

LOCATION, HOUSING AND EMPLOYMENT OPPORTUNITIES EVIDENCE FROM A RANDOMIZED CONTROLLED TRIAL WITH VULNERABLE YOUTH IN FRANCE

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Primera versión: Febrero 2026
Esta versión: Febrero 2026

Documento de Trabajo Nº 180
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Abstract

Housing conditions, residential location, and employment are key determinants of individual welfare, particularly for vulnerable populations facing credit constraints and information frictions. We examine how housing assistance affects employment outcomes using a randomized controlled trial in France that provided vulnerable youth (aged 18-25) with both job search assistance and housing support, including rent guarantees. The program successfully improved housing conditions: beneficiaries experienced better accommodation stability, reduced precarious situations, and increased satisfaction with their housing. However, despite substantial social worker support, the program did not improve employment rates, contract types, or earnings. Strikingly, beneficiaries moved to neighborhoods with objectively worse employment opportunities and lower socioeconomic indicators, yet reported higher satisfaction with their residential areas. This apparent paradox reveals that beneficiaries appear to prioritize housing affordability and conditions over employment access. Our results suggest that successful interventions may need to explicitly balance housing improvements with maintaining access to employment opportunities.

*This paper is based on the impact evaluation of the Pass'Accompagnement program, prepared initially for the Departmental Council of Bas-Rhin. Funding for these reports was provided by the Departmental Council of Bas-Rhin. The original impact evaluation of the program was carried out by the Centre de recherche en économie et statistique (CREST) and the Ecole d'économie de Paris (EEP), in partnership with the Fonds d'expérimentation pour la Jeunesse (FEJ). The evaluation was registered with the AEA RCT registry with number AEARCTR-0001892. It was reviewed by PSE's IRB with file number 2010-004, updated in 2013 (reference 2013-002). The responsibility for opinions expressed in this article rests solely with the authors, and publication does not constitute an endorsement by the Departmental Council of Bas-Rhin or the FEJ of the opinions expressed in it. The authors wish to warmly thank Clémence Kieny for comments and excellent research assistance in every step of the impact evaluation, including the analysis. Marie Sestier and Nicolas Studer participated in an earlier stage, while Gaia Gaudenzi, Magnus Bjrnbeek, Joaquin Varvasino, Estefanía Lotitto and Fabián González participated in a later stage. The authors also thank Inés Berniell, Emma Chiodi, the editor and two anonymous referees for valuable comments and suggestions.

Corresponding author: vera.chiodi@sorbonne-nouvelle.fr - JEL codes: J8, J60, O18, R23.

1 Introduction

Where one lives, in which conditions, and whether one works or not are key factors to understand individual welfare. Low-income individuals face credit constraints, information frictions, and other barriers that limit their residential location choices and the jobs they can access and sustain. Moreover, there is often an interplay between location and housing decisions and labor market prospects. Better and more affordable housing tends to be further away from employment hubs (Duranton and Puga, 2015), which may hamper job opportunities. The complex relationship between housing improvements, residential location choices, and labor market outcomes may involve several underlying mechanisms, including spatial mismatch (Gobillon et al., 2007), which especially affects youth, low-skilled workers and vulnerable populations (Bertrand and Crépon, 2014; Gobillon and Selod, 2021). We leverage a randomized controlled trial of a program that provided housing and job search assistance simultaneously, which allows us to study the potential trade-offs between location and housing decisions and labor market outcomes.

A large fraction of young adults aged 18-25 in France remain outside the labor market, with high unemployment rates and low salary levels for those who find work. Moreover, a substantial part of this group has problems with housing and accommodation, due to demanding financial requirements, credit constraints and high rent levels, among other factors. To address these challenges, the departmental council of the Bas-Rhin region in France implemented the Pass'Accompagnement (PA) program in 2010, targeting young adults aged 18-25 in precarious situations, with the primary goals of securing stable housing and fostering employment opportunities. The main innovation for a program of this type was the emphasis on housing and accommodation issues simultaneously with employment problems. The program had two main tools to achieve its objectives. First, it provided housing aid in the form of support for accommodation search, and with the local authority providing a rent guarantee for beneficiaries' tenancy contracts when needed (one of the main barriers to access to independent private housing arrangements for youth in France). Second, the

program attempted to boost employment prospects by providing job search assistance and counseling. Our analysis is based on two sets of results. We first study a series of individual level outcomes (housing, labor markets and welfare receipt, among others) based on comparisons between outcomes for individuals in the treatment and control groups. We then compare area of residence-level indicators between the two groups to explore whether the program might have induced differences between the neighborhoods selected by treated and control individuals.

Our results indicate that beneficiaries received support from the program’s social workers, and were satisfied with these services, and was effective in improving housing and accommodation conditions, suggesting that it successfully removed credit and other constraints faced by beneficiaries. Second, beneficiaries moved to neighborhoods with more limited employment opportunities and lower socioeconomic levels, although they reported high levels of satisfaction with their new residential environments and with their housing conditions when compared to those in the control group. Third, the program did not lead to significant improvements in labor market outcomes on average, but suggestive evidence indicates that it was effective for some groups. Taken together, these findings reveal complex trade-offs between the program’s housing and employment objectives, with successful housing and neighborhood satisfaction improvements not translating into employment gains.

Several mechanisms could explain the null employment effects. First, job search assistance programs often simply fail to generate employment improvements for disadvantaged populations, as documented in extensive meta-analyses showing that only about one-third of youth employment programs achieve positive results (Card et al., 2018; Kluve et al., 2019). These difficulties may be particularly pronounced for disadvantaged youth with lower educational attainment and unstable housing situations — characteristics that define our study population. Additionally, several mechanisms related to the housing component may have operated. These include spatial mismatch from relocation to areas with fewer job opportunities, income effects from housing assistance reducing job search urgency, changed

reservation wages due to improved housing stability, and program implementation trade-offs affecting the quality of job search assistance. We explore these channels and find that multiple mechanisms likely operated simultaneously, making it difficult to isolate their individual contributions.

The interplay between housing decisions and labor market outcomes can be understood through two complementary frameworks. The Rosen-Roback model (Rosen, 1979; Roback, 1982) explains how regional differences in wages and rents reflect variations in local amenities. Workers may rationally accept lower wages to access desirable non-traded amenities such as better housing quality, public services, or community characteristics, while these same amenities drive up rents through increased residential demand. The spatial mismatch literature complements this framework by highlighting how physical distance from employment opportunities can limit labor market outcomes, particularly for disadvantaged workers (Gobillon et al., 2007; Gobillon and Selod, 2021). The mechanisms include higher costs of job search and commuting, as well as information frictions that increase with distance. While our beneficiaries likely face both imperfect information and mobility constraints, these frameworks help explain our finding that individuals might rationally choose residential locations that offer better housing and amenities at the expense of employment accessibility.

Our study is also related to the large literature on housing programs. Bergman et al. (2019) examine whether beneficiaries have a preference for lower quality neighborhoods, for example, because they are also associated with economic or social benefits, or, conversely, whether their choice of residence is the result of existing frictions in the housing market. Their results indicate that when assistance is provided, participants move to higher quality areas without sacrificing the quality of their housing. This implies that barriers in access to housing have a more important role in beneficiaries' outcomes than their preferences. This is consistent with Kling et al. (2005) and Collinson and Ganong (2018), who also find that housing programs that condition vouchers on moving to low poverty areas or include subsidies indexed to local rents allow beneficiaries to apply to accommodation in

higher quality neighborhoods. Our own study considers a program similar in content to the “Creating moves to opportunity” program in Bergman et al. (2019). Our results show that in the context of youth at risk of housing insecurity, reducing frictions in the housing market enables them to obtain stable, good quality housing. However, our results also indicate that it may be important to consider multiple dimensions of neighborhood quality when assessing interventions of this type: beneficiaries report being more satisfied with their neighborhood in our study, despite these areas exhibiting significantly worse socioeconomic levels and employment opportunities.

A subset of the literature has studied the impact of housing programs on labor supply. Jacob and Ludwig (2012) show that means-tested housing programs have a negative impact on labor supply and increase participation in social programs. This result is also present in van Dijk (2019), who also highlight the heterogeneity of the impact with respect to the type of neighborhood: the impact on labor supply is negative only when the move takes place to neighborhoods of lower or equal quality. Our results show that improved housing is associated with greater access to social programs without leading to reduced (or increased) labor supply, probably because of the degradation in the quality of the socioeconomic environment of the areas of residence chosen by beneficiaries.

Recent work on developing countries has also highlighted similar issues. Franklin (2020) found that South African recipients of free government housing saw income boosts driven by increased female employment, suggesting housing improvements might enhance women’s labor force participation. Kumar (2021), in turn, observed that Mumbai’s subsidized housing lottery winners experienced improved housing and employment outcomes, even though they live in neighborhoods with lower employment rates than non-winners. However, and more in line with our findings, Picarelli (2019) finds a decline in labor supply following a housing relocation in South Africa, attributed to increased distances to employment hubs. These findings resonate with our Bas-Rhin study, indicating that while housing conditions might improve, labor market outcomes can be influenced by spatial and localized factors.

Finally, this paper is also related to the large literature on active labor market programs, which has found mixed results for developed countries. The existing meta-analyses by Card et al. (2010, 2018) find that job search assistance programs, like the one implemented in the PA program, have better results than other active labor market policies. Crépon et al. (2013) study the impacts of job placement assistance on the labor market outcomes of young, educated job seekers in France. They find that eligible, unemployed youths who were assigned to the program were significantly more likely to have found a stable job than those who were not, but these gains were transitory and at the expense of eligible workers who did not benefit from the program. In a meta-analysis of employment programs for youth specifically, Kluve et al. (2019) find that only one-third have positive and statistically significant effects, and these are relatively low, with better outcomes for programs that integrate multiple employment services. The PA program offers several job search assistance interventions, but it also includes a strong housing assistance component. Our analysis sheds light on whether these two components’ objectives may be at odds with each other. Our analysis contributes to this body of evidence with original results from a program that integrates diverse interventions devoted to addressing potentially conflicting objectives.

Our findings have important implications for the design of housing assistance programs. The tension we identify between housing quality and employment access, combined with beneficiaries’ satisfaction with objectively “worse” neighborhoods, suggests that policies should move beyond one-dimensional measures of neighborhood quality to consider multiple dimensions that matter for welfare. Programs might benefit from more nuanced approaches that consider both housing quality and employment accessibility while respecting beneficiaries’ diverse preferences. We expand on these policy implications in the conclusion.

The paper is organized as follows. Section 2 presents some background on the program, its context, and the experimental design and setup, as well as characteristics of the program’s population. Section 3 presents the empirical methods and the results for individual-level experience with the program and labor market and housing outcomes. Section 4 explores

potential mechanisms behind these results by examining differences between beneficiaries and non-participants' area of residence characteristics. Section 5 concludes by presenting our interpretation of the findings and a discussion of alternative hypotheses.

2 Background of the program and of the experiment

2.1 Characteristics of the program

To address the challenges of housing and labor markets faced by youth in France, in 2010 the departmental council of the Bas-Rhin region implemented the PA program, which targeted young adults aged 18-25 in precarious situations. The primary goals of the intervention were securing stable housing and fostering employment opportunities.

The program centralized assistance through a “single point of contact,” a social worker assigned to each beneficiary that operated as a personalized guide and accompanied participants throughout their stay in the program. The program targeted youth in precarious conditions. Eligibility criteria were: (i) no education diploma, (ii) unstable and unsatisfactory housing conditions (i.e., living with parents in contexts of interpersonal conflicts, living with friends, overcrowding, living in shelters), (iii) no job or very unstable job with low income, (iv) financial distress.

The social worker was tasked with three objectives over the period of up to two years. First, she should seek to stabilize beneficiaries' financial resources by helping them acquire stable employment and ensuring adequate revenues. The social worker would normally give advice concerning work etiquette, guidance on how and where to find a job, as well as assistance with budgeting and with application to welfare programs and available aids. Second, the social worker would guide beneficiaries in the process of finding adequate housing, and would provide advice on housing financial aid from external sources and on communicating with landlords. The social worker could also establish the local authority as the beneficiary's rent guarantor for private properties (the program did not intend to direct beneficiaries to

social housing). Access to independent private housing in France often hinges on having a guarantor (someone who assumes the risk in case of any problem arising with the tenant, payments, etc.).¹ Finally, social workers helped beneficiaries in maintaining the acquired housing by ensuring adequate revenue, budgeting correctly and behaving properly as a tenant. We can thus distinguish two broad components of the intervention: general advice (budgeting, access to social welfare programs) and job search assistance, and housing assistance. To stay in the program, beneficiaries had to participate in regular meetings with the assigned counselor in order to discuss i/ housing, ii/ labor market and training, iii/ financial constraints and iv/ other issues (emotional, personal, health, etc).

2.2 The experiment

The estimation of the program’s effect builds on the random assignment of participants to the program or to a control group. The local authorities received applications on a rolling basis and carried out random assignment of beneficiaries at periodic intervals. A total of 981 youth complied with the inclusion rules (age, socioeconomic conditions) and registered in the experiment, of which 486 were randomly assigned to the treatment group and 495 to the control group. We label all applicants as participants, and then distinguish between beneficiaries (the treatment group) and individuals in the control group. We exploit this setup to assess the effects of the program along several dimensions: on the beneficiaries’ receipt of assistance from social workers, on welfare receipt, on housing and on labor market outcomes. Second, we leverage detailed administrative information on the beneficiaries’ areas of residence to establish potential program effects on beneficiaries’ neighborhoods employment opportunities.

¹Financial requirements in the private rental market are related to a guarantor who demonstrates liquidity (e.g., high income or assets) and the tenant needs to prove they have a source of income (in some cases up to three times the amount of the rent). In this program, the local administration (Conseil Général) acted as the beneficiaries’ guarantor.

2.3 Data collection and effectiveness of randomization

The PA program began in April 2010 and ended in February 2013. It ran continuously during this period. While the initial schedule indicated that midline and endline surveys would take place on a rolling basis, one and two years after a participant’s enrollment, the midline survey began nine months late in April 2012 due to delays in program implementation. Hence, midline surveys were carried out between April 2012 and March 2014 and endline surveys between April 2013 and August 2015.

Table 1 presents an exhaustive list of descriptive statistics on the beneficiaries and those in the control group, and also tests for differences in these variables between the two groups. Column 1 reports the mean of the control group and column 2 the treatment group’s difference with the control group and level of significance of these differences. Column 3 presents the standard errors, and column 4 the number of observations. The table indicates statistically significant differences between the control and treatment groups in some dimensions, which could be expected given the relatively small sample size and the large number of characteristics being considered. There are significant differences in the variables that indicate whether the individual had a permanent job previous to the intervention, in an indicator if she lived under temporary third-party accommodation, and on indicators of bad or acceptable housing conditions. Reassuringly, these differences are small and not economically meaningful. Furthermore, the result of the F-test indicates that the differences in baseline characteristics in Table 1 are not jointly significant at standard levels. In the regression analysis presented in Section 3, we control for the unbalanced characteristics as well as a set of basic socioeconomic variables to increase statistical power and to control for these few pre-existing differences amongst the two groups.

The program intake ran smoothly over time (see Appendix Table A1 and corresponding text for more details on program intake). The average treatment duration lasted around one year and four months (the maximum duration was two years). As in most multi-year interventions and data collection processes, there was some degree of attrition from the PA

program and between data collection rounds. Since more participants completed one year in the program than two, the midline survey attained a slightly higher response rate of 82.9% than the endline survey (77.98%). The tests in Table 1 indicate that attrition from these surveys was balanced between the control and treatment groups.

2.4 Characteristics of the program’s population

Housing represented a major challenge for disadvantaged youth in the Bas-Rhin region at the time of the program. As Table 1 shows, more than 70% of the sample reported having problems with housing and more than half reported living in a precarious situation, characterized by having short-term housing, provisional accommodation or expulsion threats. Conversely, only 30% of the sample reported having stable housing and a 35% of the sample was accommodated temporarily by a third party. As for the type of accommodation arrangements, around a third of the sample lived with their parents and less than 20% lived by themselves. Individuals also reported difficulties in moving out of their parents’ homes and in acquiring stable, non-precarious housing.

Additionally, finding a stable job posed a considerable challenge for the program’s objective population. Half of the sample was unemployed or searching for employment, which is approximately 30 percentage points higher than the national average (OECD, 2018). Whereas 41% of the sample was employed, 48% of youths aged 20-24 were employed nationally. Moreover, only 10% of the sample had a permanent job, and almost 16% had temporary contracts. On average, participants earned 528 euros per month, slightly less than the national average of 576 euros per month for employees under 25. Regarding educational achievements, two thirds of the sample completed secondary education, but only 10% pursued a tertiary education.

Finally, the PA program was specific to the Bas-Rhin region. More than half of the sample lived in the city of Strasbourg, 15% in the larger urban area of Strasbourg and 27% in the Bas-Rhin region outside Strasbourg. At baseline, almost a fifth were dissatisfied or

very dissatisfied with the area in which they lived.

3 Empirical methods and results

3.1 Empirical methods

The program intake was designed to ensure adequate statistical power to test the null hypothesis that the program would improve beneficiaries’ housing and labor market outcomes.

In the context of a randomized controlled trial, we use a simple ordinary least squares regression model with controls of the following form:

$$y_i = a + b \textit{Treated}_i + X_i\gamma + \mu_i \tag{1}$$

where y_i is our outcome variable for recipient i , a is the constant, and $\textit{Treated}_i$ is an indicator of whether the youth i belongs to the treatment (1) or the control (0) group. The variables in X_i correspond to a series of socioeconomic controls (including those that were not balanced between the treatment and control groups in Table 1) and program-implementation related characteristics, such as the participant’s implementation organization.² Besides controlling for chance imbalances in the outcomes of interest, these controls are meant to increase statistical power given the relatively small final experimental sample for the program. Most importantly, when available, X_i includes the value of the dependent variable y_i at baseline, which implies that we are estimating ANCOVA models for the variables with repeat measurements (McKenzie, 2012).³ Finally, since there was a change in the probability of being selected into the program, the regressions are estimated with a weight to account for the differential probabilities of being randomized into the program – the results remain virtually unchanged without this adjustment (see Appendix A1 for details). We estimate this

²Please see the note to Table 2 for a complete list of these controls.

³As a robustness check we also run the same regression using the double-post-lasso procedure (Belloni et al., 2014). Results are virtually the same. We report the procedure with the fixed list of controls as we also implement randomization inference with these common controls, as described below.

model separately for outcomes in the midline and endline surveys. In addition to asymptotic standard errors for each coefficient in Equation 1, we also report the exact p-values of the estimates based on 9,999 iterations based on Young (2020)’s randomization inference procedure.⁴ Also following Young (2018), we report in each table Westfall–Young stepdown p-values (FWER-adjusted) for outcome families. These are p-values adjusted for multiple testing for all midline outcomes, for all endline outcomes, and for all outcomes in each table. In Table 4, moreover, we include the same test for outcome families for all the results in Tables 2, 3 and 4 – again, for all midline outcomes, all endline outcomes, and all outcomes considered in these tables jointly. This represents a test of the experiment’s overall significance where the null is that the experiment is irrelevant – that is, that all coefficients are zero.

For each outcome of interest, Tables 2 to 4 report the mean for the control group in column 1 and the treatment effect in column 2 with the asymptotic level of significance denoted by stars. Column 3 shows the standard errors of these treatment effects, column 4 presents the exact randomization inference p-values of the estimates, and column 5 portrays the number of observations. It should be noted that the questionnaires from the baseline, midline and endline survey differed substantially in some items – we always report the effects on the same variable at the two survey rounds when available.

3.2 Personal support: Effectiveness of the program

The premise of the PA program was the provision of housing and job search assistance by means of a dedicated social worker that would take care of a beneficiary throughout the duration of the program. In this context, we first want to assess if the program was in effect successful at providing this support and assistance.

The results in Table 2 indicate that program participants received more support from social workers and were satisfied with their services with respect to the control group. It

⁴we report RI-c p-values—exact randomization-inference p-values computed with a studentized test statistic.

should be noted that individuals in the control group did not have access to the PA program’s “single point of contact” but they were still eligible for other forms of assistance from local and national welfare, aid and support programs. Compared to this status quo, we observe that PA beneficiaries report significantly higher levels of engagement with social workers, mostly at the time of the midline survey. For instance, we observe substantial increases in the proportion of youngsters who effectively met with a social worker: treated individuals experience an increase of 21.62 percentage points regarding meetings with a social worker in the midline survey (significant at the 1% level), although this effect is small and not significant for the endline survey (there were no statistically significant effects for meetings with local mission employees in either of the two survey rounds). These meetings were satisfactory: the proportion who manifested trust in social workers in the midline survey was 25.19% for the control group and 14.81 percentage points higher for individuals in the treatment group (significant at the 1% level), whereas those declaring to be satisfied with housing assistance at the endline survey were 30.89% in the control group, and 17.64 percentage points higher in the treatment group (significant at the 1% level). Regarding specific aspects of support, the program induced an increase of 11.18 percentage points in budget assistance when compared to the control group (significant at the 1% level).

Moreover, this assistance resulted in more access to housing and welfare programs. The proportion of individuals in the control group receiving support from the means tested National Housing Benefit program is 41.24% in the midline and 47.64% in the endline survey, with significantly higher levels for treated individuals by 6.55 and 8.59 percentage points respectively. We observe a similar pattern for Local Housing Benefit program: 15.95% and 9.52% in the control group received them in the midline and endline surveys respectively, with 19.99 and 4.18 percentage points higher levels for the treated group (significant at the 1% and 10% levels respectively). The increase in assistance is clearly related to the target group (youth) and objectives (housing) of the program: we find no significant differences for

other benefits, such as income support or unemployment benefits.⁵

All in all, the PA program induced fruitful interactions between beneficiaries and social workers, reflected in higher levels of trust, assistance and access to benefits. These strong positive effects fade out over time, with fewer significant effects reported in the endline survey, though participants still experience a statistically significant difference in the receipt of financial benefits (both youth and housing benefits), as well as in satisfaction with housing assistance. This is also reflected in the very low p-values for the Westfall-Young tests of multiple outcomes.

3.3 Housing outcomes

The main component of the program was the provision of housing advice and assistance, including the local authority providing a rent guarantee. The evidence in Table 2 indicated that participants were more satisfied with housing assistance than individuals in the control group.

Table 3 presents a series of additional outcomes directly related to participants' accommodation situation and housing conditions. The percentage of treated respondents reporting to be in a good accommodation situation (having chosen their living arrangement and not being in institutional accommodation) is higher than that for the control group by 9.03 percentage points in the midline survey and 7.68 percentage points in the endline survey (significant at the 1% and 5% levels respectively), with a level of 41.50% and 43.72% respectively for the control group. Accordingly, the percentage of individuals reporting a precarious accommodation situation was 6.85 lower in the treated group in the midline survey and 1.31 percentage points lower in the endline survey, although only the former is statistically significant (at the 5% level). In addition to affecting accommodation arrangements, the program also had an impact on the quality of housing. Treated individuals had a higher probability of reporting

⁵The Local Housing Benefit program is aimed at providing *financial aid for access to housing, maintenance in the premises, prevention of water, energy and telephone service cuts*. One objective of the PA program was to improve the ability of youth to access this program.

no housing problems (such as humidity, heating or vandalism) of 4.53 percentage points for the midline survey and 9.72 percentage points for the endline survey (although only the latter is statistically significant at the 5% level), with levels for the control group of 30.75% and 38.92% respectively. Overall, 58.79% (midline) and 55.91% (endline) of individuals in the control group reported having some form of accommodation arrangement or housing issues, with 9.02 and 7.98 percentage points lower levels for individuals in the treatment group at the midline and endline surveys respectively (both coefficients significant at the 5% level).

Besides accommodation arrangements and housing quality issues, the program also had an impact on other housing related variables. For instance, 14.29% of individuals in the control group reported having missed their rent at the time of the midline survey, whereas those in the treatment group had a 4.17 percentage points lower probability of doing so (significant at the 10% level – the difference for the endline level is close to zero and not significant). This may be due to the increased access to housing aid or to higher level of budgeting assistance induced by the program.

Finally, individuals in the treatment group also reported to be more satisfied with their housing and with their area of residence (information available for the endline survey only): 34.55% of individuals in the control group manifested to be very satisfied with their housing, with a large 9.64 percentage points difference for the treatment group (significant at the 1% level). Notably, the effect is concentrated in the higher category of satisfaction for this question – there is no significant difference in reporting to be satisfied or very satisfied with housing between the two groups. Most importantly, beneficiaries are more satisfied with their area of residence than individuals in the control group: 71.99% of individuals in the control group reported to be satisfied or very satisfied with their area, with a 5.29 percentage points increase for those in the treatment group (significant at the 10% level). The difference is similar, but not statistically significant, for those who reported to be very satisfied with their area (we explore the program’s effect on area characteristics in Section 4 below. Figure 1 depicts the full distribution of the housing (panel a) and neighborhood

(panel b) satisfaction variables for the treatment and control groups. The Epps-Singleton tests of difference between the distributions (Goerg and Kaiser, 2009) indicates that the treatment group is more satisfied for both variables (significant at the 10% level).

3.4 Labor market outcomes

The program also had as an objective integrating beneficiaries to the labor market. Table 4 presents the results on the impact of the PA program on labor market outcomes. The evidence in Table 2 indicated that the program did not have a significant impact on beneficiaries' satisfaction with job search assistance at the time of the endline survey, despite higher satisfaction with housing assistance. The evidence in Table 4 is telling: the program as a whole did not achieve economically nor statistically significant effects on youngsters' labor market outcomes. There is no significant effect on employment for the treatment group at midline and endline surveys. Interestingly, however, the employment rate rose from close to 41% to almost 50% from the baseline to the endline survey, but this cannot be attributed to the program itself.

For contract type or duration (permanent or temporary jobs, full time work, being unemployed), we observe no impact produced by the program either. The same can be said regarding the average monthly salary, and the percentage of participants who were interviewed in the month before the survey. Regarding the acceptance of an offer for a temporary employment agency, no statistically significant differences were found in the midline survey, and though they were present in the endline survey, the effect is actually negative: -5.06 percentage points (significant at the 5% level) — i.e., treated individuals exhibit a lower probability of having accepted an offer of this type. We find a positive and significant effect for being in education or training of 4.2 percentage points (significant at the 10% level) at the midline survey, which may be attributed to the social workers orienting beneficiaries to training, but this effect disappears in the medium run — the difference at the endline survey is small and not statistically significant. While beneficiaries may have increased their

training or education because of the program, this did not translate into better labor market outcomes later on.

The null employment effects we observe⁶ align with a substantial body of evidence suggesting mixed effectiveness of job search assistance programs, particularly for disadvantaged youth. This interpretation is further supported by our own findings in Table 2: whereas beneficiaries reported significantly higher satisfaction with housing assistance (a 17.64 percentage point increase), we find no statistically significant effect on beneficiaries' satisfaction with job search assistance. This lack of perceived effectiveness by the beneficiaries themselves suggests this component may have had limited impact. Card et al. (2018) find that only 38% of active labor market programs show statistically significant positive effects, with particularly weak results for programs targeting youth. The challenges are particularly acute for programs targeting less-educated youth like ours – Kluve et al. (2019) report that only one-third of youth employment programs generate positive effects, with weaker results for those with lower educational attainment. For the French context specifically, Crépon et al. (2013) found that while job placement assistance initially improved employment for educated youth, these effects were temporary and came at the expense of non-treated workers through displacement effects.

4 Potential mechanisms: Location and employment opportunities

The evidence presented so far indicates that the PA program was effective in improving housing and accommodation conditions, but did not improve employment outcomes for beneficiaries. In this section, we explore potential mechanisms that might explain these findings. While the experimental design does not identify mechanisms, information on beneficiaries'

⁶This is reflected in the lack of significance of the Westfall-Young multiple testing procedure for the family of outcomes in Table 4, with p-values of 0.4 for midline outcomes in the table, 0.29 for endline outcomes, and 0.49 for all outcomes in the table jointly.

neighborhoods sheds light on how the housing and employment elements may interact.

The results of this analysis are presented in Table 5. Leveraging detailed administrative information on participants’ areas of residence, we compare a series of neighborhood characteristics and indicators between beneficiaries and individuals in the control group.⁷ All variables considered are the rank of area-level indicators among all areas in the sample. We transform all ranks such that higher values indicate worse situations or higher vulnerability. Because single indicators such as the neighborhood unemployment rate show limited variation, we build indices combining several of these variables to analyze whether there were re-locations to worse-off areas. The indices are computed as the simple averages of the ranks for the underlying variables.

Table 5 shows that treated individuals’ neighborhoods exhibited different characteristics in terms of employment opportunities,⁸ as reflected by the results on the labor market indicators index, which shows significantly higher vulnerability levels in this dimension for both the midline and endline surveys (at the 10% level in both cases).⁹ The same pattern emerges for socioeconomic indicators. The results suggest that beneficiaries moved to more vulnerable areas. The differences are positive, but larger and statistically significant only for the endline survey (significant at the 10% level).¹⁰

⁷This is similar to Jacob and Ludwig (2012) analysis of the effects of a housing voucher program in Chicago on beneficiaries’ resulting neighborhood environment (Table 5, Section D). The authors find no effects of the vouchers on beneficiaries’ neighborhoods poverty rates, proportion of minorities, crime and social capital. Kling et al. (2005), in the context of the Moving to Opportunity experiment, find that the program’s experimental arms conditioning the voucher on moving to low poverty area successfully induced beneficiaries to move to areas with lower poverty rates.

⁸Although some low-socioeconomic-status neighborhoods could, in principle, have similar physical access to jobs—with worse employment outcomes arising mainly from compositional sorting—in France the pattern is different: areas with lower socioeconomic indicators also tend to offer fewer nearby employment opportunities. Consistent with this, Gobillon et al. (2011) report that individual characteristics account for only about 30% of spatial disparities in unemployment duration, while roughly 70% is captured by local (area) indicators. We thank an anonymous referee for suggesting this clarification.

⁹The labor market indicators index is the simple average transformed rank for the following variables: the area’s unemployment rate, the proportion of workers with contracts of indeterminate duration, the labor force participation for the adult population (15 to 64) and the labor force participation rate for men in the same age range. Appendix Table A2 presents results for each of the index components separately.

¹⁰The socioeconomic indicators index is the simple average transformed rank for the following variables: the proportion of adults with no secondary school diploma or above, the areas’ average fiscal revenue, the proportion of adults for which social assistance represents 50% or more of household income, and the proportion of immigrants in the area. Appendix Table A2 presents results for each of the index components

Finally, we present the results for an index that combines the underlying variables for both the labor market and socioeconomic indicators indices.¹¹ The results for this aggregate vulnerability index exhibit statistically significant differences for the midline and endline periods, with a higher and more statistically significant difference at endline (at the 10% and 5% levels respectively).¹² To probe the null employment effects further, we implement an analysis to test whether the program had direct employment effects separate from its impact on residential location.¹³ The logic behind this exercise is that if the program improved employment capabilities but beneficiaries moved to areas with poor job opportunities, controlling for neighborhood characteristics should unmask positive employment effects. This approach assumes that, conditional on our control variables, selection into neighborhoods is independent of unobserved factors affecting employment outcomes. As reported in Appendix Table A3, when we add a control for the labor-market indicators index of the respondent’s area of residence at the time the outcome is measured,¹⁴ the null employment treatment effects remain virtually unchanged. This stability is informative in two ways. First, conditioning on observed local labor-market conditions does not uncover a hidden employment impact. Second, while these controls are post-treatment and thus the specification identifies a direct (conditional) effect rather than a total effect, the near-identical estimates suggest that any bias introduced by conditioning on location is negligible in practice.

Given the importance of area characteristics for employment opportunities and socioeconomic conditions, we provide a heterogeneity analysis of the program’s effects by area separately.

¹¹The aggregate vulnerability index is the simple average transformed rank for the eight variables underlying the labor markets indicators and the socioeconomic indicators indices. Appendix Table A2 presents results for each of the indices’ components separately.

¹²Additionally, we also examine the effect of program participation on social links. Our surveys involve several questions about social ties, such as on how frequently participants met with friends, or whether they could count on friends/family to face specific problems. The results (not reported) show no effect on these variables.

¹³We owe this suggestion to the editor.

¹⁴As discussed above, this index summarizes local labor market conditions at respondents’ areas of residence. Alternative specifications controlling for the area’s socioeconomic indicators index, or by the variables that make up those indices separately, yields virtually the same results (available upon request). We thus prefer the more parsimonious specification controlling for the simple labor indicators index.

of residence at baseline in Table 6. We show the proportion employed and the proportion residing in above or below-median employment opportunity areas,¹⁵ and we also examine the joint distribution of employment and location outcomes. The results indicate that the program shifted residential patterns—treated individuals were 4.79 percentage points less likely to reside in above-median areas at midline and 5.52 percentage points less likely at endline (both significant at the 10% level), which is consistent with the program’s effects on area of residence characteristics presented in Table 5. When examining joint outcomes, we observe some changes in the distribution of employment and location, such as a positive effect on being employed in below-median areas at midline (4.87 percentage points, significant at 10%).

We also analyze heterogeneity in these outcomes by the characteristics of the area of residence at baseline (again, dividing areas as those above and below the median employment opportunities index). For those originally from worse-off neighborhoods (bottom panel of Table 6), there is a large and positive effect on the probability of being employed in a below-median neighborhood at the time of the endline survey (8.43 percentage points, significant at 10%), but this is offset by a similarly large negative effect on the probability of being employed and residing in an above-median neighborhood (-7.37 percentage points, significant at 5%). For those who started in above-median employment opportunity neighborhoods, we observe much more limited effects. While these results show some heterogeneity, they do not provide definitive evidence that the program improved employment outcomes for specific subgroups. The evidence does not allow us to disentangle changes in residential sorting from genuine employment improvements.

The null findings in terms of employment combined with the positive housing effects and changes in neighborhood characteristics could be explained by several mechanisms, which are not mutually exclusive and may have operated simultaneously. First, the job search

¹⁵In this table, the index measures opportunity – it has not been reverted as in Table 5 for a more intuitive presentation of results: above-median areas are better off neighborhoods and below-median are those worse off.

assistance component may simply have been ineffective for this population, consistent with the broader literature on youth employment programs (Crépon et al., 2013; Card et al., 2018; Kluve et al., 2019). The challenges of providing effective employment services to disadvantaged youth—including skills mismatches, lack of work experience, and employer discrimination—are well-documented (Caliendo and Schmidl, 2016). Our population faced particularly severe barriers: two-thirds had at most secondary education, over 70% had housing problems, and half were unemployed at baseline, suggesting they may have required more intensive or different types of support than the program provided. Additionally, several mechanisms related to the housing component may have operated. Spatial mismatch is one possible explanation—the housing assistance may have enabled beneficiaries to move to neighborhoods with better housing but fewer job opportunities, potentially offsetting any positive effects of the job search assistance component, though the unchanged employment effects when controlling for location (Table A3) suggest this mechanism alone cannot fully explain our results. Income effects from housing assistance may also have played a role, as the housing support, particularly the rent guarantee provision, could have functioned as an implicit income transfer, reducing the urgency of job search—the increased access to housing benefits we document (Table 2) represents a substantial income supplement for this population. Additionally, improved housing stability might have fundamentally altered beneficiaries’ job search behavior, with secure housing no longer contingent on immediate employment allowing beneficiaries to become more selective about job opportunities, as the reduction in precarious accommodation situations (Table 3) could have extended job search duration. Finally, program resource allocation trade-offs may have affected implementation, with social workers facing time constraints potentially prioritizing the more tractable housing assistance over the more complex job placement efforts—our finding that beneficiaries reported high satisfaction with housing assistance but no significant effect on job search assistance satisfaction (Table 2) suggests possible asymmetries in program delivery.

5 Discussion and conclusion

By leveraging the random assignment of a program that tackled housing and employment difficulties among youth, we established that removing information and financial constraints resulted in better housing and improved accommodation conditions for the program’s beneficiaries. Our analysis reveals an important trade-off: while the program effectively improved housing and accommodation conditions, but it did not raise employment. Furthermore, beneficiaries were more likely to reside in areas with more limited employment opportunities and lower socioeconomic levels. This movement to objectively worse neighborhoods occurred despite beneficiaries reporting high levels of satisfaction with their new residential environments and housing conditions.

The apparent puzzle of beneficiaries choosing and being satisfied with objectively worse neighborhoods can be understood through a broader conception of residential choice. While these areas have lower employment opportunities, they may offer other valuable amenities that make them attractive to beneficiaries: more affordable housing that allows for better dwelling conditions and larger living spaces, proximity to family and existing social networks, better access to public services and transportation, or cultural amenities and community characteristics that match beneficiaries’ preferences. Indeed, the Rosen-Roback framework suggests that individuals may rationally accept fewer employment opportunities in exchange for these local amenities that represent significant non-monetary benefits. This interpretation is consistent with beneficiaries reporting higher satisfaction with their neighborhoods despite worse objective indicators.

Taken together, these results suggest that while the program was effective in improving housing and accommodation conditions and neighborhood satisfaction, the relationship between housing assistance, neighborhood choice, and employment outcomes is multifaceted. The evidence is consistent with multiple mechanisms operating simultaneously, though we cannot definitively identify which, if any, predominate. First, the job search assistance component may have simply failed to generate employment improvements for disadvantaged

populations like ours, which is consistent with the existing evidence on the limited impact of interventions of this type. The lack of employment effects despite substantial contact with social workers suggests that the standard job search assistance model may be insufficient for populations facing multiple disadvantages. Additionally, the fact that beneficiaries report higher satisfaction with their neighborhoods despite moving to objectively worse areas in terms of employment and socioeconomic indicators suggests that neighborhood quality has multiple dimensions, with beneficiaries potentially prioritizing housing quality, affordability, or proximity to social networks over employment access.

Our findings provide important nuance to the notion of neighborhood quality in the housing programs literature. While studies like Bergman et al. (2019) find that housing assistance enables moves to “higher quality” neighborhoods - typically defined by socioeconomic status, poverty rates, or school quality - our results reveal a more complex picture. Our beneficiaries systematically move to areas that would be classified as ‘lower quality’ by standard socioeconomic metrics, yet report higher satisfaction with these locations. This apparent contradiction highlights that neighborhood quality cannot be captured by a single index or dimension. Different aspects of quality – housing affordability and conditions, employment accessibility, public amenities, social networks, cultural characteristics, community attributes – may trade off against each other, and individuals may rationally prioritize certain dimensions over others. The fact that objectively “worse” neighborhoods might be optimal choices for some households suggests that policy interventions should account for this multidimensionality rather than focusing solely on moving people to areas with higher socioeconomic indicators. This is particularly relevant when programs target specific populations, as different demographic groups may value neighborhood attributes differently based on their circumstances and preferences.

These insights have important policy implications. Housing assistance programs might benefit from incorporating explicit geographical targeting or providing incentives for residence in areas with stronger labor markets. Alternative approaches could include enhancing

transportation links between affordable housing areas and employment hubs, or developing place-based policies to attract employers to areas with more affordable housing. The challenge lies in designing interventions that respect beneficiaries' diverse neighborhood preferences while ensuring access to economic opportunities. These issues are present in the current literature on place-based policies that provide incentives for firms to locate near deprived neighborhoods. In a context related to ours, Gobillon et al. (2012) and Givord et al. (2013) find that the French enterprise zone program, in which firms had to hire local employees in exchange for substantial tax reductions, decreased unemployment only modestly, induced displacement effects, and it was likely not cost-effective. Some recent work has also studied the impact of transportation subsidies. Phillips (2014) studies a program that provided transit subsidies in the Washington DC area to randomly selected clients of a non-profit employment agency, and finds that reducing transport costs increases job search intensity and short-run employment. The paper also provides a model that links these costs to the persistence of neighborhood poverty. Le Gallo et al. (2017) in turn study the impact of subsidizing driving lessons in France. They do not discern any significant impact on access to permanent jobs, but they do find a positive yet weak effect on access to temporary jobs.

Ultimately, our study contributes to the broader debate on place-based versus people-based policies. The tension we identify between housing quality and employment access, combined with the multidimensional nature of neighborhood quality, suggests that successful interventions may need to combine both approaches - helping people access better housing while also investing in the economic development of affordable housing areas. Future research should focus on understanding how different populations value various neighborhood attributes, to understand poor individual's neighborhood choice (Galiani et al., 2015; Collinson and Ganong, 2018), and how policy can better accommodate these heterogeneous preferences while promoting economic opportunity.

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Table 1. Baseline Characteristics and Experimental Balance, *Pass'Accompagnement* Program. Full Baseline Sample

	Control	Treatment Diff.	Diff.	N
	Mean	w/Control	S.E.	
<i>Individual Characteristics</i>				
Age at baseline	21.26	-0.01	0.13	979
Female	57.89	0.29	3.16	979
Has children	16.80	0.44	2.40	979
Single	84.21	-0.75	2.35	979
Born in France	72.27	-1.10	2.89	979
French father	55.47	-1.81	3.19	979
French mother	59.31	-1.35	3.15	979
Primary education	0.40	0.01	0.41	979
Some secondary education	10.12	1.60	1.99	979
Completed secondary education	66.19	-2.86	3.06	979
Technical secondary education	9.31	0.56	1.89	979
Tertiary education	10.53	0.25	1.97	979
Foreign education category	1.01	-0.01	0.64	979
Education unknown	2.43	0.46	1.03	979
Monthly income (Euros)	528.13	33.96	43.27	979
<i>Employment</i>				
Employed	41.09	-3.45	3.13	979
Permanent job	10.93	-4.13**	1.81	979
Temporary job	15.79	-0.40	2.34	979
Full-time job	18.22	-1.28	2.45	979
Unemployed for the last 3 months	50.20	0.92	3.20	979
Monthly labor earnings (Euros)	240.99	-22.12	22.45	979
<i>Housing and Accommodation</i>				
Major housing problems	48.79	1.58	3.20	979
Minor housing problems	23.89	-3.98	2.63	979
No housing problems	21.66	0.45	2.65	979
Stable accommodation	29.76	1.45	2.94	979
No fixed accommodation	4.25	1.64	1.42	979
Institutional accommodation	6.88	-0.22	1.62	979
Temporary 3 rd -party acc.	35.63	-5.26*	3.00	979
Other accommodation	16.80	2.32	2.47	979
Accommodation: DK/DA	6.68	0.07	1.60	979
Autonomous accommodation	18.02	1.06	2.48	979
Parental accommodation	31.78	2.07	3.00	979
Precarious accommodation	51.01	-3.09	3.20	979
Housing conditions: V. Good	13.56	-1.87	2.13	979
Housing conditions: Good	21.05	0.28	2.62	979
Housing conditions: Acceptable	44.94	-6.02*	3.15	979
Housing conditions: Bad	14.78	5.66**	2.43	979
Housing conditions: N/A	5.67	1.95	1.60	979
Problems with housing	72.67	-2.40	2.90	979
<i>Social Assistance</i>				
Housing support	34.21	-0.50	3.04	979
Unemployment benefits	15.79	-2.20	2.27	979
Income support	15.79	-1.28	2.30	979

Provider: NGO	24.49	-3.18	2.70	979
Provider: Urban Gov. Office	36.84	6.25**	3.13	979
Provider: Rural Gov. Office	18.42	1.09	2.50	979
Provider: Local Gov.	20.24	-4.15*	2.45	979
<i>Area Characteristics</i>				
Strasbourg	57.69	-1.29	3.17	979
Strasbourg Metro Area	15.38	0.73	2.33	979
Outside Strasbourg	26.92	0.56	2.84	979
Very satisfied with area	27.33	1.87	2.89	979
Satisfied with area	20.85	-1.81	2.55	979
Somewhat satisfied with area	19.43	-0.20	2.52	979
Dissatisfied with area	8.10	0.25	1.76	979
Very dissatisfied with area	12.15	-1.07	2.05	979
Satisfaction with area-N/A	12.15	0.96	2.14	979
<i>Area Indicators</i>				
Labor market-indicators index	50.61	-1.26	1.61	979
Socioeconomic-indicators index	50.51	-0.94	1.60	979
Aggregate vulnerability index	50.51	-0.99	1.48	979
<i>Other Characteristics</i>				
First Semester	13.56	4.17*	2.30	979
Second Semester	24.29	-2.69	2.65	979
Third Semester	22.47	-0.67	2.65	979
Fourth Semester	15.59	-1.34	2.32	979
Fifth Semester	24.09	0.53	2.78	979
Attrition (present at baseline only)	30.97	-3.42	2.91	979
Attrition at midline	19.03	-3.65	2.41	979
Attrition at endline	22.67	-1.06	2.65	979

F test p-value: 0.85

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). Values in percentages except for income/earning amounts.

Source: PA administrative records (baseline, midline, and endline data collections).

Table 2. Social Worker's Support to Youth and Social Assistance Receipt. Treatment Effects with Controls

	Control	Treatment	T.E.	R.I.	N
	Mean	Effect	S.E.	p-value	
Midline Survey					
<i>Support from Social Workers</i>					
Budgeting assistance	20.35	11.18***	3.12	0.0003	803
Housing support ⁺	18.25	4.26	2.87	0.1394	809
Would get help if homeless	15.62	4.85*	2.74	0.0754	802
Meeting with local mission	60.96	-2.08	3.51	0.5493	802
Meeting with social worker	38.99	21.62***	3.48	<0.0001	800
Trust in social workers	25.19	14.81***	3.31	<0.0001	802
<i>Social Assistance Receipt</i>					
Any benefit (non-housing)	50.00	1.51	3.15	0.6215	802
Income support ⁺	21.80	1.36	2.40	0.5736	808
Housing benefit	41.24	6.55*	3.51	0.0683	782
Unemployment benefits ⁺	18.80	-0.41	2.71	0.8760	808
Youth benefits	15.95	19.99***	3.12	<0.0001	796
Endline Survey					
<i>Support from Social Workers</i>					
Budgeting assistance	11.32	-0.03	2.32	0.9887	756
Housing support ⁺	11.52	0.60	2.47	0.8062	762
Meeting with local mission	51.58	-2.86	3.76	0.4960	755
Meeting with social worker	34.04	2.01	3.43	<0.0001	755
Satisfied w/job search assistance	41.62	2.56	3.72	0.4523	762
Satisfied with housing assistance	30.89	17.64***	3.54	0.0563	762
<i>Social Assistance Receipt</i>					
Any benefit (non-housing)	49.60	3.39	3.17	0.2863	762
Income support ⁺	26.51	1.81	2.83	0.5230	760
Housing benefit	47.64	8.59**	3.66	0.0198	762
Unemployment benefits ⁺	21.78	-3.10	2.99	0.2897	760
Youth benefits	9.52	4.18*	2.38	0.0833	753
<i>Westfall-Young multiple testing</i>					
Midline outcomes				0.0001	
Endline outcomes				0.0009	
All outcomes				0.0005	

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). The R.I. p-value for each coefficient is the exact randomization-c p-value from 9,999 randomized inference iterations. The Westfall-Young is a test of the coefficients' overall significance adjusted for multiple testing for midline, endline and all outcomes, obtained from the same iterations (Young, 2018; 2020). Values in percentages except for income/earning amounts. The treatment effect is obtained by a regression that includes baseline controls for: birth year indicators, a gender indicator, an indicator for whether the individual has children, an indicator for marital status, a seven-tier educational level indicator, an indicator for employed individuals, an indicator for open-ended salaried workers, an indicator for the individual's monthly income, indicators for area of residence (Strasbourg, Strasbourg Metro Area and outside Strasbourg), indicators for type of program implementer (NGO, urban government office, local government office, rural government office), indicators for accommodation situation (stable accommodation, no fixed accommodation, institutional accommodation, temporary third-party accommodation, other accommodation arrangements, or does not know/not available), indicators for housing conditions (very good, good, acceptable, bad, and not available), and indicators for the semester of the individual's application (participation in the program's random assignment process). When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript).

Table 3. Housing and Accommodation Outcomes and Neighborhood Vulnerability. Treatment Effects with Controls

	Control	Treatment	T.E.	R.I.	N
	Mean	Effect	S.E.	p-value	
Midline Survey					
<i>Housing and accommodation</i>					
Precarious accommodation	34.50	-6.85**	3.19	0.0345	810
Good housing situation	41.50	9.03***	3.50	0.0116	810
No housing problems ⁺	30.75	4.53	3.28	0.1687	810
Some housing/accommodation issues	58.79	-9.02**	3.50	0.0104	804
Missed rent payment ⁺	14.29	-4.17*	2.38	0.0828	808
Endline Survey					
<i>Housing and accommodation</i>					
Precarious accommodation	23.30	-1.31	2.98	0.9171	762
Good housing situation	43.72	7.68**	3.66	0.0377	762
No housing problems ⁺	38.92	9.72**	3.87	0.0111	678
Some housing/accommodation issues	55.91	-7.98**	3.67	0.0335	760
Missed rent payment ⁺	14.14	0.41	2.55	0.5622	762
Very satisfied with housing	34.55	9.64***	3.59	0.0086	762
Satisfied or very sat. with area ⁺	71.99	5.29*	3.20	0.0930	762
<i>Westfall-Young multiple testing</i>					
Midline outcomes				0.0564	
Endline outcomes				0.0650	
All outcomes				0.1028	

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). The R.I. p-value for each coefficient is the exact randomization-c p-value from 9,999 randomized inference iterations. The Westfall-Young is a test of the coefficients' overall significance adjusted for multiple testing for midline, endline and all outcomes, obtained from the same iterations (Young, 2018; 2020). Values in percentages. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript).

Table 4. Labor Market Outcomes. Treatment Effects with Controls for Baseline Characteristics

	Control	Treatment	T.E.	R.I.	N
	Mean	Effect	S.E.	p-value	
Midline Survey					
Employed ⁺	44.50	4.07	3.49	0.2496	810
Permanent job ⁺	14.75	1.07	2.43	0.6643	810
Temporary job ⁺	18.00	0.24	2.72	0.9284	810
Full-time job ⁺	29.00	1.18	3.12	0.7119	810
Unemployed for the last 3 months ⁺	38.25	-1.33	3.36	0.7008	809
Accept an offer through interim empl. agency ⁺	15.50	-1.18	2.55	0.6427	809
Job interview last month	17.25	-4.21	2.58	0.1047	809
Labor earnings ⁺	437.89	-10.97	35.66	0.7521	808
In education/training program	9.75	4.20*	2.34	0.0653	810
Endline Survey					
Employed ⁺	49.21	-2.04	3.72	0.5880	762
Permanent job ⁺	20.16	-1.09	2.93	0.7028	762
Temporary job ⁺	17.02	-0.84	2.77	0.7690	762
Full-time job ⁺	32.20	-1.63	3.41	0.6325	762
Unemployed for the last 3 months ⁺	37.17	1.05	3.55	0.7673	762
Accepted an offer through interim empl. agency ⁺	15.71	-5.06**	2.57	0.0447	762
Job interview last month	16.23	-0.96	2.79	0.7223	762
Labor earnings ⁺	468.34	-2.21	40.49	0.9530	762
In education/training program	8.90	-0.60	1.92	0.7658	762
<i>Westfall-Young multiple testing – this table</i>					
Midline outcomes				0.3979	
Endline outcomes				0.2898	
All outcomes				0.4895	
<i>Westfall-Young multiple testing – tables 2,3 and 4 jointly</i>					
All midline outcomes				0.0016	
All endline outcomes				0.0011	
All outcomes				0.0010	

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). The R.I. p-value for each coefficient is the exact randomization-c p-value from 9,999 randomized inference iterations. The Westfall-Young is a test of the coefficients' overall significance adjusted for multiple testing for midline, endline and all outcomes, obtained from the same iterations (Young, 2018; 2020). Values in percentages except for income/earning amounts. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript).

Table 5. Neighborhood Vulnerability Indices. Treatment and Control Differences (with Controls)

	Control	Treatment	T.E.	R.I.	N
	Mean	Effect	S.E.	p-value	
Midline Survey					
<i>Area Vulnerability Indices</i>					
Labor market-indicators index ⁺	48.34	2.84*	1.50	0.0597	810
Socioeconomic-indicators index ⁺	49.16	2.04	1.59	0.2016	810
Aggregate vulnerability index ⁺	48.78	2.39*	1.39	0.0661	810
Endline Survey					
<i>Area Vulnerability Indices</i>					
Labor market-indicators index ⁺	48.14	3.11*	1.74	0.0688	762
Socioeconomic-indicators index ⁺	48.33	3.38*	1.88	0.0723	762
Aggregate vulnerability index ⁺	48.21	3.33**	1.68	0.0364	762

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). The R.I. p-value for each coefficient is the exact randomization-c p-value from 9,999 randomized inference iterations. Values in percentages. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript). The labor market-indicators index is the simple average transformed rank for the following variables: the area's unemployment rate, the proportion of workers with contracts of indeterminate duration, the labor force participation for the adult population (ages 15 to 64), and the labor force participation for men in that same age range. The socioeconomic-indicators index is the simple average transformed rank for the following variables: the proportion of adults with no secondary school diploma or above, the area's average fiscal revenue, the proportion of adults for which social assistance represents 50% or more of household income, and the proportion of immigrants in the area. The aggregate vulnerability index is the simple average transformed rank for the eight variables underlying the labor markets-indicators and the socioeconomic-indicators indices. The indices are constructed so that higher values indicate fewer employment opportunities or higher levels of vulnerability.

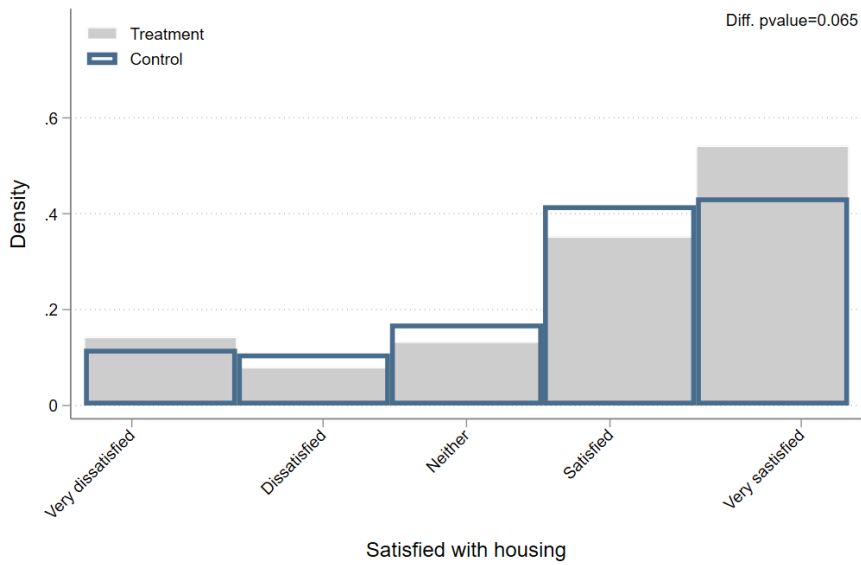
Table 6. Labor Market and Location in Above and Below Median Employment Opportunity Areas at Survey Points – Joint Outcomes and Heterogeneity by Baseline Area of Residence. Treatment Effects with Controls

	Control Mean	Treatment Effect	T.E. S.E.	N
<i>Full sample</i>				
Outcome at Midline Survey				
Employed ⁺	44.50	4.07	3.49	810
Resident in above-median area ⁺	52.25	-4.79*	2.91	810
Employed, resident in above-median area	24.00	-0.80	2.90	810
Employed, resident in below-median area	20.50	4.87*	2.84	810
Unemployed/inactive, resident in above-median area	28.25	-3.43	2.98	810
Unemployed/inactive, resident in below-median area	27.25	-0.69	3.00	810
Outcome at Endline Survey				
Employed ⁺	49.21	-2.04	3.72	762
Resident in above-median area ⁺	52.88	-5.52*	3.28	762
Employed, resident in above-median area	28.53	-4.84	3.11	762
Employed, resident in below-median area	20.68	2.66	3.06	762
Unemployed/inactive, resident in above-median area	24.35	-0.23	3.06	762
Unemployed/inactive, resident in below-median area	26.44	2.21	3.22	762
<i>Heterogeneity: Individuals resident in above-median employment opportunity area at baseline</i>				
Outcome at Midline Survey				
Employed ⁺	47.96	0.37	5.14	402
Resident in above-median area ⁺	82.14	-5.51	3.84	402
Employed, resident in above-median area	40.31	-1.69	4.99	402
Employed, resident in below-median area	7.65	2.73	2.86	402
Unemployed/inactive, resident in above-median area	41.84	-4.03	5.01	402
Unemployed/inactive, resident in below-median area	10.20	2.99	3.11	402
Outcome at Endline Survey				
Employed ⁺	53.59	-5.19	5.69	372
Resident in above-median area ⁺	76.24	-5.12	4.97	372
Employed, resident in above-median area	40.88	-2.16	5.60	372
Employed, resident in below-median area	12.71	-2.49	3.82	372
Unemployed/inactive, resident in above-median area	35.36	-2.87	5.32	372
Unemployed/inactive, resident in below-median area	11.05	7.52*	3.94	372
<i>Heterogeneity: Individuals resident in below-median employment opportunity area at baseline</i>				
Outcome at Midline Survey				
Employed ⁺	41.18	7.06	5.11	408
Resident in above-median area ⁺	23.53	-3.39	4.42	408
Employed, resident in above-median area	8.33	0.12	3.22	408
Employed, resident in below-median area	32.84	6.21	4.93	408
Unemployed/inactive, resident in above-median area	15.20	-3.73	3.40	408
Unemployed/inactive, resident in below-median area	43.63	-2.60	5.12	408
Outcome at Endline Survey				
Employed ⁺	45.27	2.34	5.44	390
Resident in above-median area ⁺	31.84	-6.23	4.65	390
Employed, resident in above-median area	17.41	-7.37**	3.61	390
Employed, resident in below-median area	27.86	8.43*	5.05	390
Unemployed/inactive, resident in above-median area	14.43	1.43	3.65	390
Unemployed/inactive, resident in below-median area	40.30	-2.49	5.22	390

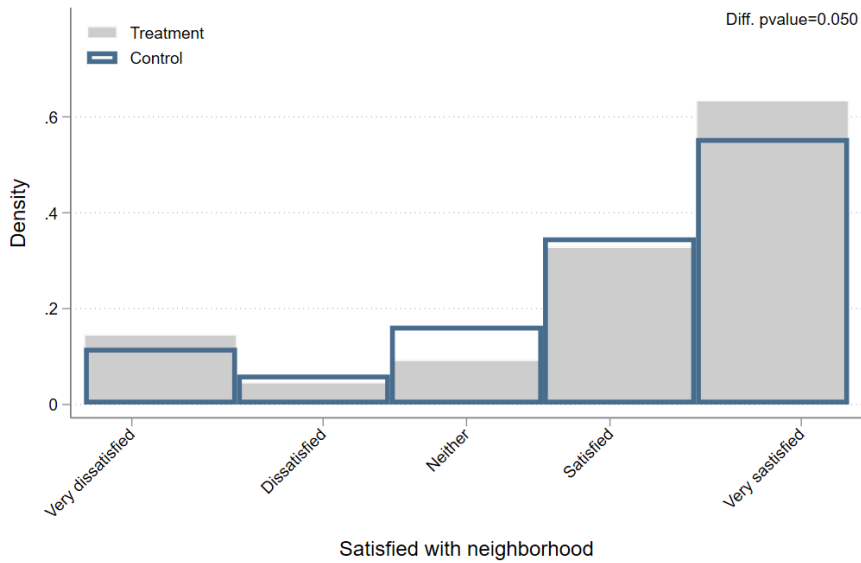
Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). Values in percentages. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. The location outcomes correspond to each survey wave. The regressions include the baseline value of the outcome of interest as an additional control. In this table, the indices have not been reverted as in Table 5 for a more intuitive presentation of results: above-median are better off neighborhoods and below-median are those worse off.

Figure 1. Housing and Neighborhood Satisfaction, Endline Survey

a) Satisfaction with housing



b) Satisfaction with neighborhood



Note: The p-value of the difference between treatment and control distributions is that of the Epps–Singleton characteristic function test of equality of two distributions, a two-sample version of the Kolmogorov–Smirnov test of equality of distributions based on the empirical characteristic function that is valid for discrete data (Goerg and Kaiser, 2009).

Appendix A1. Further details about the program's intake process

The PA program began in April 2010 and ended in February 2013. It ran continuously during this period and new participants entered on a weekly basis, resulting in approximately 29 new participants each month. While the initial schedule indicated that midline and endline surveys would take place on a rolling basis, one and two years after a participant's enrollment, the midline survey began nine months late in April 2012 due to delays in program implementation. Hence, midline surveys were carried out between April 2012 and March 2014 and endline surveys between April 2013 and August 2015.

The originally planned intake was 1400, but it was adjusted to 1000 during the program. Early applicants had a probability of being selected into the treatment group of $4/7$ but this was later adjusted to $1/2$. The regressions in the paper include weights based on the empirical assignment probabilities over these two periods to adjust for these differential selection probabilities. The results of the analysis remain unchanged without this adjustment.

Finally, the intake process experiment included an informed consent session in which the program's principles were explained, as well as the experiment and the randomization. The program was open to youth between 18 and 25 years old, and applicants were asked to fill the baseline questionnaire and to commit to keep their status updated, irrespective of being selected to the program or not (so that the evaluation would have information for treated and control individuals). They were also informed of their right to leave the program or the experiment at their will.

Table A1. Participants in Treatment and Control Groups.

Date of randomization	Treatment group	Control group	Total
June 2010 – May 2012	166	171	337
June 2011 – May 2012	180	191	371
June 2012 – February 2013	140	132	272
Total	486	494	980

Source: Program administrative records.

Appendix A2. Area vulnerability indicators

Table A2. Disaggregation of Area Vulnerability Indices (higher values indicate fewer employment opportunities or higher levels of vulnerability). Treatment Effects with Controls

	Control	Treatment	T.E.	N
	Mean	Effect	S.E.	
<i>Midline Survey</i>				
Labor markets				
Unemployment rate rank ⁺	48.59	2.16	1.78	810
Employees w/indeterminate duration contracts rank ⁺	48.15	2.82	1.76	810
Labor force participation rank, all men ⁺	48.08	3.61**	1.81	810
Labor force participation rank, all 15-64 ⁺	48.56	2.72	1.84	810
Labor market-indicators index ⁺	48.34	2.84*	1.50	810
Socioeconomic indicators				
Adults with no secondary school diploma rank ⁺	48.58	2.41	1.95	810
Adults with social assistance >50% of hh. income ⁺	49.60	1.88	1.87	810
Immigration proportion rank ⁺	49.51	1.35	1.69	810
Fiscal revenue rank ⁺	48.94	2.60	1.83	810
Socioeconomic-indicators index ⁺	49.16	2.04	1.59	810
Summary of indicators				
Aggregate vulnerability index ⁺	48.78	2.39*	1.39	810
<i>Endline Survey</i>				
Labor markets				
Unemployment rate rank ⁺	48.33	2.49	2.11	762
Employees w/indeterminate duration contracts rank ⁺	48.00	3.12	2.04	762
Labor force participation rank, all men ⁺	47.83	4.09*	2.11	762
Labor force participation rank, all 15-64 ⁺	48.42	2.58	2.13	762
Labor market-indicators index ⁺	48.14	3.11*	1.74	762
Socioeconomic indicators				
Adults with no secondary school diploma rank ⁺	48.70	2.30	2.27	762
Adults with social assistance >50% of hh. income ⁺	48.59	3.63	2.21	762
Immigration proportion rank ⁺	48.38	3.38*	2.04	762
Fiscal revenue rank ⁺	47.67	4.39**	2.15	762
Socioeconomic-indicators index ⁺	48.33	3.38*	1.88	762
Summary of indicators				
Aggregate vulnerability index ⁺	48.21	3.33**	1.68	762

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Values in percentages. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript). The labor market-indicators index is the simple average transformed rank for the following variables: the area's unemployment rate, the proportion of workers with contracts of indeterminate duration, the labor force participation for the adult population (ages 15 to 64), and the labor force participation for men in that same age range. The socioeconomic-indicators index is the simple average transformed rank for the following variables: the proportion of adults with no secondary school diploma or above, the area's average fiscal revenue, the proportion of adults for which social assistance represents 50% or more of household income, and the proportion of immigrants in the area. The aggregate vulnerability index is the simple average transformed rank for the eight variables underlying the labor markets-indicators and the socioeconomic-indicators indices.

Appendix A3. Additional analysis

Table A3. Labor Market Outcomes. Treatment Effects with Controls for Baseline Characteristics – Mediation Specification (Additional Control for Labor Market Indicators Index at Time of Outcome)

	Control	Treatment	T.E.	N
	Mean	Effect	S.E.	
Midline Survey				
Employed ⁺	44.50	4.14	3.49	810
Permanent job ⁺	14.75	1.21	2.44	810
Temporary job ⁺	18.00	0.41	2.71	810
Full-time job ⁺	29.00	1.24	3.11	810
Unemployed for the last 3 months ⁺	38.25	-1.67	3.36	809
Accept an offer through interim empl. agency ⁺	15.50	-1.22	2.55	809
Job interview last month	17.25	-4.38*	2.58	809
Labor earnings ⁺	437.89	-9.09	35.76	808
In education/training program	9.75	3.78	2.33	810
Endline Survey				
Employed ⁺	49.21	-1.70	3.73	762
Permanent job ⁺	20.16	-0.99	2.94	762
Temporary job ⁺	17.02	-0.88	2.76	762
Full-time job ⁺	32.20	-1.30	3.40	762
Unemployed for the last 3 months ⁺	37.17	0.85	3.57	762
Accepted an offer through interim empl. agency ⁺	15.71	-5.20**	2.58	762
Job interview last month	16.23	-1.00	2.79	762
Labor earnings ⁺	468.34	3.50	40.54	762
In education/training program	8.90	-0.69	1.93	762

Note: Robust standard errors. * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level (asymptotic standard errors). Values in percentages except for income/earning amounts. The treatment effect is obtained by a regression that includes baseline controls for variables as described in the note to Table 2. In this mediation specification, the regressions also include the value of the labor market indicators index at the time of the outcome as a control. When available, the regression includes the baseline value of the outcome of interest as an additional control (indicated by the ⁺ superscript).