

A Foot in the Door: Ethnic Employment Quotas and Democratic Attitudes in Uruguay

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Abstract

Does bringing a marginalized group into the state raise its members' trust in democratic institutions? Uruguay's 2013 Ley 19.122, reserving eight percent of public-sector hires for Afro-descendant citizens, offers a rare test on the dimension representation theory names. Using the Uruguayan household survey and eight waves of the Latinobarómetro, we document a genuine but small first stage, Afro-Uruguayan public employment rose about 1.3 percentage points, a composition shift rather than a net gain, against a target the state met only fractionally. In addition, as second stage delivered a precise attitudinal null, with equivalence tests rejecting effects larger than roughly 0.15–0.20 standard deviations. The result establishes a scope condition rather than a refutation: at this level of exposure, bureaucratic inclusion produces no measurable attitudinal return. How much inclusion is required before the representation mechanism fires remains an open and newly quantifiable question.

Keywords: affirmative action; Afro-descendants; Uruguay; democratic attitudes; institutional trust; representative bureaucracy

I. Introduction

Does giving a historically excluded ethnic group access to the state change how its members relate to democracy? Ethnic and racial minorities in stratified societies consistently exhibit lower voter turnout, lower trust in public institutions, and weaker support for democracy than majority populations. Part of this gap is explained by observable characteristics such as income, education, and place of residence. But as historically marginalized groups, ethnic minorities also face composite disadvantages: discrimination, political under-representation, and exclusion from the institutions that govern them, that can depress their attachment to the democratic order itself (Booth and Seligson 2009; Madrid 2012). If exclusion erodes minority support for democracy, a natural question follows: can deliberate institutional inclusion rebuild it?

This paper studies that question in the setting of an unusually direct policy experiment. In 2013 Uruguay enacted Ley No. 19.122, which reserves a minimum of eight percent of all new public-sector hires for citizens who self-identify as Afro-descendant (hereafter *AD*). Employment quotas of this kind are rare. While the literature on affirmative action is dominated by educational quotas, a broader literature shows that group-targeted mandates can reshape access to state institutions themselves. For example, Pande (2003) finds that political reservation for India's Scheduled Castes increased the share of public-sector jobs reserved for members of those groups, while Chattopadhyay and Duflo (2004) show that randomly assigned village-council quotas altered the policies governments delivered. These studies suggest that quotas can affect not only economic outcomes but also the relationship between marginalized groups and the state. The large literature on ethnic affirmative action is dominated by university admissions and by the United States,

and it focuses overwhelmingly on the economic outcomes of beneficiaries (enrollment, wages, employment) rather than on whether inclusion reshapes their political attitudes (Holzer and Neumark 2000; Schotte, Gisselquist, and Leone 2023). Uruguay's law lets us ask a different question: when a marginalized group is brought into the apparatus of the state, do its members come to see democratic institutions more favorably?

We approach the question in two linked steps that mirror the policy's intended causal chain. First, we ask whether the quota had its intended first-stage effect on the labor market: did it raise the share of Afro-descendants in public employment? Administrative records and our own estimates using the Uruguayan household survey (ECH) indicate that it did, narrowing the AD/non-AD gap in public-sector employment. Second, and centrally, we ask whether the policy moved the democratic attitudes of Afro-descendants, their support for democracy and their trust in political institutions, relative to comparable groups in countries without such a policy, using eight waves of the *Latinobarómetro*. Throughout, we interpret any attitudinal effect through the mechanism that connects the two steps: the idea, drawn from the literatures on descriptive representation and representative bureaucracy, that seeing one's own group inside the state, and gaining material access to it, raises trust in government (Bobo and Gilliam 1990; Riccucci and Van Ryzin 2017).

This analysis contributes to the existing literature in several ways. First, while most evidence on ethnic affirmative action concerns education and the United States, we study a quota in public employment in Latin America, a policy instrument and a region that remain comparatively under-examined (Htun 2004; Francis and Tannuri-Pianto 2012). The Uruguayan law also offers an unusually clean design: a single, datable national reform targeting a well-defined group, in a country with high-quality household and opinion data.

Second, we move the study of affirmative action beyond its usual economic outcomes to a political one. Whether inclusionary policy changes how beneficiaries evaluate democratic institutions is theoretically central to debates about representation and legitimacy, yet there is little causal evidence on it. Third, we take the mechanism seriously rather than asserting it. Because the law operates through public employment, we can test whether the attitudinal response is larger precisely where the employment response was larger, a dose-response test that links the labor-market first stage to the political outcome at the level of the locality.

Our findings are, at this stage, asymmetric in their strength. The labor-market effect is clear and consistent with the government's own administrative figures: Afro-descendants became more likely to hold public-sector jobs after 2013, and the AD/non-AD employment gap narrowed. The attitudinal effects are more limited and concentrated in institutional trust, trust in government and in the legislature, rather than in abstract support for democracy, and the mechanism (dose-response) evidence is suggestive but imprecise. We therefore read the political results as promising but preliminary, and treat the employment results as the firmer of the two. This pattern is itself informative: it suggests that institutional inclusion registers first, and most legibly, in citizens' evaluations of the specific institutions doing the including, rather than in their diffuse commitment to democracy as a system.

II. Race, Affirmative Action, and Political Inclusion: Review of the Literature

This paper sits at the intersection of three literatures: the economics and policy analysis of affirmative action; the study of race, ethnicity, and nation-building in Latin America; and the political-behavior literature on how representation and institutional

inclusion shape the attitudes of minority citizens. We take each in turn and then state the gap we address.

Affirmative action: rationale and effectiveness.

Affirmative action policies are designed to counteract systematic disadvantage and unequal access to education, employment, and government for groups defined by race, ethnicity, caste, or gender. The empirical balance of evidence is broadly favorable. In a systematic review of 194 quantitative studies from around the world, Schotte, Gisselquist, and Leone (2023) find that close to two-thirds (about 63%) report a dominantly positive effect of affirmative action on the targeted group's education and employment outcomes, though they caution that the qualitative implications are more ambiguous, that the evidence is concentrated in a few country cases, and that rigorous causal studies remain scarce. For the United States, Holzer and Neumark's influential review concludes that affirmative action measurably improved the representation and outcomes of women and minorities, with at most modest efficiency costs (Holzer and Neumark 2000). In Latin America, the best-identified evidence comes from Brazil's university quotas: Francis and Tannuri-Pianto (2012) exploit the University of Brasília's 2004 racial quota and find that it raised the share of black students and shifted the socioeconomic composition of admits, without reducing applicant effort—while also inducing some applicants to reclassify their racial identity. Two themes carry into our setting. The first is that affirmative action generally “works” on the margin it targets, but its broader and second-order effects are far less studied. The second is that such policies are frequently contested and can provoke backlash, both among non-beneficiaries and in public opinion at large (Htun 2004)—which makes the question of how beneficiaries themselves respond all the more important.

A related literature on policy feedback suggests that public policies can shape how citizens perceive government and politics. The most direct precedent in the Uruguayan context is Manacorda, Miguel, and Vigorito (2011), who find that participation in Uruguay's PANES cash-transfer program increased beneficiaries' support for the incumbent government by 11–13 percentage points, with effects persisting after the program ended. This raises the possibility that policies aimed at inclusion may influence not only material outcomes but also citizens' political attitudes toward the institutions implementing them.

Race and the politics of mestizaje in Latin America.

The Latin American context gives the affirmative-action question a distinctive shape. For much of the twentieth century, Latin American states managed their ethnic diversity through the ideology of *mestizaje* and “racial democracy”: an official color-blindness that celebrated racial mixture as the essence of the nation while denying the salience of race and, in practice, sustaining durable racial hierarchies (Paschel 2016; Telles, Ethnicity, and (PERLA) 2014). One consequence was statistical invisibility—censuses and surveys across the region largely stopped recording ethnoracial identifiers between the 1940s and 1980s, which is why empirical study of racial inequality in countries like Uruguay only became possible once such questions were reintroduced (Bucheli and Porzecanski 2011). Indeed, the return of ethnoracial categories to official statistics reflects a broader regional shift toward recognizing previously invisible populations (Loveman 2014).

The comparative data assembled by the PERLA project nonetheless document that skin color and ethnoracial classification remain strongly associated with socioeconomic

status across the region (Telles, Ethnicity, and (PERLA) 2014). Soler-Lopez (2016) document this as well for Latin America. Beginning in the 1980s and accelerating in the 1990s and 2000s, Afro-descendant and Indigenous movements (aided by a favorable international ethno-racial field and by constitutional moments such as Colombia's 1991 charter) pressed states to recognize ethnic difference and to adopt targeted policies (Hooker 2005; Madrid 2012; Paschel 2016). Htun (2004) documents the resulting reversal in Brazil, where a state that had long defended racial democracy endorsed quotas for Afro-Brazilians in higher education and the civil service. Uruguay's Ley 19.122 belongs to this regional turn, but with a twist that makes it analytically useful: it places its quota in public employment rather than university admissions, and it does so in a country whose national self-image as a homogeneous, egalitarian “exception” had long suppressed the very idea of racial policy.

Ethnic minorities and democratic attitudes.

Why might inclusion matter for how minorities view democracy? A long line of work argues that economic and political marginalization breeds disaffection: groups excluded from power and stigmatized in public life have reason to doubt that democratic institutions represent them, and this is often invoked to explain lower minority trust and participation (Madrid 2012; Bueno and Dunning 2017). Yet the Latin American evidence on this point is surprisingly mixed. Evidence from Brazil and other Latin American contexts shows that skin color stratification is strongly associated with socioeconomic inequality but does not always translate into consistent political differences in attitudes toward democracy or institutions (Bailey, Loveman, and Muniz 2013).

Using comparative survey data across eighteen countries, Sánchez, Doherty, and Dobbs (2020) find little support for the expectation that darker-skinned respondents are less favorably disposed toward the political system, even as skin color strongly predicts socioeconomic status—a socioeconomic “pigmentocracy” that does not translate cleanly into a political one. Research on political incorporation provides additional reasons to expect attitudinal effects. Inclusion in government programs often increases political engagement and support for state institutions. Using the randomized rollout of Mexico’s Progresa program, De La O (2013) find that beneficiaries became more likely to vote and support incumbents. At the same time, such effects may have limits: Zucco (2013) shows that Brazil’s transfer programs increased incumbent vote shares without generating durable partisan realignment. These findings suggest that policy-induced support may be strongest for specific institutions rather than for broader political identities.

More broadly, Booth and Seligson's study of political legitimacy in Latin America shows that the link between citizens' grievances and their support for the regime is looser and more multidimensional than simple exclusion accounts imply (Booth and Seligson 2009). This unsettled state of the evidence is precisely what makes the Uruguay case valuable: rather than inferring the attitudinal consequences of exclusion from cross-sectional correlations, we can observe how attitudes move when an excluded group is, for the first time, deliberately brought in.

The mechanism: representation, inclusion, and trust.

The strongest reason to expect an attitudinal response runs through the classic conceptual distinction between descriptive and substantive representation established by Pitkin (1967), and the literatures on descriptive representation and representative

bureaucracy. This perspective is consistent with constructivist accounts of ethnic identity, which emphasize that political and institutional incentives can shape the salience and activation of ethnic identities themselves (Chandra 2012).

The classic finding is that of Bobo and Gilliam (1990): African Americans living in “black-empowered” cities—those with black mayors and council representation—display higher political trust, knowledge, and participation than observably similar individuals in unempowered cities. Subsequent work generalizes the result: descriptive representation can raise minority trust, efficacy, and engagement (Banducci, Donovan, and Karp 2004; Gay 2002; Mansbridge 1999), though the effect is conditional and not universal. A parallel literature in public administration makes the mechanism concrete for a policy like Uruguay's. The theory of representative bureaucracy holds that when the people who staff the state visibly resemble the citizens they serve, even passively or “symbolically,” citizens extend more trust to public institutions and become more willing to cooperate with them (Ricucci and Van Ryzin 2017). This expectation is consistent with evidence showing that bureaucratic representation shapes both policy outcomes and perceptions of fairness in public administration (Keiser et al. 2002). Survey-experimental evidence supports this too: minority and female respondents report greater trust in, and perceived legitimacy of, public agencies when those agencies are more representative of them (Ricucci, Van Ryzin, and Lavena 2014). Field evidence from local bureaucracies similarly finds that greater descriptive representation increases citizen satisfaction and trust in public services (Theobald and Haider-Markel 2009). More recent survey-experimental work directly shows that citizens evaluate representative bureaucracy itself as normatively legitimate and perceive it as fair under conditions of active representation (Van Ryzin

2021). Baker, Rubin, and Weinberg (2024) further shows that greater workforce diversity in government agencies improves perceptions of distributive justice and fairness of policy outcomes among citizens.

The importance of labor-market incorporation as a mechanism of broader social and political integration is also evident outside the affirmative-action literature. Studying an Italian amnesty program, Devillanova, Fasani, and Frattini (2018) show that the prospect of legal status significantly increased employment among undocumented immigrants, even before legalization itself occurred. Likewise, Hainmueller, Hangartner, and Pietrantuono (2015) find that naturalization fosters the long-run political integration of immigrants. Together, these studies suggest that formal inclusion into state-recognized institutions can reshape both economic and political incorporation. In the context of Ley 19.122, public-sector employment may therefore constitute an important channel through which institutional inclusion affects attitudes toward the democratic system. A complementary policy-feedback tradition adds that the design of policies teaches citizens what to expect from government and how much voice they have, shaping their political attitudes and participation downstream (Soss 1999). This line of argument builds on foundational work showing that public policies systematically shape political behavior and attitudes through feedback effects (Campbell 2003; Mettler 2005). A broader synthesis of the policy-feedback literature reaches a similar conclusion. Public policies not only distribute resources but also shape citizens' political identities, expectations, and orientations toward government (Mettler and Soss 2004; Soss 1999). This perspective implies that affirmative-action policies may influence democratic attitudes even among individuals who do not directly benefit from employment gains, insofar as the policy signals recognition and

inclusion by the state. An employment quota sits squarely at the junction of these literatures: it is at once a material transfer (jobs) and a form of symbolic representation (visibly placing the minority inside the state), and both channels point toward higher institutional trust among beneficiaries.

A complementary literature on immigrant integration reaches similar conclusions regarding the importance of labor-market incorporation. Studying refugee integration policies in Europe, Fasani, Frattini, and Minale (2022) and Fasani, Frattini, and Pirot (2024) argue that access to stable employment serves as a key mechanism through which disadvantaged groups become socially and institutionally integrated into the host society. Although Afro-Uruguayans are a native minority rather than an immigrant population, the underlying logic is comparable: labor-market inclusion may alter how historically marginalized groups perceive and interact with public institutions. This perspective reinforces our focus on public employment as the primary channel through which Ley 19.122 may affect democratic attitudes.

Taken together, recent advances in representative bureaucracy and policy feedback research suggest a unified mechanism in which descriptive and symbolic representation within state institutions shapes perceived fairness, institutional trust, and willingness to see the state as legitimate (Johnston and others 2024; Baker, Rubin, and Weinberg 2024; Van Ryzin 2021).

Descriptive representation, symbolic inclusion, and institutional trust.

A closely related body of work, originating in the study of women's political representation, shows that the visible presence of historically marginalized groups in positions of authority can reshape citizens' attitudes toward political institutions beyond

any change in policy. Gender quotas, in particular, increase not only the numerical presence of women in office but also citizens' attitudes toward politics and democratic institutions, operating through a policy-feedback logic: by publicly recognizing a group's under-representation and creating visible opportunities for inclusion, quotas signal that institutions are responsive to longstanding inequalities, leading citizens to update their beliefs about who belongs in politics and whether institutions represent people like themselves (Fernández and Valiente 2021; Soss 1999). Exposure to women in office has been linked to greater political efficacy, stronger beliefs in women's competence, and higher engagement, with effects often strongest among women themselves (Wolbrecht and Campbell 2007; Beaman et al. 2009). Critically for the question of whether affirmative action undermines legitimacy, recent cross-national survey experiments find that citizens evaluate gender-balanced institutions as more legitimate, fairer, and more democratic than all-male bodies, and that these gains largely persist even when balance is achieved through quotas (Clayton, O'Brien, and Piscopo 2025).

This literature offers a template for ethnic affirmative action. Afro-descendants and women differ in important respects, but both have faced barriers to political inclusion and under-representation within state institutions. If citizens read women's growing presence in politics as evidence that institutions are becoming more inclusive, a similar mechanism may operate when members of a marginalized ethnic group gain visibility within the state. On this view, an employment quota may generate benefits beyond its direct beneficiaries: by increasing the visibility of Afro-descendants within the state apparatus, it may signal institutional openness and recognition, strengthening perceptions of representation and

fairness among Afro-descendant citizens; attitudinal changes that, in principle, could precede any material gain. We test this possibility directly below.

The gap.

What is missing is causal evidence on whether an ethnic affirmative-action policy actually changes the democratic attitudes of its intended beneficiaries, and whether any such change operates through the inclusionary channel the theory specifies. The affirmative-action literature documents economic effects but rarely political ones; the representation literature documents attitudinal effects of elected and bureaucratic representation but rarely studies them as the outcome of a specific, datable policy in a developing democracy; and the Latin American race literature is divided on whether exclusion even depresses minority system support in the first place. Ley 19.122 lets us speak to all three: a clean policy shock, a measurable first stage in public employment, and rich opinion data on the targeted group.

III. Background: Afro-Uruguayans and Ley No. 19.122

Afro-Uruguayans and the “exceptional” nation.

Uruguay has long presented itself as a homogeneous, European-descended, and egalitarian exception within Latin America (the “Switzerland of South America”) an image that, as elsewhere in the region, rendered its Afro-descendant minority statistically and politically invisible for much of the twentieth century (Bucheli and Porzecanski 2011). That image is misleading. Afro-Uruguayans make up roughly eight percent of the population Using the first household survey to record racial descent, Bucheli and Porzecanski (2011) estimate an Afro-descendant/white wage ratio of about 0.72 for men and 0.78 for women, and attribute a sizable share of the gap—roughly half for men—to

labor-market discrimination operating partly through occupational segregation, with the remainder driven by differences in schooling. Afro-Uruguayans are also poorer (poverty among AD runs at roughly double the national rate) and under-represented in stable, formal, public-sector employment. Political inequalities mirror these socioeconomic disparities. Across Latin America, Afro-descendants remain under-represented in elected office and face substantial barriers to political representation (Janusz 2022; Bueno and Dunning 2017; Janusz and Sells 2022). These patterns reinforce the importance of policies that increase minority visibility within state institutions. These disparities persist despite Uruguay's comparatively low overall poverty and high per-capita income, which makes the country a useful “least-likely” setting: if even Uruguay exhibits durable ethnoracial inequality, the racial-democracy self-image is hard to sustain.

The policy: Ley No. 19.122 (2013).

Against this backdrop, and as part of the broader regional turn toward ethnic recognition, Uruguay enacted Ley No. 19.122 on 21 August 2013. The law formally recognizes that the Afro-Uruguayan population has been the victim of historical racism and discrimination, and adopts affirmative-action measures to redress it. Its central provision reserves a minimum of eight percent of vacancies filled by the state (across the central government, departmental governments, autonomous agencies, and other public bodies) for persons who self-identify as Afro-descendant, with ethnoracial self-identification as the operative criterion. The law also establishes complementary measures in education (including scholarship quotas) and vocational training, and it assigns monitoring and annual-reporting obligations to public entities, with the National Civil Service Office (*Oficina Nacional del Servicio Civil*, ONSC) compiling compliance and the

Ministry of Social Development (MIDES) and the national statistics institute (INE) supporting implementation.

Uruguay thereby became one of the few countries in the region (and one of very few anywhere) to adopt an explicit ethnic quota in public employment rather than in university admissions.

Implementation and compliance.

Compliance with the quota has been partial and uneven, a fact that is important both substantively and for our research design. Official monitoring indicates that Afro-descendants accounted for only about 2.7% of public-sector entries in 2021— well short of the eight-percent target—and that a large share of public bodies (around 43% in 2021) failed to submit the required annual reports (United Nations Committee on the Elimination of Racial Discrimination (CERD) 2023). In July 2023 the government issued a formal implementation guide intended to make the eight-percent quota operational across agencies. For our purposes, imperfect compliance does not undermine identification; it bounds it. The treatment we study is the introduction of the policy (an intent-to-treat), and even a partial increase in Afro-descendant public employment generates the first-stage variation whose downstream attitudinal consequences we estimate. Indeed, the administrative record confirms a real, if sub-target, rise in the AD share of public employment after 2013, consistent with our ECH estimates.

Why Uruguay is a useful case.

Three features make Ley 19.122 well suited to the question of this paper. First, the treatment is sharp and datable: a single national reform, enacted in 2013, targeting a clearly defined group, which supports a difference-in-differences design against comparable Latin

American countries that adopted no such policy. Second, Uruguay's data are unusually good for the region—a large continuous household survey (the ECH, with on the order of 120,000 individuals per year) for the employment first stage, and the Latinobarómetro for the opinion outcomes—allowing us to measure both the labor-market and the attitudinal links in the causal chain. Third, the country's racial-democracy self-image works in the design's favor: because race was so long denied salience, the 2013 quota represents an unusually discrete shift from non-recognition to explicit ethnic targeting, with little prior policy of the same kind to confound it.

IV. Data

Latinobarómetro

The data used in this analysis comes from the Latinobarómetro surveys (Corporacion Latinobarometro 2024). These public opinion surveys are carried out in an annual basis (with few exceptions) across 18 Latin American and the Caribbean countries (and on occasion in Spain), providing a comprehensive overview of political attitudes, behaviors, and socio-economic conditions in the region. The survey data spans from 1995 to the most recent wave in 2024, offering rich information about opinions on democracy, governance, as well as economic development, and social issues.

The Latinobarómetro includes detailed demographic information, such as age, gender, income, ethnicity, and education, alongside key variables on political participation, trust in democratic institutions, and perceptions of government performance. Additionally, and depending on the rooster, the survey also captures attitudes toward electoral processes, with focus on local and national elections; but also includes questions on contentious issues like egalitarian marriage, abortion, or legalization of soft drugs.

The surveys are largely harmonized across countries, but changes in methodology over time can introduce inconsistencies. To address potential issues from these changes, we invested significant effort in further harmonizing the remaining parts of the survey to ensure consistency across waves and countries.

Table 1. Country Frequencies and Cumulative Percentages

Country	Freq.	Percent	Cum.
Argentina	9,900	5.94	5.94
Bolivia	9,954	5.98	11.92
Brasil	10,650	6.39	18.31
Chile	10,083	6.05	24.37
Colombia	9,837	5.91	30.27
Costa Rica	8,422	5.06	35.33
Rep. Dominicana	8,736	5.24	40.57
Ecuador	10,583	6.35	46.93
El Salvador	7,463	4.48	51.41
Guatemala	8,034	4.82	56.23
Honduras	8,122	4.88	61.11
Mexico	8,644	5.19	66.30
Nicaragua	8,216	4.93	71.23
Panama	8,600	5.16	76.39
Paraguay	9,146	5.49	81.88
Peru	9,703	5.83	87.71
Uruguay	10,121	6.08	93.79
Venezuela	10,349	6.21	100.00
Total	166,563	100.00	

Source: Latinobarómetro, pooled 2007–2018 waves.

In our analysis, we use data from 2007 to 2018 waves, which comes up to 8 waves, considering that the survey was not done in several years. We cannot include data for other years as the race variable was not asked. We restrict our sample to all countries in the region except Spain. Additionally, we exclude observations with missing or null information to ensure data quality and consistency. Our workable sample includes observations from 17 countries in the region with total of 166,563 observations. Table 1 presents the sample composition of the pooled data.

Dependent Variables: Democratic Institution Support

To capture support for democratic institutions we use six binary outcomes from the Latinobarómetro. The first three measure institutional Confidence (or trust): respondents are asked how much Trust they have in the government, the judiciary, and congress, on a four-point scale ("a lot," "some," "a little," "none"); we code each variable 1 if the respondent reports "a lot" or "some" Trust and 0 otherwise. The remaining three capture broader evaluations of the political system. *Country governed for all* equals 1 if the respondent says the country is governed "for the good of all the people" rather than "by a few powerful groups in their own interest." *Approval of government* equals 1 if the respondent approves of the way the current administration is doing its job, and 0 if they disapprove. *Country is progressing* equals 1 if the respondent says the country is progressing, and 0 if they see it at a standstill or in decline. "Don't know" and refused responses are coded as missing throughout.

In addition to these, for the summary specification, we also construct an institutional-trust index: the standardized mean of the three (standardized) Trust items.

Main independent variable: Ethnicity

The race variable is created based on respondents' self-identification with the question, "With which ethnicity or race do you identify better?" Respondents were given several options to choose from, including "Asian," "Black," "Indigenous," "Mestizo," "Mulato," "White," "Other race,". The categories might change from country to country, for example, in Brazil, the category of Preto is generally recognized as black, while Pardo is a more general, afro-descendant.

For the purposes of analysis, the responses are categorized as follows:

itemize “Asian,” “Black,” “Indigenous,” and “Mulato” are used to identify minorities, both as pan-group and as individual ethnicities. Table two summarises the percentage of the different groups by country. “Not applicable,” “No answer/Refused,” and “Don’t know” are treated as missing and excluded from the analysis. itemize

Table 2. Afro-Descendant, Black, and Indigenous Population Distribution by Country (%)

Country	Afro-Des. (%)	Black (%)	IP (%)
Argentina	1.6	1.6	3.3
Bolivia	1.4	0.5	27.3
Brazil	30.5	16.7	18.5
Chile	1.3	0.5	7.1
Colombia	12.6	12.6	19.0
Costa Rica	17.9	17.9	22.2
Rep. Dominicana	47.6	22.8	29.1
Ecuador	6.7	3.1	8.7
El Salvador	8.0	4.1	10.9
Guatemala	2.3	0.9	44.1
Honduras	6.3	3.1	15.5
Mexico	3.2	0.7	19.5
Nicaragua	7.5	4.9	13.0
Panama	19.5	14.2	21.4
Paraguay	2.5	1.2	3.6
Peru	3.7	2.0	10.3
Uruguay	5.9	5.9	7.6
Venezuela	22.8	6.8	11.3
Total	11.2	6.6	15.9

Source: Latinobarómetro, pooled 2007–2018 waves. IP = Indigenous peoples.

Other Variables – Controls

Survey Control Variables: We include an array of demographic variables (gender, age, marital status, urban); and education variables (Primary, Secondary and Tertiary). We also include household wealth and economic variables: (1) a wealth index created with Principal component analysis using several household goods/conditions (internet, washing

machine, motorcycle, overcrowding, potable water and tablet/PC). (2) current and future perceived economic state.

Encuesta Continua de Hogares (ECH)

To measure the *employment* effect of the policy we use the Uruguayan *Encuesta Continua de Hogares* (ECH), the continuous household survey run by the National Statistics Office (INE). The ECH covers 2009–2022 with roughly N per year ≈ 120,000 individuals per year and records ethnicity, labour-market status (including public- vs. private-sector employment), education, department, and standard demographics.

Table 3. ECH Summary Statistics, by Afro-Descendant Status and Pre/Post 2013

	Afro-descendant				Non-Afro-descendant			
	Pre-2013		Post-2013		Pre-2013		Post-2013	
Public-sector empl.	0.143	(0.350)	0.158	(0.365)	0.171	(0.377)	0.172	(0.378)
Unemployed	0.060	(0.238)	0.068	(0.251)	0.042	(0.202)	0.047	(0.211)
Inactive	0.188	(0.391)	0.190	(0.392)	0.182	(0.386)	0.186	(0.389)
Inactive (no students)	0.088	(0.283)	0.097	(0.296)	0.096	(0.294)	0.109	(0.311)
Female	0.543	(0.498)	0.546	(0.498)	0.531	(0.499)	0.533	(0.499)
Age	42.606	(11.319)	43.227	(11.441)	44.290	(11.548)	44.865	(11.637)
Years of education	8.128	(3.446)	8.697	(3.549)	9.777	(3.988)	10.199	(3.962)
Observations	27,761		34,771		290,397		321,677	

Notes: ECH 2009–2019, individuals aged 25–65. Means with standard deviations in parentheses; the pre/post split is at Law 19.122 (2013).

Table 4. Latinobarómetro Summary Statistics, by Afro-Descendant Status and Pre/Post 2013

	Afro-descendant				Non-Afro-descendant			
	Pre-2013		Post-2013		Pre-2013		Post-2013	
Socio-demographics								
Female	0.46	(0.50)	0.46	(0.50)	0.52	(0.50)	0.53	(0.50)
Urban	0.60	(0.49)	0.60	(0.49)	0.73	(0.44)	0.58	(0.49)
Years of education	9.28	(4.73)	9.98	(4.56)	10.74	(4.20)	11.17	(4.14)
Age	40.70	(16.11)	40.98	(16.67)	42.04	(16.75)	43.65	(17.01)
Married	0.57	(0.50)	0.34	(0.47)	0.57	(0.50)	0.37	(0.48)

Wealth quintile	3.44	(1.27)	3.10	(1.31)	3.01	(1.43)	2.82	(1.35)
Supports democracy	0.59	(0.49)	0.52	(0.50)	0.65	(0.48)	0.64	(0.48)
Baseline Opinion								
Democracy best system	0.80	(0.40)	0.68	(0.47)	0.84	(0.37)	0.77	(0.42)
Satisfied with democracy	0.39	(0.49)	0.30	(0.46)	0.43	(0.50)	0.33	(0.47)
Country governed for all	0.31	(0.46)	0.23	(0.42)	0.25	(0.43)	0.19	(0.39)
Approves government	0.71	(0.45)	0.42	(0.49)	0.58	(0.49)	0.40	(0.49)
Country is progressing	0.40	(0.49)	0.28	(0.45)	0.37	(0.48)	0.24	(0.43)
Government Trust	0.44	(0.50)	0.26	(0.44)	0.43	(0.50)	0.28	(0.45)
Judiciary Trust	0.35	(0.48)	0.25	(0.43)	0.32	(0.47)	0.25	(0.44)
Congress Trust	0.31	(0.46)	0.20	(0.40)	0.33	(0.47)	0.22	(0.42)
Life Satisfaction	0.77	(0.42)	0.81	(0.40)	0.77	(0.42)	0.79	(0.41)
Economy Now	0.76	(0.43)	0.64	(0.48)	0.72	(0.45)	0.62	(0.49)
Own Economy Future	0.86	(0.35)	0.82	(0.38)	0.85	(0.36)	0.78	(0.41)
Own Economy Now	0.81	(0.39)	0.83	(0.37)	0.89	(0.31)	0.80	(0.40)
Observations	2,300		1,366		29,609		13,797	

Notes: Pooled Latinobarómetro waves, comparison-country set used in the analysis. Means (standard deviations) weighted; pre/post split at 2013.

Population Census

The 2011 Uruguayan population census provides two inputs. First, it serves as the benchmark against which we validate the Latinobarómetro sample: the census records a 7.6% Afro-descendant share among adults, closely matching the 6.6% (weighted) Afro share in our Latinobarómetro Uruguay sample, which indicates that the survey captures the target population well. Second, we use the census microdata to compute the Afro-descendant population share of each locality, which serves as the treatment-intensity “dose” in Strategy 3.

V. Methods

Conceptual Framework and Mechanism

The empirical strategy is guided by a simple theory of change: the quota raises AD access to stable public employment, and improved labour-market inclusion in turn raises

AD trust in, and support for, democratic institutions. The estimation therefore proceeds in three linked steps, mirroring the three objectives stated in the Introduction: (i) the effect of the policy on AD public employment (the “first stage”), (ii) the reduced-form effect on AD democratic attitudes, and (iii) whether the attitudinal effect is stronger where the employment effect was larger (the mechanism / dose–response). See Figure 1.

Figure 1. Mechanism

Notes: Implicit mechanism; the affirmative-action intervention might also have a direct effect on views.

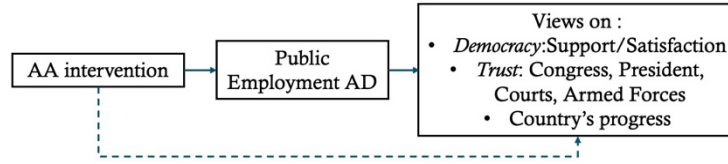


Figure 1: Implied Mechanism

To analyze the relationship between democracy and ethnicity, we first use a linear probability model of the sort:

$$DemocracySupport_i = \beta_0 + \beta_1 Ethnicity_i + \beta_4 X_i + \Psi + \varepsilon_i \quad (1)$$

where X_i is a vector of control variables for the household, which include socioeconomic status, household finances, and other relevant factors. We include country fixed effects (Ψ) to control for common factors at the country level.

Additionally, we estimate a difference-in-differences specification using a multilevel model of the sort:

$$y_{igt} = \lambda_t + \alpha_g + x_{igt}\beta + z_{igt}\lambda_{gt} + v_{gt} + u_{igt} \quad (2)$$

- λ_t is the year fixed effect,

- α_g is the country fixed effect,
- x_{igt} is 1 for Uruguayan minority individuals after 2013, and β is the coefficient capturing the effect of the policy,
- z_{igt} includes other covariates with country-time-varying coefficients,
- v_{gt} is a country-year random error term.

Three Estimation Strategies

Strategy 1 — Public employment (first stage, ECH). A two-way fixed-effects (TWFE) model on the ECH panel estimates the effect of the policy on the probability of holding public-sector employment:

$$PublicEmp_{idt} = \gamma(AD_i \times Post_t) + x'_{idt}\delta + \theta_t + \mu_d + \varepsilon_{idt} \quad (3)$$

where $AD_i \times Post_t$ is the treatment (Afro-descendant individuals from 2013 onward), θ_t are year fixed effects, μ_d are department fixed effects, and x_{idt} are controls (education, age, age², and demographics). Estimated by OLS and logit. *Data:* ECH 2009–2022.

Strategy 2 — Democratic attitudes (cross-country and within-Uruguay). Equation (2) applied to the opinion outcomes (support for democracy, the democracy scale, and institutional-trust measures), with Uruguayan AD after 2013 as the treated group and a donor pool of comparison countries (Argentina, Chile, Colombia, Costa Rica, and Paraguay) used as controls. *Data:* Latinobarómetro. We treat the within-Uruguay difference-in-differences as the primary specification and the cross-country version as a robustness check; because the latter has few country clusters and a single treated country, we report wild-cluster-bootstrap and permutation p-values rather than relying on cluster-robust standard errors.

Strategy 3 — Dose–response (mechanism, $LB \times ECH$ at municipality level).

To test whether attitudinal effects run through employment, the treatment is interacted with the *municipality-level* change in AD public employment between the pre- and post-treatment periods:

$$y_{imt} = \pi(AD_i \times Post_t \times \Delta PubEmp^{AD}_m) + x'_{imt}\delta + \theta_t + \alpha_m + \varepsilon_{imt} \quad (4)$$

Here $\Delta PubEmp^{AD}$ at the municipality level is built by merging Latinobarómetro municipalities to the ECH (constructed by merging Latinobarómetro municipalities to the ECH). A positive π indicates that the attitudinal effect is larger where the employment effect was larger, consistent with the labour-market channel.

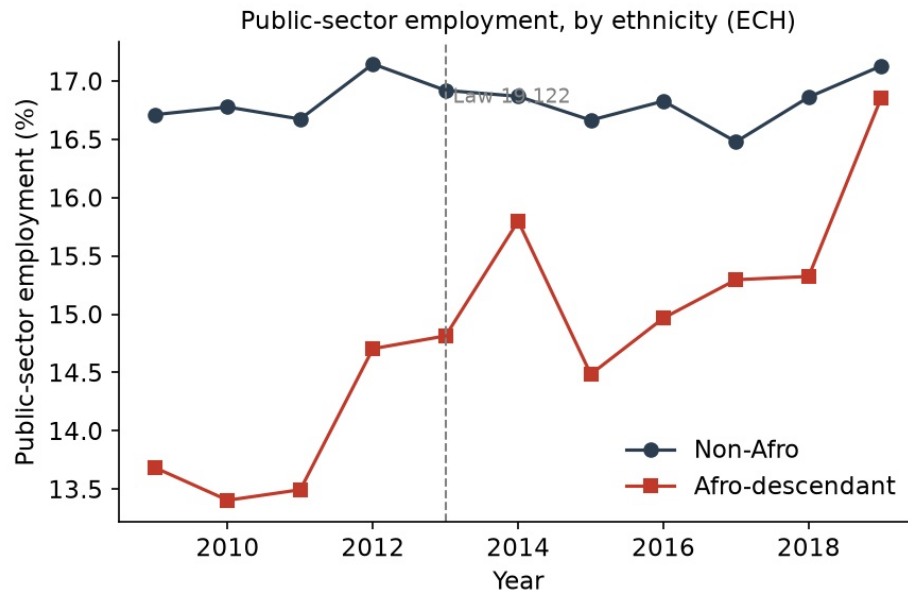
VI. Results

Effect on Public Employment (Strategy 1)

The clearest effect of the policy is on the labour market. Table 5 reports the difference-in-differences estimates from the ECH. Afro-descendants became about **1.3 percentage points** more likely to hold public-sector employment after 2013 (column 1, OLS, significant at the 1% level), a result corroborated by the logit estimate in column 2 (+0.111 log-odds). Figure 2 shows the underlying trajectories: the Afro-Uruguayan public-employment rate rose from about 13.5% in 2009–2011 to about 16.9% by 2019, narrowing its gap with non-Afro Uruguayans. Two further coefficients qualify the interpretation: the same treatment is associated with small *increases* in inactivity (+0.006) and unemployment (+0.008). Because labour-force states sum to one, this implies that the quota shifted Afro-Uruguayans *out of private and into public* employment (and, at the margin, into job search), rather than raising their overall employment. The first stage is therefore best described as

a public-sector *composition* effect, consistent with administrative reports of rising—if sub-target—Afro hiring.

Figure 2. Public-Sector Employment Rate by Ethnicity, ECH 2009–2019



Notes: The vertical line marks Law 19.122 (2013).

Table 5. Effect of Law 19.122 on Labour-Market Outcomes (ECH Difference-in-Differences)

	Public employment		Inactive		Unemployed
	(1)	(2)	(3)	(4)	(5)
	OLS	Logit	All OLS	No Students OLS	OLS
Treated (AD x post-2013)	-0.015*** (0.004)	0.118*** (0.033)	-0.005 (0.004)	-0.003 (0.003)	0.002 (0.003)
Afro-descendant	-0.033*** (0.003)	-0.248*** (0.028)	0.021*** (0.003)	0.010*** (0.002)	0.016*** (0.002)
Post-2013	0.004 (0.003)	0.028 (0.018)	-0.011*** (0.002)	0.008*** (0.001)	0.003** (0.001)
Year FE	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes
Sex, age controls	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.172	0.172	0.146	0.060	0.046

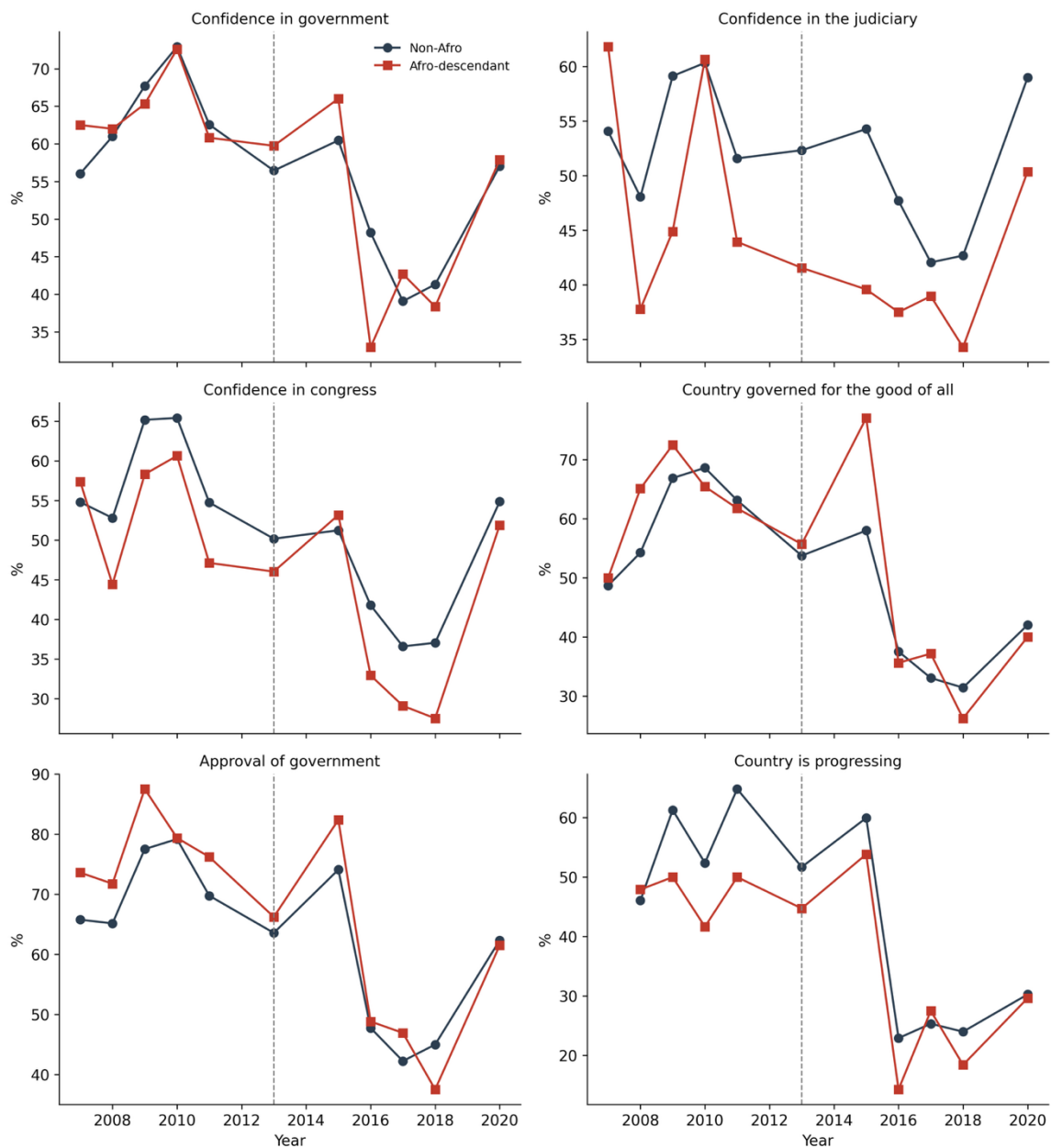
Observations	440,387	440,387	546,374	496,387	465,968
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Notes: ECH 2009–2019 (excl. 2010); robust standard errors in parentheses. Treated is the difference-in-differences coefficient (Afro-descendant $\times$ post-2013). Column 2 reports logit coefficients (log-odds). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Effect on Democratic Attitudes (Strategy 2)

In sharp contrast to the labour market, the policy produced no detectable change in Afro-Uruguayan political attitudes. We begin with the cleanest design: a within-Uruguay difference-in-differences that compares Afro-descendants to non-Afro Uruguayans before and after 2013, with year fixed effects and demographics (Table 6). The coefficient on AD $\times$ Post is small and statistically insignificant for the institutional-trust index (-0.050), for Trust in government (-0.039), and for every other outcome. Figure 3 makes the point visually: Trust in government among Afro-Uruguayans tracks that of non-Afro Uruguayans throughout the period, including after 2013, with no post-policy divergence. The same logic applies to the other measures.

Figure 3. Trust in Government by Ethnicity, Latinobarómetro (Uruguay)



Notes: The vertical line marks Law 19.122 (2013).

Table 6. Within-Uruguay Difference-in-Differences on Political Attitudes (Afro-Descendant $\times$ Post-2013)

	(1) Trust index (SD)	(2) Government Trust	(3) Judiciary Trust	(4) Congress Trust	(5) Country is for all	(6) Approval of Government	(7) Country is progressing
AD x post-2013	-0.050 (0.099)	-0.039 (0.050)	0.005 (0.052)	-0.009 (0.051)	0.027 (0.052)	-0.031 (0.050)	0.061 (0.049)
Afro-descendant	-0.029 (0.079)	0.030 (0.041)	-0.066 (0.043)	-0.015 (0.042)	0.008 (0.044)	0.030 (0.040)	-0.078* (0.043)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.004	0.513	0.479	0.449	0.463	0.574	0.412
Observations	6,444	6,413	6,241	6,194	5,923	6,080	6,384

Notes: Latinobarómetro, Uruguayan respondents. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This null is precise rather than merely uninformative. Standardising each outcome and applying two one-sided (equivalence) tests, the data reject true average effects larger than roughly 0.13–0.22 standard deviations across outcomes, and larger than 0.15 SD for Trust in government, at the 5% level. One can therefore statistically rule out an average attitudinal response as large as a “small” effect (0.2 SD), while remaining consistent with negligible effects below 0.1 SD.

The cross-country design tells the same story once inference is done honestly. Table 7 reports difference-in-differences estimates that use Afro-descendants in comparable countries as an additional counterfactual. Because the design has few country clusters and a single treated country, cluster-robust standard errors are unreliable; we therefore report wild-cluster-bootstrap and permutation p-values. The estimates are uniformly small and statistically indistinguishable from zero once few-cluster inference is applied: Trust in government is -0.010 (wild-bootstrap $p \approx 0.60$) and Trust in congress +0.001 ($p \approx 0.96$). The

only nominally significant coefficient, trust in the judiciary (-0.052, cluster $p < 0.10$), does not survive the wild bootstrap ($p \approx 0.51$) or permutation inference ($p \approx 0.67$). The cross-country design therefore reinforces, rather than overturns, the within-Uruguay null.

Table 7. Cross-Country Difference-in-Differences on Institutional Trust, with Few-Cluster Inference

	(1) Government Trust	(2) Judiciary Trust	(3) Congress Trust
Treated (URY AD x post-2013)	-0.010 (0.017)	-0.052* (0.024)	0.001 (0.017)
Afro-Uruguayan	0.019 (0.013)	-0.016 (0.022)	-0.049** (0.014)
Pre-determined controls	Yes	Yes	Yes
Race FE	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean dep. var.	0.348	0.318	0.280
Observations	49,042	48,425	48,305
Wild-bootstrap p	0.598	0.508	0.959
Permutation p	1.000	0.667	1.000
Placebo countries	6	6	6

Notes: Latinobarómetro. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Heterogeneity by Local Afro Share (Strategy 3, Exploratory)

The one place an attitudinal signal appears is in the geography of the response. We interact the treatment with the local Afro-descendant population share (from the 2011 census) among Uruguayan respondents, with locality and year fixed effects and standard errors clustered on locality (Table 8). The institutional-trust index responds positively where Afro-Uruguayans are more concentrated—about +0.14 standard deviations per standard deviation of local share (wild-bootstrap $p \approx 0.10$)—and this is driven entirely by

Trust in government (+0.095, wild-bootstrap $p \approx 0.03$), with no response in the democracy-support index.

We treat this result as exploratory for two reasons. First, an event-study version (Table 9) shows that the trust gradient was already drifting upward before 2013, so part of it reflects pre-existing divergence rather than the policy. Second, an alternative “dose” built from where Afro public employment actually rose—a department-level intensity measure constructed from the ECH—does *not* reproduce the result. Together these suggest that any attitudinal response operates through local *exposure*, being surrounded by co-ethnics for whom the policy is salient, rather than through individual labour-market benefit; but the evidence is too fragile to bear weight on its own. Because no Uruguayan locality has a zero Afro share, this is best read as a relative dose-response rather than a clean causal response function (Callaway and Sant’Anna 2021).

Table 8. Dose-Response: Attitudes and the Local Afro-Descendant Share (Latinobarómetro, Uruguay)

	(1) Government Trust	(2) Judiciary Trust	(3) Congress Trust	(4) Democracy best system	(5) Satisfied with democracy	(6) Life Satisfaction	(7) Economy Now	(8) Own Economy Future	(9) Own Economy Now
AD x Post x dose (census, SD)	0.096*** (0.030)	0.018 (0.049)	0.006 (0.039)	0.002 (0.020)	0.002 (0.034)	-0.012 (0.035)	-0.023 (0.027)	-0.045 (0.030)	0.020 (0.027)
Pre-determined controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Locality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.597	0.531	0.538	0.918	0.735	0.775	0.827	0.841	0.903
AD obs.	624	604	601	600	608	640	640	640	640
Clusters	111	111	111	111	111	111	111	111	111
Observations	9,637	9,381	9,301	9,398	9,477	9,784	9,784	9,784	9,784
Wild-bootstrap p	0.010	0.725	0.882	0.903	0.956	0.770	0.429	0.193	0.497

Notes: Uruguayan respondents only. Dose is the locality Afro-descendant population share (2011 census). The coefficient of interest is

AD × Post × local Afro share. Locality and year fixed effects; standard errors clustered on locality. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 9. Dose-Response Event Study (Afro Respondents): Local Afro Share × Year

	(1) Trust index	(2) Democracy index
dose x 2007	0.055 (0.141)	0.024 (0.297)
dose x 2008	0.208 (0.288)	-0.026 (0.219)
dose x 2009	0.243 (0.243)	-0.142 (0.476)
dose x 2010	0.240** (0.110)	0.013 (0.276)
dose x 2013	0.402** (0.158)	0.212 (0.179)
dose x 2015	0.496* (0.250)	0.114 (0.401)
dose x 2018	0.345 (0.363)	-0.213 (0.265)
dose x 2020	0.128 (0.095)	-0.036 (0.247)
Observations	630	628

Notes: Latinobarómetro, Uruguayan Afro-descendant respondents. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

VII. Conclusion

We asked whether an ethnic employment quota—an unusually direct form of state inclusion—changes the political attitudes of its beneficiaries. The answer is nuanced and, in part, sobering. The policy did what it was designed to do in the labour market: Afro-Uruguayan public employment rose by about 1.3 percentage points, shifting the group's employment toward the public sector. But this material and representational inclusion did not produce a measurable change in how Afro-Uruguayans evaluate democratic institutions. The average effect on trust is a precise null (small enough to rule out effects of even modest size) and the only positive signal, a concentration-based dose-response, is exploratory and does not survive the design's own robustness checks.

These results speak directly to the representation and policy-feedback literatures reviewed above. Models of descriptive representation and representative bureaucracy (Bobo and Gilliam 1990; Gay 2002; Riccucci and Van Ryzin 2017), and the gender-quota evidence on symbolic legitimacy (Clayton, O'Brien, and Piscopo 2025; Fernández and Valiente 2021), lead one to expect that bringing a marginalized group into the state would raise its members' political trust. The Uruguayan case shows that this expectation can fail even when inclusion occurs on the relevant dimension. The contrast with more salient, direct transfers is instructive: Manacorda, Miguel, and Vigorito (2011) find that Uruguay's PANES program raised beneficiaries' support for the government by 11–13 points, and Layton, Donaghy, and Rennó (2017) show that Brazil's Bolsa Família raised trust in core institutions and incumbents while leaving diffuse democratic support largely unmoved. A hiring rule that reaches a small minority indirectly, and that was only partially implemented, may simply lack the salience and direct personal benefit that move political trust.

Several non-exclusive interpretations are consistent with the evidence: the quota reached too few people, too indirectly, to register in mass attitudes; implementation was too weak (a 2.7% realised share) to be salient; or attitudinal change of this kind requires more time, or more visible co-ethnic authority, than a hiring rule provides. The honest summary is that, within its first seven years, the quota changed jobs but not, on average, minds.

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