

Come In: Comprehensive Care, Access Frictions, and Social Inclusion*

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Abstract

Can comprehensive care reduce access frictions and promote social inclusion in a minimum-income system? We study ACCEDE—Spanish for “come in”—a clustered randomized controlled trial implemented by Cáritas across 64 parish clusters in Spain, alongside the national Minimum Income Scheme. Treated parishes opened a dedicated space offering personalized inclusion itineraries, benefit-claiming assistance, job-search and skills support, digital access, group activities, and intensive one-on-one accompaniment. Control parishes continued to provide Cáritas’s standard support. Both arms remained eligible for the same usual cash and in-kind material aid, so the experiment identifies the effect of the access and accompaniment bundle rather than the effect of additional transfers. The program increased average monthly household income, constructed from income received over the previous six months, by about €133, or 16 percent of the control-group mean. Intermediate outcomes point to several relaxed frictions: job-market engagement increased across employability measures, access to social rights improved, digital skills and connectivity rose, and participation in community groups increased by nearly half a standard deviation. We do not observe a corresponding improvement in regular contact and perceived support, and we cannot decompose the income gain into benefit take-up and earnings. The results nevertheless show that, even when a national income floor formally exists, informational, administrative, digital, and behavioral frictions can materially reduce its effective reach.

JEL Classification: I32, I38, H53

Keywords: non-take-up of social benefits, access frictions, social exclusion, minimum income scheme, comprehensive care, randomized controlled trial

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1 Introduction

A statutory transfer is not necessarily an effective transfer. Between eligibility and receipt, households face informational, administrative, digital, and behavioral frictions: learning about benefits, understanding rules, completing procedures, maintaining contact with case workers, and coordinating support with job search or training. These frictions are central to the public economics of redistribution because they shape effective incidence: systems with the same statutory generosity may deliver very different resources depending on how easily households can access them.

Spain provides a useful setting in which to study this issue. The country introduced a national guaranteed minimum income in 2020, the Minimum Income Scheme (*Ingreso Mínimo Vital*, IMV), with the goal of placing a floor under household resources. Yet the share of the population at risk of poverty or social exclusion (the AROPE rate) was still 26% in 2022, around twelve million people.¹ Spain’s independent fiscal authority documents a persistent gap between eligibility and receipt: 55 percent of eligible households did not apply for the IMV in 2024, so the scheme reached only 20 percent of households in poverty even though its design would allow reaching more than 50 percent. Non-take-up changed little over the first four years of the benefit’s existence: 57 percent in 2021, 58 percent in 2022, 56 percent in 2023, and 55 percent in 2024 (AIReF, 2026). The documented barriers are precisely the ones emphasized in the broader take-up literature: limited information, administrative complexity, procedural costs, and difficulties navigating the application process.

These barriers are not specific to Spain. Across OECD countries, social assistance programs typically go unclaimed by 20 to 60 percent of eligible households (Hernanz et al., 2004; Bargain et al., 2012). The standard explanations are lack of information about entitlements, stigma, administrative complexity, and the cognitive and time costs of claiming benefits (Currie, 2006). Experimental evidence shows that reducing these costs can matter. Bhargava and Manoli (2015) show that salience and simplification affect take-up of tax benefits. In the closest experimental analogue to our setting, Finkelstein and Notowidigdo (2019) find that informing likely eligible elderly households about SNAP raised enrollment from 6% to 11%, while adding hands-on application assistance raised it to 18%. Assistance, not information alone, closed a large part of the take-up gap.

¹The AROPE indicator (At Risk Of Poverty or social Exclusion) was agreed in 2010 to measure relative poverty in Europe by broadening the at-risk-of-poverty rate, which considers income alone.

This paper asks whether a more comprehensive non-cash access layer can improve economic and social inclusion in a high-income country where an income floor already exists. We study ACCEDE (meaning *come in* in Spanish), a clustered randomized controlled trial implemented by the Catholic charity Cáritas across 64 parish clusters in Spain. The design isolates a policy margin that is usually hard to observe. Both treatment and control parishes continued to provide Cáritas’s usual support: cash and in-kind material aid, referrals to social services, and information on procedures. Treated parishes added a dedicated and easily identifiable space, equipped with computers and internet access, in which participants received personalized inclusion itineraries, benefit-claiming assistance, active job-search and skills support, digital access points, group activities using a participatory methodology, and intensive one-on-one accompaniment. Since the usual material aid was available both for treated and control parishes, the experiment estimates the effect of adding access, training, and accompaniment around the existing safety net rather than the effect of adding cash.

Our main result is that this non-cash bundle increased household resources. Treated households report about €133 more in average monthly household income, constructed from income received over the previous six months, equivalent to roughly €800 over the six-month recall period and 16 percent of the control-group mean. This is a sizeable gain for an intervention that did not provide additional cash transfers. Consistent with this increase, the program improved several margins that mediate access to income and support. Participants applied for more jobs, attended more interviews, and participated more in occupational training and career-guidance actions. They also reported better access to social rights and public services, as well as higher digital skills and internet access—an input that is increasingly necessary both for benefit claiming and for job search. Although the treatment did not significantly change the ability to pay household bills over the horizon we observe, the pattern of intermediate outcomes points to a coherent mechanism: ACCEDE raised reported resources by relaxing several informational, administrative, digital, and labor-market frictions at once. We therefore interpret the income gain as the reduced-form effect of an integrated access and accompaniment model, rather than as the effect of a single separately identified channel.

The social dimension of the intervention also responded, but along a specific margin. ACCEDE did not significantly affect the index of regular contact with others and the perceived support. It did, however, substantially increase participation in community groups—by nearly half a standard deviation. This is an important result in its own right. The program increased an objective,

behavioral form of social connection: participation in parishes, neighborhood associations, sports groups, and voluntary organizations. The absence of a corresponding short-run effect on perceived support does not negate this gain; rather, it suggests that changes in social participation may precede changes in subjective feelings of support. This distinction is consistent with evidence that objective social isolation and subjective loneliness are related but separate dimensions of social experience, with objective isolation carrying important consequences of its own (Fawaz and Mira, 2023). For a policy aimed at reducing social exclusion, helping participants enter community structures is therefore a meaningful outcome.

The contribution is threefold. First, the paper contributes to the public economics of benefit take-up and administrative burdens by studying a high-touch accompaniment model rather than a purely informational or application-assistance intervention. The trial shows that the effective value of a transfer system can be increased by investing in the infrastructure that helps households reach and use the system. Second, it contributes to the cash-versus-services debate in anti-poverty policy. Multifaceted “graduation” programs combine transfers with coaching, training, and support (Banerjee et al., 2015); ACCEDE differs because it operates in a high-income country with an existing national income floor and adds no extra cash. The design therefore holds the usual material-aid component fixed and estimates the value of the non-cash wrapper. Third, the paper speaks to the design of safety nets with participation and earnings responses. In models of optimal transfers, the interaction between benefits, participation, and earnings is a central input (Saez, 2002). Our evidence shows that administrative and digital access, job-search support, and relational accompaniment can jointly affect those margins.

We also emphasize the limitations of the evidence. The treatment is a bundle, so the trial identifies the policy package rather than the marginal contribution of each component. Income is self-reported and measured over a six-month recall period. Attrition is higher in the control group and mildly selective, so the estimates are best interpreted for the endline analysis sample. Finally, without administrative records on benefit receipt, earnings, and program costs, we cannot separate take-up from labor-market channels or conduct a full cost-benefit analysis. These limitations define the next step for the research agenda, but they do not undermine the central experimental contrast: with the usual cash and in-kind material aid held fixed, adding an access and accompaniment layer raised household income and several pre-specified dimensions of inclusion.

The rest of the paper is organized as follows. Section 2 describes the intervention, experimental design, and data. Section 3 presents the empirical strategy and discusses identification. Section 4 reports the main results and Section 5 complementary analyses. Section 6 concludes.

2 Experimental design and data

2.1 *The ACCEDE intervention*

ACCEDE is one of a set of randomized pilots promoted by the General Secretariat for Inclusion (SGI) of the Spanish Ministry of Inclusion, Social Security and Migration, to complement Spain’s Minimum Income Scheme and strengthen the economic and social resilience of vulnerable households ([Secretaría General de Inclusión del Ministerio de Inclusión, Seguridad Social y Migraciones, 2025](#)).

ACCEDE program was implemented by Cáritas and targeted families already attending collaborating parishes at launch, as well as families who sought help during the recruitment period and agreed to participate. The experimental design was pre-registered in the AEA RCT Registry under the RCT ID AEARCTR-0010632.

The intervention builds on, but goes beyond, Cáritas’s traditional support model. In the control group, parishes continued to provide the usual assistance: financial and in-kind aid, referrals to social services, and information on administrative procedures. Treated parishes provided the same material support, but added a dedicated and easily identifiable space equipped with computers and internet access.² In this space, participants received a comprehensive-care bundle organized around three dimensions: the management of material resources, access to public services, and the development of relational spaces.

The treatment consisted of eight actions in addition to usual support: (i) an agreed personal and community itinerary; (ii) training in administrative procedures and complaints; (iii) training on access to the Minimum Income Scheme and other benefits; (iv) active job-search training; (v) training in skills, service management, and access to resources; (vi) the creation of groups using a participatory methodology; (vii) the provision of digital access points, including devices and connectivity; and (viii) intensive accompaniment across all areas of action.

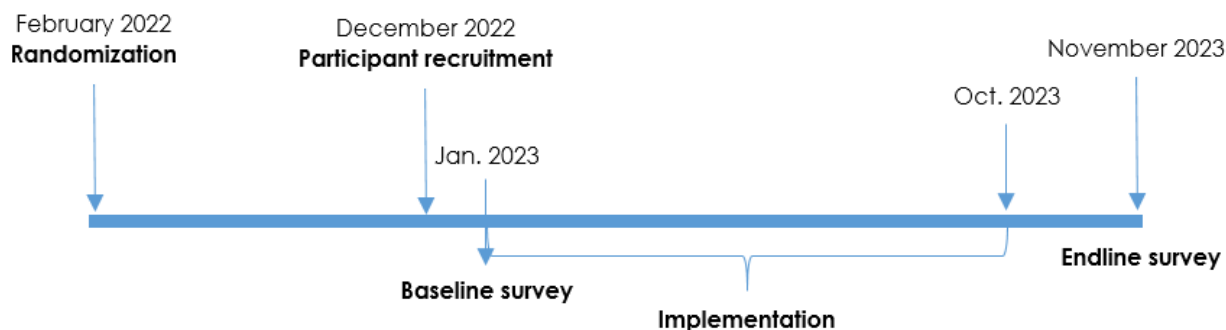
²Pictures of an ACCEDE space are included in Appendix A.

Because both treated and control parishes had access to the same usual cash and in-kind material aid, the experiment identifies the effect of the non-cash access and accompaniment bundle, rather than the effect of additional transfers.

The program’s stated objectives are defined over three horizons and correspond closely to the outcome domains we study. In the short run, the aim was to help participants stabilize their situation by covering basic needs and engaging in training and collaborative activities. In the medium run, the objective was to improve access to social goods and services, strengthen digital and labor-market skills, and improve participants’ relational situation. In the long run, the program aimed to promote more durable socio-labor integration and stronger support networks.

Randomization took place at the level of the parish or group of parishes, hereafter a cluster. In February 2022, 64 clusters were randomly assigned, stratified by diocese: 32 clusters were assigned to treatment and 32 to control. Recruitment began in December 2022, after cluster assignment. A total of 2,625 individuals enrolled on behalf of their families and completed the baseline questionnaire. Implementation ran from January to October 2023, and the endline survey was completed in November 2023. Figure 1 summarizes the program timeline.

Figure 1: Program design and implementation chronology



Because enrollment occurred after cluster assignment, the estimates should be interpreted as effects for families who enrolled in parishes assigned to each experimental arm. We therefore examine baseline balance carefully and use covariate-adjusted analysis-of-covariance (ANCOVA) specifications in the analysis below, controlling for the baseline value of the outcome whenever available. We evaluate impacts on five pre-specified domains: household economic circumstances, employability, access to rights and public services, digital inclusion, and social relationships.

2.2 Data and sample

In total, 2,625 individuals responded to the baseline questionnaire on behalf of their families. Following the randomization carried out by the SGI at the parish level, 1,205 families are in the control group (46%) and 1,420 in the treatment group (54%).

Table 1 reports descriptive statistics for the variables used in the analysis, drawn from the baseline survey: the characteristics of the families and the outcome indicators measured before the intervention.³ For each variable, the table reports the number of observations, the mean, the standard deviation, and the minimum and maximum.

The sample is predominantly female and economically vulnerable. Women account for 73% of respondents, 46% hold Spanish nationality, and the average age is 44. More than 65% of participants are unemployed, and most live in rented or sublet housing (72%) and in individual rather than shared accommodation (84%).

2.3 Balance

Table 2 reports baseline balance between the treatment and control groups. For each variable, we show the mean in each group, the number of observations, and the p -value from a test of equality across arms. These tests are estimated as regressions of the baseline characteristic on treatment assignment, including randomization-stratum fixed effects and clustering standard errors at the parish-cluster level.

Overall, the two groups are broadly comparable at baseline. Some differences nevertheless reach conventional significance levels, which is not unexpected given that randomization was conducted at the parish-cluster level rather than at the individual level. Among demographic characteristics, significant differences appear for age, sex, household size, inactivity, education, residence in rented or sublet housing, and the small category of non-EU citizens who are family members of Spanish or EU nationals. Among baseline outcome indicators, three variables differ across arms: the number of job offers applied for in the previous six months, participation in a community group in the previous six months, and internet access by any means.

³Appendix B details the construction of all final and intermediate outcome indicators, together with the survey variables entering each one. Missing values are imputed using the mean of the variable in the corresponding treatment or control group.

Table 1: Sample descriptive statistics

	Obs.	Mean	Standard deviation	Minimum	Maximum
Treatment	2625	0.54	0.50	0.00	1.00
Age	2625	43.67	12.84	18.00	92.00
Male	2625	0.27	0.44	0.00	1.00
Country of birth Spain	2625	0.39	0.49	0.00	1.00
Spanish nationality	2625	0.46	0.50	0.00	1.00
No. of members	2625	3.09	1.58	1.00	11.00
Citizenship: non-EU	2625	0.46	0.50	0.00	1.00
Citizenship: Spanish	2625	0.46	0.50	0.00	1.00
Citizenship: EU non-Spanish	2625	0.04	0.20	0.00	1.00
Citizenship: EU family members	2625	0.02	0.12	0.00	1.00
Citizenship: non-EU family member of Spanish/EU	2625	0.02	0.13	0.00	1.00
Working	2625	0.16	0.36	0.00	1.00
Unemployed	2625	0.66	0.47	0.00	1.00
Inactive	2625	0.18	0.38	0.00	1.00
Unliterate	2625	0.14	0.34	0.00	1.00
Primary education or ESO or basic FP	2625	0.51	0.49	0.00	1.00
Baccalaureate or intermediate FP	2625	0.21	0.40	0.00	1.00
University or higher FP	2625	0.13	0.33	0.00	1.00
Individual housing	2625	0.84	0.36	0.00	1.00
Shared housing	2625	0.14	0.34	0.00	1.00
Substandard housing or no housing	2625	0.02	0.13	0.00	1.00
Property paid or paying	2625	0.16	0.36	0.00	1.00
Rented or sublet	2625	0.72	0.45	0.00	1.00
Leased, occupied, accommodation center	2625	0.12	0.33	0.00	1.00
Non-payments for household supplies in the last 6 months	2625	1.35	2.00	0.00	6.00
Job offers you have applied for in the last 6 months	2625	4.53	12.86	0.00	120.00
Interviews conducted in the last 6 months	2625	1.02	4.02	0.00	80.00
Occupational training actions in the last 6 months	2625	0.33	1.13	0.00	22.00
Job training actions in the last 6 months	2625	0.51	1.86	0.00	48.00
Degree of participation in a group in the last 6 months	2625	1.34	0.70	1.00	3.00
Sum of income in the last 6 months	2625	3823.92	3339.74	0.00	32900.00
Level of success in managing public services in the last 6 months	2625	3.47	1.28	1.00	6.00
Level of skill in using the Internet for personal, work, etc. purposes	2625	4.15	1.48	1.00	6.00
Level of social inclusion	2625	3.41	1.12	1.00	5.00
Internet access at your home	2625	0.71	0.45	0.00	1.00
Internet access by any means	2625	0.84	0.37	0.00	1.00

Table 2: Baseline balance by treatment status

Variable	Control		Treatment		t-test	
	Obs./Clusters	Mean/(Var)	Obs./Clusters	Mean/(Var)	Obs./Clusters	p-value
Age	1205 32	45.25 (6487.85)	1420 32	42.34 (7282.44)	2625 64	0.00***
Male	1205 32	0.26 (7.44)	1420 32	0.28 (9.15)	2625 64	0.10*
Country of birth Spain	1205 32	0.41 (9.42)	1420 32	0.38 (10.76)	2625 64	0.10
Spanish nationality	1205 32	0.48 (9.70)	1420 32	0.45 (11.33)	2625 64	0.20
No. of member	1205 32	3.02 (107.53)	1420 32	3.15 (104.74)	2625 64	0.01**
Citizenship: non-EU	1205 32	0.46 (9.64)	1420 32	0.47 (11.40)	2625 64	0.54
Citizenship: Spanish	1205 32	0.48 (9.69)	1420 32	0.45 (11.34)	2625 64	0.24
Citizenship: EU non-Spanish	1205 32	0.04 (1.37)	1420 32	0.04 (1.85)	2625 64	0.31
Citizenship: EU family members	1205 32	0.02 (0.63)	1420 32	0.01 (0.64)	2625 64	0.53
Citizenship: non-EU family member of Spanish/EU	1205 32	0.01 (0.48)	1420 32	0.02 (1.04)	2625 64	0.04**
Working	1205 32	0.15 (5.00)	1420 32	0.16 (6.21)	2625 64	0.21
Unemployed	1205 32	0.65 (8.80)	1420 32	0.67 (10.10)	2625 64	0.31
Inactive	1205 32	0.20 (6.12)	1420 32	0.17 (6.37)	2625 64	0.05**
Unliterate	1205 32	0.17 (5.38)	1420 32	0.12 (4.56)	2625 64	0.00***
Primary education or ESO or basic FP	1205 32	0.50 (9.32)	1420 32	0.51 (11.06)	2625 64	0.49
Baccalaureate or intermediate FP	1205 32	0.19 (5.73)	1420 32	0.24 (7.99)	2625 64	0.00***
University or higher FP	1205 32	0.13 (4.19)	1420 32	0.13 (4.93)	2625 64	0.87
Individual housing	1205 32	0.83 (5.43)	1420 32	0.85 (5.69)	2625 64	0.37
Shared housing	1205 32	0.15 (4.88)	1420 32	0.13 (5.13)	2625 64	0.46
Substandard housing or no housing	1205 32	0.02 (0.76)	1420 32	0.01 (0.64)	2625 64	0.27
Property paid or paying	1205 32	0.16 (5.20)	1420 32	0.15 (5.82)	2625 64	0.21
Rented or sublet	1205 32	0.70 (7.89)	1420 32	0.73 (8.90)	2625 64	0.08*
Leased, occupied, accommodation center	1205 32	0.13 (4.32)	1420 32	0.12 (4.69)	2625 64	0.27
Non-payments for household supplies in the last 6 months	1205 32	1.36 (156.24)	1420 32	1.34 (180.66)	2625 64	0.80
Job offers you have applied for in the last 6 months	1205 32	4.08 (4678.82)	1420 32	4.91 (9299.00)	2625 64	0.05*
Interviews conducted in the last 6 months	1205 32	1.07 (664.76)	1420 32	0.97 (701.83)	2625 64	0.51
Occupational training actions in the last 6 months	1205 32	0.36 (67.16)	1420 32	0.30 (39.99)	2625 64	0.10
Job training actions in the last 6 months	1205 32	0.53 (90.20)	1420 32	0.49 (203.40)	2625 64	0.20
Degree of participation in a group in the last 6 months	1205 32	1.37 (19.90)	1420 32	1.32 (20.95)	2625 64	0.07*
Sum of income in the last 6 months	1205 32	3856.87 (4.17e+08)	1420 32	3795.96 (5.27e+08)	2625 64	0.85
Level of success in managing public services in the last 6 months	1205 32	3.44 (65.58)	1420 32	3.50 (74.05)	2625 64	0.29
Level of skill in using the Internet for personal, work, etc. purposes	1205 32	4.13 (92.88)	1420 32	4.16 (91.49)	2625 64	0.37
Level of social inclusion	1205 32	3.41 (47.23)	1420 32	3.41 (59.65)	2625 64	0.93
Internet access at your home	1205 32	0.70 (7.93)	1420 32	0.72 (9.10)	2625 64	0.35
Internet access by any means	1205 32	0.81 (5.94)	1420 32	0.86 (5.49)	2625 64	0.00***

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports baseline means by treatment status. The p -values are obtained from regressions of each baseline variable on treatment assignment, controlling for randomization-stratum fixed effects and clustering standard errors at the parish-cluster level. Parenthetical statistics under the means are variance statistics reported by the balance routine, not individual-level standard deviations.

The empirical analysis accounts for these baseline differences by controlling for the baseline value of the outcome whenever available and for the covariates that are unbalanced at baseline. The estimates reported below should therefore be interpreted as covariate-adjusted intent-to-treat effects.

2.4 Attrition

Of the 2,625 individuals who completed the baseline survey, 2,265 also completed the endline survey, implying an overall follow-up rate of 86.3 percent. Follow-up was higher in the treatment group than in the control group: 88.8 percent of treated participants completed the endline, compared with 83.3 percent of control participants (Table 3). Since the main outcomes are measured at endline, this differential response reduces the estimation sample and could threaten the experimental comparison. We therefore examine both the extent and the selectivity of attrition.

Table 3: Endline survey completion

Group	Baseline sample	Endline completed
Treatment	1,420	1,261 (88.8%)
Control	1,205	1,004 (83.3%)
Total	2,625	2,265 (86.3%)

Table 4 reports regressions in which the dependent variable is an indicator for not completing the endline survey. Column 1 includes only treatment assignment and randomization-stratum fixed effects. The estimated treatment coefficient is -0.051 and statistically significant at the 1 percent level, confirming that attrition is lower in the treatment group. Column 2 adds baseline covariates and their interactions with treatment assignment to assess whether attrition is selective with respect to observable characteristics. In this interacted specification, the treatment coefficient refers to the reference covariate profile and is estimated jointly with the treatment-covariate interactions. The larger standard error on the treatment indicator in column 2 should therefore not be interpreted as contradicting the average attrition difference documented in column 1. Among the interaction terms, only the interaction between treatment assignment and being male is statistically significant, indicating that treated men are especially likely to be retained in the endline sample.

Our main outcome regressions control for the baseline male indicator, together with the other baseline covariates discussed above, but do not include the raw treatment-by-male interaction. This keeps the reported treatment coefficient interpretable as the average intent-to-treat effect for

the endline analysis sample, rather than as the treatment effect for the omitted reference group. Because attrition is both differential and mildly selective, we interpret the estimates as applying to participants observed at endline.

The direction of the attrition difference is also informative. Treatment-group participants are retained at a higher rate, so under Lee (2009) trimming bounds the conservative lower bound would remove the highest-outcome treatment observations. For an average monthly income effect of roughly €133 against a control-group mean of about €818 per month, trimming from the top of the treatment distribution would still yield a positive lower bound, suggesting that differential attrition is unlikely to reverse the sign of the main result.

Table 4: Attrition from the endline survey

Final survey not completed	(1)	(2)
Treatment	-0.051*** (0.013)	-0.053 (0.069)
Treatment x Age from the respondent		-0.001 (0.001)
Treatment x Sex from the respondent: man		-0.073** (0.034)
Treatment x Nationality from the respondent: Spanish		0.047 (0.036)
Treatment x Number of members in the household		-0.008 (0.011)
Treatment x Employment situation: working		-0.002 (0.037)
Observations	2625	2625

Notes: The dependent variable is an indicator equal to one if the participant did not complete the endline survey. Column 1 includes treatment assignment and randomization-stratum fixed effects. Column 2 adds baseline covariates and their interactions with treatment assignment to assess selective attrition. In column 2, the coefficient on treatment refers to the reference covariate profile and should not be interpreted as the average treatment-control difference in attrition. Standard errors are clustered at the parish-cluster level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3 Empirical strategy

The empirical strategy follows directly from the clustered randomized design. We estimate intent-to-treat effects of assignment to an ACCEDE parish on a set of pre-specified outcome families that capture the program’s main objectives: household resources, employability, access to rights and public services, digital inclusion, and social relationships. The section first defines these outcomes and the summary indices used for confirmatory inference, then presents the covariate-adjusted ANCOVA specification and discusses inference, outcome scaling, and possible spillovers across clusters.

3.1 Outcomes

The evaluation was designed around a set of pre-specified outcomes, registered in advance in the pre-analysis plan (Fawaz et al., 2023), and grouped into five families. The first is *income*: average monthly household income, constructed as total household income received over the previous six months divided by six, and the household’s ability to meet its usual bills, such as electricity, water, and gas. The second is *employability*, measured over the previous six months by the number of job interviews, the number of job offers applied for, participation in occupational training for access to employment, and participation in career-guidance actions. The third is *access to rights*: the household’s experience in dealing with social services and in claiming public benefits and aid related to health, education, and housing. The fourth is *digital inclusion*: digital skills for everyday personal, family, work, and administrative tasks, and access to the internet at home and by any means. The fifth is *social relationships*: regular contact with other people, perceived support, and participation in community groups. Appendix B gives the exact construction of each indicator.

Where a family of outcomes combines several survey items, we summarize them using an Anderson index (Anderson, 2008). These indices reduce the multiple-inference problem that arises when many related outcomes are tested separately and provide one summary measure for each pre-specified domain. The family-level estimates are therefore our preferred confirmatory evidence. Results for individual components within a family are reported to help interpret mechanisms and should be read as component-level evidence; their p -values are not adjusted for family-wise multiple testing unless explicitly stated. No outcome family was added or removed after randomization.

3.2 Specification

Because treatment is randomly assigned, the causal effect of ACCEDE on an outcome is identified by the difference in mean outcomes between the treatment and control groups, conditional on the randomization strata. We estimate the following ANCOVA specification:

$$Y_{i,t=1} = \alpha + \beta T_i + \gamma Y_{i,t=0} + X_i \delta + \epsilon_i$$

where $Y_{i,t=1}$ is the outcome for family i measured after the intervention, T_i indicates assignment to treatment ($= 1$) or control ($= 0$), $Y_{i,t=0}$ is the baseline value of the outcome, and X_i is a vector of controls comprising randomization-stratum indicators and baseline covariates that are unbalanced in Table 2 or predictive of selective attrition in Table 4. The coefficient of interest is β , the intent-to-treat effect of assignment to an ACCEDE parish for the endline analysis sample. Importantly, X_i does not include raw treatment–covariate interactions in the main specification, so β is not a subgroup coefficient. Conditioning on the baseline outcome $Y_{i,t=0}$, where available, absorbs persistent differences across families and improves precision. Standard errors are clustered at the level of randomization, the parish or group of parishes, of which there are 64.

All non-monetary outcomes are standardized before estimation to facilitate comparison across domains; coefficients are therefore reported in standard-deviation units. The standardization uses the pre-intervention distribution, so endline control means reported in the tables need not equal zero. The one exception is average monthly household income, which we report in euros per month to convey the magnitude of the main result directly. It is constructed as total household income received over the previous six months divided by six.

One threat to the experimental comparison is interference between units. Treatment and control clusters operate within the same dioceses, raising the question of whether treated parishes could affect control parishes through shared resources, staff attention, or participant mobility. Several features of the design limit this concern. The intervention is delivered in a dedicated physical space specific to each treated parish; participants cannot access it from control parishes. The participating organizations have separate parish-level administrative structures, limiting the scope for resource reallocation. We nonetheless acknowledge that diocese-level spillovers cannot be fully ruled out. If anything, positive spillovers from treatment to control would attenuate our estimates.

4 Results

This section reports the effects of ACCEDE on the five pre-specified outcome domains. We start with household resources and then examine employability, access to rights and public services, digital inclusion, and social relationships as potential margins of response. We focus on the covariate-adjusted estimates, while also reporting specifications with only strata and baseline-outcome controls.

4.1 Income

The main economic result is that ACCEDE increased household resources, even though the treatment did not provide additional cash transfers. Table 5 reports the effect on average monthly household income, constructed as total income received over the previous six months divided by six. Columns 1 and 2 report the income estimates: column 1 controls for randomization strata and the baseline outcome, while column 2 adds the baseline covariates described in Section 3. The estimated effect ranges from €151 to €133 per month and is statistically significant at the 1 percent level. Relative to a control-group mean of about €818 per month, the preferred estimate represents an increase of 16 percent. This is a sizeable effect for an intervention whose incremental component consisted of accompaniment, training, digital access, and a dedicated physical space. Columns 3 and 4 show no statistically significant effect on the household’s ability to pay usual bills. We therefore interpret the income result as an increase in reported household resources.

Table 5: Effect on average monthly household income and bill payment

	Average monthly household income		Non-payment of household supplies	
	(1)	(2)	(3)	(4)
Treatment	150.544*** (32.935)	132.842*** (31.758)	0.024 (0.075)	0.016 (0.077)
Observations	2265	2265	2265	2265
R^2	0.23	0.26	0.12	0.13
Control mean dep. var.	817.829	817.829	-0.023	-0.023
Initial value dep. var.	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Income is measured in euros per month, calculated as total household income received over the previous six months divided by six, and is not standardized. The bill-payment outcome is standardized, so its coefficient is in standard-deviation units. All specifications include the baseline value of the outcome when available; even columns add the additional baseline controls described in Section 3. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Employability

We next examine whether the intervention affected labor-market engagement, one of the main margins through which household resources could improve. Table 6 reports effects over the previous six months on the number of job offers applied for, the number of interviews, participation in occupational training, and participation in career-guidance actions.

Table 6: Effect on employability over the previous six months

	Job offers presented		Interviews conducted		Occupational training actions		Job orientation actions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.136** (0.058)	0.121* (0.062)	0.118*** (0.038)	0.074** (0.033)	0.397*** (0.070)	0.375*** (0.070)	0.243** (0.103)	0.231** (0.104)
Observations	2265	2265	2265	2265	2265	2265	2265	2265
R^2	0.10	0.11	0.05	0.07	0.12	0.13	0.13	0.14
Control mean dep. var.	-0.076	-0.076	-0.060	-0.060	-0.211	-0.211	-0.126	-0.126
Initial value dep. var.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The job-offers outcome refers to job offers the participant applied for, not job offers received from employers. Outcomes are standardized; coefficients are in standard-deviation units. The component-level p -values are not adjusted for multiple testing within the employability family. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All four employability outcomes move in the expected direction. In the preferred covariate-adjusted specification, the effect is 0.121 standard deviations for job offers applied for, 0.074 standard deviations for interviews, 0.375 standard deviations for occupational training, and 0.231 standard deviations for career-guidance actions. The strongest individual evidence is for occupational training, while the estimate for job offers applied for is significant only at the 10 percent level. These are component-level p -values and are not adjusted for multiple testing within the employability family. Nevertheless, the consistent pattern across all four pre-specified components suggests that ACCEDE increased labor-market engagement across a broad set of margins, rather than only affecting a single indicator.

4.3 Access to rights

A second potential channel is access to public benefits and services. We measure this using participants' reported experience in dealing with social services, the tax authority, public health, and education. Table 7 shows a positive effect of 0.142 standard deviations in the preferred specification.

This result is consistent with accompaniment helping participants navigate administrative procedures and claim rights, benefits, or services to which they were entitled. It is also the outcome family most closely related to the benefit take-up literature: as in the assistance arm of [Finkelstein and Notowidigdo \(2019\)](#), hands-on support may help households move from formal eligibility to actual access.

Table 7: Effect on access to social rights over the previous six months

	Degree of access to social rights	
	(1)	(2)
Treatment	0.183** (0.088)	0.142* (0.082)
Observations	2265	2265
R^2	0.20	0.23
Control mean dep. var.	-0.109	-0.109
Initial value dep. var.	Yes	Yes
Controls	No	Yes

Notes: The outcome is an Anderson index combining reported experience with social-services, health, education, and tax-authority procedures. Coefficients are in standard-deviation units. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Digital inclusion

Digital inclusion is a third margin of interest because both job search and public administration increasingly require online access. Table 8 reports effects on digital skills and connectivity. In the preferred specification, participants' ability to carry out personal, family, work, and administrative tasks online increased by 0.296 standard deviations. Internet access also improved, both at home (0.193 standard deviations) and by any means, including public places and the homes of neighbors or friends (0.160 standard deviations). These effects are statistically significant at conventional levels.

The magnitudes are meaningful in this population. Among control households, only about 70 percent had internet access at home by endline. By improving digital skills and connectivity, ACCEDE likely expanded participants' ability to use job-search platforms, benefit portals, and online public-service procedures. Digital inclusion should therefore be read not only as an outcome in itself, but also as a complementary input to the employability and access-to-rights channels.

Table 8: Effect on digital inclusion

	Degree of internet use for personal, work, or educational purposes		Internet access at home		Internet access by any other means	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.380*** (0.066)	0.296*** (0.065)	0.248*** (0.071)	0.193*** (0.066)	0.199*** (0.063)	0.160** (0.059)
Observations	2265	2265	2265	2265	2265	2265
R^2	0.36	0.42	0.22	0.25	0.17	0.18
Control mean dep. var.	-0.217	-0.217	-0.145	-0.145	-0.144	-0.144
Initial value dep. var.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Notes: Outcomes are standardized; coefficients are in standard-deviation units. Internet access by any means includes access at home and access through other locations or devices. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Social relationships

The final outcome family concerns social relationships, a central dimension of the program’s social-inclusion objective. Table 9 shows that ACCEDE did not significantly affect the index of regular contact with other people and perceived support. It did, however, substantially increase participation in community groups. The estimated effect is 0.476 standard deviations in the preferred specification and is statistically significant at the 1 percent level. This outcome captures participation in parishes, parents’ associations, neighborhood groups, sports groups, and voluntary organizations.

This dissociation is the interesting part. A behavioral, objective measure of social connection responds strongly to the program while a subjective measure does not. We read this not as a weak social result but as evidence that policy can build the observable structures of social capital, getting people into community organizations, well before, or independently of, any change in how connected people feel. The distinction matters because objective social isolation and subjective loneliness are only weakly correlated and capture different aspects of social experience, and controlling for loneliness barely weakens the association between objective social isolation and mortality, indicating that the documented health and mortality consequences run through the objective dimension rather than the subjective feeling of loneliness (Fawaz and Mira, 2023). For a program aimed at combating social isolation, the margin ACCEDE moves is therefore the consequential one: participation in the community structures that sustain support networks, even if subjective satisfaction has not yet caught up over the horizon we observe.

Table 9: Effect on social relationships

	Degree of regular contact with other people		Degree of participation in a group in the last 6 months	
	(1)	(2)	(3)	(4)
Treatment	0.082 (0.066)	0.056 (0.065)	0.474*** (0.107)	0.476*** (0.105)
Observations	2265	2265	2265	2265
R^2	0.24	0.27	0.21	0.21
Control mean dep. var.	-0.044	-0.044	-0.234	-0.234
Initial value dep.var	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes

Notes: Outcomes are standardized; coefficients are in standard-deviation units. The first outcome is an index of regular contact and perceived support. The second outcome measures regular participation in community groups. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.6 What drove the income effect?

The intervention increased average monthly household income by roughly €133 without providing additional cash transfers. A natural question is whether this increase reflects higher benefit receipt, higher labor earnings, or both. The trial was not designed to separately identify these channels, and the survey does not provide administrative measures of benefit take-up or earnings that would allow a clean decomposition. We therefore interpret the income result as the reduced-form effect of the package.

The intermediate outcomes help discipline this interpretation. Access to social rights and public aid improved, consistent with participants reaching benefits or services they had previously been unable to claim. Labor-market engagement increased across the pre-specified employability measures, consistent with a potential earnings channel through search, training, and guidance. Digital inclusion also improved, which is a complementary input to both mechanisms because job applications and benefit procedures increasingly require online access.

Taken together, the evidence supports a friction-based interpretation: ACCEDE increased household resources by helping participants convert existing entitlements and labor-market opportunities into actual income. The evidence does not support a narrower claim that any single component of ACCEDE, or any single channel, explains the full effect.

4.7 Implications for minimum-income and social-assistance design

The experimental contrast is useful for public economics because it separates statutory transfer generosity from the infrastructure that makes entitlements usable. Both arms had access to the same usual material support from *Cáritas*; the treatment added the non-monetary layer of information, accompaniment, digital access, and job-search support. The results therefore speak to a policy margin that sits between cash transfers and traditional active labor-market programs: the capacity of households to navigate and use the institutions already meant to insure them.

This distinction matters for the design of anti-poverty policy. Raising benefit levels and reducing access frictions are not substitutes in general; sufficiently low benefit levels cannot be solved by better navigation alone. But when non-take-up is high, as in Spain’s minimum-income system, the marginal return to improving access may be large because it increases the effective value of existing public spending. ACCEDE provides experimental evidence that this margin can be quantitatively meaningful. A full welfare assessment would require program costs and administrative records on benefit receipt and earnings. In their absence, the appropriate conclusion is narrower but still policy-relevant: a non-cash access layer can raise reported household income and several dimensions of inclusion even without increasing the cash transferred by the implementing organization.

5 Effects by cluster size

The clusters that were the unit of randomization vary in how many parishes they contain, which raises the question of whether the intervention operated differently where it was spread across more parishes. Table 10 splits the sample into clusters with fewer than six parishes and clusters with six or more, and re-estimates the main specifications interacting treatment with this distinction.

The main effects are broadly similar across cluster sizes. The treatment effects on income and occupational training are positive, significant, and of comparable magnitude in both groups. Internet access shows a positive point estimate, although it does not reach conventional significance. For access to social rights and participation in community groups, the point estimates are larger in clusters with six or more parishes, but these additional effects are imprecisely estimated, and we cannot reject equality across cluster sizes.

Table 10: Effects by cluster size

	Income	Occupational training actions	Access to social rights	Access to internet by any other means	Degree of participation in some group
	(1)	(2)	(3)	(4)	(5)
Diocesan with ≥ 6 parishes	233.011 (178.038)	0.084 (0.072)	0.537*** (0.077)	-0.283*** (0.057)	-0.042 (0.085)
Treatment	1149.710*** (245.144)	0.390*** (0.132)	0.021 (0.130)	0.096 (0.079)	0.260* (0.130)
Treatment \times Diocesan with ≥ 6 parishes	-511.811 (349.873)	-0.022 (0.155)	0.175 (0.166)	0.093 (0.116)	0.314 (0.187)
Observations	2265	2265	2265	2265	2265
R^2	0.26	0.13	0.24	0.23	0.22
Control mean dep. var.	4906.974	-0.211	-0.109	-0.144	-0.234

Notes: Diocesan with ≥ 6 parishes refers to the randomization cluster containing six or more parishes. A positive interaction term indicates that the treatment effect is larger in those clusters than in clusters with fewer than six parishes. Standard errors, clustered by parish cluster, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We therefore read this exercise as evidence that the main findings are not driven by a particular implementation scale, rather than as evidence of systematic heterogeneity. The experiment was designed and powered to detect average treatment effects, not heterogeneous effects across cluster types. We therefore do not push the cross-cluster comparisons further. Heterogeneity by participant characteristics, such as baseline employment, poverty, or household composition, is an important question for future evaluations with larger samples.

6 Conclusion

Minimum-income systems are often evaluated by the generosity of their statutory benefits. This paper shows that their effective reach also depends on whether disadvantaged households can navigate them. ACCEDE added a non-cash access and accompaniment layer to an existing network of support. Because usual material aid was available in both arms, the experiment isolates the effect of this layer rather than the effect of additional cash transfers. The intervention increased average monthly household income by about €133, or 16 percent of the control-group mean, and improved several dimensions of inclusion: employability, access to social rights, digital inclusion, and participation in community groups.

These effects should be interpreted as the impact of an integrated policy package. The trial does not separately identify the contribution of benefit assistance, job-search support, digital access, group activities, or one-on-one accompaniment, nor can it split the income gain between benefit take-up and earnings. Nevertheless, the pattern of results is consistent with an access-frictions interpretation. Participants became more engaged with the labor market, more able to use digital tools, more successful in dealing with public services, and more connected to community organizations.

The social results are informative in their own right. ACCEDE did not significantly change regular contact and perceived support, but it substantially increased participation in community groups. The intervention therefore affected an objective, behavioral dimension of social connection before, or independently of, changes in perceived support. For a program aimed at reducing social exclusion, helping participants enter community structures is a meaningful outcome.

Several limitations remain. Attrition was higher in the control group and mildly selective, so the estimates apply most cleanly to the endline analysis sample. Income is self-reported and constructed from a six-month recall window. The trial was powered to detect average effects, not detailed heterogeneity or mechanisms. Finally, a full public-finance assessment would require program costs and administrative records on benefit receipt and earnings. Such data would allow future work to compare the marginal value of increasing benefit levels with the marginal value of reducing the frictions that prevent eligible households from using existing support.

The broader implication is that access infrastructure is a substantive component of minimum-income and social-assistance systems, not merely an implementation detail. In settings with high non-take-up, formal entitlements may translate only imperfectly into actual resources. ACCEDE shows that relational, administrative, and digital support can increase income and inclusion even when the implementing program transfers no additional cash.

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A Pictures of the ACCEDE space

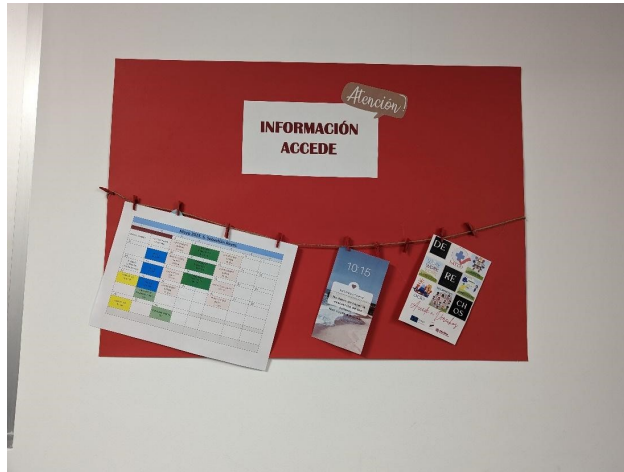
Figure A.1: Scheduling and activities



Figure A.2: ACCEDE space



Figure A.3: ACCEDE information



B Definition of result indicators

Table B.1 shows the description of the variables that make up each of the result indicators, using the original names of the survey variables.

Table B.2 includes the description of the survey variables included in the calculation of each indicator.

Table B.1: Description of the result indicators

Code	Description	Original variable or formula
ER01	Average monthly income per household	Sum of income from different sources in the last 6 months divided by six: VIER011 (money from social benefits), VIER012 (money from work income), VIER013 (money from other sources)
ER02	Payment level of basic housing expenses	VIER022 VIER022
EE03_1	Number of job interviews carried out	VIEE032
EE03_2	Number of job offers applied for	VIEE031
EE03_3	Number of training actions for employment that have been taken	VIEE033
EE03_4	Number of career guidance actions in which they have participated	VIEE034
SDA04	Degree of access to social rights in the last 6 months	Anderson index with: VISDA041, VISDA042, VISDA043, VISDA044
SDA05	Degree of internet use for personal, work, educational, family, and administrative purposes	Anderson index with: VISBD051, VISBD052, VISBD053, VISBD054
SBD06_1	Internet access at home	VISBD061
SBD06_2	Internet access by any means	VISBD062
RAS09	Degree of regular contact with other people in their environment and receipt of the necessary support	Anderson index with: VIRAS091, VIRAS092, VIRAS093, VIRAS094, VIRAS095, VIRAS096, VIRAS097
RCS10	Level of participation in group activities (community involvement)	VIRCS101

Table B.2: Description of the survey variables included in the calculation of indicators

Code	Description	Moment	Range
VIER011	How much money has come into the household from SOCIAL BENEFITS in the last 6 months?	PRE POST	Euros
VIER012	How much money has come into the household from WORK INCOME in the last 6 months?	PRE POST	Euros
VIER013	How much money has come into the household from OTHER SOURCES in the last 6 months?	PRE POST	Euros
VIER022	On how many occasions in the last 6 months have you not been able to pay for household supplies (electricity, water, etc.)?	PRE POST	0-6 (PRE) 0-20 (POST)
VIEE031	How many job offers have you applied for in the last 6 months?	PRE POST	0-120 (PRE) 0-100 (POST)
VIEE032	How many job interviews have you done in the last 6 months?	PRE POST	0-80 (PRE) 0-72 (POST)
VIEE033	In how many occupational training actions to access a job have you participated in the last 6 months?	PRE POST	0-22 (PRE) 0-16 (POST)
VIEE034	How many career guidance actions have you participated in in the last 6 months?	PRE POST	0-48 (PRE) 0-24 (POST)
VISDA041	What is your experience with SOCIAL SERVICES procedures in the last 6 months?	PRE POST	1-6 (PRE) 1-6 (POST)
VISDA042	What is your experience with PUBLIC HEALTH procedures in the last 6 months?	PRE POST	1-6 (PRE) 1-6 (POST)
VISDA043	What is your experience with EDUCATION procedures in the last 6 months?	PRE POST	1-6 (PRE) 1-6 (POST)
VISDA044	What is your experience with the HACIENDA procedures in the last 6 months?	PRE POST	1-6 (PRE) 1-6 (POST)
VISBD051	Would you know how to do the following procedures online? Personal management (maintaining	PRE POST	1-6 (PRE) 1-6 (POST)

Code	Description	Moment	Range
	relationships with family, friends, etc.)		
VISBD052	Would you know how to do the following procedures online? Family management (tutoring at your children's school...)	PRE POST	1-6(PRE) 1-6 (POST)
VISBD053	Would you know how to do the following procedures online? Public Administration management	PRE POST	1-6 (PRE) 1-6 (POST)
VISBD054	Would you know how to do the following procedures online? Labor or training procedures (carry out school activities...)	PRE POST	1-6 (PRE) 1-6 (POST)
VISBD061	Do you have internet access at your home?	PRE POST	0-1 (PRE) 0-1 (POST)
VISBD062	Do you have access to the internet through other means? (anywhere, including your home)	PRE POST	0-1 (PRE) 0-1 (POST)
VIRAS091	How many visits have you received or made to your friends and family in the last month?	PRE POST	0-60 (PRE) 0-100 (POST)
VIRAS092	Indicate your perception of this statement: I receive love and affection	PRE POST	1-5 (PRE) 1-5 (POST)
VIRAS093	Indicate your perception of this statement: I have the possibility to talk to someone about my problems at work	PRE POST	1-5 (PRE) 1-5 (POST)
VIRAS094	Indicate your perception of this statement: I receive invitations to distract myself and go out with other people	PRE POST	1-5 (PRE) 1-5 (POST)
VIRAS095	Indicate your perception of this statement: I receive useful advice when an important event happens to me	PRE POST	1-5 (PRE) 1-5 (POST)
VIRAS096	Indicate your perception of this statement: I get help when I'm sick in bed	PRE POST	1-5 (PRE) 1-5 (POST)

Code	Description	Moment	Range
VIRAS097	Indicate your perception of this statement:	PRE	1-5 (PRE)
	I receive help with matters related to my home	POST	1-5 (POST)
VIRCS101	Do you participate regularly in any group in your environment in the last 6 months? (AMPA, parish, neighborhood organization...)	PRE	1-3 (PRE)
		POST	1-3 (POST)