	0.07
share born in different village	0.13	0.14	0.003	0.003	0.81

Columns 1 and 2 display the means of each variable in the respective treatment group. Sample sizes are 191 and 211 communities respectively. Column 3 shows the raw difference in means, while column 4 shows the conditional difference after controlling for ward fixed effects. Columns 5 shows the p-value of a test for no difference in means, controlling for ward fixed effects. The data underlying Panel A stems from the GFC database. Panel B uses data from the Gambian Census 2003. Panel C is based on our own calculations.

Table C.15: Testing for Differential Pre-Trends in Treatment and Control Villages in the Pre-Treatment Period (2001-2007)

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
treatment × year	0.019	-0.001	0.023
	(0.24)	(0.93)	(0.19)
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	2814	2814	2814
villages	402	402	402
control mean annual loss (ha.) 2001-2007	0.56	11.60	1.22

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. All regressions control for village-fixed effects and ward-year fixed effects, to account for the fact that wards constitute the strata of the treatment randomization.

dex.[8] Despite its small size, The Gambia exhibits large variation in the fractionalization of ethnic groups within small geographical units (which makes the country different from others in West Africa, as described by Arcand and Jaimovich (2019)). The ethno-linguistic fractionalization (ELF) index of the villages ranges from zero (complete homogeneity) to 0.80, with a mean of 0.24. The average shares of the main ethnic groups in the villages (Mandinka, Fula, Wollof and Jola) are close to the national shares in rural areas. The composition of the village groups change slowly over time, as only an average of 13% of people were born outside the village. In most variables from the Census 2003, the difference in means between the control and the treatment group is not statistically significant at conventional levels, with the exception of the ELF and some ethnicity shares. We have also observed this imbalance in other sub-samples of treatment villages (Heß et al., 2021), and our results remain qualitatively comparable when we include ethnicity-period interactions as additional controls.

In order to test some of the channels through which the CDD program may affect deforestation we will use some additional data sources for post-program characteristics, like the Census 2013 and the IHS 2015, which are further described in Section 4.4.

C.3 Exploring potential mechanisms using household-level data

A second source of data we use for testing potential channels is the IHS 2015, an extensive survey conducted by The Gambia Bureau of Statistics with the support of several external donors. The IHS 2015 includes 680 settlements distributed across all districts

[8]The poverty index is the average of four variables: the share of villagers who do not know how to read and write; the share of villagers without access to electricity (either directly or through a generator); the share of villagers without access to private toilets; and the share of villagers without access to an improved source of water.

of The Gambia, with data for close to 13,000 households and 105,000 individuals. The survey follows the Living Standards Measurement Study structure, with additional detailed information about agricultural production as well as political and environmental attitudes.

About one-third of the villages in the sample used in our empirical analysis are covered in the IHS 2015. This represents a total of 266 villages, 133 treatment and 133 control, covering 69 out of the 72 wards of our main sample. We exclude wards from the analysis with only control or only treated villages, resulting in a sample of 244 villages in 58 wards covering 4,462 households and 39,305 individuals.[9] When only high forest cover wards are used, 64 treatment and 62 control villages remain, covering 18,495 individuals in 2,225 households. As households were randomly drawn from enumeration areas based on population, they are not equally distributed across villages. We weight each observation in our empirical analysis by the inverse of the number of sampled households per village.[10]

The IHS 2015 is a very comprehensive survey that provides a large number of potential variables to measure the impact of the CDD program. To avoid "cherry picking" only individual statistically significant estimates among those indicators, we aggregate the household-level variables into indices related to the four hypotheses discussed above, and add two additional hypotheses that can be tested with these data (but not with the data from the Census 2013). To create the indices we use the method proposed by Anderson (2008), following Casey et al. (2012).

The variables entering into the computation of the indices are listed in Table C.17. This table also reveals that, among the many individual outcomes, some have statistically significant differences between the treatment and control group. While individually significant differences might suggest that the CDD program affected outcomes related to the above stated hypotheses, the results in Table C.16 do not support this. Table C.16 shows the estimation results of the treatment effect on the indices that summarize the hypotheses. None of the program treatment effect estimates for any of the indices is statistically significant at conventional levels and magnitudes are consistently below 0.1 standard deviations. Treatment effect estimates in wards with above-median baseline forest cover are also statistically insignificant throughout (Table C.16, Panel A).

In conclusion, we cannot reject that the CDD program had no long-lasting effect on the outcomes studied in *H1-H4* that could explain the increased deforestation in treatment villages. Considered jointly with the census-based results, the results for the IHS 2015 suggest that there was at most a modest increase in general economic welfare. Estimates

[9] The treatment effect of the CDD program on deforestation in this sub-sample is not statistically different from the average treatment effect reported in the main empirical analysis.

[10] The IHS 2015 sampling design made use of enumeration areas (EAs) from the Census 2013, which divide the country into groups to facilitate the division of tasks between census enumerators. EAs were delineated targeting an average group size of around 500 persons per EA while following village demarcations whenever possible. Very small villages, however, were grouped into single EAs, while larger villages, especially in urban locations, were divided into multiple EAs. Overall, 89% of EAs contain persons from a single village (including villages that are spread over more than one EA) and 70% of EAs directly correspond to villages. The IHS 2015 randomly sampled EAs with a probability proportional to population and within each EA targeted 20 randomly selected households for interview.

Table C.16: Household Channels: Indices for Six Hypotheses

	(H1) welfare	(H2) livestock	(H3) land-intensive goods	(H4) population	(H5) agric. production	(H6) social capital
Panel A: Villages with Above Median Forest Cover						
treatment	0.101	-0.009	-0.011	-0.000	0.039	-0.063
	(0.18)	(0.90)	(0.87)	(1.00)	(0.64)	(0.26)
observations	2414	2407	2412	2415	2407	2415
villages	134	134	134	134	134	134
Panel B: All Villages						
treatment	0.028	0.030	-0.020	0.017	-0.017	-0.003
	(0.59)	(0.50)	(0.67)	(0.73)	(0.78)	(0.93)
observations	4896	4881	4891	4898	4880	4894
villages	267	267	267	267	267	267

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The unit of observation is the household. Dependent variables are indices for each of the six hypotheses stated in the text. The index is created using the inverse covariance weighting as proposed by Anderson (2008) and it is standardized to have mean zero and standard deviation one in the control group. All specifications include ward fixed effects as well as the village-level poverty index and village population in 2003.

related to *H1* and *H2* appeared marginally significant in the Census 2013, but were insignificant using data collected two years later in the IHS 2015. This may indicate that the effects were larger in years closer to the project and dissipated over time.

In addition to the four hypotheses described above, the data in IHS 2015 allow us to test additional hypotheses that relate specifically to the Gambia CDD program, in particular to agricultural production and village institutions.

Hypothesis 5 (H5): The CDD program affects agricultural production.

Every household in the IHS 2015 sample cultivates some kind of crop. As a large share of the CDD program sub-projects focused on agriculture, deforestation in treatment villages may be related to an expansion of agricultural production. We build an index for agricultural production considering inputs (plot size and use of fertilizer), grain processing and indicators for each type of crop (groundnut, rice, millet, maize and vegetables). The estimates of the treatment effect on the *H5* index in column 5 of Table C.16 are small and insignificant, though larger in wards with high baseline forest cover. We do not find evidence for a medium-term treatment effect on agricultural production. However, we cannot rule out short-run changes that had dissipated by the time the IHS 2015 was conducted.

Hypothesis 6 (H6): The CDD program affects institutions and social capital.

An aspect that distinguishes the Gambian CDD program from other development programs is that it does not only provide financial resources, goods and services to the village, but also attempts to positively influence local institutions and decision-making processes. These outcomes are defined as "software" outcomes in Casey et al. (2012). Changes in "software" may relate to deforestation directly (e.g., empowering forest management groups) and indirectly (e.g., affecting the willingness to contribute to public goods). We build an index to capture forestry and environment-related participation in village politics, based on household-level indicators for participation in projects at the ward-level, voting in the last local elections, participating in a village forestry group, contributing to village's tree planting, contributing to building a buffer to protect the forest from bush fires, and listing the environment as one of the main village problems. The estimates for the treatment effect on the *H6* index in Table C.16 are close to zero and statistically insignificant. Thus, we do not find evidence that there were changes in institutions and social capital brought about by the CDD program that can explain its effects on deforestation.[11]

Overall, our results indicate that the CDD program has a modest impact on economic welfare (wealth and livestock), which could be related to the increase of deforestation in treatment villages. Nonetheless, we do not find evidence that the effect on deforestation is driven by other channels described in previous literature, such as an increase in the consumption of resource-intensive goods or an increase in village population. Nor do we find evidence that suggests a channel specific to the CDD program such as changes in agricultural production and villages institutions played an active role in deforestation. The lack of clear results on channels could also be related to power issues. Statistical power could be a bigger issues for the analysis of household-level channels than for the deforestation analysis for a number of reasons. First, the IHS 2015 only covers a comparatively small subset of villages and a sample of households of our main analysis. Second, both data sources in this section are based on surveys and the indices are built from a limited number of items. Third, while treatment effect estimates for the deforestation analysis use repeated measurements in a 14-year panel, for the household-channel analysis we only have cross-sectional data available. We can thus not rule out that some of the hypothesized changes at the household level actually occurred, but are not picked up by our analysis with the data that we use.

[11] In Heß et al. (2021) we use detailed data for a sub-set of eligible villages to show that the CDD program is likely to have induced internal disputes related to unequal benefits and failed sub-projects. An increase in within-village disputes may imply a reduction in the coordination for the management of common resources such as the forest. We do not have data to directly test this hypothesis on the full set of villages.

Table C.17: Variables Entering into the Indices Used in Table C.16

hypothesis	variable	variable type/unit	weight in index		TE (high forest cover)		
			high forest cover sample	full sample	estimate	p-value (CRSE)	p-value (RI)
H1	assets	PCA	0.25	0.26	0.01	0.36	0.38
	annual income	log(income in GMD + 1)	0.19	0.15	0.34	0.07*	0.03**
	paid job	dummy	0.24	0.27	0.05	0.06*	0.08*
	food spending	log(food spending/week in GMD + 1)	0.23	0.24	-0.03	0.68	0.70
	non-food spending	log(non-food spending/year in GMD + 1)	0.10	0.09	0.00	0.99	0.99
H2	cattle	count	0.12	0.11	0.53	0.34	0.45
	oxen	count	0.21	0.19	0.02	0.75	0.78
	goats	count	-0.02	0.03	-0.08	0.72	0.76
	sheep	count	0.10	0.05	0.37	0.16	0.17
	donkey	count	0.13	0.18	-0.17	0.01***	0.01***
	any cattle	dummy	0.11	0.11	-0.04	0.18	0.20
	any goat	dummy	0.17	0.13	0.01	0.76	0.78
	any sheep	dummy	0.10	0.10	0.06	0.03**	0.04**
	any donkey	dummy	0.07	0.10	-0.09	0.00***	0.00***
H3	fuel consumption	dummy	0.22	0.22	-0.01	0.68	0.71
	firewood cooking	dummy	0.27	0.26	-0.01	0.48	0.51
	rooms	count	0.13	0.13	-0.09	0.67	0.67
	beef	dummy	0.13	0.13	-0.03	0.22	0.26
	other meat	dummy	0.14	0.14	0.03	0.08*	0.12
	milk	dummy	0.11	0.12	0.04	0.22	0.28
H4	born outside village	dummy	0.38	0.38	0.03	0.31	0.32
	household size	count	0.13	0.08	-0.47	0.26	0.28
	children	count	0.19	0.23	-0.29	0.30	0.33
	any births past year	dummy	0.30	0.31	0.00	0.89	0.88
H5	plot size	hectares	0.08	0.08	-0.07	0.89	0.88
	fertilizer	dummy	0.09	0.08	-0.03	0.45	0.46
	processes grain	dummy	0.15	0.13	-0.06	0.17	0.20
	groundnut	dummy	0.06	0.08	-0.06	0.06*	0.10*
	rice	dummy	0.19	0.20	0.05	0.42	0.46
	millet	dummy	0.05	0.08	-0.03	0.40	0.45
	maize	dummy	0.15	0.14	0.03	0.44	0.47
	vegetables	dummy	0.22	0.20	0.08	0.05**	0.07*
H6	participate in ward proj.	dummy	0.19	0.18	-0.04	0.04**	0.06*
	voted in elec.	dummy	0.21	0.21	0.01	0.56	0.56
	forestry group	dummy	0.15	0.15	-0.02	0.51	0.56
	tree planting activity	dummy	0.13	0.12	-0.01	0.67	0.69
	environmental concerns	dummy	0.22	0.21	0.00	0.97	0.98
	buffer creation activity	dummy	0.11	0.13	-0.02	0.52	0.52

This table contains a comprehensive list of all variables used to compute the indices used in Table C.16. The weight of each variable in the final index is based on the variance-covariance matrix in the control group of that specific sample of all variables included in the same index (following Anderson, 2008). The weights are computed from and applied to the normalized variables (mean 0, variance 1). The relative weights shown in this table sum up to 1. Treatment effect estimates and p-values are based on Equation (4). "Buffer creation activities" refers participation in the collective clearing of land in the village surroundings for the purpose of preventing wildfires.

C.4 Land prices

The IHS survey questionnaire has a section on land plots that includes a question on land prices, elicited through the survey question "If [household] were to sell this parcel today, how much could it fetch?" These data allow us to obtain estimates for hypothetical land prices and to investigate treatment-control differences in these. However, these analyses are subject to a number of caveats. First, transactions of land in The Gambia are embedded in a traditional system of land exchange that does not rely on payment (see Heß et al., 2021). According to the IHS data 95% of all land plots were acquired through inheritance. Only 1.6% of households that are from wards with meaningful initial forest cover (our main estimation sample) and that were sampled for survey in the IHS report to own plots that were purchased. Since plot price expectations are elicited in the IHS through a question based on a hypothetical scenario and households can base their answers only on a very limited number of actual land transactions, these numbers must be assumed to be measured with significant noise. The same applies to the measurement of plot areas, which likely suffer from measurement error. Lastly, not all villages and not all households own or report plots in this survey. We address this last issue in the regression below by using the inverse number of plots per village as weights in the regression.

Table C.18 shows regressions using the hypothetical per-hectare land price as dependent variable. Because of the mentioned concerns regarding measurement error, also in independent control variables, we present results both with and without plot-level control variables. These results imply that expected land prices are approximately 13.7% larger in treatment villages (p-value=0.118). Based on the treatment effect heterogeneity with respect to road infrastructure, which we document for the deforestation effect in Table 2, we also test for treatment effect heterogeneity with respect to road infrastructure for the hypothetical land prices. We find a pattern that is consistent with the heterogeneous effect on deforestation, namely that the observed average increase in hypothetical land prices appears to be predominantly driven by villages that are far from a road. The point estimate in column 4 implies an increase by approximately 28.3%. This result mirrors the result that the forest loss also seems to be more pronounced in areas with limited road infrastructure.

Table C.18: Land prices

	plot level				village level	
	log(plot price)	log(plot price)	log(plot price)	log(plot price)	log(median plot price)	log(median plot price)
Panel A: Villages with Above Median Forest Cover						
treatment	0.122	0.137	-0.0505	-0.00867	0.184*	0.0136
	(0.178)	(0.118)	(0.660)	(0.938)	(0.090)	(0.929)
far from road			-0.138	-0.117		-0.264
			(0.321)	(0.397)		(0.156)
far from road × treatment			0.334*	0.283		0.347
			(0.065)	(0.104)		(0.143)
village controls	✓	✓	✓	✓	✓	✓
plot controls		✓		✓		
household controls		✓		✓		
observations	5417	5413	5417	5413	134	134

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The price data is collected in a household-level interview where for each plot—aside from questions about plot area and plot slope and size, the estimated current price is elicited through the survey question "If [household] were to sell this parcel today, how much could it fetch?". Most transactions of land in The Gambia are embedded in a traditional system of land exchange that does not rely on payment (see Heß et al., 2021), so these price estimates have to be regarded as rough estimates by the household heads for current market prices for land that is traded on a market. Similarly, the plot area that is used for the computation of the price per hectare and as a control must be assumed to be measured with significant measurement error. All specifications include ward fixed effects. Village controls are the village-level poverty index and village population in 2003. Plot controls are plot size, whether the plot is flat or sloped, and whether the plot is located in the same district as the respondent household. Plot-level observations are weighted by the inverse of the number of plots per village, so that each village receives the same weight in the regression.

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