

Single large or several small? Willingness to pay for conservation in the Río de la Plata grasslands

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Abstract: We use a contingent valuation survey to estimate Uruguayan households' willingness to pay (WTP) for a payment-for-ecosystem-services (PES) program targeting 320,571 hectares of natural grassland in the Río de la Plata region, combining a national in-person probability sample (n=400) with a larger online non-probability sample (n=2,994). In both samples respondents answer a referendum-style payment question with an assigned price. Within the online sample we vary the spatial configuration of the protected area and

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the payment horizon (one, five, or ten years). In the former, respondents were randomly assigned to two large zones versus fourteen smaller, dispersed zones covering the same total hectares, bringing the ecological Single Large or Several Small (SLOSS) debate into stated-preference valuation for the first time. Across the full sample we test the sensitivity of WTP estimates to respondents' self-reported certainty. The median household is willing to pay UYU 371.70 (USD 9) per month in the in-person probability sample. We find no statistically detectable difference in median WTP between the two spatial configurations, across payment horizons, or between the in-person and online samples. The in-person/online comparison confounds sampling design with survey mode, and the horizon tests rest on subsamples of 431–544, so those two nulls are not evidence of equivalence. WTP estimates remain stable under moderate certainty corrections but weaken under more aggressive recoding. These findings point to substantial public support for financing grassland conservation, but limited sensitivity of stated preferences to spatial and temporal design features that were not explained to respondents.

Keywords: Contingent valuation; Biodiversity; SLOSS; Habitat fragmentation; Payment for ecosystem services; Uruguay.

1 Introduction

Natural grasslands constitute one of the planet's most extensive biomes. Including prairies, savannas, steppes, and meadows, they cover roughly 20-40% of the Earth's terrestrial land surface (Dixon et al., 2014). Moreover, they provide essential services for societal well-being, supporting the livelihoods of millions of people (White et al., 2000). These services include the provision of basic inputs for the production of food, fibers, medicines and energy, as well as regulating services such as carbon sequestration and the control of erosion and agricultural runoff, cultural services such as recreation and ecotourism, and supporting services such as the provision of habitat for diverse species of birds, amphibians, mammals, plants, insects, and soil microarthropods (Havstad et al., 2007; Sala et al., 2017; Sala and Paruelo, 1997).

Many biodiversity-related services provided by natural grasslands have public goods characteristics. This situation creates challenges for biodiversity conservation on private lands because, in the absence of markets for these services, private owners cannot profit from providing them. As a result, the value of these services is not internalized in their land-use decisions, and they are underprovided.

Policy makers could correct this market failure if they knew how much households value biodiversity in natural grasslands. Nevertheless, the same market failure prevents them from easily observing households' preferences for these goods and services. Providing conservation policy makers with non-market estimates of households' willingness to pay for biodiversity conservation in natural grasslands could overcome this barrier, as well as help them make a more compelling case for public investment in biodiversity conservation to finance officials.

This paper provides evidence of households' willingness to pay for the conservation of biodiversity in Río de la Plata grasslands. Building on the consideration of a PES program by Uruguayan biodiversity conservation authorities, we conduct a contingent valuation survey among Uruguayan households and ask them for their willingness to contribute to a fund to cofinance the PES scheme. We find that the median Uruguayan household is willing

to pay ~USD 9 per month (UYU 371.7) to finance a scheme that targets 320,571 hectares of natural grasslands in private lands, divided into two areas located in different parts of the country.

This paper makes the following contributions to the literature. First, we provide the first estimates of household willingness to pay for natural grassland conservation in the Río de la Plata, and we do so for a conservation problem that differs from the one this literature has mostly addressed. Eleven of the twelve primary stated-preference studies we identify are set in European agricultural landscapes (the exception is Resende et al., 2017, on a protected area in the Brazilian Cerrado), where the good valued is the maintenance of semi-natural grassland against abandonment or intensification. That is, a cultural landscape sustained by farming. In the Río de la Plata the threat runs in the opposite direction: native grassland is being converted to forestry plantations, crops and sown pasture, so the good is the retention of a native ecosystem rather than a farmed one. These studies also differ in how biodiversity enters the valuation: some treat it as an attribute whose level varies across respondents (Barkmann and Zschiegner, 2010; Christie and Rayment, 2012; Hasund et al., 2011; Hynes and Campbell, 2011), others use the extent or conservation state of grassland habitat as its indicator (Hanley et al., 1998; Mazzocchi and Sali, 2018; Rewitzer et al., 2017), and others value conservation programs or agricultural landscapes as bundles in which biodiversity is described but never separately priced (Cortignani et al., 2018; Garrod and Willis, 1995; Marzetti et al., 2011; Meyerhoff et al., 2012; Resende et al., 2017). Our design belongs to the second and third of these approaches: we value a program that conserves grassland habitat identified as critical for priority species, rather than treating biodiversity itself as a measured and varying attribute.

The absence of estimates for this region is particularly consequential in the Río de la Plata, one of the world's largest temperate grassland ecosystems: grasslands are a basic part of the economy and landscape of Argentina, Uruguay and Rio Grande do Sul (Brazil), and governments in the region face urgent social needs that compete with conservation for limited public funds.

A second contribution of this paper is that we bring the classic but still active question of Single Large or Several Small (SLOSS) protected areas (Diamond, 1975; Simberloff and Abele, 1976; Fahrig, 2013) into stated-preference valuation. The SLOSS choice is a relevant one for conservation agencies, and public willingness to fund either option may differ. Just as recent landscape experiments have isolated the effect of patch configuration from habitat amount on species occurrence (Fletcher et al., 2023), we isolate the effect of spatial configuration from total protected area on stated preferences. To our knowledge, this is the first study to test whether households value changes in the spatial configuration of a conservation program while holding the total protected area constant.

Third, we provide evidence on whether stated preferences for conservation vary with the duration of the payment commitment. Existing studies have focused primarily on recovering discount rates or testing intertemporal consistency (Vásquez-Lavín et al., 2019, 2021; Grammatikopoulou et al., 2020a, 2020b) rather than on how WTP itself responds to commitment length. This has direct implications for the financing and feasibility of long-term PES programs. We address it by varying the initial commitment across one-, five-, and ten-year periods, each with the possibility of renewal.

Fourth, we examine whether stated preferences differ across sampling strategies and survey modes, comparing a national in-person probability sample with a lower-cost, social-media-recruited online sample. WTP estimates may differ systematically between probability-based and non-probability samples (Maguire, 2009; Bell et al., 2011; Whitehead et al., 2023; Sandstrom-Mistry et al., 2023). The former is generally regarded as the benchmark for drawing inferences about a target population, but it is substantially more costly than online opt-in recruitment. This creates a practical trade-off central to applied stated-preference research given the growing reliance on lower-cost online recruitment.

Finally, we assess the sensitivity of our WTP estimates to respondents' stated degree of certainty in their responses, under progressively more conservative or alternative assumptions regarding the informational content of low-certainty responses. By doing this, we address the potential overestimation of WTP arising from the hypothetical nature of the

valuation exercise, one of the main concerns in contingent valuation studies (Seip and Strand, 1992; Loomis et al., 1996; Brown et al., 2003; Hausman, 2012), using numerical certainty scales, one of the two prominent approaches together with cheap talk (Vásquez-Lavín et al., 2016).

The results show a positive and statistically significant median WTP, but no statistically detectable difference between the two spatial configurations, across payment horizons, or between in-person and online samples; the last comparison cannot isolate sampling strategy from survey mode. WTP is stable under moderate certainty adjustments and weakens under more aggressive recoding.

From a policy perspective, the estimates provide the aggregate figure that a request for public conservation funding requires. Because stated values proved insensitive to a spatial design the survey did not explain, conveying the ecological consequences of fragmentation would be a precondition for those consequences to be reflected in public support.

2 Methodology

2.1 Survey design and administration

The study combines a benchmark valuation based on a national probability sample with an experimental online component. These two components place different demands on the data. Estimating the population level of WTP requires a probability sample. The treatment comparisons require random assignment and enough observations per cell. The study was conducted under a fixed budget, which bought us an in-person probability sample of 400 adults, representative of the Uruguayan adult population living in households nationwide. In-person probability interviewing at the scale the experiments require was beyond it. Replicating the full design in person would have needed six cells — two spatial configurations by three payment horizons — leaving fewer than seventy respondents each. Social-media recruitment delivers responses for a small fraction of the per-response cost. We therefore use the in-person probability sample for the benchmark valuation exercise and the online non-probability sample for the experiments. For the benchmark exercise, we conducted a contingent valuation survey eliciting households' willingness to contribute to a

proposed fund to finance a payment for ecosystem services program under consideration by the government. The 400 interviewed adults were presented with the baseline conservation scenario and a one-year renewable payment commitment. Random assignment within the online sample makes the treatment comparisons internally valid. This is true whether or not the sample reproduces the composition of the population.

The in-person sample was drawn from a frame of census zones built from the 2011 Census, using a multistage design with 12 strata and 100 census zones selected with probability proportional to the number of private households; the Supplementary material describes the design in full. Fieldwork was carried out by Equipos Consultores in February 2025.

The online sample was recruited between February and March 2025 through advertisements placed on Facebook and Instagram. The survey was distributed nationwide. The questionnaire reached respondents as an advertisement and was self-administered on the web, yielding 2,994 responses. The platform recorded which advertisement each respondent entered through. This information is unavailable for 304 respondents, 10% of the online sample. Respondents were assigned at random across the thirty combinations of spatial configuration, payment horizon and bid amount by the survey platform's randomizer.

Our questionnaire had three sections. The first collected information on respondents' sociodemographic and household characteristics, with total household income recorded in ten brackets; we classify a household as high income when it reports one of the top five brackets.

The second section contained the valuation exercise. It began by explaining that natural grasslands, locally known as *campo natural*, are the predominant ecosystem in Uruguay and are characterized by native grasses, forbs, shrubs, and, occasionally, trees. The questionnaire explicitly clarified that *campo natural* does not include areas covered by sown pastures established for livestock forage. A follow-up question then asked respondents whether they were able to distinguish *campo natural* from sown pasture.

Respondents were then informed that natural grasslands provide beneficial products and services to human beings, 'known as ecosystem services', and were given six examples, the last of which was grassland as habitat for biodiversity conservation. The questionnaire then focused on the latter, telling respondents that 'Uruguay's natural grasslands provide habitat for nearly 300 species identified as priorities for conservation' and that 'some of these species would be threatened if natural grasslands were not conserved' (wording based on Pérez Rocha, 2020; our translation). It then named five of these species and showed their photographs (Figure 1). The full wording is reproduced in the Supplementary material.

These five species were selected to serve as flagships for the valuation exercise: recognizable, visually salient animals that make concrete an otherwise abstract attribute (habitat for roughly 300 priority species). All five are grassland-dependent species included in the national conservation priority list (Pérez Rocha, 2020), so the set is drawn from the good being valued rather than selected for appeal alone. Respondents were shown the photographs alongside the description of grassland habitat; no species-specific population information was provided, and no individual species was presented as an object of valuation in its own right.

Figure 1. Flagship species of the Uruguayan natural grasslands. Photographs reproduced from the images shown to respondents, which were labelled in Spanish. Top row, left to right: *ave dragón* (Saffron-cowled Blackbird), *ñandú* (Greater Rhea), *viuda blanca* (Black-and-White Monjita). Bottom row: *mulita* (Southern Long-nosed Armadillo), *gato de pajonal* (Pampas cat). Credits where known: *viuda blanca*, D. Gil; *mulita*, Gustavo Casás.

Respondents were then informed that natural grasslands occupied 63.2% of the national territory in 2000 and 55.7% by 2020, supported by the two maps shown in Figure 2, and that this decline is driven by the greater profitability of substituting them with forestry plantations, agricultural crops, and sown pastures. They were also informed that overgrazing represents an additional pressure that degrades grassland in place and is therefore not visible in the Figure 2 maps.

Figure 2. Land cover maps presented in the survey. Land cover in Uruguay in 2000 (left) and 2020 (right). Between the two dates natural grassland declines while cropland and

forestry plantations expand. Authors' elaboration with data from the MapBiomass Pampa Trinacional project. The maps were labelled in Spanish: *Campo natural*, natural grassland; *Cultivos*, cropland; *Forestación*, forestry plantations; *Otros*, other (water bodies, native forest, urban)

After presenting the threats, the questionnaire moved to the central part where the WTP is estimated. The approach chosen was to propose a hypothetical referendum regarding the co-financing of a grassland conservation policy based on the instrument of payments (compensation) for ecosystem services (PES) to landowners. This program, under actual evaluation by the Uruguayan biodiversity conservation officials, would establish a payment to landowners who commit to conserving natural grasslands in selected areas, aiming to incentivize protection. This payment is designed to compensate the owner for the opportunity cost of conservation; specifically, the lost profit from dedicating those hectares to conservation instead of a use with greater private benefit, such as agriculture or forestry.²

The compensation program would be financed by a 'Fund for the conservation of natural grasslands,' with money contributed by the State and managed by an entity (public or private) defined by contributors. Due to the scarcity of public funds, the fund would also require contributions from taxpayers. Citizen contribution would be mandatory only if the majority of citizens voted in favor in a plebiscite. If approved, all families would pay; if rejected, the program would not be implemented, and no one would pay. This simulated referendum approach aims to emulate a real democratic decision regarding an environmental public good, thereby increasing the credibility of the payment scenario and minimizing voluntary payment bias. A single binary choice of this form is incentive compatible provided respondents believe the survey may influence policy (Carson and Groves, 2007). This condition is more plausible

² This opportunity cost naturally arises only in those grassland areas threatened by more profitable alternative land uses, not across all natural grassland hectares.

here than in most contingent valuation applications, since the program was under active evaluation by the national conservation authority at the time of the survey.

The exact question posed was: ‘Would you vote in favor of this environmental compensation program, if your family were required to pay \$X pesos per month to the Fund for the conservation of natural grasslands, for a period of Y years, renewable? (Equivalent to \$12X per year.)’.

This question seeks to estimate both the WTP and the probability of payment acceptance. The amount \$X varied across respondents in both samples, though it was allocated differently in each. The amounts used were UYU 50, UYU 150, UYU 300, UYU 450 and UYU 650 (approximately USD 1.2, USD 3.6, USD 7.3, USD 11 and USD 15.7, respectively, at the average exchange rate for February 2025). These values were determined by calculating the average opportunity cost of land (rental prices) in the selected implementation areas and adjusting the value dispersion through a pilot test. In the online sample respondents were assigned to a bid at random. In the in-person sample the number of interviews per bid was fixed in advance at 60, 80, 120, 80 and 60 respectively. The figure in parentheses is twelve times the monthly bid and does not vary with the payment period.

2.2 Spatial and temporal treatments

The number of years Y (payment periods) also varied randomly, but only for online respondents, who were consulted about participation for 1, 5, or 10 years. In-person respondents were only asked about a 1-year period. In every version, the period was described as renewable, so the treatment varies the length of the initial commitment rather than the point at which the program and its payments would end.

The in-person survey (400 interviews) showed Figure 3, highlighting two prioritization zones: one in the North and one in the East. The two large priority zones were selected through a multicriteria ecological prioritization process. The selected zones retain substantial areas of natural grassland in relatively good condition, are exposed to current or potential land-use transformation pressures, and are ecologically complementary. Together, they

represent five grassland vegetation communities and support 26 priority bird species. The selected zones contain 320,571 hectares of natural grassland.

Figure 3. Maps of the program's target zones — two large zones. On the left, land cover in Uruguay in 2020 with the two large target zones highlighted. On the right, enlarged views of these zones: northern zone above, eastern zone below. The maps were labelled in Spanish: *Campo natural*, natural grassland; *Cultivos*, cropland; *Forestación*, forestry plantations; *Otros*, other (water bodies, native forest, urban).

To study whether the preferences vary with the spatial configuration of the program's targeted area, we presented the online respondents with two different distributions of this area. The first version (1,509 respondents) featured the same Northern and Eastern Zones. The second version (1,485 respondents) presented fourteen smaller zones whose locations were drawn at random from the areas mapped as natural grassland in 2020 (Figure 4). Their total area is identical to that of the two large zones, and this total was stated to respondents in both versions. The two versions were otherwise identical, including the description of how the areas had been selected, so that they differ only in the map displayed.³ The classic position in the SLOSS debate holds that a single large reserve conserves more species than several small reserves of the same total area (Diamond, 1975): larger, less subdivided patches support larger populations, contain proportionally less edge habitat, and permit movement within the protected area. Whether this holds in general remains contested (Simberloff and Abele, 1976; Fahrig, 2013). We take the classic position as the benchmark against which to test public preferences, and hypothesize that median WTP for the two large zones would exceed median WTP for the fourteen dispersed zones.

³ Both versions stated that the areas had been prioritized on ecological criteria and on account of transformation pressure. This corresponds to the multicriteria process applied to the two large zones. The fourteen were drawn at random from areas of natural grassland and were therefore not subject to that process. Holding the description constant is deliberate: describing one configuration as expert-selected and the other as randomly drawn would confound spatial configuration with perceived selection quality.

Figure 4. Maps of the program's target zones — fourteen dispersed zones. Land cover in Uruguay in 2020 with the fourteen dispersed target zones shown as circles in more intense colours. The fourteen zones total the same number of hectares as the two zones in Figure 3. The maps were labelled in Spanish: *Campo natural*, natural grassland; *Cultivos*, cropland; *Forestación*, forestry plantations; *Otros*, other (water bodies, native forest, urban).

Finally, the third section included follow-up and debriefing questions on respondents' certainty, on the credibility and acceptance of the valuation scenario, and on whether the proposed compensation would be sufficient to achieve the program's conservation objective. Respondents who voted against the program were asked to report their reasons, from a list that included motives unrelated to the value of the program itself. No response was excluded on this basis.

2.3 The good being valued

A note on the good being valued is in order. An exercise of this kind values a conservation program and the existing biological diversity it protects, rather than biodiversity levels. In our design, biodiversity is not a measured attribute whose level varies across respondents. The commodity valued here is a biodiversity conservation program to be implemented on the targeted natural grassland, an ecosystem that is habitat for roughly 300 priority species and the exclusive habitat of some of them. The presented counterfactual of the conservation program is explicit: between 2000 and 2020 natural grassland cover contracted from 63.2% to 55.7% of the national territory. We also specified the provision mechanism through which the commodity is delivered, namely compensation to landowners for the opportunity cost of conservation.

Habitat is also the most common way this literature operationalizes biodiversity. In Bartkowski, Lienhoop and Hansjürgens's (2015) review of 123 valuation studies, habitat-based proxies are the single most frequent choice (51 studies), ahead of species-based proxies (43), and contingent valuation pairs with habitat proxies more often than choice experiments do. Two justifications for our choice are the following. First, and most important, habitat area is the currency of the policy instrument itself. The PES contract would pay a landowner for

hectares of *campo natural* retained, but not for an outcome based on a metric of diversity. So valuing hectares values what the program can actually deliver. Second, valuing several attributes simultaneously points to a choice experiment rather than a single referendum question. A multi-attribute design would also have competed for statistical power already committed: we randomize the bid, the spatial configuration and the payment horizon, and adding varying biodiversity attributes would have multiplied the number of cells and further reduced the precision of the treatment comparisons we set out to make.

2.4 Econometric estimation

Because the contingent valuation response is dichotomous, we define $y_i = 1$ when respondent i votes in favor of the conservation program and $y_i = 0$ otherwise. Although individual willingness to pay is not directly observed, it can be inferred from the relationship between the stated response and the proposed payment using the random utility framework (Haab and McConnell, 2002).

Let Δv_i denote the difference in utility between the conservation program, at a requested amount A_i , and the status quo, and assume it is linear, $\Delta v_i = \alpha - \beta A_i$, $\beta > 0$, where respondent characteristics may enter through α . The probability of supporting the program can be estimated using a logit or probit model. We use a logit specification, which assumes that the utility-error difference follows a logistic distribution. The median willingness to pay is the amount that makes the respondent indifferent between supporting and rejecting the program, $WTP = \frac{\alpha}{\beta}$. Standard errors for median WTP are calculated using the delta method, applied to the heteroskedasticity-robust variance–covariance matrix of the logit estimates. The estimates reported for the in-person sample are unweighted.

2.5 Treatment of respondent uncertainty

We incorporate respondent certainty information using four complementary treatments: two symmetric treatments (a weighting scheme and a recoding rule) addressing preference uncertainty; an asymmetric recoding addressing hypothetical bias; and a semi-symmetric treatment combining elements of the symmetric and asymmetric approaches. Comparing

them allows us to assess the sensitivity of WTP estimates to different interpretations of the certainty scale rather than relying on a single behavioral assumption. Empirical comparisons indicate that symmetric treatments of certainty can yield welfare estimates that differ meaningfully from those obtained under asymmetric corrections, without identifying a universally preferred approach (Loomis and Ekstrand, 1998; Shaikh, Sun and van Kooten, 2007; Akter and Bennett, 2013).

The first two treatments are symmetric and serve as robustness exercises grounded in a preference uncertainty framework. Early structural contributions formalize dichotomous-choice responses as probabilistic outcomes arising from uncertainty about underlying preferences, rather than as deterministic reflections of true WTP. From this perspective, both acceptance ('Yes') and rejection ('No') responses may reflect ambivalence or response noise, particularly when stated certainty is low, and therefore contribute symmetrically to the likelihood function (Li and Mattsson, 1995).⁴ In our first symmetric treatment of certainty, each observation receives a weight proportional to the respondent's reported confidence level, $w_i = c_i/10$, where $c_i \in \{1, \dots, 10\}$ is the respondent's stated certainty. This approach treats certainty as a measure of response precision rather than as a basis for recoding. In our second symmetric treatment of uncertainty, low-certainty responses on either side of the dichotomous choice are reclassified as:

$$y_i^* = \begin{cases} y_i & \text{if } c_i > k \\ 1 - y_i & \text{if } c_i \leq k \end{cases} \quad (1)$$

Where $y_i \in \{0,1\}$, ($Yes = 1, No = 0$), and k is a certainty threshold.

Our third treatment of uncertainty is specifically designed to mitigate hypothetical bias by recoding low-certainty acceptance responses (Akter et al., 2008). In this treatment we recode

⁴ Subsequent studies extend this framework by eliciting graded or polychotomous certainty information and integrating it symmetrically into estimation, either through ordered response models or probability-based weighting schemes (Ready, Whitehead and Blomquist, 1995; Ready, Navrud and Dubourg, 2001; Logar and van den Bergh, 2012).

low-certainty affirmative responses as rejections based on a certainty threshold, while rejection responses are left unchanged. Formally,

$$Yes_i^* = \begin{cases} 1 & \text{if } Yes_i = 1 \text{ and } c_i > k \\ 0 & \text{if } Yes_i = 1 \text{ and } c_i \leq k \\ 0 & \text{if } Yes_i = 0 \end{cases} \quad (2)$$

This asymmetric treatment mitigates the empirically documented directionality of hypothetical bias, which primarily manifests as overstated acceptance rather than overstated rejection (Champ et al., 1997; Blumenschein et al., 1998, 2008). Estimates obtained under this treatment can be interpreted as conservative lower-bound measures of willingness to pay.

Finally, we also adopt a semi-symmetric treatment of respondent certainty. Building on both strands of the literature, it combines elements of asymmetric bias correction with insights from preference uncertainty models. More specifically, under this approach, low-certainty ‘Yes’ responses are corrected following the asymmetric calibration, while low-certainty ‘No’ responses are not mechanically reversed but instead treated as weak informational signals. This specification acknowledges the directional nature of hypothetical bias while allowing for uncertainty in both acceptance and rejection decisions. Operationally, low-certainty acceptances are first recoded as rejections. Then, all observations classified as low-certainty rejections — including those just recoded — receive reduced weight in the estimation, reflecting lower informational content without mechanically converting rejections into acceptances. Weights are defined as:

$$w_i = \begin{cases} 1 & \text{if } c_i > k \\ \omega & \text{if } c_i \leq k, 0 < \omega < 1 \end{cases} \quad (3)$$

The weighted log-likelihood function is given by: $\mathcal{L} = \sum_i w_i [Yes_i \ln P_i + (1 - Yes_i) \ln(1 - P_i)]$, where P_i denotes the model-implied probability of acceptance.

Rather than fixing a single cutoff, we report estimates over a grid of certainty thresholds: $k = 1$ to 8 for the symmetric recoding, $k = 1$ to 5 for the asymmetric recoding, and cutoffs from $k < 2$ to $k < 8$ combined with weights ω of 0.30, 0.50, 0.70 and 0.90 for the semi-symmetric

treatment. One observation with missing certainty information is dropped, so all certainty specifications use 3,393 observations, including a specification without any certainty correction that serves as the benchmark for comparison.

3 Results

3.1 Descriptive statistics

We begin by comparing the composition of the samples. Tables 1 to 4 report the balance of covariates across sampling modes, spatial treatments, social-media recruitment channels, and payment horizons.

Table 1 shows that the two samples differ significantly across most sociodemographic and attitudinal characteristics: relative to respondents interviewed in person, online respondents are older, more likely to be female, report higher household income and more tertiary education, live in smaller households, are less likely to report being able to distinguish natural grasslands from sown pasture, and report lower response certainty. Environmental NGO membership and perceived policy sufficiency are not statistically different. Table 3 shows the same pattern across recruitment channels, with Facebook and Instagram respondents differing significantly on most observed characteristics.

Tables 2 and 4 report the two randomized comparisons. Neither the spatial configuration nor the payment horizon shows a statistically significant difference on any of the sociodemographic, household, environmental-knowledge or attitudinal variables considered; all associated p-values exceed 0.10. The payment-horizon comparison is restricted to the online sample, since the horizon was randomized only there.

Taken together, the balance results reveal systematic compositional differences across sampling modes and social-media recruitment channels, but no statistically detectable imbalance across the randomized spatial treatments or payment horizons within the online sample. These findings motivate the inclusion of observed respondent characteristics and recruitment indicators in the pooled models, while supporting the internal comparability of the spatial-treatment and payment-horizon groups.

3.2 WTP estimates

The probability sample provides the benchmark estimate, while the treatment comparisons are made within the online sample. Figure 5 shows the empirical share of 'yes' responses by bid, by sampling design and spatial configuration. Acceptance falls with the bid in every panel, and the panels differ mainly in the level of acceptance rather than in its gradient.

Figure 5. Empirical distribution of 'yes' responses, by sampling design and spatial configuration.

Figure 6 disaggregates further, by payment horizon. A few horizon–configuration cells are not strictly monotonic at every bid, which is unsurprising at these cell sizes. The BID coefficient is nonetheless negative and significant within every subgroup (Tables 5 and 6), so the fitted acceptance probability is decreasing in the bid throughout.

Figure 6. Empirical distribution of 'yes' responses in the online sample, by spatial configuration and payment horizon.

To estimate individuals' WTP for the conservation of natural grasslands, we employ a logistic regression ('logit') model for a dichotomous dependent variable. The dependent variable represents the decision whether or not to support the natural grassland conservation program. The covariates are the monthly contribution requested from the household (BID), our variable of interest, and sociodemographic variables. We first report estimates that exclude the latter, separately by spatial configuration and payment horizon, and then a pooled specification that includes them.

3.2.1 Estimation of WTP for the two large zones

This subsection presents the econometric results for respondents who were shown the conservation configuration consisting of two large priority zones, one located in northern Uruguay and the other in the eastern part of the country. This group includes the 400 respondents interviewed in person as part of the probability sample and 1,509 respondents from the online non-probability sample. Table 5 reports the results separately for the in-

person sample, the full online subsample assigned to this spatial configuration, and the online respondents assigned to payment horizons of one, five, and ten years.

Across all specifications, the BID coefficient is negative and statistically significant, and of broadly similar magnitude across the samples and payment horizons.

The bottom portion of Table 5 reports the estimated median WTP, defined as the monthly household contribution at which the predicted probability of voting in favor of the referendum equals 50%. For the in-person probability sample, which was presented with a one-year payment horizon, the estimated median WTP is UYU 371.70 per household per month. For the full online subsample assigned to the two-large-zone configuration, pooling the three payment horizons, median WTP is UYU 272.86.

The statistical comparisons among the horizon-specific estimates, and between the in-person and online one-year samples, are reported in Table 7.

3.2.2 Estimation of WTP for the fourteen dispersed zones

Table 6 reports the logit estimates for the 1,485 respondents in the online non-probability sample who were shown the conservation configuration consisting of fourteen dispersed priority zones. This spatial configuration was presented only in the online survey. The table reports results for the full subsample and separately for respondents assigned to payment horizons of one, five, and ten years.

The BID coefficient is again negative and statistically significant in every specification. For the full online subsample assigned to the fourteen-dispersed-zone configuration, pooling all three payment horizons, the estimated median WTP is UYU 283.27 per household per month. As reported in Panel C of Table 7, we do not reject equality among the three payment-horizon estimates at conventional significance levels. Comparisons between the two spatial configurations are reported separately in Panel D of Table 7.

3.2.3 Pairwise comparisons of median WTP

Table 7 reports pairwise comparisons of the median WTP estimates presented in Tables 5 and 6. Each comparison is constructed so that, as far as possible, only one design dimension changes at a time. Pairwise differences are calculated as WTP A minus WTP B, and their standard errors are obtained from the delta-method standard errors of the two estimates. Because the compared subsamples are mutually exclusive, the standard error of each difference is calculated as the square root of the sum of the two estimated variances.

Panel A compares the in-person probability sample with the online non-probability sample while holding constant both the spatial configuration and the payment horizon. Respondents in both groups were presented with the two-large-zone configuration and a one-year payment commitment. The estimated median WTP is UYU 371.70 in the in-person sample and UYU 228.07 in the online sample, producing a difference of UYU 143.63. However, the difference is not statistically significant at conventional levels ($z = 1.58$, $p = 0.113$). Because survey mode and sampling strategy vary simultaneously, this comparison should be interpreted as a joint difference between the in-person probability survey and the online non-probability survey, rather than as the isolated effect of either sampling design or survey mode.

Panels B and C examine whether median WTP varies across payment horizons within the online sample. Panel B reports the comparisons for respondents assigned to the two-large-zone configuration, while Panel C reports the corresponding comparisons for respondents assigned to the fourteen-dispersed-zone configuration. In neither spatial configuration do we reject equality of median WTP across the one-, five-, and ten-year payment horizons. Nevertheless, some point estimates differ substantially, particularly between the five- and ten-year horizons in the two-large-zone treatment, and the associated standard errors are relatively large. Therefore, these results should be interpreted as an absence of statistically detectable differences rather than evidence of exact equivalence across horizons.

Panel D compares the two spatial configurations within the online sample, separately by each payment horizon and pooled. None of the horizon-specific differences is statistically significant, and they do not share a common sign: the fourteen-dispersed configuration yields a higher median WTP at one and five years, the two-large configuration at ten. Pooling the

three horizons, the estimated difference is only UYU -10.41 ($p = 0.835$), less than 4% of either estimate.

Overall, the comparisons provide no evidence, at conventional significance levels, that median WTP differs across the two survey samples, payment horizons, or spatial configurations. However, the confidence intervals around several differences remain wide.

3.2.4 Pooled data estimation

To summarize the overall patterns in the combined data and assess whether the bid response varies across samples and experimental conditions, we estimate a series of pooled logistic models. Table 8 presents the results using the combined in-person probability and online non-probability samples. Rather than interpreting the absence of statistically significant differences in Table 7 as evidence that the samples and treatments are equivalent, we use the pooled models as a complementary framework for examining the overall relationship between the requested contribution and referendum support while controlling for observed respondent characteristics.

The table reports three specifications estimated on the same sample. Model 1 includes only a constant and the requested monthly household contribution (BID), applying to the pooled sample the specification used separately in Tables 5 and 6. Model 2 adds indicators for the payment horizons, the spatial configuration, the recruitment channel and membership in the in-person probability sample, their interactions with BID, and the sociodemographic controls listed in the table. The reference categories are a one-year payment horizon, the fourteen-dispersed-zone configuration, and an online respondent recruited through Facebook. Because all respondents in the probability sample were interviewed in person, the indicator for that sample captures the joint difference in survey mode and sampling design; these two dimensions cannot be identified separately. Model 3 retains only the variables significant at the 10% level in Model 2. It is descriptive rather than inferential, because its standard errors and p-values do not account for that selection step.

Across all three models, the BID coefficient is negative and highly statistically significant. This result confirms that a higher required monthly contribution reduces the probability of supporting the conservation referendum. Although the estimated coefficient varies somewhat across specifications, particularly in the full model, its sign and statistical significance remain stable.

None of the interactions between BID and payment horizon, spatial configuration, recruitment channel, or membership in the in-person probability sample is statistically significant. Therefore, the estimates provide no evidence that the BID coefficient differs systematically across these groups. This result should be interpreted as an absence of statistically detectable heterogeneity in price sensitivity, rather than as proof that respondents react identically across samples and treatments.

The main-effect indicators for payment horizon, spatial configuration, recruitment channel, and the in-person probability sample are also not statistically significant in the full model.

Among respondent characteristics, being female, having tertiary education, and belonging to an environmental NGO are positively and statistically significantly associated with support for the conservation program. Environmental NGO membership exhibits the largest estimated association among the sociodemographic variables, but it applies to only 2% of the estimation sample (63 respondents). Therefore, we read its sign and significance more as a validity check than a quantitatively important determinant of support. In contrast, age, high-income status, household size, the number of household members under 18, and response certainty are not statistically significant in the full model.

Perceived policy sufficiency is also positive, large, and highly statistically significant. Respondents who consider the proposed compensation policy sufficient are substantially more likely to support the referendum. This variable was elicited in the debriefing section, after the referendum question, so the association should not be read causally: respondents who voted in favor may subsequently assess the policy as adequate. Response certainty was likewise collected after the vote, so we re-estimated Models 2 and 3 excluding both variables.

The BID coefficient, the implied median WTP and the sociodemographic coefficients are virtually unchanged.

The bottom panel of Table 8 reports the implied median WTP for each specification. All models are estimated on the same 3,168 observations, the sample that remains once respondents who did not report income or whose NGO response was invalid are excluded. In Model 1 the estimated median WTP is UYU 301.92 per household per month (SE 22.21). In Model 2, evaluated for the reference group, it is UYU 210.60 (SE 73.42); in Model 3, evaluated at the covariate means, UYU 300.53 (SE 23.02).

The three figures are not alternative estimates of the same quantity. Those from Models 1 and 3 summarize the pooled sample. The Model 2 figure instead describes respondents recruited through Facebook, facing fourteen dispersed zones and a one-year payment commitment, with the remaining covariates at their sample means. Its level is therefore a reference point rather than an alternative estimate of the pooled median. The same restriction explains its standard error: with BID fully interacted, the main BID coefficient is the price slope for that cell alone and is estimated from a small fraction of the sample.

3.3 Robustness to uncertainty of responses

We did not exclude ‘No’ responses that would conventionally be classified as protest responses. The reason is that the valuation question concerns contributing to the fund under the payment mechanism presented, so a refusal to pay remains relevant for policy implementation regardless of whether it reflects a budget constraint, low perceived benefits, or disagreement with that mechanism. Excluding respondents who reject the payment vehicle would remove part of the observed opposition to the proposed policy and raise the estimated WTP; retaining them is therefore the conservative choice. Our robustness analysis addresses the opposite concern: that low-certainty affirmative responses may overstate willingness to pay.

We checked the robustness of our results to the reported degree of certainty of respondents. Figure 7 reports the distribution of stated certainty levels by bid amount and response type,

focusing on the focal points of the scale (1, 5, 8, and 10). Two patterns stand out. Rejection is a confident decision and largely insensitive to the bid: for 'No' responses, certainty is strongly concentrated at the upper end of the scale at every bid level. Acceptance is not: the share of 'Yes' responses at maximum certainty declines monotonically as the bid rises, shifting towards certainty level 8, while very low certainty levels remain rare throughout. Although the differences are moderate, the patterns are directionally consistent with rational choice and support the internal validity of the contingent valuation responses.

Figure 7. Certainty patterns for 'No' and 'Yes' answers at different bid levels. Share of responses at focal certainty levels 1, 5, 8 and 10, by bid amount, separately for respondents who rejected the bid (left) and accepted it (right).

We apply the four treatments defined in Section 2.5 over the grids of thresholds and weights described there. Median WTP remains positive and statistically significant under continuous certainty weighting and under moderate recoding, and weakens only under the strictest thresholds; Tables S1 to S4 in the Supplementary material report the estimates treatment by treatment.

Certainty corrections affect estimated WTP primarily through the constant rather than through price responsiveness, though this holds over a well-defined range of thresholds rather than universally. Under the symmetric weighting (Table S1) and across every asymmetric threshold (Table S3), the estimated bid coefficient is negative, highly significant and close to its uncorrected value, varying only between -0.00133 and -0.00165 . The movement in estimated WTP is therefore driven almost entirely by the constant, which falls from 0.443 in the original specification to 0.236 at $k = 4$. The deterioration of the bid coefficient under symmetric recoding beyond $k = 4$ (Table S2) is what one would expect mechanically: reversing an increasing share of responses on *both* sides of the dichotomous choice is a form of misclassification of the dependent variable, which attenuates the estimated slope toward zero and makes the ratio α/β progressively less well determined. The peak of UYU 401.57 at $k = 5$ and the loss of significance at $k = 8$ are therefore symptoms of a shrinking denominator rather than of a genuine rise and collapse in willingness to pay.

3.4 An illustrative comparison with the opportunity cost of land

As an illustrative comparison, and not as a benefit-cost analysis, we compare the WTP estimates with the opportunity cost of the land the program would target.

On the benefit side, we aggregate the benchmark estimate from the in-person probability sample, the only sample drawn from a frame that covers the national population. A median WTP of UYU 371.70 per household per month is approximately USD 108 per household per year at the average February 2025 exchange rate of 41.3 UYU/USD. Applied to the 1,373,546 households in Uruguay (Instituto Nacional de Estadística, 2023), this gives an aggregate of approximately USD 148 million per year.

On the cost side, the relevant magnitude is what landowners forgo by retaining natural grassland, which the PES contract must cover. The market rental price of land in livestock use is the natural measure. DIEA reports an average rental price for land destined to livestock production of USD 81 per hectare per year in 2024 and USD 85 in 2025 (DIEA, 2025, 2026). Rents in the departments containing the two priority zones are between USD 83 and USD 120 per hectare per year in both years. Applied to the 320,571 hectares the program would target, the annual opportunity cost is approximately USD 26–27 million at the national livestock average, rising to about USD 38 million at the upper end of the departmental range.

Aggregate household WTP therefore exceeds the opportunity cost of the targeted land by a factor of roughly four to six. The ordering is not sensitive to the choice of estimate: repeating the exercise with the more conservative one-year online estimate of UYU 228.07 per month yields an aggregate of about USD 91 million, still some three times the annual rental cost.

4 Discussion of results

The main contribution of this study is to examine whether households value the spatial design of a biodiversity conservation program when the total protected area is held constant. This brings the classic Single Large or Several Small question into a stated-preference valuation setting. Despite the ecological relevance of habitat configuration, we find no statistically detectable difference in median WTP between a program protecting two large areas and one

protecting fourteen smaller, dispersed areas covering the same total number of hectares. This finding should not be interpreted as evidence that respondents are indifferent between the two configurations, because some of the estimated differences are imprecise and economically meaningful differences cannot be ruled out. Rather, the results indicate that the survey did not reveal a clear WTP premium for the configuration in which the same total area is concentrated in fewer, larger patches.

One possible explanation is that respondents focused primarily on the total amount of land protected and the implementation of the conservation program, rather than on its spatial organization. This interpretation is plausible because the survey deliberately did not explain the ecological consequences of habitat fragmentation. The benefits of a less subdivided habitat (larger populations, species movement with the resulting genetic exchange) as well as the isolation, edge effects and local extinction risks that fragmentation brings, are not necessarily apparent to respondents without additional information (Terborgh, 1976; Gilpin and Diamond, 1980; Higgs and Usher, 1980; Saunders et al., 1991; Hanski, 1998; Fahrig, 2003; Haddad et al., 2003, 2015; Heller and Zavaleta, 2009). Consequently, the absence of a detectable WTP difference may reflect limited public awareness of these ecological mechanisms rather than an absence of social value for less subdivided habitat.

This result has an important implication for conservation policy. When spatial configuration affects ecological effectiveness, policymakers may need to communicate these consequences explicitly rather than assuming that citizens will infer them from maps alone. At the same time, such communication would change the object being valued. The current design identifies preferences for spatial configuration without explaining its ecological consequences, whereas a survey that provides information on expected biodiversity outcomes would estimate preferences for the ecological benefits associated with that configuration. Future research could compare these two approaches directly.

A related caveat concerns how the biodiversity attribute itself was presented. The program was described in terms of hectares of natural grassland and the roughly 300 species identified as national conservation priorities, but that description was accompanied by photographs of

five flagship species. We cannot rule out that stated values are anchored partly on the depicted species rather than on the full set of priority species the program would protect. This would not affect the treatment comparisons, which hold the biodiversity presentation constant across arms, but it is relevant for interpreting the level of WTP.

A second important finding is the limited sensitivity of monthly WTP to the duration of the initial payment commitment. Within the online sample, we do not reject equality of median WTP across one-, five-, and ten-year commitments, even though the cumulative financial burden over the initial commitment period differs substantially. Concerns about temporal embedding and the treatment of payment duration have been raised in the stated-preference literature (Carson and Roth Tran, 2009) and could explain this pattern. A more benign interpretation deserves priority. The questionnaire described the payment period as renewable, so respondents had an explicit basis for reading it as the length of the initial commitment rather than the date at which the conservation program would stop. If benefits and payments accrue over the same horizon and are discounted at the same rate, the annuity factor is common to both sides of the participation condition and cancels: the respondent votes in favor whenever the per-period benefit exceeds the per-period payment, regardless of the horizon. Invariance of monthly WTP to commitment length is then the prediction of the standard model rather than a departure from it.

Should respondents read the horizon as applying to payments alone, several behavioral mechanisms consistent with the mental-accounting framework, according to which recurring payments may be treated as current expenditures and evaluated separately from their cumulative cost, would predict the same pattern (Thaler, 1985, 1999; Prelec and Loewenstein, 1998). Behavioral and experimental research also documents duration neglect, whereby individuals respond more strongly to the magnitude of a recurring outcome than to its temporal extent (Frederick, Loewenstein and O'Donoghue, 2002; Read, Frederick and Airoldi, 2012). Such behavior may be particularly relevant for environmental public goods, which frequently involve long or indefinite provision horizons and relatively modest recurring payments (Brouwer et al., 2008; Vossler and McKee, 2006). Our design cannot distinguish among these mechanisms, since all imply the same observable pattern.

The comparison between the in-person probability sample and the online non-probability sample provides a complementary finding. The two samples differ significantly in several observed sociodemographic and attitudinal characteristics, confirming that online opt-in recruitment does not reproduce the composition of the probability sample. Nevertheless, when respondents face the same spatial configuration and one-year payment horizon, we do not detect a statistically significant difference in median WTP. This result is relevant because online surveys offer a substantially less costly alternative for stated-preference data collection. However, it should be interpreted cautiously. Survey mode and sampling strategy vary simultaneously, and the estimated difference is relatively imprecise. Consequently, the comparison cannot separately identify a sampling effect from an interview-mode effect, nor does the absence of statistical significance demonstrate equivalence between the two approaches (Maguire, 2009; Bell et al., 2011; Whitehead et al., 2023; Sandstrom-Mistry et al., 2023).

The pooled models reinforce the main findings. The requested contribution is consistently negative and highly statistically significant, confirming that referendum support responds coherently to the program's cost. By contrast, interactions between the BID and spatial configuration, payment duration, recruitment channel, and membership in the in-person probability sample are not statistically significant. Thus, the data provide no statistically detectable evidence that price sensitivity differs systematically across these dimensions. Support for the program is also positively associated with being female, having tertiary education, belonging to an environmental NGO, and perceiving the proposed policy as sufficient. In particular, the strong association with perceived policy sufficiency is consistent with respondents evaluating not only the financial cost of the program but also whether the proposed intervention appears adequate to achieve its conservation objective. As noted in Section 3.2.4, however, this variable was elicited after the referendum question, so the association is equally consistent with respondents rationalizing their vote after the fact.

Finally, the certainty analyses provide a demanding assessment of the robustness of the welfare estimates. Continuous certainty weighting and moderate asymmetric and semi-symmetric adjustments preserve a positive and statistically significant median WTP. Under

these treatments, the BID coefficient remains negative and comparatively stable. More aggressive recoding rules, however, substantially reduce or destabilize the WTP estimates. This indicates that certainty corrections are not behaviorally neutral: strict thresholds may eliminate informative acceptance responses rather than simply correct hypothetical bias. The range of results should therefore be interpreted as a sensitivity analysis rather than as evidence that one particular certainty treatment necessarily recovers the respondents' true WTP (Champ et al., 1997; Blumenschein et al., 1998, 2008; Loomis and Ekstrand, 1998; Shaikh, Sun and van Kooten, 2007; Akter et al., 2008; Akter and Bennett, 2013).

One further consideration concerns the illustrative comparison reported in Section 3.4, which compares aggregate household WTP with the rental value of the land that the program would target and finds that the former is roughly four to six times larger. Several qualifications apply. The comparison aggregates a median, which is a conservative basis: under the logistic specification the median coincides with the untruncated mean. Restricting WTP to non-negative values would raise the implied mean substantially.⁵ Rental prices measure the landowner's opportunity cost and therefore the transfer the program must make. They exclude the administrative, contracting and monitoring costs of a PES scheme, which a full appraisal would add. The elicited commitment is a one-year renewable payment, whereas conservation benefits would accrue over a longer and unspecified horizon. For these reasons the comparison should be read as establishing that household willingness to finance the program is of the same order as, and plausibly well above, the cost of compensating landowners, not as a demonstration that the program passes a benefit-cost test.

Ex post, the bid vector would have benefited from a higher upper bound. Acceptance at UYU 650, more than twice the pooled median estimate, remains close to 40% in both samples. The right tail of the willingness-to-pay distribution is therefore never traced out, which is one

⁵ Restricting it to non-negative values gives a mean of $\left(\frac{1}{\beta}\right) \ln(1 + e^{\alpha}) \approx \text{UYU } 648$ per household per month, which would roughly double the aggregate. We report the median throughout as the conservative basis.

source of the width of the confidence intervals around the treatment comparisons. This is a lesson for future studies.

5 Conclusions

This study brings the classic Single Large or Several Small question into stated-preference valuation, comparing two conservation designs covering the same 320,571 hectares. We find no statistically detectable difference in median WTP between them. Because several of the estimated differences have relatively wide confidence intervals, this is an absence of detectable differences rather than evidence of equivalence.

In the in-person probability sample, facing the baseline spatial configuration and a one-year renewable payment commitment, median WTP is UYU 371.70 per household per month. In the comparable online cell it is UYU 228.07. We do not reject equality, but sampling strategy and survey mode vary jointly, so the comparison cannot separate them. Within the online sample, median WTP does not differ across one-, five- and ten-year commitments. Across the pooled specifications it ranges from approximately UYU 211 to UYU 302 and the estimates survive continuous certainty weighting and moderate recoding while weakening under stricter thresholds.

Taken together, these results indicate substantial willingness to finance grassland conservation and limited differentiation across the design features we varied. An illustrative aggregation puts household WTP at roughly four to six times the rental value of the targeted land — an initial economic case for a PES scheme rather than a benefit–cost assessment. A definitive evaluation would also need to account for administrative and transaction costs, the temporal structure of benefits and payments, implementation constraints, and uncertainty surrounding aggregate WTP.

The policy implication is a communication one. If public support turns on the existence, perceived adequacy and monthly cost of a program rather than on its ecological design, then conveying why configuration and duration matter is itself part of designing a program that can be financed.

CRedit authorship contribution statement

Marcelo Caffera: Conceptualization, Methodology, Investigation, Formal analysis, Writing – original draft, Writing – review & editing, Funding acquisition, Project administration.

Felipe Vásquez Lavín: Conceptualization, Methodology, Investigation, Formal analysis, Software, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used Claude (Anthropic) in order to improve the language and readability of the manuscript and to check internal consistency. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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Data availability

The data that support the findings of this study are available from the corresponding author upon reasonable request.

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