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Grillos, Tara; Bottazzi, Patrick; Crespo, David; Asquith, Nigel; Jones, J.P.G.

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Tara Grillos*
Department of Political Science, Purdue University, 100 N. University Street, West Lafayette, IN, United States; tgrillos@purdue.edu

Patrick Bottazzi[1]
School of Environment, Natural Resources and Geography, Thoday Building, Deniol Road, Bangor, LL57 2UW

David Crespo
Universidad Mayor de San Simón
Calle Nataniel Aguirre N°0466, Cochabamba, Bolivia; dcrespoforestal@gmail.com

Nigel Asquith[2]
Harvard Forest, 324 North Main Street, Petersham, MA 01366
Sustainability Science Program, Kennedy School of Government, Harvard University, 79 JFK Street Cambridge, MA 02138

Julia P.G. Jones[3]
School of Environment, Natural Resources and Geography, Thoday Building, Deniol Road, Bangor, LL57 2UW

[1] Present address: Institute of Geography, University of Bern, Hallerstrassse, 12, 3012 Bern, Switzerland; patrick.bottazzi@giub.unibe.ch
[2] Present address: Cuencas Sustentables, Calle Río Totaitú 15, Barrio Las Palmas, Santa Cruz de la Sierra, Bolivia; nigelasquith@yahoo.com
[3] Present address: School of Natural Science, College of Environmental Sciences and Engineering, Thoday Building, Deniol Road, Bangor, LL57 2UW; julia.jones@bangor.ac.uk

In-kind Conservation Payments Crowd in Environmental Values and Increase Support for Government Intervention: A Randomized Trial in Bolivia

ABSTRACT

There is growing use of economic incentives such as Payments for Ecosystem Services (PES) to encourage sustainable land management. An important critique is that such approaches may unintentionally disrupt environmental and social values, 'crowding out' pre-existing motivations to conserve. Some scholars suggest that the use of in-kind payments and norm-based framing, rather than financial transfers and a market framing, can mitigate these risks. There are calls to use more robust methods for impact evaluation in environmental policy. We use one of the only Randomized Controlled Trials of a conservation incentive scheme to evaluate its impact on self-stated environmental and social values and beliefs. Data from before and after the intervention, from households in villages randomly selected to receive the program or not, demonstrate that the program increased prioritization of environmental values (evidence of crowding-in as opposed to crowding out) and altered social beliefs related to inequality and the role of government. The findings demonstrate that this conservation program had a positive impact on environmental values and increased the belief that government involvement is appropriate. The scheme, with its use of in-kind payments and reciprocity framing, offers lessons to those seeking to develop effective schemes to incentivize positive environmental stewardship.

Keywords: payments for ecosystem services; motivation crowding; social norms; framing; environmental values; Bolivia

64

1. Introduction

65 **1. Introduction**

66 Payments for Ecosystem Services (PES) are voluntary transactions whereby land managers are

67 incentivized to carry out natural resource management actions believed to generate ecosystem

68 services for another group of users or society as a whole. PES schemes have proven to be a valuable

69 tool for promoting conservation in vulnerable and critically important ecosystems (Jayachandran

70 et al. 2017). This approach has been adopted worldwide (Kinzig et al. 2011; Pattanayak, Wunder

71 & Ferraro 2010) with over 550 ongoing PES programs, representing around US$36-42 billion in

72 transactions (Salzman et al. 2018).

73

74 A common critique of PES programs is that financial incentives may have adverse effects on pre-

75 existing motivations for conservation, including both environmental and social values and beliefs

76 (Rode, Gómez-Baggethun & Krause 2015). In particular, critics view PES as engaging in

77 "commodity fetishism", reducing complex ecosystem functions to tradeable services (Kosoy &

78 Corbera 2010, Muradian et al. 2010). A large body of social science research demonstrates that

79 financial incentives sometimes result in the opposite of their intended effect (Bowles 2008; Deci,

80 Koestner & Ryan 1999; Frey 1994; Gneezy & Rustichini 2000; Titmuss 1971). This phenomenon,

81 known as "motivation crowding", could potentially lead to a *decrease* in conservation behavior,

82 especially after the incentive payments end (Andersson et al. 2018). However, the original

83 psychology literature on motivation crowding suggests that external interventions can sometimes

84 enhance pre-existing motivations rather than displace them (crowding "in" rather than "out"), if

85 the external intervention is perceived as congruent with one's own values and identity (Deci,

86 Koestner & Ryan 1999).

87

88   Another, closely related critique of PES programs is that, in practice, they often present barriers to

89   entry that exclude the poorest members of communities (Bremer, Farley & Lopez-Carr 2014;

90   Pagiola, Arcenas & Platais 2005). Compensation programs often feature participation skewed

91   toward wealthier members of a community (Greig-Gran, Porras & Wunder 2005; Zbinden & Lee

92   2005), and there is a risk that PES schemes may exacerbate pre-existing inequalities between

93   landowners and others (García-Amado et al. 2011). The tension between equity and efficiency of

94   market-based mechanisms has been an important concern in the literature (Brown & Adger 2007;

95   Landell-Mills 2002; Pascual et al. 2010; Wunder 2008). Some caution that a failure to consider

96   social equity can undermine environmental protection in the long-run through disenchantment with

97   the program (Pascual et al. 2014). These studies raise concerns about unintended social impacts

98   that may arise from PES programs, leading scholars to a call for greater inclusion of the poor in

99   PES (Farley & Costanza 2010). They also raise questions about the potential for motivation

100   crowding with respect, not only to intrinsic environmental values, but also to pro-social values and

101   beliefs, especially as they relate to inequality.

102

103   Apart from direct material benefits provided to participants, policies and programmatic

104   interventions also have interpretive effects (Pierson 1993) which may shape participants'

105   "psychological predisposition to participate in public life" and perceptions of "their status in

106   relation to other citizens and government" (Mettler 2002, p.352). Studies of this dynamic

107   interaction, aka policy feedback, have largely been focused on social policy in the United States

108   and Europe (Béland 2010, Campbell 2012), with little attention to the developing country context,

109   where government policies often co-exist and overlap with programs implemented by international

110   and non-governmental organizations. If PES influences the values and beliefs of community

111   members, this may have long-run implications not only for the specific conservation behavior it

112   was designed to influence, but also for subsequent policy efforts related to equity in the same

113   communities. This represents a potential spillover of motivation crowding to other areas of policy

114   intervention, and warrants attention to the influence of PES on beliefs and values related to

115   inequality and government intervention, in addition to environmental conservation.

116

117   The introduction to a recent special section on motivation crowding in *Ecological Economics*

118   (Ezzine-de-Blas et al. 2019) argued that particular programmatic design features of PES programs,

119   including for example payment type and communication, will influence the likelihood of crowding

120   in vs. crowding out, to the extent that they stimulate feelings of competence, autonomy, and

121   social/environmental relatedness. Prior laboratory research suggests that in-kind payments may be

122   less prone to crowding out than cash, likely because they evoke social norms rather than a "market

123   logic" (Heyman & Ariely 2004). Scholars have suggested such in-kind payments may be more

124   effective in the application of PES (Kerr, Vardhan & Jindal 2014, Chan et al. 2017) but this has

125   not been tested in a field-based experiment.

126

127   In addition, a growing literature suggests that simply framing an intervention in a particular way

128   can change how people react to it (Chong and Druckman 2007, Clot et al. 2017) and that the

129   effectiveness of a particular framing depends on pre-existing norms and beliefs (Andrews et al.

130   2013). One such pre-existing norm is reciprocity, or the relational notion that people should give

131   back to those who help them. Recent research exploring the drivers of environmental values has

132   shown that they can be driven by a perceived relationship with nature (Bremer et al. 2018, Chan

133   et al. 2016, 2017). Reciprocity is considered one of several shared principles of moral psychology,

134   common across many cultures (Haidt 2007) and has been observed to motivate human behavior in

135   a variety of decision contexts (Axelrod & Hamilton 1981; Falk & Fischbacher 2006). Taken as a

136   whole, this suggests that the combination of in-kind compensation and reciprocity framing may

137   reduce the risks of motivation crowding in incentive schemes.

138

139   A blossoming literature has explored the psychological impact of PES, and PES-like, programs on

140   individuals, through their motivations, values, beliefs and internalized norms, with few consistent

141   results. This literature has included a range of methods including ethnographic analyses (Bose,

142   Garcia & Vira 2019, Van Hecken et al. 2019), structured interviews (García-Amado, Pérez &

143   García 2013), quasi-experimental approaches (Agrawal, Chhatre & Gerber 2015; Chervier, Le

144   Velly & Ezzine-de-Blas 2019), regression discontinuity designs (Alix-Garcia et al. 2018) and

145   framed field experiments (Andersson et al. 2018; Cook et al. 2019; Handberg & Angelsen 2019;

146   Kaczan, Swallow & Adamowicz 2019; Moros, Valez & Corbera 2019, Kolinjivadi et al. 2019).

147   There is substantial interest in the use of Randomized Controlled Trials (RCTs) in conservation

148   (Ferraro & Pattanayak 2006, Bayliss et al. 2015), where units are randomly allocated to receive an

149   intervention or not, as a robust method of impact evaluation (Banerjee & Duflo 2009). RCTs

150   overcome many of the challenges of other approaches to allow causal inference (the ability to

151   conclude that the intervention resulted in the result observed). However, the use of randomized

152   trials is still very rare in the study of environmental management interventions (Ma et al. 2017).

153   There are only two published RCT evaluations of PES schemes (Jayachandran et al. 2017; Pynegar

154   et al. 2018) and none that examine psychological effects on the values and beliefs of participants.

155

156   We present the results of the only Randomized Controlled Trial to date that measures the effects

157   of a conservation incentive scheme on environmental and social values and beliefs among

158   community members. In this paper, we experimentally evaluate the impact of a PES-like program,

159   called *Watershared*, that features two specific design features intended to reduce the risk of

160   motivation crowding: the use of in-kind payments and framing that references local reciprocity

161   norms. We use before and after data from households in communities randomly allocated to be

162   offered *Watershared* agreements (treatment communities) or not (control communities) to evaluate

163   the extent to which the scheme resulted in motivation crowding related to environmental and social

164   values and beliefs.

165

166   **2. Environmental and Social Values and Beliefs**

167   The theory of motivation crowding primarily focuses on how motivation for future behavior will

168   be affected after incentive programs end and the new, external motivation is no longer a direct

169   driver (Andersson et al. 2018). However, as they have not yet happened, future environmental

170   behaviors and the motivations behind them are difficult to observe directly. As a result, studies of

171   motivation crowding often focus instead on values and beliefs which are commonly understood to

172   be important precursors to motivations for pro-environmental behaviors. If PES and PES-like

173   programs affect motivations for future environmental behaviors, they likely do so through changes

174   to individual values and beliefs.

175

176   Terms such as values and beliefs can be used to mean subtly different things. Our goal is not to

177   contribute to the theoretical arguments relating to these definitions but to examine shifts in mental

178   assessments that people might make as the result of experiencing a PES or PES-like intervention,

179   and which, in turn, could influence later motivations for environmental behavior. Values can be

180   understood as universally held guiding principles for decisions that people make in their lives

181   (Schwartz 1992, p.21), thus providing a direct precursor to motivations for behavior. Much of the

182   literature on motivations for environmental behavior, specifically, focuses on four key types of

183   values: (i) hedonic or short-term pleasure-seeking values, (ii) egoistic or market values, (iii)

184   altruistic or pro-social values and (iv) biospheric or environmental values (Steg & DeGroot 2012,

185   Steg et al. 2014b). Given the importance of both environmental and social values in motivating

186   environmental behaviors (Rode, Gómez-Baggethun & Krause 2015), there is a risk if either or

187   both are crowded out by financial incentives.

188

189   There is evidence that those who endorse either environmental or social values are typically more

190   motivated to engage in pro-environmental behaviors (Steg et al. 2014a, Steg et al. 2014b, Nordlund

191   & Garvill 2002, Stern et al. 1995, Thogersen & Olander 2002). However, it is well recognized that

192   values alone are insufficient to motivate action. In order to take action, people must not only place

193   value on something, but also hold related beliefs, for example believing that the thing they value

194   is affected through their own individual actions (Schwartz 1970, 1977, Stern et al. 1995). We

195   define a belief as "any proposition that is accepted as true" (Colman 2001, as cited in Kenter et al.

196   2015), which is broad enough to include both value-laden attitudes and norms, as well as mere

197   descriptive perceptions of the world. The particular beliefs we measure in this paper are those that

198   seem most directly related to PES interventions: perceptions of a trade-off between environmental

199   conservation and economic growth, and views on inequality and egalitarian norms (see Table 1).

200   These touch directly on the two primary critiques of mainstream PES: (i) that they may lead to

201   "commodity fetishism" whereby perceptions shift toward viewing the forest as an economic

202 commodity, and (ii) that they may exacerbate inequalities in communities, with related impacts on

203 perceptions of those inequalities. The intervention we examine here made explicit efforts to avoid

204 these pitfalls through its use of in-kind incentives and reciprocity framing.

205

206 **3. The Intervention: *Watershared***

207 In 2003 the non-governmental organization Fundación Natura Bolivia (Natura), in cooperation

208 with several municipal governments, began using in-kind incentives to encourage conservation in

209 the Andean region of Bolivia. Their program, now called *Watershared,* aims to slow deforestation

210 and maintain supplies of high quality water available to communities. The program provides

211 modest development support in exchange for avoiding deforestation and excluding livestock from

212 riparian forest. Natura first visited each treatment community to offer a series of information

213 sessions presenting their compensations as "reciprocal watershed agreements" and likening the

214 arrangements to existing reciprocity norms that are common in the region (Bétrisey & Mager 2014;

215 Capuma 2007). The information sessions characterized the program as establishing a reciprocal

216 relationship between (i) Natura and those entering into the *Watershared* agreements, (ii) upstream

217 and downstream water users, as well as between (iii) human beings and the natural environment.

218 The original definition of PES involves buyers and sellers of services (Wunder 2007),

219 while *Watershared* simply incentivizes landowners to conserve their watersheds. However, the

220 intervention does involve "voluntary transactions between service users and service providers that

221 are conditional on agreed rules of natural resource management for generating offsite services"

222 (Wunder 2015) and so consideration of the *Watershared* scheme is relevant to those interested in

223 the design of conservation incentive schemes such as PES. As of 2016, 210,000 hectares of forest

224   owned by 4,500 households were under some version of Natura's *Watershared* conservation

225   agreements (Asquith 2016).

226

227   In our study setting, households enrolling land in Watershared agreements were provided with

228   development projects with a value of $100 (as a one-off enrollment bonus) plus a variable amount

229   (ranging from $1-$10) per hectare conserved, depending on the type of land and the rules they

230   agreed to follow, which could include restrictions on both deforestation and degradation due to

231   cattle grazing. Between the 1960s and early 2000s, deforestation in the Bolivian lowlands

232   increased from about 4.7×10^4 hectares/year to more than 2.9×10^5 hectares/year (Killeen et al.

233   2008). During the ten years prior to our baseline survey, deforestation in our specific study area

234   was approximately 4,147hectares, with a mean deforestation rate of 1.2% per community (Wiik et

235   al. 2019). The goal of the *Watershared* program was to limit forest degradation, as well as

236   deforestation. In particular, the agreements targeted the issue of cattle grazing in the watershed,

237   which can lead to fecal contamination of the water source (Crane et al. 1983, Sunohara et al. 2012)

238   and creates risks for biodiversity (Stern et al. 2002).

239

240   Payments were made in the form of inputs for sustainable livelihoods, such as fruit trees,

241   beekeeping equipment, irrigation tubing, or barbed wire (to help enclose the cattle and keep them

242   away from the watershed). Agreements (for three years) were offered on an individual basis

243   (Pynegar et al. 2018). Previous research on *Watershared* found that take-up was determined by a

244   combination of financial and social characteristics, with poorer community members less able to

245   participate (Grillos 2017) and that those motivated by pro-nature instrumental motivations were

246   more likely to enroll land which resulted in additional conservation (Bottazzi et al. 2018). In this

247 study, we examine whether experiencing the intervention has changed prioritization of

248 environmental and social values, and whether it affected self-stated agreement with normative

249 statements related to inequality and the environment.

250

251 **4. Research Design**

252 **4.1 A Randomized Controlled Trial in Bolivia**

253 The *Watershared* intervention we study here took the form of a randomized controlled trial within

254 the Río Grande Valles Cruceños (RG-VC) Natural Integrated Management Area. The RG-VC is a

255 mixed-use protected area, meaning that, while it is identified as an important ecosystem in need of

256 protection, the government also recognizes the rights of pre-existing forest dwellers to use their

257 own land as they deem appropriate. Natura identified 129 villages inside the RG-VC and

258 conducted a pre-intervention survey with households in all of those communities in late 2010.

259 After stratifying by municipality, village size and number of cattle in the community, they then

260 randomly selected 65 villages out of the original 129 included in the survey.[4] Individuals in these

261 randomly selected villages were offered the opportunity to enroll their land in *Watershared*

262 agreements, while the remaining communities constituted a control group (Pynegar et al. 2018).

263 Five years later, in late 2015, we implemented a follow-up survey with the same households in all

264 villages (those that received the program as well as those that did not), generating a panel dataset

265 (Bottazzi et al. 2017). Two papers have been published using the RCT. Pynegar et al. (2018)

266 examined the impact of the intervention on water quality (in terms of *E. coli* contamination of

267 water used for human consumption) and found no impact. Wiik et al. (2019) showed that the

---

[4] The study sample originally involved 130 villages, but one of the randomly selected control villages later turned out to be located outside the designated study area, so the baseline survey was not conducted there and it was dropped from all analyses.

268   intervention had limited impact on slowing deforestation (using the Global Forest Change data).

269   Ours is the first paper to make use of the household survey data related to this intervention.

270

271   The randomized design of the intervention eliminates concerns over selection bias (Duflo et al.

272   2007), and balance tests confirm that the treatment and control groups did not differ substantially

273   at the outset on neither demographic characteristics nor our key outcome variables (See Appendix

274   A). However, those sampled in the treatment group were less likely to be active members of the

275   community council (called the *organización territorial de base*, or "OTB"), which previous

276   research also cited as an important predictor of program take-up (Grillos 2017). We address this

277   issue in the analytic methods section.

278

279   **4.2 The Dataset**

280   Two thousand, six hundred and one (2,601) households were included in the pre-treatment baseline

281   survey. Of these, 55% (1,443 households) reside in one of the 65 treatment villages, and the other

282   45% (1,158) reside in one of the 64 control villages. Of those initially surveyed within treatment

283   villages, 38% (548 households) took up *Watershared* agreements. Since some families live in one

284   community but simultaneously own land in another, there was a small amount of contamination in

285   the control group, with 32 (out of 1,158 control households) reporting they took up a *Watershared*

286   agreement. We directly address this two-sided noncompliance in the analytic methods section

287   below. The post-treatment endline resurveyed 1,672 of those covered in the baseline. Attrition was

288   due to a combination of people moving away (there is high rural depopulation in this part of

289   Bolivia) and not being available.[5] Attrition was not correlated with any of our key outcome

290   variables, although the subset that was re-surveyed at endline did differ on some control variables.[6]

291   Of those households surveyed at both baseline and endline, 58% (970 households) were in the

292   treatment group, and 38% (548 households) of those had entered into *Watershared* agreements.

293

294   The full survey instrument is archived alongside the full dataset [dataset] (Bottazzi et al. 2017).

295   The full text of the particular questions we identified as measuring environmental and social values

296   and beliefs is included in the next section (translated into English). Some of these questions were

297   initially removed from the post-treatment survey because of concerns about the length of the

298   survey. Due to their scholarly interest, they were then reintroduced in the remaining surveys. For

299   this reason, the sample size for some of these analyses is much more limited than the full set of

300   households included in the more general survey. There were 333 households that received the full

301   set of all our values and beliefs questions at both baseline and endline (i.e. 666 observations in the

302   panel dataset), and 69% of these (231 households) were part of the randomly assigned treatment

303   group. Of those in the treatment group, 40% (92 individuals) had taken up *Watershared*

304   agreements. Balance tests confirm that this smaller subsample is representative of the broader

305   study region based on statistics from the full baseline survey (See Appendix B).[7]

306

307   **4.3 Outcome Measures: Values and beliefs**

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[5] In addition, some additional households were also picked up in the endline survey without having been included in the original baseline survey, but these do not figure into any of our analyses or tables and represent less than 3% of the total households with whom we made contact throughout this process.

[6] Attrition was associated with, on average, less cattle ownership, slightly fewer people in the household, and less OTB membership. See Appendix B for comparisons across subsets.

[7] The two groups did differ slightly in that the smaller sample (who received all the questions at endline) oversampled the treatment group relative to the true proportions (69% of the households included in the values sub-sample resides in the treatment group villages).

308　The survey included questions about demographic characteristics, assets, education and

309　livelihoods as well as questions relating to environmental and social values and beliefs. Table 1

310　includes the full text of the survey questions (translated from Spanish) used to construct our

311　outcome measures related to environmental and social values and beliefs.

Table 1: Survey Questions on Environmental and Social Values and Beliefs
(*Original Spanish in Italics*)

314

Construct	Survey Question
Values	I'm going to present you with some values that may be taught to children in the home. Of these values, can you choose the two that you think are the most important? (a) Independence, (b) Creativity, **(c) Protecting the Environment, (d) Sharing with Others, being altruistic** (e) Obedience, (f) Being a Good Student, (g) Being Successful *Voy a presentarle algunos valores que se puede enseñar a los niños en casa. ¿De estos valores, puede elegir los dos que piensa que son los más importantes?* *(a) Independencia, (b) Creatividad, **(c) Cuidar el medio ambiente, (d) Compartir con los demás, ser altruisto,** (e) Obediencia, (f) Ser un buen estudiante, (g) Búsqueda del éxito*
Beliefs	Now I will read some statements and I would like to know if you agree with each one. There is no correct answer, I just want to know your opinion. 　　　　　[1= completely disagree… 5= completely agree] *Ahora voy a leer unas afirmaciones y me gustaría saber si usted está de acuerdo con cada una. No hay una respuesta correca, sólo quiero saber su opinión sobre cada una de las afirmaciones.* *[1=completamente en desacuerdo… 5=completamente de acuerdo]*
Environmental beliefs	• "In order to improve quality of life, it is necessary to harm the environment." [*"Para mejorar las condiciones de vida, es necesario dañar el medio ambiente."*] • "We can have higher economic incomes if we protect the environment." [*"Podemos tener mejores ingresos económicos si protegemos el medio ambiente"*]
Social beliefs	• "It is the responsibility of the government to reduce income inequality between people with a lot of money and people with little money." [*"Es responsibilidad del gobierno reducir la desigualdad de ingresos entre las personas con mucho dinero y las personas con poco dinero."*]

	• "If a person works more than others, it's fair that they earn more money." ["*Si una persona trabaja más que otras personas, es justo que gane más dinero.*"] • "If a person earns more than others, they must share with the rest." ["*Si una persona gane más que otras, tiene que compartir con los demás.*"]

315

316   To measure the relative priority placed on environmental and social values, we included a question

317   in the survey that asked respondents to choose their top two priorities from a list of values that

318   could be taught to children in the home. Among this list of possible values were the options

319   "protecting the environment" (biospheric/environmental values) and "sharing with others"

320   (altruistic/pro-social values). Our outcome variables related to values were two binary variables:

321   whether an individual chose, respectively, protecting the environment (environmental values) or

322   sharing with others (social values), as one of their top two priorities. This question was adapted

323   for the local context from one that appeared on the World Values Survey questionnaire (Inglehart

324   et al. 2014).

325   While environmental values have been measured in variety of ways in the past (Dietz 2005), we

326   find this relative priority version of the question to be the most compelling for various reasons.

327   First, we believe it is less prone to social desirability bias. Since all of the values are potentially

328   viewed as socially desirable, asking about each one individually could lead participants to simply

329   state that all are important. Asking them to choose between them, however, forces them to identify

330   those that are of utmost priority, even if all could be seen as desirable. Second, this type of question

331   is likely more comparable across individuals. Likert-scales can be interpreted differently by

332   different people, as the dividing line between agreeing "completely" and "somewhat" is less

333   objectively obvious than what it means to prefer one thing to another. Finally, this type of measure

334   is theoretically supported by much of the literature on the link between values and environmental

335   behavior. Steg (2016) argues that the link between environmental values and related behaviors is

336   mitigated when individuals are operating in a choice environment where competing values are also

337   at play. According to Schwartz, "attitudes and behavior are guided… by tradeoffs among

338   competing values that are implicated simultaneously" (1996, p.121). Values may be culturally

339   shared, but individuals prioritize those values differently, leading to different individual choices

340   and actions in practice (Steg et al. 2014b). Thus, it is an individual's relative prioritization of

341   values, not their absolute magnitude (which is difficult to measure in a comparable way across

342   individuals anyway) that is the relevant driver of environmental behaviors.

343

344   This approach is similar to the strategy employed by Agrawal, Chhatre & Gerber (2015) in which

345   they ask respondents what reason is more important for conserving forests and force them to

346   choose between economic and environmental reasons.[8] Our measurement strategy differs in that

347   it focuses specifically on values and includes a wider variety of values, based loosely on

348   categorizations provided by the previous literature on values. An implication of this measurement

349   strategy is that identification with one value is mechanically linked to the measure of others. Thus,

350   an increase in the prioritization of environmental values must, by necessity, correspond with a

351   decrease in the prioritization of other values. However, given the theoretical justification for a

352   focus on relative prioritization of values, rather than absolute agreement with them, we view this

353   as a design feature, rather than a bug, of our measurement strategy.

354

355   The survey also included five questions relating to environmental and social beliefs, asking

356   respondents to what extent they agreed with various statements. These were designed to assess

---

[8] Agrawal, Chhatre & Gerber (2015) also mention in a footnote that they piloted a version of the question that allowed respondents to choose "both." When they did so, nearly all of the participants chose that option. This demonstrates the potential for social desirability bias in questions that do not require trade-offs between competing values.

357   two main perceptions that were deemed likely to change as a result of a PES-like program, based

358   on the two major critiques in the literature identified earlier in this paper. The environmental beliefs

359   questions aim to assess people's perceptions of a trade-off between environmental conservation

360   (biospheric values) and economic growth (egocentric values). The social beliefs questions aimed

361   to gauge participants' views on inequality and egalitarian norms.

362

363   **4.4 Analytic Methods**

364   We assess the effects of the intervention on self-stated environmental and social values and beliefs

365   using a difference-in-differences analysis, comparing the change in relevant survey responses in

366   the treatment group to the change in those same questions in the control group (for all those who

367   answered the questions at both baseline and endline). This difference-in-differences approach is

368   preferable, because it does not assume that the treatment and control group would have been

369   identical absent the intervention, only that the trend would have been similar (Angrist & Pischke

370   2008). The parallel trends assumption is often violated when there is some sort of selection bias

371   into the treatment group that is endogenous to the outcome variables (Besley & Case 2000).

372   However, randomization into the treatment group solves the selection bias problem (Duflo &

373   Kremer 2005). The combination of randomization with difference-in-differences is particularly

374   robust, as the randomization means there are likely to be no systematic differences in unobservable

375   characteristics of the sort that could violate the parallel trends assumption. The intervention was

376   cluster-randomized at the village level, but balance tests suggest that the treatment and control

377   group do not differ significantly with respect to most key variables at the household level (See

378   Appendix A).

379

380   For the purpose of these analyses, the data were stacked, meaning they were structured as a panel

381   dataset, with two observations for each household: one from the pre-treatment (baseline) survey

382   and one from the post-treatment (endline) survey. For each model, we restrict the sample to only

383   those households who answered each question at both baseline and endline. In the basic difference-

384   in-differences model, each hypothesized effect of the intervention is regressed on a simple model

385   including three explanatory variables: a dummy for whether the observation was in the treatment

386   group or not, another indicating whether the observation was from the baseline or endline survey,

387   and finally, an interaction term between the treatment and endline variables. The coefficient on

388   this interaction term represents the effect of the intervention (Angrist & Pischke 2008; Puhani

389   2012).

390

391   *4.4.1 Intent-to-Treat Approach*

392   To explore the effectiveness of the intervention as delivered to the whole population (ie comparing

393   those in the control communities to all those who were randomized into the treatment group

394   regardless of whether they entered into an agreement), we first ran basic intent-to-treat models. In

395   the equation below, we describe the basic linear version of our difference-in-differences models,

396   using an intent-to-treat approach. Y_{ivt} is the outcome variable for person *i* in village *v* at time *t*. D_v

397   represents the treatment, T_t represents the post-treatment period, and the interaction term, $D_v \cdot T_t$

398   takes on the value of 1 only for observations from the treatment group that were surveyed in the

399   post-treatment period. The coefficient δ is the estimator for our treatment effect: the difference

400   between the difference in the treatment group after the intervention and the difference in the control

401   group over the same time period. As treatment, endline and the interaction are included in the

402   model, the de facto reference category is individuals in the control group at baseline.

403
$$Y_{ivt} = \alpha + \beta D_v + \gamma T_t + \delta(D_v \cdot T_t) + \varepsilon_{ivt}$$

404   While the equation above represents the basic linear model specification, our final models take the

405   form of either ordered logit (in the case of the categorical outcome variables) or logit (in the case

406   of the binary outcome variables) models, all with clustered standard errors by village. Results from

407   the basic intent-to-treat models can be found in Appendix C.

408

409   This basic intent-to-treat analysis applies the difference-in-differences analysis to a comparison of

410   the time trend between all observations in the control group and all observations in the treatment

411   group, including those who did not choose to sign up for agreements through the program. This is

412   an appropriate approach because we wish to understand the overall impact of the program

413   (including the effect of the offer itself and its likelihood to be adopted). It is also appropriate due

414   to the likelihood that there are spillover effects within the treatment group, whereby those who did

415   not directly participate as an agreement-holder might still be affected, for example through the

416   spread of social norms from those in their village who did participate or through the effect of the

417   information sessions delivered in all treatment villages.

418

419   In the case of the outcomes derived from the respondent's prioritization of environmental and

420   social values, we also include one additional covariate to capture some heterogeneity in the number

421   of responses offered. While the question asked the respondent to choose only the top two priorities,

422   in some cases enumerators allowed respondents to name three. Thus, we include a control variable

423   for the number of responses given, as this of course directly affects the likelihood of choosing any

424   particular option from the list. Unsurprisingly, this variable is highly significant as a predictor of

425   choosing any particular response. Our main results are consistent whether this additional variable

426   is included or not. No other covariates are included in the main model described above.

427

428   *4.4.2 Intent-to-Treat Plus Matching*

429   The randomized nature of the program implementation should eliminate the need to include control

430   variables (Mutz 2011). However, because the subsample that received the values questions was

431   not randomly selected and did differ slightly from the broader sample, this introduces the

432   possibility that the members of the treatment and control groups who were included in the final

433   sample differ in some systematic way. Though we have no reason to suspect that there is selection

434   bias into this smaller sample that is systematically related to our outcomes of interest, we cannot

435   wholly rule it out. Thus, to address this possibility, we also include a matching analysis to further

436   confirm the validity of our results. While matching alone cannot guarantee causal inference, it may

437   be combined with traditional ITT estimates to strengthen confidence in the results (Sekhon 2009).

438   Using Stata's psmatch2 command and the mahalanobis distance matching specification with

439   replacement (Leuven & Sianesi 2018), we identify matched pairs between the treatment and

440   control group. The mahalanobis distance matching uses a specified set of covariates from the

441   baseline survey, based on prior research into the key determinants of taking up the agreements

442   (Grillos 2017).

443

444   The covariates used in the matching include demographic controls (the age and education of the

445   head of household) as well as a combination of financial factors (formal land ownership, cattle

446   ownership, the number of rooms in the home, whether the household has alternative sources of

447   income apart from that derived from the land, whether anyone in the household has taken out loans

448   in recent months, trust in institutions), community involvement (participation in community work

449   projects, generations living in the community, participation in the formal community decision-

450   making body), and environmental values (inability to identify forest benefits, whether they

451   prioritized environment as a value at baseline, and agreement with various statements about

452   environmental conservation). These covariates were used to identify a matched sample based on

453   baseline characteristics, which were then incorporated into the basic intent-to-treat regression

454   models using frequency weights. (Full output of the regression models based on the matched

455   sample can be found in Appendix E.)

456

457   *4.4.3 Instrumental Variables Approach (Take-up)*

458   The intent to treat model considers outcomes of all households in the treatment community the

459   same, regardless of whether they took up the treatment or not. However, if we believe that the

460   intervention should only have an effect on those who directly entered into *Watershared*

461   agreements, then it is appropriate to instead calculate the Complier Average Causal Effect

462   (CACE). In our case of two-sided noncompliance, one widely accepted method of estimating the

463   effect of the treatment on the treated is to use treatment assignment as an instrumental variable

464   predicting take-up (Gerber & Green 2012; Glennerster & Takavarasha 2013). Because treatment

465   assignment was randomly assigned and affects outcomes through its effect on actual treatment, it

466   is an ideal instrumental variable. It can be used to estimate the treatment effect through a two-stage

467   least squares regression process in which we estimate predicted take-up as a function of treatment

468   assignment, and then use that predicted take-up as the key predictor of our outcomes of interest.

469

470   However, this approach assumes that the instrumental variable (treatment assignment) can only

471   affect outcomes through take-up of the *Watershared* agreements (Gerber & Green 2012;

472   Glennerster & Takavarasha 2013). Thus, this approach is likely inappropriate in the case of this

473   intervention, where treatment assignment means exposure to framed information sessions with

474   potential effects on those in the treatment group, even if they do not ultimately take up agreements.

475   We include the instrumental variables approach mainly as a robustness check related to the issue

476   of two-sided noncompliance. We apply it using Stata's ivregress command, specifying treatment

477   assignment as the instrument for agreement take-up. The instrumental variables regressions can be

478   found in Appendix D, and their results are consistent with those of our basic intent-to-treat models.

479

480   *4.4.4 Predicted Probabilities and Other Robustness Checks*

481   Some scholars argue that, in logit models, interaction terms should not be interpreted the same

482   way as in other models, and that a statistically significant interaction term is neither necessary nor

483   sufficient for a true interaction to exist (Ai & Norton 2003; Berry et al. 2010). Instead they

484   recommend focusing on predicted probabilities. To address this potential critique, for our two

485   binary outcome variables (relating to the prioritization of environmental and social values,

486   respectively), we also confirm our main results using predicted probabilities (Berry et al. 2010).

487   See Appendix F for the test of second differences confirming our finding with respect to

488   environmental values. Though not presented in the paper, we have also confirmed that results are

489   robust to the use of 2 or 3 nearest neighbors, as opposed to 1, to the application of the instrumental

490   variables regression to the matched dataset, and to a matched comparison of endline values only

491   as opposed to the difference-in-differences estimator.

492

5. Results

Using pre- and post-intervention data from a randomized controlled trial, we measured the causal effect of *Watershared* on environmental and social values and beliefs. In Table 1 below, we summarize the findings to come out of three different models: (1) an approach that uses treatment assignment as an instrumental variable to measure the effect of entering into agreements (i.e. take-up), (2) an intent-to-treat approach that measures the effect of being in a village where agreements were offered, irrespective of individual take-up, and (3) the intent-to-treat model applied to a matched sample, where the treatment and control group has been selected to be as similar as possible, based on baseline characteristics previously shown to influence take-up of agreements. The first row in Table 2 shows the mean value of each variable at baseline (for the full sample used in analysis), and the subsequent rows show the treatment effect on that outcome variable that is attributable to the intervention, as predicted by each analytical approach. (Full output from the regression models used to generate this table can be found in Appendix C-E.)

Table 2: Synthesis of the model results

	Environ. Values	Environmental Beliefs		Social Values	Social Beliefs		
	Prioritizes environment as value for kids	"Must harm environment to improve life"	"Environment improves incomes"	Prioritizes sharing/ altruism as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Government responsible address inequality"
Baseline Mean	0.414	1.432	4.621	0.237	2.886	4.636	3.320
Take-Up	0.542*	-0.143	-0.020	-0.440*	0.187	1.806***	1.491*
Intent to Treat	0.199**	-0.058	-0.004	-0.142*	0.258	0.513***	0.485*
(+ Matching)	0.285**	-0.075	-0.047	-0.064	0.085	0.632**	0.525+
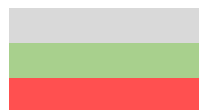
No significant effect
Positively influenced $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$
Negatively influenced

Our results show that the intervention increases the likelihood that people choose environmental protection as a value that should be prioritized for their children, suggesting that *Watershared* may

510   have resulted in 'crowding in' of environmental values. In addition, the intervention alters social

511   beliefs within the treated communities. It tends to support an individualistic and/or meritocratic

512   view as opposed to a more communalized model of local redistribution. At the same time, it also

513   increased the view that the government should play a direct role in addressing inequality. Below,

514   we discuss results with respect to each of our key outcome variables in more detail.

515

516   **5.1 Environmental Values & Beliefs: Crowding in of Environmental Values**

517   Prior to the intervention, fewer than half (~41%) of respondents prioritized "protecting the

518   environment" as one of the most important values to teach their children. After the intervention,

519   people in the treatment group were more likely to prioritize environmental protection (Figure 1).

520   This result was both highly significant across all three model types and relatively large in

521   magnitude. The difference in differences is estimated to be 0.285, meaning that the estimated effect

522   of the intervention was for an additional ~28% of participants to prioritize environmental values

523   who previously did not. This demonstrates a "crowding in" of environmental values.

524

525   Figure 1 illustrates the shift in prioritization of environmental values. On the left-hand side, we

526   show (for the matched sample) the raw proportion of participants who rank environmental values

527   above others for (i) the control group, (ii) those in the treatment group who did not take up

528   agreements, and (iii) those in the treatment group who did take up agreements, both before and

529   after the intervention. This shows that the proportion prioritizing the environment increased in the

530   treatment group both for those with and without agreements, although the jump is larger for the

531   agreement-holders. On the right-hand side of Figure 1, we show the predicted proportions based

532   on the intent-to-treat model plus matching. It demonstrates that, on the whole, the intervention

533   group increased their prioritization of environmental values to a statistically significant degree,

534   while the control group remained more or less constant.
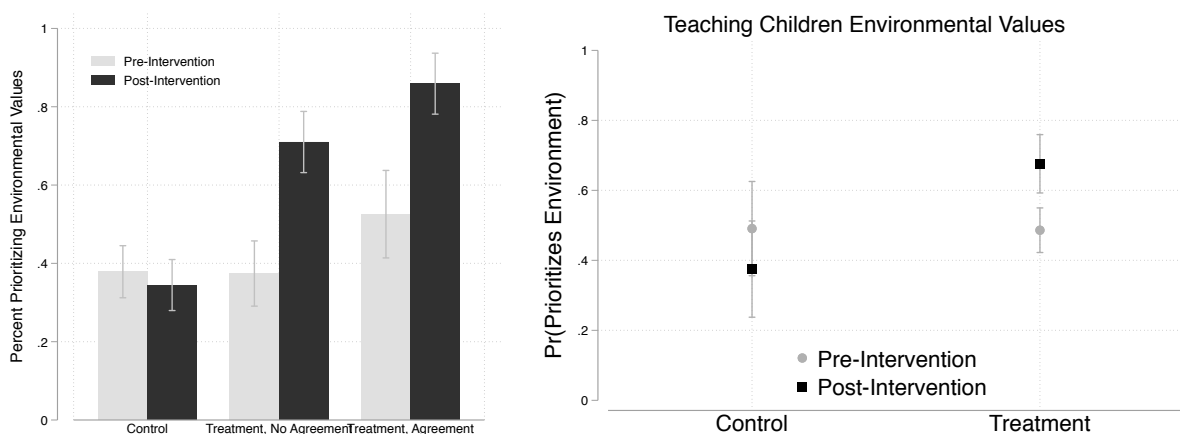
536   Figure 1: Prioritization of Environmental Values
537 (Left: Distribution of Raw Data –Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

539   Descriptive statistics and visual inspection of the raw data suggest that the increase in prioritization

540   of environmental values occurred among both agreement-holders and non-agreement-holders in

541   the treatment group. Community members within the treatment villages who did not sign

542   compensation agreements would still have received the informational components of the

543   intervention, which the NGO delivered through community meetings as part of the initial program

544   offer. Thus, residents of the treatment villages would have been exposed to the reciprocity framing

545   even if they did not receive any compensation, and be subject to any socialization effects that could

546   arise from the communication alone.

547

548   With respect to environmental beliefs, there was no statistically significant effect on how likely a

549   person was to agree with the statements "To improve quality of life, it is necessary to harm the

550   environment" and "We can improve our incomes if we protect the environment". Most people in

551   both groups already strongly agreed with pro-environment statements at baseline (more than 75%

552   choosing the most extreme pro-environmental position on a 5-point likert scale on each question)

553   and these opinions remained relatively stable over the period of the study.

554

555   **5.2 Social Values & Beliefs: Altered Beliefs on Inequality and Government Involvement**

556   In the treatment group, there was a negative shift in the likelihood that a household prioritizes

557   sharing or altruism as a value to teach their children. This effect was statistically significant in two

558   of our three models, but it was not robust to the use of the matched sample. This implies that the

559   shift likely was related to particular characteristics of those sampled in the treatment group, rather

560   than a result of the intervention itself. We thus hesitate to put too much weight on this finding, but

561   it would be consistent with other results discussed below, including a regional trend toward

562   individualism, and the notion that the intervention may have strengthened or accelerated that

563   existing trend.

564

565   With respect to the first of the three questions on social beliefs, the intervention had no effect on

566   agreement with the statement "If a person earns more than others, they must share with the rest",

567   but more than 45% of people at baseline already disagreed or strongly disagreed with that

568   statement. That percentage increased to more than 70% in the treatment group after the

569   intervention but as disagreement increased in the control group as well, this effect was not

570   statistically significant and cannot be directly attributed to the intervention. In both the treatment

571   and control group, individuals are less likely at endline to favor the form of redistribution suggested

572   by this question. This may be interpreted as a general trend toward individualism across the region

573   over time (in both treatment and control groups), unrelated to the *Watershared* intervention.

574

575   Relatedly, after exposure to the intervention, treated respondents were more likely to agree that "If

576   a person works more than others, it is fair that they earn more money" (Figure 2). This result was

577   statistically significant and consistent across all model specifications. While most people agreed

578   with this statement even at baseline, an increase in the percentage of people in the treatment group

579   stating that they "strongly agree" (the most extreme option on a 5-point Likert scale) is what drives

580   the change in the treatment group. This reflects an increased identification with the notion of

581   "meritocratic inequality." It suggests that the intervention may have further strengthened existing

582   trends toward individualism in the region, as evidenced by the result described in the previous

583   paragraph.
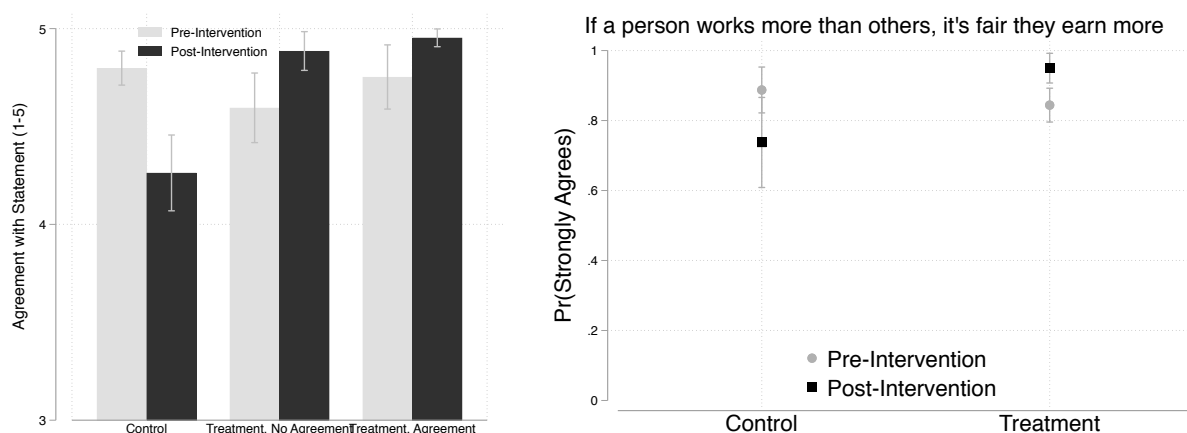
584

585   Figure 2: Agreement with Meritocratic Inequality
586   (Left: Distribution of Raw Data – Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

587

588   However, there was also a positive effect on agreement with the statement "It's the responsibility

589   of the government to reduce inequality of income between people with a lot of money and people

590   with a little money" (Figure 3). In the matching analysis with one nearest neighbor, the statistical

591   significance for this latter outcome drops but is still marginally significant (p=0.069). (Using 2 or

592   3 nearest neighbors, the p value is below 0.05.) This result with respect to government

593   responsibility moves in the opposite direction of the regional trend. (In the control group, support

for government involvement decreases over this same time period.) We view this result as demonstrating an increased belief that the social security net should be transferred from the community to the government level. After experiencing this intervention (presented as a government-NGO partnership), individuals in the treatment communities are more likely to believe that the government should shoulder the responsibility for helping out the poor.
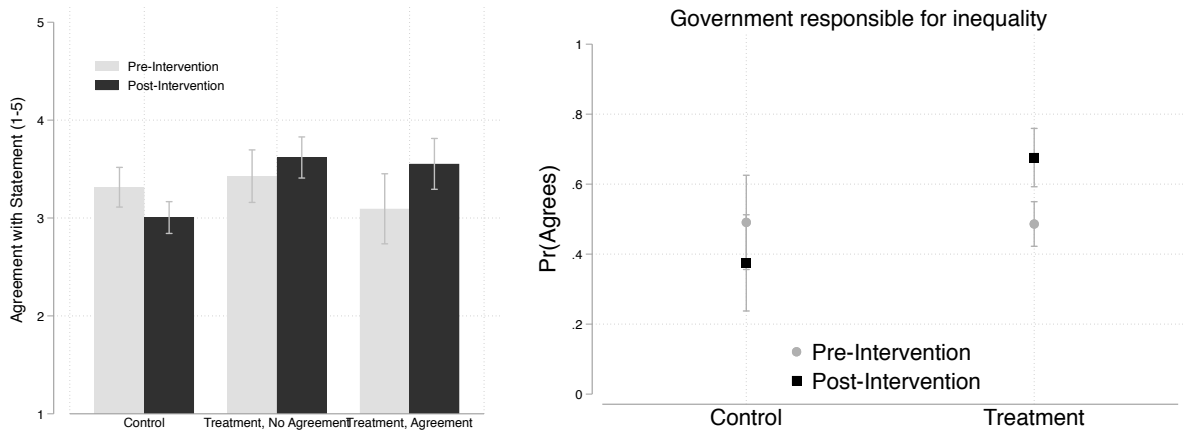
Figure 3 Agreement with Government Responsible for Inequality
(Left: Distribution of Raw Data – Matched Sample, Right: Predicted Probabilities from Matched Regression Model)

Respondents simultaneously felt that income inequalities cannot be the responsibility of local villagers themselves, but that they must be dealt with somehow through government intervention. Taken as a whole, we interpret these results as an indication that the treatment increased the acceptability of government intervention with respect to income inequality, despite a strong regional trend toward disagreement with redistribution in general. The treatment provoked a change in local perception of distributional values, from a community-based system to a state-based system. After exposure to the intervention (presented as a partnership with municipal governments), respondents are more likely than the control group to agree with meritocratic inequality but also to support the role of government in addressing inequalities. In general, the

612 program had a countervailing effect on the overall regional trend toward individualism and limited

613 government involvement, by increasing the acceptability of government intervention.

614

615 **6. Discussion**

616 Understanding the effects of incentive programs like PES on environmental and social values is

617 important in order to improve the chances for such interventions to make deep and permanent

618 socio-ecological change toward more sustainable development. Two major concerns in the

619 literature have been (i) the risk of crowding out values that are supportive of conservation behavior

620 (Rode, Gómez-Baggethun & Krause 2015, Ezzine-de-Blas et al. 2019) and (ii) the tension between

621 market-based efficiency and the equity of PES interventions (Pascual et al. 2010). Our study

622 speaks to both of these ongoing discussions.

623

624 First, our case illustrates that, contrary to fears around motivation crowding, *Watershared* had a

625 positive influence on self-stated pro-environmental values. Notably, this program influenced

626 environmental values even among those who did not receive any compensation. This suggests not

627 only that the program avoided the crowding out often associated with financial incentives, but that

628 it did so, at least in part, through the introductory information sessions, which included framing

629 related to reciprocity. This complements other literature which emphasizes the role of 'nudges', or

630 subtle contextual cues – as opposed to direct information about outcomes – in influencing

631 environmental behaviors (Thaler & Sunstein 2008, Ölander & Thogersen 2014). Since framed

632 information sessions are generally inexpensive relative to other programmatic design features, this

633 also represents a promising and cost-effective approach for policy-makers.

634

635   Our results also illustrate that the scheme had influences on social beliefs. The program was

636   associated with an increased acceptance of 'meritocratic inequality' but also increased support for

637   government involvement in reducing inequality. Prior work demonstrated that barriers to entry

638   (such as a lack of formal land title) limit the ability of the poorest community members to

639   participate in this program (Grillos 2017), echoing concerns in the PES literature that barriers to

640   entry could potentially exacerbate pre-existing inequalities. But our results here show that even

641   among those in the treatment group who have not entered into *Watershared* agreements, the shift

642   in social beliefs moves in the same direction. That is to say, people who have *not* directly benefitted

643   from the compensations (but have been exposed to the reciprocity framing) *also* strongly agree

644   with the meritocratic inequality statement and simultaneously favor government action on

645   inequality. This echoes another recent study which concluded that payments programs based on

646   meritocratic principles need not be in conflict with equity (Loft et al. 2019).

647

648   Our results with respect to views on the role of government could, at first glance, be interpreted as

649   increased support for redistribution, but our other results contradict the notion that there is support

650   for direct redistribution between people within the community. (Most people in both the treatment

651   and control group disagree that individuals are obligated to share their wealth.) Instead this result

652   seems to speak directly to views about the government itself. This intervention was conducted as

653   a collaborative effort between Natura and several municipal governments. (Though the municipal

654   governments did not directly contribute funds in the early years of the intervention during which

655   these data were collected, the program was always presented to the communities as being

656   conducted in partnership with the municipal government.) As a result of this quasi-governmental

657   intervention, people's views of government and its role have been altered. Contrary to concerns in

658   the literature that disenchantment with program-related inequalities may decrease support for later

659   efforts, in this case we find that there may in fact be greater support for future governmental policy

660   interventions as a result of this particular program.

661

662   The use of a Randomized Controlled Trial to evaluate impacts of this program lends increased

663   internal validity to our study, providing one of the clearest examples of causal inference in this

664   literature to date. However, there are of course still limitations associated with our research design

665   that we wish to acknowledge here. First, the use of self-stated data on values and beliefs is limited

666   by social desirability bias and experimenter demand effects, in that respondents may say what they

667   believe researchers want to hear (Tourangeau et al. 2000). This is somewhat mitigated by the fact

668   that we collect pre- and post- intervention data in both treatment and control groups (since social

669   desirability bias is likely to be at play across all interviews), and the research team made efforts to

670   ensure that the interviewers were not seen as affiliated with the NGO (while Natura did manage

671   the initial hiring of the enumerators, the unaffiliated researchers trained and supervised them).

672   Second, given that this experiment spanned multiple years, we cannot completely rule out the

673   possibility of spillover effects. If control communities heard about incentive programs in other

674   villages, they may perceive that others have opportunities to earn more to which they have not had

675   access – thus decreasing their degree of comfort with the notion of meritocratic inequality. This is

676   of particular concern for the meritocratic inequality finding, since a corresponding *decrease* in

677   agreement in the control group contributes to the statistical significance of the result.

678

679   Our results contrast with other prior studies that found evidence of motivation crowding in

680   conservation incentive programs (Agrawal, Chhatre & Gerber 2015; Chervier, Le Velly & Ezzine-

681   de-Blas 2019; García-Amado, Pérez & García 2013; Moros, Valez & Corbera 2019), and

682   corroborate arguments that fears of motivation crowding in PES may be overstated (Andersson et

683   al. 2018; Handberg & Angelsen 2019; Kaczan, Swallow & Adamowicz 2019). However, we are

684   cautious in generalizing these results to other incentive programs, as we believe contrasting results

685   are due to differences in specific design features. In particular, we believe the use of in-kind

686   compensation, individual-level agreements, and the targeted framing related to local reciprocity

687   norms all likely influenced the results we present here.

688

689   As described earlier, there is reason to believe that both the use of in-kind payments and norm-

690   based framing may reduce the risk of crowding out. Here we demonstrate that these design features

691   may go even farther, leading to a *crowding in* of environmental values. At the same time, the

692   reciprocity framing may also have influenced results with respect to social beliefs. One

693   interpretation of reciprocity is an expectation of fair exchange, including potentially that of reward

694   for effort.[9] The Natura information sessions evoked reciprocity not only with respect to human-

695   environment relations generally but also specifically with respect to the compensations earned

696   through the conservation agreements. If framing successfully engaged internalized reciprocity

697   norms with respect to environmental protection, then perhaps it also led individuals to feel more

698   entitled to the goods earned through the agreements – and therefore more comfortable with any

699   potentially unequal distribution resulting from it.

700

---

[9] This interpretation is distinct from alternative definitions of reciprocity in, for example, the anthropological literature, which views reciprocity not as a direct exchange of goods and services, but rather as an indirect and delayed system of exchange based on trust and internal cohesion.

701 Finally, *Watershared* involves individual land owners entering into agreements. Several studies

702 have suggested that individual payments have different psychological implications for motivation

703 crowding compared with communal payments (Agrawal, Chhatre & Gerber 2015; Kerr, Vardhan

704 and Jindal 2014; Midler et al. 2015; Moros, Vélez and Corbera 2019; Narloch, Pascual and Drucker

705 2012). A recent paper found that communal payments in Mexico had a positive impact on social

706 capital (Alix-Garcia et al. 2018). Programs directed at entire communities may eliminate barriers

707 faced by non-landowners, but on the other hand, community-based management programs are in

708 practice often co-opted by local elites, potentially also resulting in elite capture of benefits

709 (Bardhan & Mookherjee 2000; Iversen et al. 2006). Even if payments are not skewed within

710 communities, they may influence inequality across groups, with benefits accruing

711 disproportionately to wealthier communities relative to the additionality of their conservation

712 (Murtinho & Wolff 2015). On the other hand, individual agreements can increase the perception

713 of individual rights to natural resources and, as observed here, influence local attitudes toward

714 more individualism. We thus recommend caution in assuming that the same results may be found

715 in communal payment settings.

716

717 **5.1 Conclusions**

718 Our results are supportive of the continued use of incentives to promote conservation, and they

719 highlight a particular approach that has successfully increased pro-environmental values (these

720 show 'crowding-in' as opposed to 'crowding out'). We also provide relatively robust evidence

721 about how a carefully designed incentive scheme may influence the perspectives of community

722 members themselves, potentially affecting the acceptance and effectiveness of future policy

723 efforts. This work contributes to a large body of social science research about how the form in

724   which incentives and information are delivered can shape human perceptions and values and

725   thereby subsequent policy intervention. It also engages with a growing, policy-relevant literature

726   on psychology and environmental behavior.

727

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Appendix

1007   **A. Balance Tests - Treatment vs. Control (based on full baseline survey)**

1008

	Control Mean	SD	Treated Mean	SD	p-value
Environmental Values & Beliefs:					
"Must harm environment to improve life" (1-5)	1.41	1.02	1.45	1.07	0.355
"Environment improves incomes" (1-5)	4.63	.85	4.60	.90	0.449
Prioritizes environment as value for kids	.38	.49	.40	.49	0.346
Social Values & Beliefs:					
"Earn more, must share with others" (1-5)	2.83	1.72	2.81	1.71	0.716
"Work more, should earn more" (1-5)	4.69	.89	4.63	.98	0.119
"Government responsible address inequality" (1-5)	3.44	1.51	3.43	1.54	0.810
Prioritizes sharing and altruism as value	.25	.43	.22	.42	0.084[+]
Demographic Controls:					
Age Head of Household	49.87	16.37	49.36	16.37	0.429
Educational Level	4.65	3.47	4.82	3.67	0.225
People in Household	3.50	1.88	3.50	1.81	0.960
OTB Membership	.84	.37	.76	.42	0.000[***]
Hectares of Land Owned	25.43	61.77	26.58	63.04	0.644
Cattle Ownership	.69	.46	.69	.46	0.753
Number of Cattle	12.09	22.60	11.41	17.65	0.384
Perceptions of Current Situation:					
Forest better than 5 years ago	2.19	.81	2.22	.82	0.350
Community care better than 5 years ago	1.97	.77	2.03	.77	0.026[*]

Both water quality & quantity is a problem	.20	.40	.23	.42	0.137
All victims of problem	.87	.33	.90	.30	0.051[+]
All contribute to solution	.75	.43	.78	.42	0.205
Total Observations (exact n varies by variable)	1,158		1,443		

1009   [+] $p < 0.1$, [*] $p < 0.05$, [**] $p < 0.01$, [***] $p < 0.001$

1010

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1012 **B. Balance Tests – Included vs Excluded from Analyses (based on baseline survey)**

	All Baseline		Re-surveyed only			Values Questions Subset		
Environmental Values & Beliefs:	Mean	sd	Mean	sd	p-value	Mean	sd	p-value
"Must harm environment to improve life" (1-5)	1.43	1.05	1.43	1.06	0.797	1.48	1.12	0.414
"Environment improves incomes" (1-5)	4.62	.88	4.62	.88	0.651	4.63	.87	0.770
Prioritizes environment as value for kids.	.39	.49	.40	.49	0.126	.41	.49	0.427
Social Values & Beliefs:								
"Earn more, must share with others" (1-5)	2.82	1.72	2.80	1.70	0.472	2.89	1.73	0.435
"Work more, should earn more" (1-5)	4.66	.94	4.64	.98	0.266	4.64	.97	0.675
"Government responsible address inequality"(1-5)	3.43	1.52	3.44	1.53	0.654	3.32	1.57	0.148
Prioritizes sharing and altruism as value	.24	.42	.24	.42	0.991	.23	.42	0.796
Demographic Controls:								
Age Head of Household	49.59	16.37	49.62	15.44	0.909	49.27	16.69	0.700
Educational Level	4.74	3.58	4.85	3.54	0.053^{+}	4.84	3.58	0.589
People in Household	3.50	1.84	3.66	1.78	0.000^{***}	3.43	1.76	0.457
OTB Membership	.80	.40	.82	.39	0.001^{**}	.78	.41	0.411
Hectares of Land Owned	26.07	62.46	26.85	64.10	0.399	32.36	84.57	0.051^{+}
Cattle Ownership	.69	.46	.75	.43	0.000^{***}	.69	.46	0.952
Number of Cattle	11.71	20.00	13.10	21.42	0.000^{***}	13.68	24.97	0.055
Perceptions of Current Situation:								
Forest better than 5 years ago	2.20	.81	2.22	.81	0.148	2.19	.83	0.805
Community care better than 5 years ago	2.00	.77	1.99	.77	0.232	1.97	.77	0.433
Both Water quality & quantity is a problem	.22	.41	.22	.41	0.751	.20	.40	0.367
All victims of problem	.89	.32	.89	.31	0.863	.86	.35	0.150
All contribute to solution	.76	.42	.76	.42	0.915	.76	.42	0.969
Treatment:								
Treatment Group	.55	.50	.58	.49	0.000^{***}	.69	.46	0.000^{***}
Agreement-Holder (Treatment Group only)	.38	.49	.47	.50	0.000^{***}	.40	.49	0.528
Total Observations (exact n varies by variable)	2,601		1,672			333		

1013 (p-values compare each subsample to the rest of the households picked up in the baseline survey)
1014 $^{+}p < 0.1,\ ^{*}p < 0.05,\ ^{**}p < 0.01,\ ^{***}p < 0.001$

C. Basic Intent to Treat Regressions: Environmental & Social Values & Beliefs

	Environmental Beliefs & Values			Social Beliefs & Values			
	(1) "Must harm environment to improve life"	(2) "Environment improves incomes"	(3) Prioritizes environment as value for kids	(4) "Earn more, should share with others"	(5) "Work more, should earn more"	(6) "Government responsible address inequality"	(7) Prioritizes sharing/ altruism as value for kids
TreatmentEndline	-0.157 (0.182)	-0.0281 (0.189)	0.881** (0.328)	0.326 (0.338)	1.971*** (0.564)	0.608* (0.304)	-0.724* (0.362)
Treatment	0.00526 (0.151)	0.196 (0.143)	0.391 (0.257)	-0.0223 (0.279)	-0.106 (0.330)	-0.00324 (0.239)	-0.109 (0.286)
Endline	0.279* (0.138)	-0.287+ (0.158)	0.00453 (0.259)	-0.863*** (0.262)	-0.560+ (0.291)	-0.416* (0.199)	0.378 (0.273)
NumResponses			0.919*** (0.187)				1.144*** (0.227)
Constant			-2.450*** (0.435)				-3.379*** (0.518)
c1	1.458*** (0.117)	-3.352*** (0.147)		-0.757** (0.238)	-2.996*** (0.268)	-1.662*** (0.202)	
c2	2.300*** (0.135)	-2.884*** (0.143)		0.0185 (0.240)	-2.585*** (0.302)	-0.902*** (0.183)	
c3	2.791*** (0.151)	-2.354*** (0.122)		0.162 (0.245)	-2.344*** (0.285)	-0.174 (0.162)	
c4	3.527*** (0.191)	-1.177*** (0.116)		1.214*** (0.250)	-1.703*** (0.279)	0.968*** (0.169)	
Observations	3290	3290	642	666	664	662	642

Standard errors in parentheses

$^+ p < 0.1, \, ^* p < 0.05, \, ^{**} p < 0.01, \, ^{***} p < 0.001$

D. Instrumental Variable Regression: Environmental & Social Values & Beliefs

	Environmental Beliefs & Values				Social Beliefs & Values		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Must harm environment to improve life"	"Environment improves incomes"	Prioritizes environment as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Government responsible address inequality"	Prioritizes sharing/ altruism as value for kids
TakeUp	-0.143	-0.0195	0.543*	0.187	1.806***	1.491*	-0.440*
	(0.181)	(0.188)	(0.231)	(0.692)	(0.454)	(0.608)	(0.196)
Treatment	0.0134	0.0535	0.0933	-0.0514	-0.0827	-0.0570	-0.0194
	(0.0646)	(0.0481)	(0.0600)	(0.216)	(0.109)	(0.173)	(0.0522)
Endline	-0.0195	-0.0879	0.00419	-0.719***	-0.412***	-0.330+	0.105*
	(0.0669)	(0.0726)	(0.0620)	(0.205)	(0.125)	(0.178)	(0.0534)
NumResponses			0.184***				0.229***
			(0.0332)				(0.0407)
Constant	1.424***	4.590***	-0.0161	2.922***	4.693***	3.360***	-0.205*
	(0.0531)	(0.0417)	(0.0840)	(0.185)	(0.0890)	(0.128)	(0.0927)
Observations	3290	3290	642	666	664	662	642

Standard errors in parentheses
$^+ p < 0.1, \, ^* p < 0.05, \, ^{**} p < 0.01, \, ^{***} p < 0.001$

Note: The use of the matched sample combined with the instrumental variable regression produces results consistent with this table, except that the coefficient on "prioritizes sharing" is no longer significant.

E. Matching Analysis (Nearest Neighbors=1): Environmental & Social Values & Beliefs

	Environmental Beliefs & Values			Social Beliefs & Values			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	"Must harm environment to improve life"	"Environment improves incomes"	Prioritizes environment as value for kids	"Earn more, should share with others"	"Work more, should earn more"	"Government responsible address inequality"	Prioritizes sharing/ altruism as value for kids
TreatmentEndline	-0.239	-0.144	1.246**	0.136	2.280***	0.672+	-0.330
	(0.222)	(0.226)	(0.392)	(0.421)	(0.682)	(0.369)	(0.427)
Treatment	0.188	0.177	0.182	0.188	-0.375	0.0299	-0.0512
	(0.165)	(0.174)	(0.313)	(0.301)	(0.378)	(0.343)	(0.357)
Endline	0.377*	-0.233	-0.338	-0.632+	-1.032*	-0.431	0.122
	(0.186)	(0.196)	(0.361)	(0.370)	(0.486)	(0.304)	(0.401)
NumResponses			1.002***				0.927**
			(0.256)				(0.282)
Constant			-2.462***				-2.984***
			(0.554)				(0.620)
c1	1.720***	-3.341***		-0.463+	-3.264***	-1.699***	
	(0.130)	(0.194)		(0.276)	(0.305)	(0.317)	
c2	2.552***	-2.951***		0.210	-2.955***	-0.910**	
	(0.153)	(0.174)		(0.285)	(0.331)	(0.313)	
c3	2.972***	-2.447***		0.376	-2.743***	-0.0724	
	(0.175)	(0.161)		(0.282)	(0.334)	(0.285)	
c4	3.701***	-1.285***		1.498***	-2.064***	1.031**	
	(0.216)	(0.152)		(0.278)	(0.334)	(0.318)	
Observations	3564	3560	836	864	864	864	836

Standard errors in parentheses
$^{+} p < 0.1$, $^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

Note: Using 2 or 3 nearest neighbors rather than 1 produces results consistent with this table (and increases statistical significance in model 6.)

F. Test of Second Differences: Prioritization of Environmental Values

	Pr(Prioritizes Environmental Values)	Test of First Difference	Test of Second Difference
Control Group			
Pre-Intervention	0.44 (0.06)	$0.36 - 0.44 =$ -0.08 (p=0.342)	$0.21 - {-0.08} =$ 0.28^{**} (p=0.001)
Post-Intervention	0.36 (0.07)		
Treatment Group			
Pre-Intervention	0.48 (0.04)	$0.69 - 0.48 =$ 0.21^{***} (p=0.000)	
Post-Intervention	0.69 (0.04)		

Standard errors in parentheses
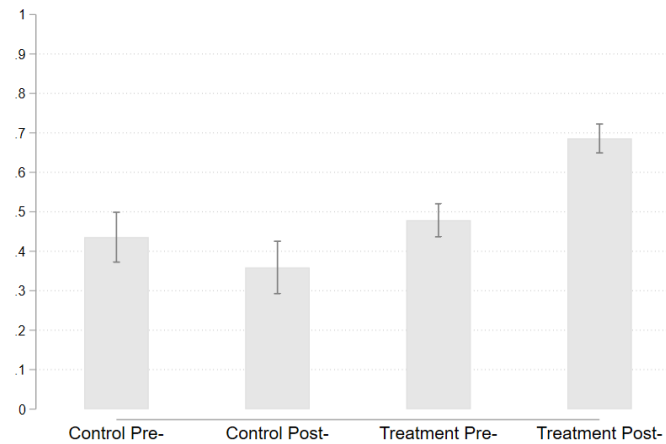$^{+}p < 0.1,$ $^{*}p < 0.05,$ $^{**}p < 0.01,$ $^{***}p < 0.001$

Figure 4: Predicted Probabilities, Prioritizing Environment