Political Incentives in the Implementation of Social Protection: Evidence from Peru's *Juntos* Programme

Marwa Ibrahim Brigitte Seim

Abstract

Rules-based social protection programmes can still generate politically patterned outcomes when implementation requires administrative discretion. We study this possibility in Peru's conditional cash transfer programme, *Juntos*, combining administrative records, nationally representative household survey data, electoral returns, and qualitative interviews. We examine two implementation margins during President Ollanta Humala's 2011–2016 administration: district incorporation and beneficiary designation. Official poverty and demographic criteria strongly predicted district incorporation, but, conditional on those criteria, districts with greater electoral support for Humala were more likely to enter *Juntos*. As complementary evidence, in three departments prioritised for expansion, Humala's vote share was positively associated in 2014–2016 with the share of male beneficiaries and male household heads, despite programme rules designating mothers and pregnant women as recipients; comparable associations are not observed nationally or in 2011–2013. Regression-discontinuity evidence provides no support for targeting narrowly won districts, while a 2016-election placebo does not reproduce the incorporation association. The findings show that technocratic targeting can coexist with politically patterned implementation, highlighting implementation as a distinct allocation stage in social protection.

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1 Introduction

Targeting is central to the design and evaluation of social protection. Governments increasingly use poverty scores, demographic criteria, and administrative registries to identify eligible households and places. Yet formal eligibility is not the same as realised participation. Between the adoption of targeting rules and the receipt of benefits lie registration, verification, enrolment, and rollout decisions that often require judgement by administrators and local actors. If those implementation decisions respond to political incentives, a programme can remain formally rules-based while its realised allocation departs from the distribution implied by those rules.

This paper studies that implementation margin in Peru's conditional cash transfer programme, *Juntos*. The programme is useful for this purpose because its formal targeting criteria were centrally defined, while implementation required several administrative steps. District incorporation was prioritised using measures of poverty, chronic child malnutrition, and demographic vulnerability. Within participating districts, household poverty status was classified through SISFOH and then subjected to demographic and administrative verification; in some rural communities, Community Validation Assemblies also participated in verification. Programme rules were unusually specific about the identity of the transfer recipient: mothers and pregnant women, rather than household heads generally, were the intended beneficiaries. The setting therefore permits us to ask whether political support was associated with implementation conditional on observable programme rules.

Research on social protection has documented the difficulty of translating targeting systems into accurate coverage: administrative registries can misclassify eligible households, and front-line administrators exercise discretion when converting formal rules into enrolment decisions on the ground (Slater et al. 2009; Tummers and Bekkers 2014). That discretion can itself become a channel through which local political considerations shape allocation even under rules-based formulas (Mookherjee and Nath 2021), and investments in administrative capacity have been shown to materially affect whether antipoverty programmes reach intended beneficiaries (Muralidharan, Niehaus, and Sukhtankar 2016). Work on distributive politics, meanwhile, shows that electoral incentives can shape the allocation of public resources and, in some settings, directly degrade the

accuracy of programme implementation (Frey 2021; Slough 2024). Studies of political allocation commonly focus on programme placement, spending, or electoral returns (Golden and Min 2013; De La O 2013; Zucco Jr. 2013; Araújo 2021; Duchoslav, Kenamu, and Thunde 2023). Our contribution is to connect these concerns by treating implementation as a separate allocation stage. Politically patterned allocation need not require governments to rewrite eligibility criteria: it may operate through the administrative decisions that convert eligibility into enrolment and determine the timing of programme expansion.

The empirical setting is the expansion of *Juntos* under President Ollanta Humala. After Humala's election in 2011, programme expansion accelerated (Figure 1). Expanding *Juntos* formed part of his broader redistributive agenda. Referring to the programme during the presidential campaign, Humala stated: "We have a program called *Juntos*, but we have to improve it. We must focus on the women heads of households, not the fathers" (Weymouth 2023). In Humala's first year, Ayacucho, Apurímac, and Huancavelica were prioritised for expansion. These were high-poverty departments with substantial numbers of districts still outside the programme, and they also gave Humala some of his strongest electoral support.

We analyse two implementation margins. The first is district incorporation: conditional on the programme's official district targeting criteria, was Humala's electoral support associated with the probability that a district entered *Juntos*? The second is beneficiary selection: within participating districts, was electoral support associated with departures from the programme's explicit gender targeting? We combine administrative records on district incorporation from 2011 to 2016, Peru's nationally representative ENAHO household survey, district-level presidential election returns, and two semi-structured interviews with Carolina Trivelli, Peru's first Minister of Development and Social Inclusion, and a senior *Juntos* official involved in programme implementation. We supplement the main specifications with a regression discontinuity analysis around Humala's district-level electoral threshold and a placebo using support for Pedro Pablo Kuczynski in the 2016 presidential election.

The results show that formal targeting and political correlates of implementation coexisted.

Official programme criteria strongly predict district incorporation. Conditional on those criteria, however, Humala's second-round vote share is positively associated with incorporation during his administration; this district-level result is the paper's most systematic evidence of politically patterned implementation, since it is estimated using the full national sample over the programme-expansion period. As a complementary and more narrowly evidenced second margin, we also examine beneficiary selection within participating districts. Here, the pattern is concentrated rather than general: in Ayacucho, Apurímac, and Huancavelica during 2014–2016, a 10-percentage-point increase in Humala's vote share is associated with a 0.75 percentage-point increase in the district-year share of male beneficiaries and a 3.7 percentage-point increase in the share of male household heads. The corresponding relationships are not present in the national sample, and the earlier 2011–2013 estimates do not show the same pattern. We therefore treat the beneficiary result as suggestive evidence that politically patterned implementation can extend to a second margin, not as a co-equal headline finding alongside district incorporation.

Two exercises probe alternative political interpretations of the district-incorporation result. A regression discontinuity design finds no discontinuous increase in incorporation in districts Humala narrowly won relative to those he narrowly lost, providing no evidence that expansion was concentrated at the electoral threshold. A placebo using PPK's 2016 vote share does not reproduce the association with incorporation during Humala's administration. These exercises do not identify the motivation of individual administrators, but they make simple swing-district targeting and a generic correlation with later electoral preferences less consistent with the observed pattern.

The paper's central contribution is specific: targeting performance cannot be inferred solely from the quality of an eligibility formula, because implementation itself constitutes a separate allocation stage. A programme can score well on every conventional measure of design quality – centrally defined criteria, transparent poverty and demographic thresholds, an explicit household-level targeting rule – and still see its realised benefit distribution diverge from that design once administrators translate eligibility into enrolment. The Peru case demonstrates this directly: official need-based criteria are strong, statistically dominant predictors of district incorporation, yet polit-

ically patterned variation remains conditional on those criteria. This distinction matters for how targeting fidelity is evaluated. Auditing an eligibility formula answers whether the rules are well designed; it does not answer whether those rules are what actually determined who received benefits and when. The paper's secondary contribution follows from this: implementation can operate on more than one margin simultaneously, since the same underlying dynamic that shapes which places enter a programme can also shape which household member within an already-eligible place is recorded as its beneficiary.

The policy implication is not that administrative discretion should be eliminated. Local verification can correct information gaps in central registries and improve targeting. The relevant design problem is instead how to preserve those informational benefits while making registration, verification, beneficiary designation, and rollout auditable. The analysis therefore links political economy to a practical implementation question for social protection: whether formal rules are sufficient to protect targeting fidelity when administrators retain discretion over how those rules are translated into participation.

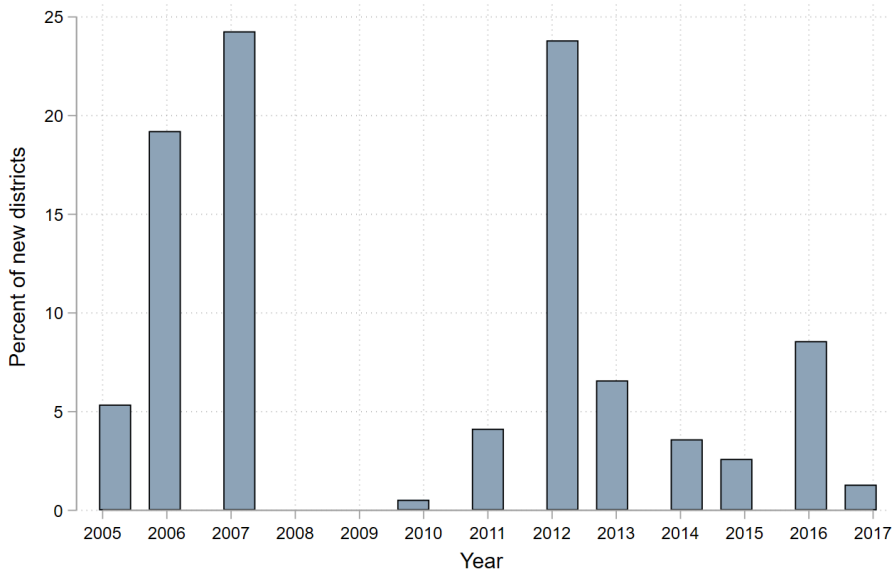
2 Institutional Background

2.1 Political context

Ollanta Humala assumed office in July 2011 after winning 51.9 percent of the vote in the second round of Peru's presidential election (Salazar 2011; Romero and Zarate 2011). His administration created the Ministry of Development and Social Inclusion (MIDIS), which assumed responsibility for major social programmes, including *Juntos*.

Although *Juntos* had been established in 2005, its geographic expansion slowed considerably after the initial 2005–2007 rollout. The percentage of new districts entering the programme remained below one percent annually between 2008 and 2010 before programme expansion resumed after Humala took office. Figure 1 illustrates the annual pattern of district incorporation between the programme's creation and 2017.

Figure 1: District incorporation into *Juntos*, 2005-17



Note: The figure reports the percentage of newly incorporated districts entering *Juntos* in each year between 2005 and 2017. District-level incorporation dates are compiled from *Juntos* administrative records.

During Humala's first year in office, Ayacucho, Apurímac, and Huancavelica were prioritised for programme expansion. The 54 districts incorporated in 2011 were selected from the 2007 *Estrategia Nacional CRECER* list, which ranked predominantly rural districts with poverty rates exceeding 50 percent. These departments also provided some of Humala's strongest electoral support, with second-round vote shares of 71 percent in Ayacucho, 65 percent in Apurímac, and 71 percent in Huancavelica.

Ayacucho, Apurímac, and Huancavelica combined several characteristics that made them especially important during the early expansion of *Juntos*. Appendix Table A1 shows that they ranked among the departments with the largest shares of districts that had not yet been incorporated into the programme at the beginning of Humala's administration. Among Peru's departments, Huancavelica, Apurímac, and Ayacucho ranked second, third, and fifth, respectively. These departments therefore combined three characteristics that made them particularly salient during the early expansion of *Juntos*: they were among the poorest departments in the country, they had provided some of Humala's strongest electoral support, and they still contained a substantial number of eligible

districts that had not yet entered the programme. Prioritising these departments therefore allowed the government simultaneously to expand coverage in high-poverty areas while extending the programme where electoral support was strongest. For this reason, they also provide the principal setting for the beneficiary-level analysis presented later in the paper.

Peru also has a documented history of political influence over social programme spending. Using data from the Fujimori administration, Schady (2000) finds that expenditures under the social fund FONCODES were systematically associated with electoral support for the incumbent. A cross-country analysis of Latin American social funds similarly identifies opportunistic political influences on FONCODES spending during the 1990s (Graham and Kane 1998). The analysis in this paper examines whether programme implementation under *Juntos* remained politically patterned despite its nationally standardised targeting criteria.

2.2 The *Juntos* programme

Established in 2005, *Juntos* is a conditional cash transfer programme for poor households containing pregnant women or children. During the study period, the qualifying age for children was initially 14 and was raised to 19 in 2014. Beneficiary households received a fixed transfer through direct bank deposit, irrespective of household size. Transfers were designated to mothers or pregnant women rather than to household heads generally. The identity of the registered beneficiary therefore formed an explicit part of the programme's design.

Receipt of the transfer was conditional on compliance with programme requirements related to children's education and health. These included school attendance, vaccinations, child growth monitoring, routine health visits, and prenatal care where applicable. In an interview conducted for this study, Carolina Trivelli, Peru's first Minister of Development and Social Inclusion, explained that the programme was presented as an investment in future generations rather than as a "handout."

Programme eligibility combined geographic and household-level targeting. During the study period, district incorporation was based on three principal criteria: district poverty, chronic child malnutrition, and an index measuring the concentration of children below age three. The poverty

threshold was initially 50 percent in 2011 and was reduced to 40 percent during 2012–2014. An updated poverty map was adopted for 2015–2016 while retaining the 40 percent threshold. The reconstruction of these targeting criteria follows the programme documentation assembled by Carpio et al. (2019).

Within participating districts, households were required to register with the *Sistema de Focalización de Hogares* (SISFOH) to obtain a poverty classification. Registration took place through a local registration unit, the *Unidad Local de Empadronamiento*. Household classification relied on a proxy means test rather than reported income or expenditure. The formula incorporated household demographic characteristics, dwelling materials, and access to services such as water, electricity, and sanitation; its parameters were not publicly disclosed (Camacho 2014).

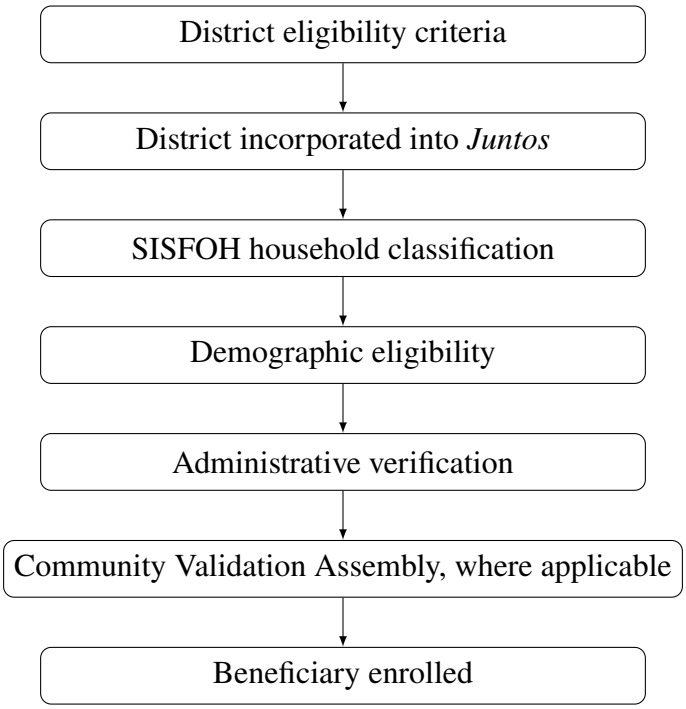
Households satisfying both the poverty and demographic criteria proceeded to administrative verification before final beneficiary enrolment. Figure 2 summarises the sequence from geographic targeting through beneficiary enrolment.

2.3 Household registration and verification

Household classification through SISFOH did not by itself result in enrolment in *Juntos*. Household information was subject to additional demographic and administrative verification. SISFOH exchanged information with Peru's National Registry of Identification and Civil Status (RENIEC), while children's eligibility and programme compliance were cross-checked against administrative records from the education and health sectors. Officials could also conduct household visits to verify changes in household composition, residence, and poverty status.

Community Validation Assemblies formed an additional stage of beneficiary verification in some rural communities. According to the senior *Juntos* official interviewed for this study, these procedures were introduced to adapt programme registration to local conditions and to verify the socioeconomic and demographic information of potentially eligible households. Committees composed of village leaders and local representatives assessed whether households satisfied the programme's poverty and demographic eligibility criteria. Potential beneficiaries could also request

Figure 2: Administrative stages from eligibility to beneficiary enrolment under *Juntos*



Note: The figure summarises the district- and household-level stages described in programme records, the reconstruction by Carpio et al. (2019), and the qualitative evidence discussed in the text. Community Validation Assemblies were used in some rural communities rather than uniformly across all participating districts.

enrolment through these assemblies.

The official explained that the registered household representative was generally the mother and, in some cases, her daughter. Community Validation Assemblies therefore played a role not only in confirming household eligibility but also in confirming the identity of the programme beneficiary ultimately recorded by *Juntos*. This feature is particularly relevant to the empirical analysis, which examines whether beneficiary selection remained consistent with the programme's intended gender targeting.

Maintaining current household classifications and beneficiary records was administratively demanding. The official interviewed for this study described delays in updating household information as an important operational challenge during programme implementation. This account is consistent with the independent evaluation by Orozco (2015), which documents bottlenecks in SISFOH's poverty classification process together with confusion among potential beneficiaries regarding programme registration and enrolment.

Although districts remained in *Juntos* once incorporated, household participation was not permanent. Household eligibility continued to be reassessed as demographic circumstances changed, including whether households still contained eligible children or pregnant women, continued to satisfy residency requirements, and remained eligible under programme rules. Households could therefore be removed from the programme if eligibility conditions were no longer met or if they relocated outside participating districts. Beneficiary enrolment therefore depended not only on the programme's formal eligibility rules but also on continuing administrative decisions regarding household status throughout programme participation.

These multiple stages of verification illustrate that, although eligibility rules were centrally defined, programme implementation required a series of administrative decisions before and after households entered *Juntos*. The empirical analysis examines whether political support was associated with these implementation decisions. Importantly, these implementation stages occurred after districts had already satisfied the programme's official targeting criteria and households had been classified as poor through SISFOH. The remaining administrative decisions therefore concerned

the translation of formal eligibility into realised programme participation rather than the design of eligibility itself.

3 Data

The empirical analysis combines three complementary data sources. Administrative records document district incorporation into *Juntos*, ENAHO identifies programme beneficiaries and their characteristics, and district-level electoral returns measure political support for Humala. Together, the data cover the geographic and beneficiary-level implementation margins studied below.

3.1 Administrative data

Administrative records from *Juntos* identify the year in which each district was incorporated into the programme between 2011 and 2016. These records are used to construct a district-year panel covering programme expansion during Humala's administration.

To distinguish political influences from the programme's official targeting rules, the empirical specifications control for the district selection criteria used by MIDIS to prioritise incorporation into *Juntos*. During the study period, district incorporation was based on three principal indicators: monetary poverty, chronic child malnutrition, and the concentration of children below the age of three. The poverty measure reflects the transition from Peru's 2009 to 2013 Poverty Maps, while the remaining indicators are constructed from official census statistics following Carpio et al. (2019).

Table 1 summarises the official district selection criteria applied during the study period. Summary statistics for the district-level variables used in the empirical analysis are reported in Appendix Table A2.

3.2 Household survey data

The household-level analysis relies on Peru's National Household Survey on Living Conditions and Poverty (ENAHO), which allows us to examine whether the characteristics of recorded programme

Table 1: *Juntos* District Selection Criteria

Years	Criteria
Pilot phase	Poverty gap
2005-07	Prevalence of chronic child malnutrition
	Percentage of population centers very affected by political violence
	Percentage of households with two or more unsatisfied basic needs
2010	By legal decree, selection of districts exposed to heightened violence and social instability.
2011	Poverty ranking of districts from three prioritized departments: Ayacucho, Apurimac, and Huancavelica.
	Poverty cutoff of 50 percent.
	Child Malnutrition.
	Index of children below age 3.
2012-14	Poverty cutoff revised to 40 percent.
2015-16	Poverty map revised while keeping the same cutoff of 40 percent.

Note: The table summarizes the official criteria used by MIDIS to prioritize districts for incorporation into *Juntos*. The criteria are compiled from MIDIS programme documentation and the reconstruction by Carpio et al. (2019).

beneficiaries remained consistent with *Juntos*' intended gender targeting. ENAHO records whether respondents received transfers from *Juntos*, together with detailed demographic and socioeconomic characteristics of beneficiary households.

We pool annual ENAHO waves from 2011 through 2016 and aggregate the data to the district-year level to match the administrative records. By the end of Humala's administration, *Juntos* operated in 1,289 districts, of which 567 entered the programme during the study period. Of the 567 districts incorporated during the study period, 462 are represented in ENAHO. Because ENAHO is a nationally representative household survey rather than a census, the smallest districts are not represented.

The principal beneficiary outcome is whether the registered programme beneficiary is male, despite programme guidelines identifying mothers and pregnant women as the intended recipients of transfers. We additionally examine whether the beneficiary is a male household head and whether that household head resides in his district of birth. The latter serves as a proxy for stronger local social ties that may facilitate access to programme information or influence community-level implementation. The beneficiary-level outcomes are ordered from broad to increasingly restrictive definitions. The first captures any departure from the programme's intended gender targeting, while the remaining outcomes examine whether these departures are concentrated among male household heads and among individuals with stronger local ties.

Male beneficiaries are uncommon, consistent with the programme's explicit designation of mothers and pregnant women as transfer recipients. Approximately 3.7 percent of registered beneficiaries are male. Restricting the sample to household heads increases this proportion to 21.6 percent, while 21.2 percent of household heads are both male and reside in their district of birth. Summary statistics are reported in Appendix Table A3.

3.3 Electoral data

Official district-level electoral returns are obtained from Peru's 2011 presidential election. Our principal measure of political support is Humala's second-round vote share, which captures support in the decisive presidential contest.

To distinguish among competing political mechanisms, we also exploit close electoral outcomes using Humala's district-level margin of victory in the second round as the running variable in the regression discontinuity analysis. Appendix Figure A3 presents the distribution of district-level vote shares, illustrating substantial variation in electoral support across districts.

4 Empirical Strategy

Programme implementation involved two distinct administrative decisions after formal eligibility had been established. We examine whether political support was associated first with the incorporation of eligible districts into *Juntos* and then with beneficiary selection within participating districts. The second analysis asks whether the identity of the recorded programme beneficiary remained consistent with the programme's intended gender targeting.

4.1 District incorporation

District incorporation constitutes the first stage of programme implementation examined in the paper. We estimate whether political support was associated with the geographic expansion of *Juntos* after accounting for the programme's official district eligibility criteria. Because eligible

districts entered the programme at different points during the study period, we estimate a discrete-time event history model in which the unit of observation is the district-year.

The dependent variable equals one in the year that a district is first incorporated into *Juntos*, zero in all preceding years, and is coded as missing thereafter. Consequently, each district exits the estimation sample once it is incorporated into the programme, while districts that remain outside *Juntos* by the end of 2016 contribute only zero outcomes throughout the observation period.

The estimating equation is

$$Incorporation_{dy} = \alpha + \beta_1 HumalaVote_d + \mathbf{X}'_{dy}\boldsymbol{\gamma} + \lambda_y + \varepsilon_{dy}, \tag{1}$$

where $Incorporation_{dy}$ equals one if district d was incorporated into *Juntos* in year y, and zero otherwise, $HumalaVote_d$ is Humala's second-round vote share in the 2011 presidential election, $\mathbf{X}_{dy}$ is a vector containing the programme's official district eligibility criteria (district poverty, chronic child malnutrition, and the child concentration index), $\boldsymbol{\gamma}$ is the corresponding vector of coefficients, and λ_y denotes year fixed effects. Standard errors are clustered at the district level.

Because district incorporation is binary, the principal specifications are estimated using logistic regression and reported as odds ratios. Districts remain at risk of incorporation until the year they first enter *Juntos*, after which they exit the risk set.

4.2 Beneficiary selection

Beneficiary selection provides the paper's most direct test of whether political support shaped programme implementation after formal eligibility had already been established.

Programme guidelines explicitly designated mothers and pregnant women as transfer recipients. Administrative verification and community validation, however, determined which eligible household member was ultimately recorded as the programme beneficiary. We therefore estimate

$$Male_{dy} = \alpha + \beta_1 HumalaVote_d + \mathbf{X}'_{dy}\boldsymbol{\gamma} + \lambda_y + \varepsilon_{dy}, \tag{2}$$

where $Male_{dy}$ alternatively denotes (i) the share of beneficiaries who are male, (ii) the share of male beneficiaries residing in their district of birth, (iii) the share of male household heads, and (iv) the share of male household heads residing in their district of birth. $\mathbf{X}_{dy}$ is a vector containing the programme's official district eligibility criteria (district poverty, chronic child malnutrition, and the child concentration index), $\boldsymbol{\gamma}$ is the corresponding vector of coefficients, and λ_y denotes year fixed effects.

Together, these outcomes distinguish between departures from the programme's intended gender targeting and broader differences in the characteristics of beneficiary households. They also allow us to assess whether any such differences were concentrated among individuals with stronger local social ties. All specifications control for the programme's official district selection criteria and include year fixed effects. Standard errors are clustered at the district level throughout.

4.3 Distinguishing political mechanisms

The preceding models estimate whether programme implementation was systematically associated with political support after accounting for the programme's official targeting criteria. They do not, however, distinguish between competing political mechanisms. A prominent explanation in the distributive politics literature is that governments strategically allocate resources to electorally competitive, or "swing," districts. An alternative possibility is that programme implementation favoured districts providing stronger electoral support for the incumbent government.

To distinguish between these mechanisms, we estimate a regression discontinuity design exploiting Humala's district-level margin of victory in the second round of the 2011 presidential election. Districts in which Humala narrowly won are compared with districts in which he narrowly lost. If programme implementation primarily favoured electorally competitive districts, one would expect a discontinuous increase in district incorporation immediately above the 50 percent vote-share threshold.

The estimating equation is

$$Incorporation_{dy} = \alpha + \beta_1 Margin_d + \beta_2 \left(Margin_d \times AboveCutoff_d\right) + \beta_3 AboveCutoff_d + \mathbf{X}'_{dy}\boldsymbol{\gamma} + \lambda_y + \varepsilon_{dy}. \tag{3}$$

where $Incorporation_{dy}$ equals one if district d was incorporated into *Juntos* in year y and zero otherwise, $Margin_d$ denotes Humala's district-level margin of victory in the second round of the 2011 presidential election, $AboveCutoff_d$ is an indicator equal to one for districts in which Humala's vote share exceeded 50 percent and zero otherwise, $\mathbf{X}_{dy}$ is a vector containing the programme's official district eligibility criteria (district poverty, chronic child malnutrition, and the child concentration index), $\boldsymbol{\gamma}$ is the corresponding vector of coefficients, and λ_y denotes year fixed effects.

The validity of the regression discontinuity design is assessed using standard diagnostic tests. Appendix Figure A1 reports covariate balance tests, while Appendix Figure A2 and Appendix Table A6 present density tests following Cattaneo, Jansson, and Ma (2018). Together, these diagnostics support the identifying assumptions underlying the research design.

5 Results

5.1 Geographic expansion of *Juntos*

We begin with district incorporation because it provides the paper's most systematic evidence of politically patterned implementation: unlike the beneficiary results presented below, it is estimated using the full national sample over the programme-expansion period. We examine whether political support was associated with district incorporation after accounting for the programme's official targeting criteria. Table 2 reports the estimates from the discrete-time event history model. Once districts satisfied the programme's official targeting criteria, administrative authorities retained discretion over when they were incorporated into *Juntos*. The empirical question is therefore whether

districts providing stronger electoral support for Humala were more likely to be incorporated into *Juntos* after accounting for the programme's official district selection criteria.

As expected, the programme's official targeting criteria strongly predict district incorporation, confirming that programme expansion broadly followed its intended targeting strategy. After accounting for these criteria, however, Humala's second-round vote share remains positive and statistically significant (Table 2).

Holding constant the programme's official district selection criteria, districts providing stronger electoral support for Humala were significantly more likely to be incorporated into *Juntos*. The estimated odds ratio implies that a 10-percentage-point increase in Humala's second-round vote share increased the odds of incorporation by approximately 8 percent. Thus, among districts with similar levels of need under the official targeting criteria, electoral support remained associated with programme incorporation.

The magnitude of the poverty coefficient nevertheless indicates that the programme's formal targeting rules remained the principal determinant of district incorporation. Political support therefore complemented rather than displaced technocratic targeting.

The association between political support and district incorporation is also evident when programme expansion is examined year by year, though the year-by-year pattern reported in Appendix Table A7 is not uniform and is worth addressing directly rather than leaving to the appendix. The coefficient on Humala's vote share is positive and statistically significant in 2011, 2015, and 2016, negative and statistically significant in 2014, and not statistically distinguishable from zero in 2012 and 2013. Several institutional developments coincide with this pattern. The 2012–2013 period, when the coefficient is not significant, corresponds to the early tenure of MIDIS and Minister Carolina Trivelli, whose establishment and stated mandate emphasised centralised, criteria-based programme administration. The negative and significant coefficient in 2014 corresponds to a year in which enrolment was concentrated in San Martín, a department with a comparatively narrow (8 percent) margin of support for Humala, alongside a small number of additional districts in Apurímac; San Martín's districts had not previously been enrolled despite a poverty rate of 46

percent, and 2014 was also the year of Peru's regional and municipal elections. The coefficient returns to positive and significant in 2015 and 2016, following the issuance of Resolution No. 260 in 2015. We do not read this year-by-year variation as evidence of a single, temporally uniform political-targeting mechanism; it suggests instead that the pooled association reflects years with differing institutional oversight and differing sets of departments entering the programme. The pooled coefficient in Table 2 is therefore best read as an average association across a heterogeneous expansion process, not as evidence that political support raised the odds of incorporation uniformly in every year.

Taken together, the district-level evidence is the paper's central empirical result. Programme expansion remained closely aligned with the official targeting criteria, yet districts providing stronger electoral support for Humala were significantly more likely to be incorporated into *Juntos* after accounting for those criteria. Political support therefore complemented rather than displaced the programme's formal targeting rules. We next examine beneficiary selection within participating districts as a complementary, more narrowly bounded test of the same underlying possibility.

5.2 Beneficiary selection

As a complementary and more narrowly evidenced test of the same underlying possibility, we next examine beneficiary selection within participating districts. Programme guidelines explicitly designated mothers and pregnant women as transfer recipients, yet administrative verification ultimately determined which eligible household member was recorded as the programme beneficiary. We therefore examine whether beneficiary selection remained consistent with the programme's intended gender targeting.

Table 3 presents the beneficiary-selection results. The evidence indicates that beneficiary selection departed from the programme's intended gender targeting in a concentrated setting. Across the national sample, there is little evidence that political support was associated with the identity of the recorded beneficiary. Within Ayacucho, Apurímac, and Huancavelica, however, districts providing stronger electoral support for Humala were significantly more likely to enrol

Table 2: District incorporation, 2011-16

	Odds ratio
Vote share for Humala	2.056**
	(0.705)
Poverty Criterion	470.372***
	(147.989)
Child Malnutrition	3.063*
	(1.919)
Child Index	1.325***
	(0.060)
2011	0.102***
	(0.018)
2013	0.457***
	(0.076)
2014	0.361***
	(0.071)
2015	0.217***
	(0.059)
2016	5.425***
	(1.285)
Constant	0.006***
	(0.003)
Observations	5,176
Pseudo R2	0.505

Note: Entries report odds ratios from district-year logistic regressions of first-time district incorporation into *Juntos*. The dependent variable equals one in the year a district first enters the programme. All specifications control for the programme's official district selection criteria and include year fixed effects. Standard errors are clustered at the district level ***p<0.01, **p<0.05, *p<0.1

male household heads despite programme guidelines identifying mothers and pregnant women as the intended recipients of transfers.

A 10-percentage-point increase in Humala's second-round vote share is associated with approximately a 0.75 percentage-point increase in the district-year share of male beneficiaries and a 3.7 percentage-point increase in the share of male household heads. These estimated effects are economically meaningful given the rarity of male beneficiaries under the programme's intended design.

The estimated relationship remains positive when beneficiary selection is further restricted to

male household heads residing in their district of birth. Although residence in one's district of birth is not an eligibility criterion under *Juntos*, it may proxy stronger local social ties that facilitate access to programme information or increase visibility during administrative verification and community validation.

By contrast, no comparable relationship is observed in the national sample. Outside the priority departments, the estimated coefficients are consistently small and statistically insignificant. Appendix Table A4 likewise shows no comparable relationship during the earlier 2011–2013 period, suggesting that these implementation differences emerged as programme expansion accelerated.

The concentration of these effects within the three priority departments is itself informative. These departments formed the initial focus of programme expansion following Humala's election and therefore experienced the greatest administrative activity associated with beneficiary enrolment. If departures from the programme's intended gender targeting simply reflected random administrative error or systematic measurement differences in ENAHO, similar patterns would be expected elsewhere in the country. Instead, the observed differences are confined to the departments that experienced the earliest phase of programme expansion. Although the empirical analysis cannot identify the precise administrative mechanism, the evidence is consistent with implementation responding differently across political contexts.

Taken together, these findings identify a concentrated departure from the programme's intended beneficiary designation, not a general nationwide change in beneficiary selection. In the three priority departments during 2014–2016, stronger electoral support for Humala was associated with a higher district-year share of male beneficiaries, particularly male household heads. Because the relationship is not present nationally or in the earlier period, we treat it as complementary evidence that politically patterned implementation can extend to a second, more granular margin – the identity of the recorded beneficiary within already-incorporated districts – rather than as a co-equal headline finding alongside the district-incorporation result.

Table 3: Beneficiaries of *Juntos*, 2014-16

	Male	Male born in this district	Male hhld head	Male hhld head born in this district
			Districts in Three Prioritized Departments	
Vote share for Humala	0.075**	0.050	0.371**	0.293*
	(0.032)	(0.032)	(0.155)	(0.169)
Constant	-0.071**	-0.051	-0.296*	-0.252
	(0.035)	(0.032)	(0.160)	(0.173)
Observations	522	503	292	267
R-squared	0.019	0.015	0.047	0.043
			All Districts	
Vote share for Humala	-0.016	-0.026	0.019	0.020
	(0.016)	(0.017)	(0.077)	(0.078)
Constant	0.055***	0.065***	0.244***	0.251***
	(0.019)	(0.025)	(0.083)	(0.092)
Observations	2,091	1,910	1,152	978
R-squared	0.002	0.004	0.007	0.009

Note: OLS estimates using district-year aggregates from ENAHO. The dependent variables are alternative measures of male programme beneficiaries. The key explanatory variable is Humala's district vote share in the 2011 presidential election. All specifications control for the programme's official district selection criteria and include year fixed effects. Standard errors are clustered at the district level. The upper panel reports estimates for the three priority departments (Ayacucho, Apurímac, and Huancavelica); the lower panel reports estimates for all districts. ***p<0.01, **p<0.05, *p<0.1

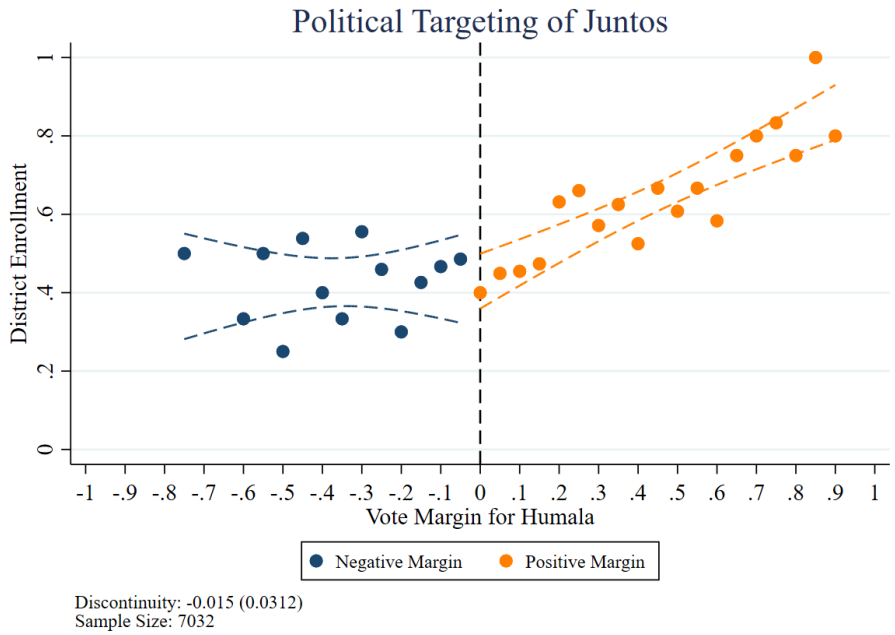
5.3 Evidence against swing-district targeting

The preceding analyses establish that political support was associated with both district incorporation and beneficiary selection. We next examine whether these patterns are consistent with strategic targeting of electorally competitive, or "swing," districts, rather than districts providing stronger electoral support for Humala.

The regression discontinuity estimates provide no evidence that programme incorporation increased discontinuously at the electoral threshold. Figure 3 presents the regression discontinuity estimates exploiting Humala's district-level margin of victory in the second round of the 2011 presidential election. Contrary to the swing-district hypothesis, we find no statistically significant discontinuity in district incorporation at the electoral threshold. Districts in which Humala narrowly won were no more likely to be incorporated into *Juntos* than otherwise comparable districts in which he narrowly lost. Appendix Figure A4, which uses a narrower bandwidth, and the corresponding regression estimates in Appendix Table A5 lead to the same conclusion.

Taken together, these findings indicate that district incorporation was not concentrated among

Figure 3: Regression discontinuity



Note: The running variable is Humala's district-level margin of victory in the 2011 presidential election. The sample includes districts not incorporated into *Juntos* prior to 2011. The fitted lines correspond to the regression discontinuity specification reported in Equation (3). The vertical line denotes the electoral cutoff.

electorally competitive districts. Instead, considered alongside the positive association between district vote share and district incorporation documented earlier, the evidence suggests that implementation responded to the intensity of electoral support rather than narrow electoral competition.

The validity of the regression discontinuity design is supported by standard diagnostic tests. Appendix Figure A1 reports covariate balance tests showing that the programme's official district selection criteria vary smoothly around the electoral threshold. Appendix Figure A2 and Appendix Table A6 similarly provide no evidence of manipulation of the running variable, supporting the identifying assumptions underlying the design. These diagnostic results increase confidence that districts immediately above and below the electoral threshold were comparable prior to treatment, allowing observed differences in programme implementation to be interpreted as evidence regarding the competing political mechanisms rather than underlying differences in district characteristics.

5.4 Placebo test: Electoral support in the 2016 presidential election

A remaining concern is that the main results capture persistent patterns of electoral support rather than political targeting specific to Humala's administration. To examine this possibility, we re-estimate the district incorporation model using Pedro Pablo Kuczynski's (PPK) district vote share in the 2016 presidential election in place of Humala's 2011 vote share. Because PPK's vote share is observed after the programme expansion examined in the main analysis, the placebo test is estimated at the district level. The dependent variable equals one if a district was incorporated into *Juntos* during Humala's administration and zero otherwise.

The placebo analysis provides no evidence that subsequent electoral support for Pedro Pablo Kuczynski (PPK) predicts district incorporation during Humala's administration. Table 4 reports the placebo estimates. In contrast to the main district incorporation results reported in Table 2, electoral support for PPK is not significantly associated with district incorporation. Districts that later provided stronger electoral support for PPK were no more likely to have been incorporated into *Juntos* during Humala's administration than districts supporting other candidates.

Taken together, the placebo estimates reported in Table 4 and the regression discontinuity results presented in Figure 3 strengthen the interpretation of the main findings. The absence of an association between district incorporation and PPK's subsequent electoral support reduces concern that the results simply reflect persistent district-level political preferences, while the absence of a discontinuity at the electoral threshold provides no evidence of swing-district targeting. Together, these robustness checks strengthen the interpretation that the implementation patterns documented in this paper were specific to electoral support for Humala during the period in which programme expansion decisions were made.

6 Conclusion

This paper examines a part of social-protection targeting that is easy to overlook: the administrative decisions between formal eligibility and realised participation. Peru's *Juntos* programme combined

Table 4: Placebo test: district incorporation and the 2016 presidential election

	Odds ratio
Vote share for PPK 2016	2.031
	(1.186)
Poverty Criterion	76.851***
	(21.404)
Child Malnutrition	1.082***
	(0.010)
Child Index	1.431***
	(0.078)
Constant	0.310***
	(0.131)
Observations	1,151
Pseudo R2	0.7411

Note: Entries report odds ratios from district-level logistic regressions. The dependent variable equals one if a district was incorporated into *Juntos* during President Humala's administration and zero otherwise. The key explanatory variable is Pedro Pablo Kuczynski's (PPK) district vote share in the 2016 presidential election. All specifications control for the programme's official district selection criteria. Standard errors are clustered at the district level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

centrally defined targeting rules with registration, verification, beneficiary designation, and district-rollout decisions that left room for implementation discretion. We ask whether electoral support for President Ollanta Humala was associated with outcomes at two of these margins during the programme's 2011–2016 expansion.

The district evidence shows that technocratic targeting remained central. Poverty and the programme's other official criteria strongly predicted incorporation into *Juntos*. Conditional on those criteria, however, districts with greater support for Humala had higher odds of incorporation. The beneficiary evidence is narrower but notable because it concerns an explicit programme rule. In the three departments prioritised for expansion, Humala's vote share was positively associated during 2014–2016 with the share of recorded beneficiaries who were male and with the share who were male household heads, even though mothers and pregnant women were the intended transfer recipients. The same pattern is not observed in the national sample or in the earlier 2011–2013 period.

These findings do not imply that formal targeting was displaced by politics. Rather, they

show why design and implementation should be evaluated separately. A programme can allocate resources largely according to poverty and demographic criteria while retaining implementation margins that are correlated with political support. For evaluations of social protection, this means that assessing the quality of a targeting formula is not sufficient to establish targeting fidelity. Registration, verification, enrolment, beneficiary designation, and the sequencing of rollout are also part of the allocation process.

The robustness exercises narrow, but do not eliminate, alternative interpretations. The regression discontinuity analysis finds no evidence of a discrete increase in incorporation at Humala's 50-percent district vote threshold, which is inconsistent with a simple strategy focused on narrowly won districts. A placebo using PPK's 2016 vote share does not reproduce the main district-incorporation relationship, reducing concern that the result merely captures a stable district characteristic associated with support for any subsequent candidate. Neither exercise reveals the motivations of administrators or identifies the precise channel linking electoral support to implementation.

That limitation is important. The analysis cannot observe individual administrative decisions or distinguish cleanly among bureaucratic responsiveness, political direction, local networks, and other institutional channels. The beneficiary results are also concentrated in three departments and in the later part of the study period, so their external validity should not be overstated. The evidence is best interpreted as showing that politically patterned implementation can arise within a formally rules-based programme, not as demonstrating a single mechanism that operates uniformly across Peru.

For policy, the relevant tension is between discretion and accountability. Local verification and administrative judgement can improve targeting when central records are incomplete, but those same processes make the translation of formal rules into participation harder to monitor. Strengthening audit trails for beneficiary designation, documenting exceptions to targeting rules, and routinely comparing administrative outcomes with stated eligibility criteria can help make this implementation margin visible. More generally, understanding who benefits from social protection requires attention not only to who is formally eligible, but also to the institutions that convert

eligibility into actual programme participation.

A Online Appendix

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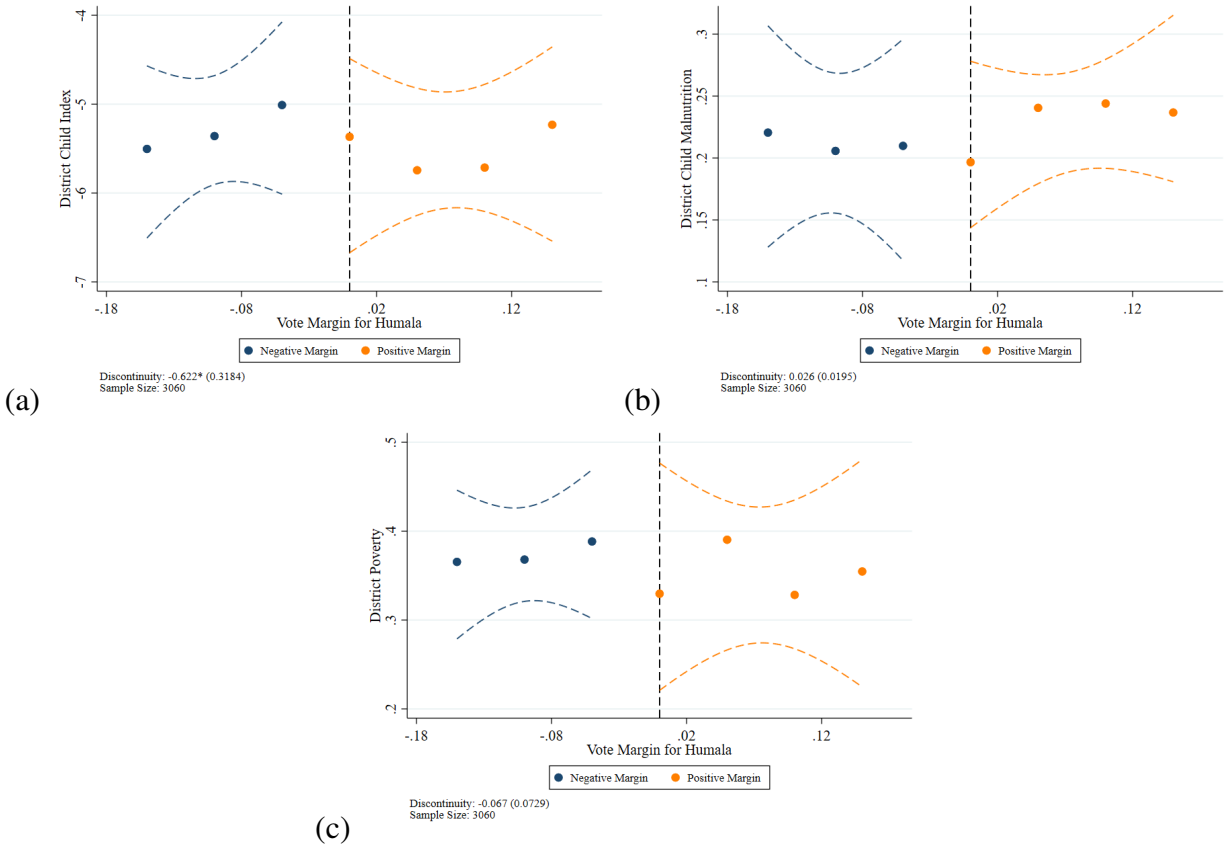
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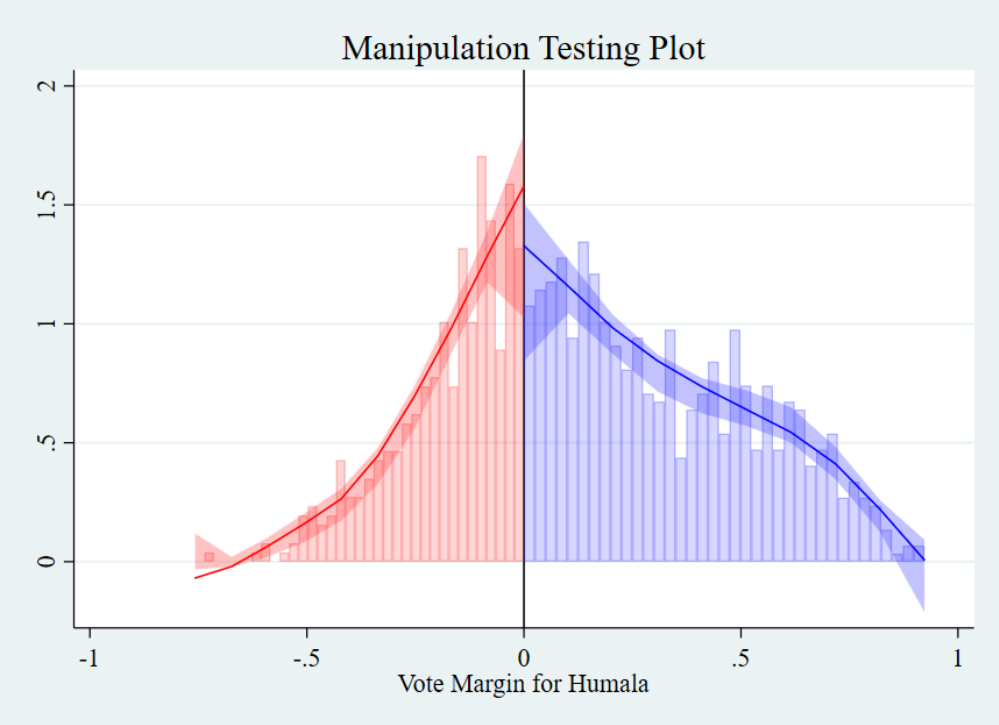
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Figure A1: Covariate Balance Test



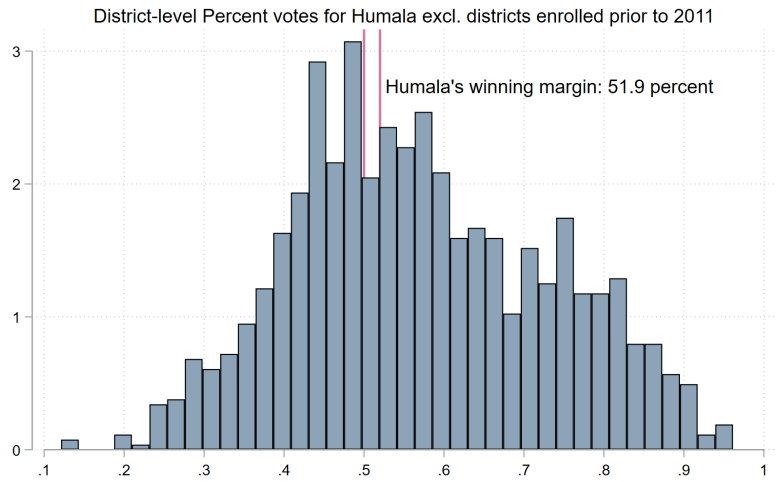
Note: Each figure displays one of the programme's official district eligibility criteria. The running variable is Humala's margin of victory around a cutoff of zero. The sample is restricted to districts that lie within a bandwidth of 18 percent. In each figure, the discontinuity is the result of a regression specified in equation 3, with year fixed effects and no controls, clustering on the running variable. Statistical significance is denoted by ***p<0.01, **p<0.05, *p<0.1.

Figure A2: Regression discontinuity manipulation test



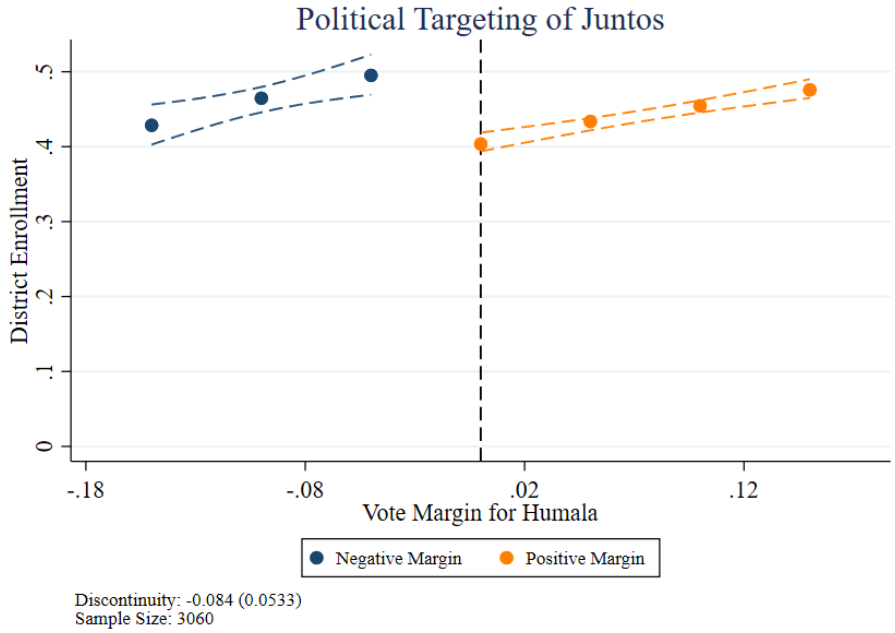
Note: The figure shows the result of a manipulation test of Humala's margin of victory in 2011. The sample is restricted to districts not enrolled in *Juntos* prior to 2011. The rddensity package finds no significant irregularities that would imply manipulation in the running variable.

Figure A3: District-level Votes for Humala



Note: Histogram of Humala's district vote share in the 2011 presidential runoff. Districts already enrolled in *Juntos* prior to 2011 are excluded. Vertical lines indicate the 50 percent electoral threshold and Humala's national vote share.

Figure A4: Regression discontinuity estimates, narrow bandwidth



Note: The running variable is Humala's margin of victory around a cutoff of zero. The sample is restricted to districts that lie within a bandwidth of 18 percent. The discontinuity is the result of a regression specified in equation 3. Statistical significance is denoted by ***p<0.01, **p<0.05, *p<0.1.

Table A1: Ranking of departments by percent of districts not yet enrolled at the start of Humala's term from lowest to highest

Departments	Percent of districts not yet enrolled (percent poor, vote margin)
HUANUCO	20.2 (71.6, 30.77)
HUANCAVELICA	**22 (72.4, 30.8)**
APURIMAC	**28.6 (75.5, 33.88)**
CAJAMARCA	39.4 (63.7, 4.35)
AYACUCHO	**41.2 (63.9, 46.8)**
LA LIBERTAD	49.4 (59.6, -12.46)
PUNO	53.6 (65.8, 65.6)
CUSCO	58 (62.4, 70)
ANCASH	60.2 (43.1, 25.1)
JUNIN	68.6 (40.3, 12.2)
PASCO	72.4 (57.4, 1.69)
PIURA	75.4 (53, 3.4)
LORETO	79.3 (67.8, 3.7)
AMAZONAS	85.7 (55.9, 8.32)
AREQUIPA	100 (36.2, 37.6)
CALLAO	100 (9.5, -1.44)
ICA	100 (18.1, 1.5)
LAMBAYEQUE	100 (38.6, -3.11)
LIMA	100 (27.4, -20.3)
MADRE DE DIOS	100 (15.7, 3.77)
MOQUEGUA	100 (23.8, 10.4)
SAN MARTIN	100 (46.1, 8.4)
TACNA	100 (27.2, 13.5)

Note: Humala's three prioritized departments are in bold.

Table A2: Summary statistics of districts not enrolled in *Juntos* prior to Humala's term

	Source	N	Max	Mean	Std. Dev.	Median
Votes for Humala, percent, 2011	ONPE	1192	96.1	57	16	55
District poverty map, percent poor, 2013	INEI	1198	90.3	34.95	19.71	32.65
District poverty map, percent poor, 2009	INEI	1191	95.4	41.67	21	40.2
Child Malnutrition, percent, 2005	INEI	1184	100	23.39	11.97	23.19
Index of children ages 0-3, 2007	INEI	1188	.54	0.016	.04	.004

Note: ONPE: election data; INEI: census data.

Table A3: *Juntos* beneficiaries aggregated at the district-year level, 2011–16

Variable	N	Mean	Std. Dev.	Min	Max
Male	3471	.037	.11	0	1
Male born here	3172	.037	.115	0	1
Male household head	1875	.216	.362	0	1
Household head	3471	.175	.231	0	1
Born here	3471	.756	.312	0	1
Male head born here	1604	.212	.362	0	1

Note: Table presents variables from the ENAHO household survey across all districts, with the data aggregated to the district-year level.

Table A4: Gender Targeting, 2011-13

	Male	Male born in this district	Male hhld head	Male hhld head born in this district
		Districts in Three Prioritized Departments		
Vote share for Humala	-0.061*	-0.065	-0.240	-0.189
	(0.036)	(0.042)	(0.254)	(0.265)
Constant	0.038	0.037	0.024	-0.024
	(0.026)	(0.029)	(0.188)	(0.236)
Observations	415	403	224	203
R-squared	0.025	0.025	0.039	0.052
			All Districts	
Vote share for Humala	-0.007	0.008	-0.090	-0.062
	(0.018)	(0.021)	(0.099)	(0.109)
Constant	0.055*	0.047	0.267**	0.310**
	(0.030)	(0.031)	(0.108)	(0.144)
Observations	1,352	1,241	702	612
R-squared	0.004	0.005	0.007	0.013

Note: OLS estimates using district-year aggregates from ENAHO. The sample period is 2011-2013. The dependent variables are alternative measures of male programme beneficiaries. The key explanatory variable is Humala's district vote share in the 2011 presidential election. All specifications control for the programme's official district selection criteria and include year fixed effects. Standard errors are clustered at the district level. The upper panel reports estimates for the three priority departments (Ayacucho, Apurímac, and Huancavelica); the lower panel reports estimates for all districts. ***p<0.01, **p<0.05, *p<0.1

Table A5: district incorporation, 2011-16

	RD Full Bandwidth	RD Narrow Bandwidth
Trend	0.025	0.213
	(0.106)	(0.352)
Discontinuity	-0.015	-0.084
	(0.031)	(0.053)
Trend*Discontinuity	0.112	0.341
	(0.117)	(0.508)
District Poverty	0.612***	0.673***
	(0.019)	(0.026)
Child Malnutrition	0.890***	0.832***
	(0.100)	(0.153)
Child Index	0.036***	0.034***
	(0.006)	(0.008)
Constant	0.237***	0.237***
	(0.039)	(0.053)
Observations	7,032	3060
R-squared	0.514	0.532

Note: Table presents regression discontinuity (RD) estimates controlling for year fixed effects. The first column shows RD estimates using the full bandwidth. The second column shows RD estimates using a narrow bandwidth of 18 percent. Sample size reflects bandwidth choice. Standard errors clustered on the running variable. ***p<0.01, **p<0.05, *p<0.1

Table A6: Regression discontinuity manipulation test

	Left of cutoff	Right of cutoff		
Number of obs.	451	741	Model	unrestricted
Eff. Number of obs.	389	481	BW method	comb
Order est. (p)	2	2	Kernel	triangular
Order bias (q)	3	3	VCE method	jacknife
BW est. (h)	0.175	0.216		
Running variable				
Method	T	P> $\|T\|$		
Robust	-0.8844	0.3765		

Note: Table reports the default parameters and optimal bandwidth selected by the rddensity package for the running variable manipulation test. The sample size is 1192 districts, excluding districts already enrolled prior to Humala's term. The bottom panel reports the manipulation test results.

Table A7: district incorporation, Year-by-Year

	2011	2012	2013	2014	2015	2016
Vote share for Humala	0.097***	-0.116	0.094	-0.135***	0.184***	0.106**
	(0.031)	(0.071)	(0.066)	(0.051)	(0.052)	(0.052)
Poverty criterion 2011	0.111***					
	(0.015)					
Poverty criterion 2012-14		0.541***	0.359***	0.331***		
		(0.023)	(0.032)	(0.038)		
Poverty criterion 2015-16					0.145***	0.815***
					(0.031)	(0.035)
Child Malnutrition	0.062	0.378***	-0.093	-0.028	0.170**	0.027
	(0.045)	(0.099)	(0.076)	(0.062)	(0.082)	(0.064)
Child Index	-0.003	0.027***	0.000	0.019***	0.003	0.024***
	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.005)
Constant	-0.080***	0.138***	-0.033	0.179***	-0.108***	0.086**
	(0.022)	(0.049)	(0.042)	(0.038)	(0.034)	(0.034)
Observations	1,181	1,128	823	738	664	642
R-squared	0.092	0.416	0.288	0.291	0.169	0.698

Note: Each column reports year-specific linear probability estimates of district incorporation. The dependent variable equals one if a district entered *Juntos* in that year. Specifications control for the official district eligibility criteria. Standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.10.

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