The Allegory of the Favela: The Multifaceted Effects of Socioeconomic Mobility

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Abstract
What are the consequences of upward socioeconomic mobility for disenfranchised individuals? This article examines this question in the context of a business training program offered to residents of Brazilian urban slums, known as ''favelas.'' The study employs a randomized controlled trial complemented by quantile regressions, field visits, and interviews. The results show that training improves favela dwellers' economic outcomes, such as by increasing income and participation in entrepreneurship, and some socio-psychological outcomes, such as by improving self-efficacy and optimism. However, these income improvements were accompanied by participants' enhanced experiences of favela stigma, an adverse socio-psychological outcome related to their residential segregation. Both quantitative and qualitative findings demonstrate the multifaceted nature of socioeconomic mobility, through which favela dwellers who prosper economically become more exposed to prejudice from people living outside favelas. The study illustrates, through the ''allegory of the favela,'' the bittersweet process of socioeconomic mobility. This abductive research contributes to the literature by showing that while interventions designed to enfranchise individuals may effectively achieve economic inclusion in terms of income gains, they may simultaneously lead participants into discriminatory systems that further stigmatize people based on the same characteristics of their prior exclusion.

Keywords: labor market discrimination, social comparison, inclusion and belonging, randomized controlled trial, Sustainable Development Goals

What are the effects of upward socioeconomic mobility for disenfranchised people? The literature has shown that entrepreneurship can be an opportune career path for disenfranchised individuals facing discrimination when they try to access formal labor markets (Hwang, 2022; Hwang and Phillips, 2023). Thus, interventions such as training programs can most effectively promote

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economic inclusion and prosperity if they focus on developing individuals' skills and capabilities to enter the labor force as entrepreneurs (McKenzie and Woodruff, 2014). A critical consideration is that as economic inclusion occurs, individuals start comparing themselves with a new set of higher-status peers (Merton, 1968; Frank, 1985; Tan and Rider, 2023). These social comparison processes may generate psychological distress as people realize and experience stigmatization, tokenization, and discrimination while conducting their work (Kanter, 2008; Turco, 2010). This study investigates the intended and unintended consequences of a training program dedicated to individuals living in ''stigmatized neighborhoods'' (Ropert and Di Masso, 2021: 53; Born, 2023). The findings reveal the multifaceted nature of socioeconomic mobility in which individuals who achieve higher income levels also experience the most prejudice due to where they live. This unintended consequence (or ''latent function,'' Merton, 1957: 68) underscores the complexity of socioeconomic inclusion in discriminatory societal systems.

The context of this study is a business training program randomly offered to individuals living in Brazilian urban slums, known as favelas. The study uses a stratified randomized controlled trial to analyze the effects of a training program offered by a non-profit organization, Banco da Providência. The program provides psychological support, technical training, and management coaching to favela dwellers in Rio de Janeiro. The analysis uses two sets of outcomes to evaluate the value generated by the training program: economic outcomes measured by job allocation and income gains and socio-psychological outcomes measured by participants' reports of self-efficacy, optimism, and experiences of social stigma.

The results indicate that the training program had significant positive effects on both economic outcomes and on two of the three socio-psychological outcomes. Yet, the training led participants to experience adverse socio-psychological feelings, namely, increased social stigma related to their residence in favelas. Additional quantitative results demonstrate that the higher the income levels individuals achieved, the more prejudice they experienced. Qualitative evidence suggests that the favela stigma arose because participants became more exposed to social interactions with higher-status, non-favela individuals (potential employers and suppliers), who classified them as untrustworthy and involved in criminal activities. I explain this process through what I call the ''allegory of the favela'': a contemporary rereading of Plato's allegory of the cave (Bloom, 1968). An allegory is conventionally understood as a story within a story; thus, following Merton (1957: 68), this study demonstrates that interventions primarily targeting economic outcomes, the ''manifest'' story, may overlook socio-psychological outcomes, the ''latent'' story, experienced by participants.

The study's findings have important theoretical and empirical implications for the literature. First, I use an abductive approach to theorize the multifaceted nature of socioeconomic mobility. Building on unexpected results of a pre-registered field experiment, I find that socioeconomic inclusion may have intended consequences (''manifest functions'') in terms of economic prosperity but also unintended consequences (''latent functions'') in terms of socio-psychological distress among participants (Merton, 1957: 68). In showing that income gains go hand in hand with increased experiences of prejudice, this study directly contributes to the literature on labor market discrimination

(Hwang, 2022; Hwang and Phillips, 2023; Rider et al., 2023), social comparison (Merton, 1968; Frank, 1985; Tan and Rider, 2023), and inclusion and feelings of belonging (Kanter, 2008; Turco, 2010; Tewfik, 2022). Second, this research is among the few studies to investigate the causal impact of a training program on economic and socio-psychological outcomes of individuals who are ''residentially 'segregated''' (Massey and Denton, 1988: 283). In emphasizing the importance of both sets of outcomes, the study contributes to the literature by examining individuals holistically and not solely based on their economic production. These themes are relevant to the achievement of the United Nations' Sustainable Development Goals (SDGs), particularly those related to poverty eradication (SDG #1), inequality reduction (SDG #10), and sustainable cities and communities (SDG #11). Finally, the study aims to call for change in the societal mindset toward disenfranchised people. Such change is essential to breaking the chains of segregation embedded in current socioeconomic systems and allowing individuals to realize their full potential.

ANTECEDENTS IN THE LITERATURE

This section briefly reviews two streams of literature for their theoretical implications: labor market bias and discrimination, as well as social comparison, inclusion, and belonging. The purpose is to identify critical contingencies and relationships stipulated in prior research that will motivate this study's empirical tests and abductive theorizing.

Labor Market Discrimination, Entrepreneurship, and Training Programs

The literature has shown that due to their condition, disenfranchised individuals often suffer from labor market discrimination (Goldin and Rouse, 2000; Kang et al., 2016; Rider et al., 2023). Recent studies have shown that entrepreneurship can be an opportune career option for these individuals to avoid the discrimination that occurs when they try to access the formal labor market (Hwang, 2022; Hwang and Phillips, 2023). Moreover, entrepreneurship can be an effective career option when institutional frameworks do not provide enough support for individual development (Thébaud, 2015); when individuals' nationalities and ethnic backgrounds are socially devalued, thereby hindering access to the labor market (Marinoni, 2023); and when individuals lack access to better outside options and need to create new ventures out of necessity (Dencker et al., 2021).

Studies demonstrate that these ventures can be a way to overcome labor market discrimination and contribute to income generation and skill development among disenfranchised people. For instance, Hwang and Phillips (2023) showed how entrepreneurship among formerly incarcerated people in the United States helps these individuals avoid discrimination when searching for formal employment, increases their income levels after being reintroduced into society, and reduces their likelihood of recidivism. In other words, it generates direct socioeconomic benefits for these individuals, their communities, and society at large.

One potential way to increase these positive effects is to foster training among disenfranchised groups to better prepare them to enter the labor

market as new entrepreneurs. Training programs have been widely used to promote local economic development by enhancing the technical and social skills of disenfranchised individuals (Attanasio, Kugler, and Meghir, 2011; de Mel, McKenzie, and Woodruff, 2012; Bulte, Lensink, and Vu, 2017; Carlson and Hager, 2021; Dimitriadis and Koning, 2022). Technical skills refer to professional, managerial, and entrepreneurial abilities, while social skills relate to emotional intelligence, rapport building, communication ability, and other socio-psychological competencies. McKenzie and Woodruff (2014) provide a more extensive literature review of these programs.

Most literature in this area points to the benefits of training programs for improving disenfranchised people's economic outcomes, specifically gains in job allocation, launch and survival of new ventures, and increases in individuals' incomes and ventures' profitability (Kraay and McKenzie, 2014). However, with notable exceptions (e.g., Dimitriadis and Koning, 2022), this literature has traditionally overlooked the effect of training programs on socio-psychological outcomes of disenfranchised people, such as interpersonal skills, self-confidence, optimism, and feelings of inclusion and belonging. To gain more-comprehensive understanding of the consequences of socioeconomic mobility for these groups of people, this study evaluates the effects of a training program on both sets of outcomes.

Social Comparison, Inclusion and Belonging, and Stigmatized Neighborhoods

A fundamental element of socioeconomic mobility, particularly related to socio-psychological outcomes, refers to changes in the peers an individual uses as a reference group for social comparison (Frank, 1985). The literature has documented that as individuals advance in their careers, they tend to compare themselves with higher-status peers (Merton, 1968). This comparison might generate socio-psychological distress since individuals (and organizations) might infer quality through a ''socially endogenous calibration'' process based on their relative social position as well as peers' perception of them (Tan and Rider, 2023: 509).

The literature on inclusion and feelings of belonging has demonstrated that minority groups and individuals in a ''token'' position within organizations experience this socio-psychologic distress more severely than do people belonging to majority groups (Kanter, 2008: 6). Scholars aiming to mitigate this distress among disenfranchised individuals typically propose limited interventions, such as nudges and framings, as possible solutions. These simple fixes range from conducting blind auditions for musicians (Goldin and Rouse, 2000) to implementing opt-out policies for managerial position tournaments (He, Kang, and Lacetera, 2021). Despite their insightfulness, these interventions do not address the root causes and the nuances of discriminatory practices in organizations and economic systems. For instance, these interventions may help women musicians to gain access to orchestras previously dominated by men (Goldin and Rouse, 2000), but they do not account for the additional discrimination burden that women newly included as orchestra members might suffer.

The complexities of inclusion and belonging are even more profound. Studies have shown that tokenism is not equal among different groups of disenfranchised individuals. Depending on local cultural contexts and contingencies, tokenism can be harsher on some groups than others (Turco, 2010). For instance, Turco (2010) described how the local cultural context of the leveraged buyout industry in the United States excludes women more than Black men despite both groups being tokenized. According to the author, cultural beliefs about motherhood's impact on work engagement and differences in taste for sports make gender a more relevant status characteristic for exclusion than race in the leveraged buyout industry context. In other words, relative differences in social status affect the socio-psychological outcomes of disenfranchised individuals in their socioeconomic mobility. Moreover, specific characteristics of these individuals determine their inclusion, self-esteem, and sense of belonging (Tewfik, 2022). Furthermore, as Rider and colleagues (2023) demonstrated in their analysis of career advancement among coaches in the National Football League, these organizational processes affect an individual's assignment to work activities (i.e., allocative bias) and promotion possibilities (i.e., valuative bias).

An additional layer of discrimination that remains understudied in this literature refers to the spatial distribution of individuals. The literature on critical geography (Santos, 1978, 1987; Melgaço and Prouse, 2017) shows that physical space has an active role in shaping and constraining socioeconomic opportunities. Examples include research on residential segregation (Massey and Denton, 1988; Massey, 1990; Charles, 2003), redlining policies (Zenou and Boccard, 2000), stigmatized neighborhoods (Ropert and Di Masso, 2021; Born, 2023), and spatial inequalities (Pongeluppe, 2022). These studies exemplify how a geographic location may represent a symbolic space (Bourdieu, 1991; Wacquant, 2022; Born, 2023) used to arbitrarily categorize the people living in neighborhoods and to stigmatize them and their communities.

This stigmatization process impedes access to educational, cultural, and labor opportunities for the individuals living in these locations. Such individuals are symbolically associated with inappropriate ''ghetto behavior'' (Smith, 2005: 23), potentially classified as violent, and are thought to be related to criminal activities (Smith, 2005; Larkins, 2015). The stigmatization of individuals based on their place of residence strengthens geographical, economic, and social segregation cycles, which are mutually reinforcing and persistent over time (Massey and Denton, 1988; Massey, 1990; Charles, 2003). This entire process acts similarly to ''poverty traps'' (Dasgupta, 1997: 5), by which individuals living in stigmatized spaces are stuck in a vicious cycle of disenfranchisement.

FAVELAS AS STIGMATIZED NEIGHBORHOODS AND THE TRAINING PROGRAM DEDICATED TO FAVELA DWELLERS

Brazilian Favelas

''Favela'' is the Brazilian Portuguese word for an urban slum. These locations are associated with numerous hurdles to economic development. For example, favelas are characterized by deficient infrastructure, lack of formally defined property rights, recurrent conflicts between gangs and police forces, and the

absence of public services such as health, legal, and education systems (Perlman, 2010; Jovchelovitch and Priego-Hernandez, 2013; Larkins, 2015; Meirelles and Athayde, 2016; Pongeluppe, 2022).

Favelas cover substantial urban areas in Brazil. According to the 2010 Brazilian Census, about 11.4 million people live in favelas, of which 88 percent are located in the 20 largest Brazilian metropolitan regions. Similar settlements also exist outside Brazil. Worldwide, 828 million people live in the equivalent of favelas, and in developing countries one of three urban dwellers lives in an urban slum (UN-Habitat, 2010).

Favelas are the archetype of a stigmatized neighborhood (Ropert and Di Masso, 2021; Born, 2023). Research has shown that favelas are poverty traps (Marx, Stoker, and Suri, 2013) where, once in, individuals cannot escape due to social ostracism and lack of opportunities. The favela stigma has been portrayed in Brazilian cinema in classic movies such as *Five Times Favela* (1962), *City of God* (2002), and *Elite Squad* (2007), in which favela residents are depicted as absorbed by delinquency and predestined by Darwinism to a criminal lifestyle.[1]

The negative representation of favela residents reinforces their spatial segregation, social stigmatization, economic disenfranchisement, and lack of self-esteem (Jovchelovitch and Priego-Hernandez, 2013: 200). Moreover, there is evidence that even (or especially) the government has historically contributed to further segregating these communities by making them invisible. For example, during the 2016 Rio de Janeiro Summer Olympics, the city administration erased favelas from the official Olympic map and erected walls alongside roads that crossed favela areas (Arsenault, 2016; Opray, 2016).

The Bolsa Família Program

Given the precarious economic conditions in Brazilian favelas, a significant portion of favela residents are beneficiaries of public welfare (distress) programs, the most prominent of which is Bolsa Família (BF). The program was initially created in 2003 and, by August 2023, had reached 21.14 million families throughout all 5,570 distinct Brazilian municipalities.[2] Given the program's national reach, the Brazilian federal government called on municipal governments to implement it locally. Municipalities are responsible for enrolling families that have a monthly per capita income less than or equal to R$85 (USD$21), which is the eligibility threshold for Bolsa Família cash transfers.[3] After locating these families, municipal governments include them in the Unified Catalog for National Social Programs (pt. CadÚnico), which stores in-depth demographic information about each family. Cash transfers are made through a national bank and are directly transferred to beneficiaries through a Bolsa Família card, which works similarly to a debit card.

Cash transfers are conditional on children's enrollment in schools and on their participation in health care programs, such as immunization. The

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[1] *Cinco Vezes Favela*, https://www.imdb.com/title/tt0055287/; *City of God*, https://www.imdb.com/title/tt0317248/; *Elite Squad*, https://www.imdb.com/title/tt0861739/.
[2] Brazil's Social Communication Secretary. https://www.gov.br/secom/pt-br/assuntos/noticias/2023/08/bolsa-familia-chega-a-21-14-milhoes-de-familias-em-agosto-241-mil-a-mais-que-em-julho.
[3] The exchange rate is calculated as USD$1 to R$4 based on the rate on December 31, 2019. This rate is used throughout the study.

program's reach is considerable mainly because municipalities work closely with beneficiaries through several local reference centers for social assistance (CRAS). Every year, eligibility for renewal of the benefits is assessed by municipal agents working for these centers, who monitor educational results, vaccination rates, and the income level of families. Through the centers, public servants also provide counseling and psychological assistance. Reach and accuracy are two major strengths of Bolsa Família. Nevertheless, the program's national scope makes it rigid to idiosyncratic local features, which often hamper participants' likelihood of progressing out of the program (Wong et al., 2016).

The Banco da Providência Program

In Rio de Janeiro, a non-profit organization named Banco da Providência (BP) acts to complement the Bolsa Família program. Banco da Providência was founded in 1959 by Dom Hélder Câmara, a Brazilian archbishop nominated four times for the Nobel Peace Prize (Câmara, 1968; Bellos, 1999; Condini, 2015). Since its founding, Banco da Providência's mission has been to ''reduce extreme poverty in Rio de Janeiro city through professional capability training and income generation for youth, adults, and families that live in poverty situations.''[4] The program is related to the Catholic Church's social movement, and historically, Banco da Providência has operated in more than 60 different favelas in Rio de Janeiro, focusing on those where poverty was particularly intense. According to program reports, their primary goal is to fulfill the United Nations Sustainable Development Goals through thoughtful stakeholder responsiveness at the local level.

Banco da Providência offers a training program that educates, coaches, and supports low-income favela dwellers to improve their psychological morale, develop technical skills, and learn business techniques. The goal is to increase disenfranchised individuals' access to the labor market and to increase income through formal employment or entrepreneurship. The program has two main eligibility criteria. First, the participant's household must have an income level below the poverty line, which is precisely the threshold for qualification for Bolsa Família's cash transfers. Second, the participant must live in a Rio de Janeiro favela where Banco da Providência operates.

The methodology of the Banco da Providência program is inspired by the work of Paulo Freire in the book *Pedagogy of the Oppressed* (1970). According to Freire (1970), the learning process among disenfranchised (i.e., oppressed) people depends heavily on a continuous and balanced dialogue between teacher and student so that the student internalizes their role as the subject of their own story. Moreover, Freire's pedagogy, which uses the Socratic method for developing critical thinking, suggests that the educational approach toward oppressed people should be customized to the reality of participants. This adaptation enables them to understand the world and to enact their liberation (Freire, 1970).

Banco da Providência participants attend two or more weekly training meetings for a total of about nine months. The program has three phases, each

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[4] Banco da Providencia website. https://www.bancodaprovidencia.org.br/en.

focusing on a specific capability set. Phase One, entitled Human and Socio-Psychological Capabilities, takes about a month. In this phase, social workers discuss topics such as human and civil rights, domestic violence, communication skills, and communitarian social network dynamics. The objective of Phase One is to improve participants' morale and psychological preparedness for further training. Phase Two, called Technical Capabilities, takes about seven months. In this phase, instructors teach general and specific technical capabilities in 15 or more different jobs, such as beauty, computing, electrical work, gastronomy, and mechanics. The goal is for each program participant to prepare for a career in one of these domains, according to their preference. Finally, Phase Three, called Managerial and Entrepreneurial Capabilities, takes one month. In this phase, instructors teach managerial skills and strategies to help participants succeed in the labor market, either as employees or entrepreneurs (see Online Appendix 1 for illustrations from the field).[5] According to a program manager,

> We train lots of people to be professionals. However, we develop a professional with a key differential, a training based on socio-psychological characteristics development. We do not train a regular electrician who only knows how to install lamps and stuff. We train an electrician with critical sense, proactivity, solidarity; we use the principles of the UN peace culture. In the end, they go out as a different type of professional. (Banco da Providência General Manager, personal interview)

Table 1 compares the public Bolsa Família and the non-profit Banco da Providência programs.

Table 1. Comparison Between the Public and Non-Profit Programs

	Bolsa Família Public Program	Banco da Providência Non-Profit Program
Activities	Conditional cash transfer on education and health indicators of children.	Socio-psychological support, technical training, and career coaching.
Eligibility	Brazilian families with monthly per capita income below R$85 (USD$21.25).	Rio de Janeiro favela families with monthly per capita income below R$85 (USD$21.25).
Strengths	Reach: the same program for the whole country, locally implemented by municipal reference centers for social assistance (CRAS). Accuracy: the program targets the ones in most need (hardest to reach).	Customization: the program is explicitly designed to enroll families from the Rio de Janeiro favelas. Flexibility: the program can be adjusted according to the participants' interests.
Weaknesses	Rigidity and lack of support for participants' emancipation.	Small-scale with limited geographical reach.

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[5] While Freire's pedagogy (1970) was developed as a revolutionary literacy method, Banco da Providência's training program is dedicated to labor market capabilities training and development. Despite this difference in the targeted public, both programs depart from the same epistemological premises and objectives related to the liberation of oppressed people through critical thinking and reasoning.

The Partnership Between Bolsa Família and Banco da Providência

In 2018, Banco da Providência established a public–private partnership with the Municipality of Rio de Janeiro, in which the program was offered to Bolsa Família beneficiaries registered in two municipal reference centers for social assistance (CRAS). The targeted centers are in two of the most violent and vulnerable favelas in the city, Cidade de Deus (City of God) and Pavuna. The partnership's objective was to run a pilot to determine whether the joint effort would lead individuals to be able to exit the Bolsa Família program. Also, the partnership goals were to foster collaboration between the public Bolsa Família program and the non-profit Banco da Providência program, to stimulate knowledge exchange, and potentially to scale up the partnership to other CRAS agencies in Rio de Janeiro. During a meeting with representatives from Banco da Providência, the two CRAS agencies, and the Rio de Janeiro Social Assistance and Human Rights Secretary, the idea behind the collaboration was stated this way: ''the reality is that in the long run, we want to check if this methodology can become public policy'' (Banco da Providência Social Assistance Coordinator, meeting at the Rio de Janeiro City Hall).

This study makes use of this partnership to perform a field experiment intended to identify the causal effects of the Banco da Providência program as a complement to the Bolsa Família program. Note that the initial purpose of the experiment, as described in pre-registration, was to evaluate whether a hybrid governance structure—public (BF) and private (BP) organizations in partnership—would lead to superior results compared to a purely public or a purely private governance structure.[6]

TESTING THE TRAINING PROGRAM THROUGH A FIELD EXPERIMENT

Relationship, Data, and Methods

Relationship. The opportunity to gain access to the field site arose through a direct contact from Banco da Providência's managers. They invited me to assist in evaluating the impact of their training program. Under our agreement, I helped with the design and execution of this evaluation, while in return, the organization authorized the use of data and results for academic research. This collaboration involved no financial transactions, as my work was entirely pro bono. Additionally, the experiment was conducted without any external funding.

Sample. To evaluate the result of the training program, I performed a stratified randomized controlled trial (Bruhn and McKenzie, 2009; Glennerster and Takavarasha, 2013) that compared individuals enrolled in both the Banco da Providência training and Bolsa Família cash transfer programs (hereafter the treatment group) with those enrolled only in the latter cash transfer program

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[6] This research was pre-registered at the American Economic Association Registry for Randomized Controlled Trials (AEA RCT Registry) under protocol number AEARCTR-0002765. All the pre-trial and post-trial information, legal documents signed both by Banco da Providência and the Rio de Janeiro Social Assistance and Human Rights Secretary, dictionary, dataset, and analysis code can be found at https://www.socialscienceregistry.org/trials/2765. The study was approved by the University of Toronto Research Ethics Board under protocol number RIS#35741.

(hereafter the control group).[7] The procedure employed was as follows: Banco da Providência recruited individuals interested in participating in their program with the support of two CRAS agencies, in Cidade de Deus and Pavuna, each of which received Bolsa Família cash transfers. Banco da Providência listed 288 interested individuals, who then participated in a lottery for a spot in the program. Before the lottery, these candidates provided basic information, through which the stratified randomized assignment was executed. The randomization occurred within three strata categories: (i) participants' age, (ii) household income level, and (iii) CRAS location. This process is described further below.

The randomization procedure occurred on March 16, 2018. All methodological steps, as well as the randomization code, were pre-registered before the lottery (see Online Appendix 2 for details). Half of the members of each stratum were allocated to the treatment group and the other half to the control group. This distribution is considered the most appropriate for the research objective, given the limitations of the sample size (Bruhn and McKenzie, 2009).

After the randomization procedure, the staff of Banco da Providência and the CRAS agencies invited all 288 individuals to return to their affiliated CRAS to perform baseline data collection (survey t_0) and to report the results of the lottery. After the survey (t_0), each candidate was told individually whether they had won the lottery and obtained a spot in the program or did not win and would not be able to attend the training program in 2018. Note that 64 individuals did not show up to learn the lottery results and to perform the initial survey. The rates of no-shows were similar across the treatment (35 individuals) and control (29 individuals) groups. As these individuals did not return before learning the result, they were removed from the analysis pool.

Banco da Providência has a quota regarding the number of participants it must enroll per year by location ($N \sim 60$ per location). As a result, a backup waitlist, randomly defined during the stratified randomization procedure, was used to migrate some individuals from the control group to the treatment group. A total of 14 individuals were migrated from the control to the treatment group following the randomly assigned order of the backup waitlist. These individuals were informed and included in the treatment group. As the backup waitlist randomly defined this assignment to the treatment, these participants were considered in the analysis pool as treated individuals.

The training program began on April 2, 2018, after baseline data collection (survey t_0). The initial sample was composed of 101 control and 123 treated individuals. Figure 1 presents the randomization within age by income level stratum. Given the particularities of the setting, a high attrition rate was expected in the program. Indeed, at the end of the nine-month program, after the endline data collection (survey t_1), the experiment had an 8 percent attrition rate, which was balanced over control and treatment. Therefore, the final sample included 207 individuals, of which 93 were in the control group and 114 were in the treatment group. Further analysis of follow-up patterns is presented in the Results section.

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[7] Online Appendix 2 presents additional details about the procedures performed for sampling, data collection, and experimental design.

Figure 1. Stratification Distribution of the Initial Sample (n = 224)*
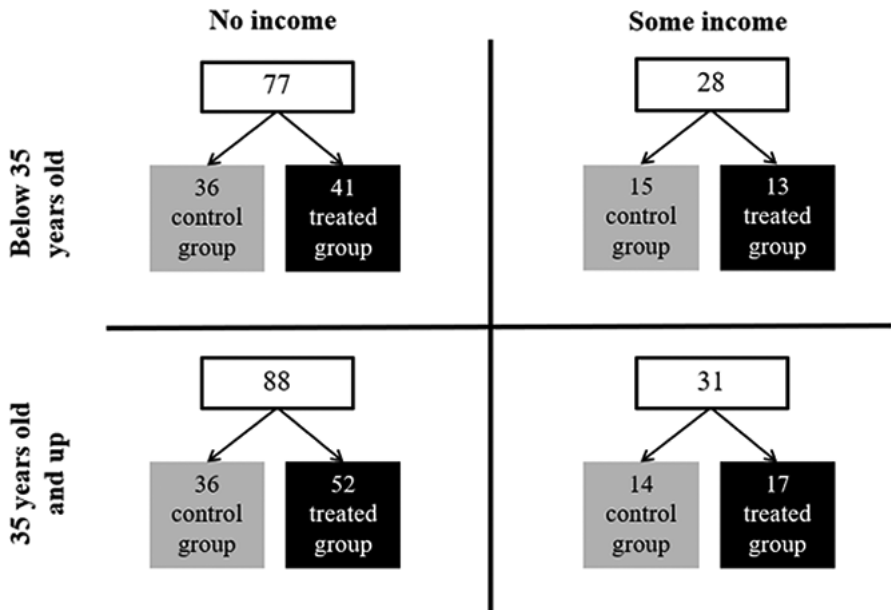
* For illustration purposes, the diagram does not show the stratification by locality (i.e., Cidade de Deus and Pavuna), only by age and income levels.

Dependent variables. I measured two main economic outcomes. First, the individual's work status was captured as an indicator (0/1) (referred to subsequently as ''job type allocation'') on one of four dimensions: no income and no job, formal employment, informal employment, or entrepreneurship activity (both formal and informal entrepreneurship). Second, the individual's income or profit measured in Brazilian reais (R$) per month was assessed on several dimensions: income per capita in the household, total household income, and individual income. Income or profit was directly surveyed with a single question. De Mel and colleagues (2009) showed that asking participants in such programs to self-report income/profit by replying to a single direct question is at least as accurate as asking for revenue and expenses/costs. Furthermore, Banco da Providência social workers (hereafter enumerators) stressed that the direct question enabled participants to understand better what was being asked.

I measured three main socio-psychological characteristics of participants, with scores ranging from 1 to 10. The selection of these three attributes was informed by the paucity of research examining the impact of training programs on these socio-psychological skills, as well as the emphasis on psychological factors during Phase One of the Banco da Providência program. First, following the literature in management and psychology (Sherer and Maddux, 1982; Chen, Gully, and Eden, 2001), I measured an individual's level of self-efficacy. This construct reflected answers to three survey questions on a questionnaire (see the questionnaire in Online Appendix 3). Internal reliability tests performed after final data collection showed a Cronbach's alpha of 0.50 for this construct.

Second, I measured the participant's optimism level. Previous literature study-
ing wealthier individuals justifies use of this measure (Englmaier, 2010; Galasso
and Simcoe, 2011). This construct was also measured through answers to
three survey questions and had a Cronbach's alpha score of 0.68. Finally, I esti-
mated individuals' awareness/experiences of social stigma related to their
place of residence, i.e., favela stigma (Jovchelovitch and Priego-Hernandez,
2013).[8] This construct was measured from responses to two survey questions,
following specialized literature (Pinel, 1999; Kang et al., 2016), with adaptations
for this setting. The construct had a Cronbach's alpha score of 0.67.

Training program (treatment). The treatment was captured by a binary var-
iable that takes the value of 1 if the individual received the actual treatment, i.e.,
enrollment in the Banco da Providência training program while already in the
Bolsa Família conditional cash transfer program, and 0 if the individual received
only the Bolsa Família conditional cash transfer program. As noted, the
characteristics of the setting drove imperfect compliance in our sample, with
the treatment variable capturing only the effect of the treatment on compliers.
The section below on the estimation model explains the implications in detail.

Fixed effects. Fully controlled models included fixed effects for the random-
ization strata and training period. For the randomization strata fixed effects, I
followed recommendations in the experimental economics literature (Angrist
and Pischke, 2008; Bruhn and McKenzie, 2009; Glennerster and Takavarasha,
2013), which suggests that stratification should be performed on variables that
could drive heterogeneous effects of the program. To balance the sample and
avoid misleading results, the randomized control trial has eight strata categories
based on the following variables: (i) age (below 35 years old vs. age 35 and
older), (ii) household income (only Bolsa Família as an income source vs. income
from this program plus any additional income), and (iii) geographical location of the
reference center (Cidade de Deus or Pavuna). In addition, the models included
a class/training period dummy to assess potential bias from the training time
sessions (morning or afternoon) or possible effects of participants' preferences
for particular sessions.

Controls. The models controlled for demographic characteristics, personal-
ity traits, and baseline dependent variable scores. The variables in the demo-
graphic characteristics set are age, gender ("woman" dummy), race (dummies
to reflect the Brazilian Census categorization of the person's identification as of
"Black" or "Brown" race), marital status ("single" dummy), number of house-
hold members, number of years of schooling, total work experience, total time
the individual was unemployed, days worked per week, and hours worked per
day. The models included controls for the big five personality traits (John and
Srivastava, 1999; Gosling, Rentfrow, and Swann, 2003). Specifically, the study
used a reduced, ten-question version of the big five personality traits question-
naire, aiming to control for potential effects that individual personality types

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[8] I also measured poverty, gender, and race stigma. Null results were found for all these stigma
types, which serves as a falsification test, reinforcing that spatial stigma is the main driver. This
analysis can be found in Online Appendix 4.

would have on participants' job allocation decisions and socio-psychological tendencies (Kerr, Kerr, and Dalton, 2019) (see Online Appendix 3). Finally, the models controlled for dependent variables' baseline values (ANCOVA).[9] This procedure followed recommendations from experimental economics (Angrist and Pischke, 2008; Glennerster and Takavarasha, 2013).

   Estimation model.   As the study has imperfect compliance, reflected in an 8 percent attrition rate, the best specification for the analysis is a local average treatment effect (LATE) (Imbens and Angrist, 1994; Angrist and Pischke, 2008), which evaluates and generalizes the causal effect of the treatment for the compliers (the individuals who complied with the $Assignment_i$ to the treatment or control group). Note that LATE uses the treatment assignment as an instrumental variable of the actual $Treatment_i$. For an instrumental variable model to hold, three main assumptions are required (Imbens and Angrist, 1994; Angrist and Pischke, 2008): (i) independence, i.e., the treatment assignment must be independent of the outcome and the actual treatment $Assignment_i \perp\!\!\!\perp$ $Outcome_{1i}$, $Outcome_{0i}$, $Treatment_{1i}$, $Treatment_{0i}$; (ii) exclusion restriction, i.e., the assignment to the training program affects the outcomes only through the actual treatment $Outcome_i(treatment, 0) = Outcome_i(treatment, 1) \equiv$ $Outcome_{di}$ for $treatment = 0,1$; and (iii) monotonicity, i.e., there are no defiers in the program $Treatment_{1i} - Treatment_{0i} \geq 0 \ \forall \ i$.
   Given the random assignment to treatment, assumptions (i) and (ii) clearly hold. Concerning assumption (iii), particularly in this setting, two factors support the assumption. First, there is no indication that participants would quit their jobs to enroll in the training program. Historically, dropouts from training arise from the opposite situation, which is that a participant leaves the program to take a job, but there are no instances of such departures in this study. Second, Banco da Providência managers reported that people who do not participate in their program usually do not have alternative training options, and the CRAS staff confirmed this information during interviews. In other words, there are no defiers. Thus, the local average treatment effect assumptions hold. The main analysis model is as follows:

$$Outcome_i = \alpha_i + \hat{\beta}\, Treatment_i + \sum\nolimits_{g=1}^{8} \delta Strata_{i,g} + \mu Period_i + \gamma' Controls_i + \varepsilon_i$$

$\hat{\beta}$ is the local average treatment effect of the treatment on the compliers for each of the outcomes.[10]

$$\hat{\beta} = \frac{E(Outcome_i|Assignment_i = 1) - E(Outcome_i|Assignment_i = 0)}{E(Treatment_i|Assignment_i = 1) - E(Treatment_i|Assignment_i = 0)}$$

$$\hat{\beta} = E(Outcome_{1i} - Outcome_{0i}|Treatment_{1i} \geq Treatment_{0i})$$

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[9] In these types of experiments, analysis of covariance models are preferable to differences-in-differences models with individual fixed effects. Controlling for baseline characteristics increases the power of the test and the accuracy of the estimation. For more information, please check https://blogs.worldbank.org/impactevaluations/why-difference-difference-estimation-still-so-popular-experimental-analysis.

[10] Online Appendix 5 presents a sequence of robustness checks that consider potential problems related to multiple hypothesis testing. The majority of the results confirm the main results.

$Outcome_i$ can be several different variables related to economic outcomes and socio-psychological outcomes. $Treatment_i \in \{0,1\}$ is the actual treatment; $Assignment_i \in \{0,1\}$ is the treatment assignment (eligibility for treatment); $\sum_{g=1}^{8} \gamma Strata_{i,g}$ is the vector of strata dummies. $Period_i \in \{0,1\}$ is a class training period dummy (1 if morning period, 0 afternoon period). $Controls_i$ is a vector of controls composed of demographic characteristics, personality traits, and baseline (survey t_0) outcome values.

RESULTS

Follow-Up Likelihood

The first set of analyses aims to understand follow-up patterns between groups. In total, 17 people did not follow up for the endline survey (survey t_1), and enumerators gathered qualitative information to obtain reasons for attrition. According to the enumerators, five people were not found (all in the control group), seven people had moved to another neighborhood/city/state (five in treatment and two in the control group), and five individuals were working and could not or did not want to answer the survey at t_1 (four in treatment and one in control).

   To make reliable inferences about attrition, the study evaluated whether the treatment had any effect on the likelihood of following up for endline data collection (survey t_1). This test checked whether treated individuals were more or less likely to respond to the endline survey (t_1) after the program ended. Table 2 shows that the treatment had no statistically significant effect on the likelihood of following up in the endline survey (t_1). A joint significance test was performed in the fully controlled model to check whether variables present in the set of controls jointly affected or are correlated with the likelihood of continuing in the sample. Similarly, in this second test, no difference between treatment and control groups is found ($p = 0.959$). In sum, the attrition rate has no relationship with either the treatment or individuals' characteristics.

   Following the local average treatment effect estimation procedure, I considered the sample of 207 compliers for which data are available at both baseline (survey t_0) and endline (survey t_1). Table 3 compares the observable

Table 2. Treatment Effect on Participants' Likelihood of Continuing (Following Up)*

	(1) Follow-up	(2) Follow-up
Treatment	0.00604	−0.00676
	(0.0358)	(0.0335)
Controls	N	Y
Test of joint significance (F-test)		0.611
p-value		0.959
Observations	224	224
R-squared	0.000	0.198

* Controls include randomization strata fixed effects, training period fixed effects, demographic characteristics, big five personality traits, and outcome values at the baseline (survey t_0). Robust standard errors are in parentheses.

Table 3. Baseline (Survey t_0) Differences Between Treatment and Control Groups*

	Control Mean	Treatment–Control Diff.		Control Mean	Treatment–Control Diff.
Income per capita in t_0 (R$)	38.04	0.968 (11.66)	Single (dummy)	0.613	0.0625 (0.0672)
Household income in t_0 (R$)	142.75	−5.849 (42.38)	Education years in t_0	9.301	0.295 (0.394)
Individual income in t_0 (R$)	29.25	−1.703 (14.59)	Total work experience in t_0 (in months)	50.043	7.738 (10.13)
No income no job in t_0	0.925	0.00509 (0.0365)	Total time is unemployed in t_0 (in months)	22.204	3.655 (5.638)
Formal job in t_0	0.000	0 (0)	Number of household members in t_0	3.409	0.451[•] (0.213)
Informal job in t_0	0.075	−0.00509 (0.0365)	Q1 Big five personality traits	8.613	0.0889 (0.263)
Entrepreneur in t_0	0.000	0 (0)	Q2 Big five personality traits	4.247	0.261 (0.432)
Self-efficacy in t_0	7.029	0.438[+] (0.247)	Q3 Big five personality traits	9.323	−0.200 (0.204)
Optimism in t_0	9.251	0.00641 (0.185)	Q4 Big five personality traits	5.817	0.253 (0.480)
Favela stigma in t_0	6.909	0.534 (0.394)	Q5 Big five personality traits	9.323	−0.0682 (0.216)
Days worked per week in t_0	0.441	−0.116 (0.183)	Q6 Big five personality traits	7.161	−0.372 (0.417)
Hours worked per day in t_0	0.817	−0.203 (0.345)	Q7 Big five personality traits	9.032	−0.0673 (0.236)
Age in t_0	35.32	1.300 (1.586)	Q8 Big five personality traits	2.817	0.235 (0.368)
Woman (dummy)	0.860	0.0433 (0.0456)	Q9 Big five personality traits	7.796	−0.0589 (0.370)
Black (dummy)	0.376	0.0535 (0.0687)	Q10 Big five personality traits	3.914	−0.0894 (0.438)
Brown (dummy)	0.462	−0.0413 (0.0697)			
Test of joint significance (F-test)		0.941			
p-value		0.555			
Observations	93	114			

+ $p < .10$; • $p < .05$.
* Robust standard errors are in parentheses.

characteristics of these individuals at the baseline. If the randomization process was successful, the two samples, i.e., treatment and control, should be statistically similar. This is confirmed, as the two groups are statistically identical based on a joint significance test ($p = 0.555$).

Table 3 lists the main characteristics of the final sample. For both control and treated individuals, the average monthly income per capita is R$39 (USD$9.75); 93 percent of individuals have neither income nor a job; approximately 88 percent are women; 84 percent are persons of color; and 65 percent are single. These descriptive statistics suggest the high intersectionality of participants in terms of gender, race, and socioeconomic condition (Crenshaw, 2017). The sample characteristics are also representative of Brazilian favela households, which are usually composed of single, unemployed mothers of color (Perlman, 2010; Jovchelovitch and Priego-Hernandez, 2013; Meirelles and Athayde, 2016). Taken together, these features suggest the potential generalizability of the results.

Table 4. Treatment Effect on Job Allocation of Participants*

DVs =	(1) No income no job in t_1	(2) No income no job in t_1	(3) Informal job in t_1	(4) Informal job in t_1	(5) Formal job in t_1	(6) Formal job in t_1	(7) Entrepreneur in t_1	(8) Entrepreneur in t_1
Treatment	−0.112	−0.146+	−0.0526	−0.0230	0.0416	0.0509	0.123••	0.118••
	(0.0698)	(0.0779)	(0.0648)	(0.0730)	(0.0465)	(0.0533)	(0.0309)	(0.0339)
Constant	0.559••	1.328•	0.333••	0.171	0.108••	−0.155	0	−0.344
	(0.0517)	(0.602)	(0.0491)	(0.511)	(0.0323)	(0.445)		(0.342)
Controls	N	Y	N	Y	N	Y	N	Y
DV average (in %)	49.76%		30.44%		13.04%		6.76%	
Observations	207	207	207	207	207	207	207	207
R-squared	0.012	0.191	0.003	0.148	0.004	0.118	0.059	0.305

+ $p < .10$; • $p < .05$; •• $p < .01$.
* Controls include randomization strata fixed effects, training period fixed effects, demographic characteristics, big five personality traits, and outcome values at the baseline (survey t_0). Robust standard errors are in parentheses.

Economic Outcomes

The next step is to understand whether the treatment had an effect on participants' job allocation and, if so, on which types of jobs. Table 4 shows that the treatment had a marginally significant effect ($p < 0.1$) on diminishing the ''no income, no job'' condition. When compared to the control group, treated individuals were 14.6 percentage points more likely to have income and a job after the program ($p < 0.1$). Analyzing the job allocation types reveals that neither informal nor formal employment shows any statistical effect of the treatment.[11]

The results show a strong significant effect of the treatment on entrepreneurship development, 11.8 percentage points ($p < 0.01$). In other words, the main path through which the treatment reduced the ''no income, no job'' condition was through small-scale entrepreneurship. Note that both formal and informal forms of entrepreneurship were considered. Figure 2 presents these results. This boost in entrepreneurship may occur as a constrained career choice (Rider et al., 2019; Dencker et al., 2021), which is a fallback option to more-rewarding and less-risky options, such as formal employment (Hwang, 2022; Hwang and Phillips, 2023).

The absence of entrepreneurs in the control group at the study's endline may seem unexpected, particularly given the prevalence of necessity entrepreneurship in countries like Brazil. There are two main reasons for this result. First, without exposure to the training program, control group individuals often overlooked entrepreneurship as a viable career option. Second, the stark lack of resources among control individuals played a crucial role. In contrast, during their training, treated individuals might have had access to essential resources such as toolkits (e.g., appliances, machines, and tools) and production inputs (e.g., cooking ingredients and sewing materials), providing a foundational platform for initiating their entrepreneurial ventures.

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[11] Notice that despite being statistically equivalent, individuals in the treated group have informal and formal jobs with higher pay. Data show that the income per capita in t_1 in informal jobs equals R$239.65 (controls) and R$300.03 (treated), and in formal jobs it equals R$405.21 (controls) and R$554.32 (treated). In other words, it seems that the treated group usually had better jobs both informally and formally.

Figure 2. Job Type Allocation After the Treatment*
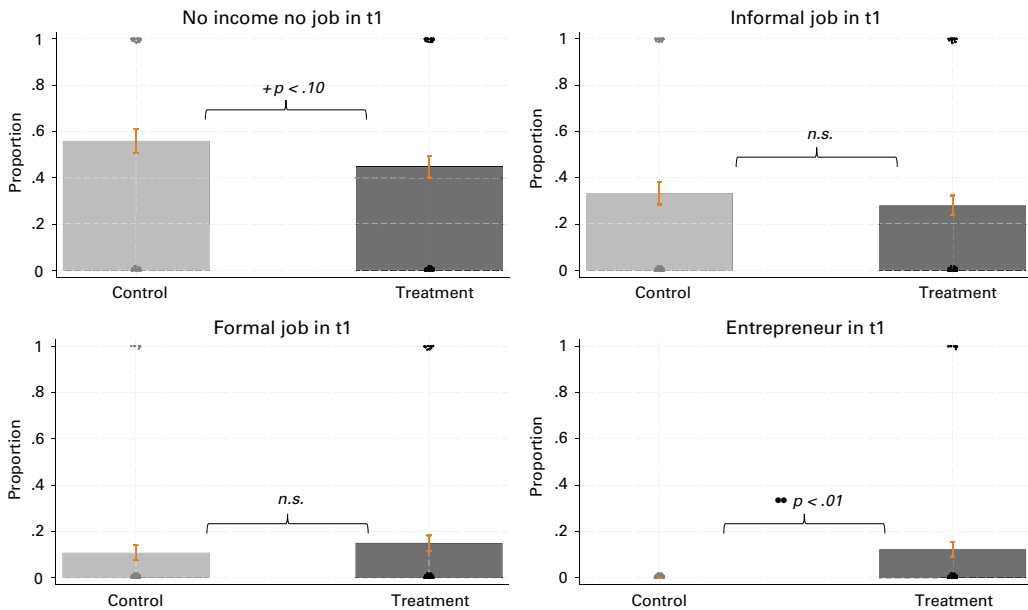
+ $p < .10$; $\bullet$ $p < .05$, $\bullet\bullet$ $p < .01$; non-significant (n.s.).
* The y-axes of the graphs are on similar scales. Since all variables are dummy variables, scattered points fall on the extremes. The plots use raw data, and *p*-values consider the fully controlled models. Data points are slightly jittered to enable an accurate interpretation of the data distribution.

Table 5 shows that the treatment had a significant effect on income generation among participants. On average, the income per capita increased by R\$106.30 (USD\$26.5) per month ($p < 0.01$). This is a 0.46 standard deviation increase in income per capita among the treated relative to control participants. On average, this change represents 2.7 times the pre-treatment monthly income per capita of the treated group.[12] The total household income increased by R\$322.50 (USD\$80.75) per month ($p < 0.01$). This is a 0.43 standard deviation increase in household income among the treated relative to control participants. On average, this change represents 2.4 times the pre-treatment monthly household income of the treated.[13] Finally, the individual income (i.e., of the surveyed participants) increased by R\$154.00 (USD\$38.5) per month ($p < 0.05$). This is a 0.32 standard deviation increase in the individual income among the treated relative to control participants. On average, this change represents 5.6 times the pre-treatment monthly individual income of the treated.[14]

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[12] Tables 3 and Table 5, such that $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (R\$106.30)/(R\$38.04 + R\$0.968) = 2.73 \equiv 273\%$.

[13] Tables 3 and Table 5, such that $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (R\$ 322.50)/(R\$142.80 - R\$5.849) = 2.35 \equiv 235\%$.

[14] Tables 3 and Table 5, such that $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (R\$154.00)/(R\$29.25 - R\$1.703) = 5.59 \equiv 559\%$.

Table 5. Treatment Effect on Income Generation of Participants*

DVs =	(1) Income per capita in t1 (R$)	(2) Income per capita in t1 (R$)	(3) Household income in t1 (R$)	(4) Household income in t1 (R$)	(5) Individual income in t1 (R$)	(6) Individual income in t1 (R$)
Treatment	98.65••	106.3••	351.2••	322.5••	133.2•	154.0•
	(30.71)	(34.25)	(97.59)	(104.1)	(65.92)	(73.00)
Constant	159.8••	57.58	565.7••	165.3	304.0••	−326.8
	(18.60)	(256.5)	(51.96)	(970.8)	(45.49)	(512.8)
Controls	N	Y	N	Y	N	Y
DV average (in R$)	R$214.08		R$759.14		R$377.33	
Observations	207	207	207	207	207	207
R-squared	0.045	0.238	0.054	0.226	0.019	0.239

• $p < .05$; •• $p < .01$.
* Controls include randomization strata fixed effects, training period fixed effects, demographic characteristics, big five personality traits, and outcome values at the baseline (survey t_0). The calculation of the treatment effect sizes among treated follows the formula $EffectSize^T = [\beta^T/AvgDV_0^T]$. Robust standard errors are in parentheses.

It is essential to be cautious about using individual income as the primary income measure. A substantial number of individuals work only occasionally as opportunities emerge or work in small family businesses. Thus, it is difficult for them to estimate their individual income precisely. Several enumerators reinforced this point based on the data collection surveys. Therefore, a total household income or an income per capita measure provides a better understanding of the monetary position of the entire household unit. This is also the reference unit that both the Banco da Providência and Bolsa Família programs employ. Figure 3 presents the income comparison between the treated and control groups.

Socio-Psychological Outcomes

Table 6 shows that the treatment has a significant effect on the socio-psychological outcomes of participants. On average, participants increased their self-efficacy by 0.524 points ($p < 0.05$) and their optimism level by 0.409 points ($p < 0.05$). These represent a 0.32 standard deviation increase in self-efficacy and a 0.29 standard deviation increase in optimism among the treated relative to control participants. On average, these changes represent a 7 percent increase in self-efficacy and a 4.4 percent increase in optimism among the treated individuals.[15]

However, the treatment group reported greater experience of stigma based on their favela residence (0.742 points) than did those in the control group ($p < 0.05$). This is a 0.27 standard deviation increase in favela stigma among the treated relative to control participants. On average, this change represents a 10 percent increase in experiences of favela stigma among the treated group, which is approximately equivalent to moving one decile up in the stigma distribution.[16] Figure 4 illustrates the socio-psychological comparison between the treated and control groups.

[15] Tables 3 and Table 6, such that $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (0.524)/(7.029 + 0.438) = 0.070 \equiv 7.0\%$. And: $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (0.409)/(9.251 + 0.00641) = 0.044 = 4.4\%$.

[16] Tables 3 and Table 6, such that $EffectSize^T = [\widehat{\beta^T}/AvgDV_0^T] = (0.742)/(6.909 + 0.534) = 0.100 \equiv 10.0\%$.

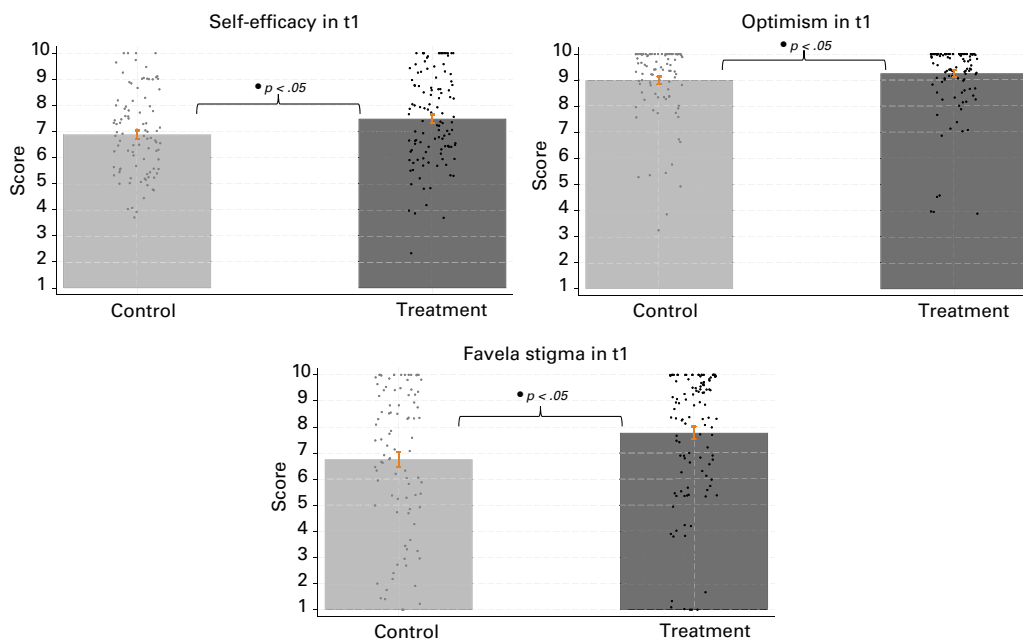## Figure 3. Monthly Income After the Treatment*
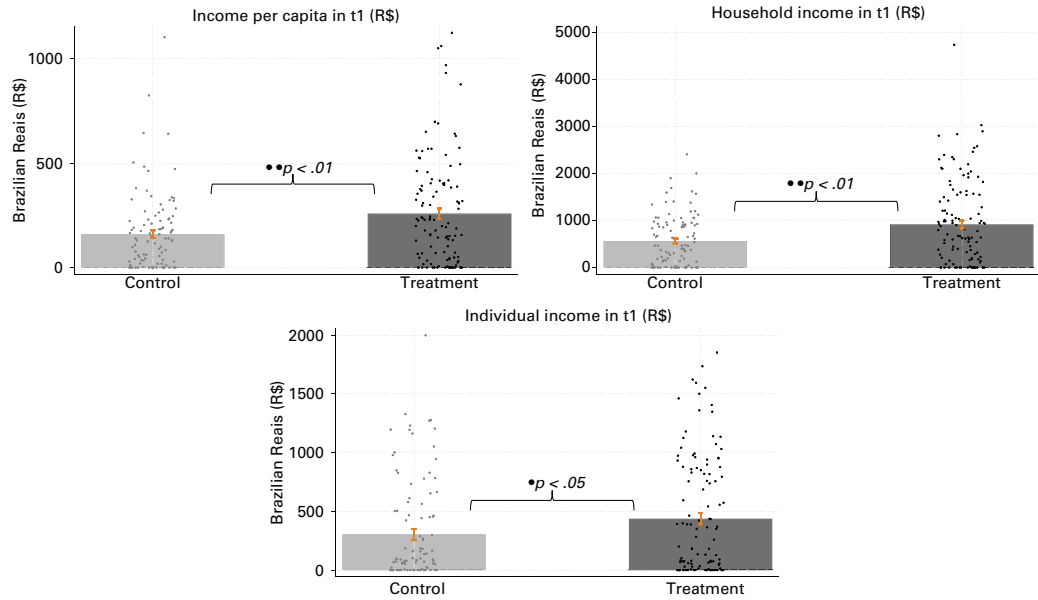
• $p < .05$; •• $p < .01$.
* The y-axes of the graphs are on different scales. The plots use raw data, and *p*-values consider the fully controlled models. Data points are intensely jittered to enable better visualization of the data distribution.

Table 6. Treatment Effect on Socio-Psychological Aspects of Participants*

DVs =	(1) Self-efficacy in t1	(2) Self-efficacy in t1	(3) Optimism in t1	(4) Optimism in t1	(5) Favela stigma in t1	(6) Favela stigma in t1
Treatment	0.604••	0.524•	0.264	0.409•	1.018••	0.742•
	(0.226)	(0.217)	(0.197)	(0.190)	(0.381)	(0.369)
Constant	6.882••	2.619	8.993••	2.599	6.758••	5.389•
	(0.160)	(2.011)	(0.151)	(1.706)	(0.296)	(2.711)
Controls	N	Y	N	Y	N	Y
DV average (in score)	7.214		9.138		7.319	
Observations	207	207	207	207	207	207
R-squared	0.033	0.285	0.009	0.288	0.034	0.326

• $p < .05$; •• $p < .01$.
* Controls include randomization strata fixed effects, training period fixed effects, demographic characteristics, big five personality traits, and outcome values at the baseline (survey t_0). Social-psychological variables are measured on a scale ranging from 1 to 10 points (see Online Appendix 3). The calculation of the treatment effect sizes among treated follows the formula $EffectSize^T = [\beta^T / AvgDV_0^T]$. Robust standard errors are in parentheses.

It is complex to describe the social significance of the favela stigma result as this would represent an attempt to measure the extent to which an emotion affects an individual's social-psychological state. To provide a refined

Figure 4. Socio-Psychological Outcomes After the Treatment*

* The y-axes of the graphs are on similar scales. The plots use raw data, and *p*-values consider the fully controlled models. Data points are intensely jittered to enable a better visualization of the data distribution.

approximation, consider that before a certain educational program, you were aware of ten individuals known to harbor prejudices against favela residents. After the program ends, you encounter an additional individual who displays bias against you due to your residence in a favela. Suppose this individual is a potential employer assessing you during a recruitment process or a supplier from whom you intend to procure products for your newly launched business venture. What emotional response would this encounter elicit in you? What would be the social significance of it?

DO INCOME GAINS AND FAVELA STIGMA GO HAND IN HAND? UNVEILING THE ALLEGORY OF THE FAVELA

The results of this study's analysis of favela stigma run opposite to what was predicted in pre-registration, which itself can be considered a contribution (it was predicted that the treatment would lead to reduction in the favela stigma). Two competing explanations exist for this unexpected result. The first explanation for the increase in individuals' experience of stigma is that the training program was biased or discriminatory against the participants. If so, the treatment itself would have generated negative emotions and experiences of stigmatization among participants. An alternative explanation for this increased experience of stigma is that after participating in the program, many individuals searched for employment outside the favela. Through heightened interactions with peers outside the favela, these individuals faced prejudice more intensely.

To test for these alternative competing explanations and to comprehensively understand the mechanisms behind the surprising result regarding favela stigma, I departed from the pre-registered study description and performed a sequence of additional tests in order to generate a theoretical contribution through abduction. Thus, I conducted the following non-preregistered additional empirical exercises: (i) computational text analysis of participants' feedback during the training program, (ii) a set of quantile regression models evaluating the correlation between the favela stigma scores and income gains at different moments of the income distribution, and (iii) triangulation of the quantitative results with qualitative evidence from field visits and interviews with participants. This section also unveils the ''allegory of the favela,'' which informs the study's theoretical implications.

Computational Text Analysis

Computational text analysis of participants' feedback during the training program reveals non-significant correlations between participants' positive or negative emotions during the program (at the end of Phase One) and their experiences of favela stigma after the program ended (survey t_1) (see Online Appendix 6 for more details). In other words, these results suggest that the training program did not trigger the stigmatization experiences among participants. However, this evidence does not corroborate the above-noted second explanation as most adequate.

Quantile Regressions

To properly evaluate which of the two explanations is the best conjecture for the increased favela stigma at the program's end, I used a quantile regression model (Koenker and Bassett, 1978; Abrevaya, 2001; Koenker and Hallock, 2001; Firpo, 2007). This estimation examined how the favela stigma's relationship with income changes conditional on which quantile of the income distribution individuals are in at the endline (survey t_1).[17] This analysis helps to shed light on the discrimination mechanism given that individuals with higher socioeconomic mobility (i.e., in higher quantiles of the income distribution) tend to interact more frequently with relatively higher-status individuals (employers and suppliers) beyond the favela boundaries.

This estimation approach has several advantages. A quantile regression model enables assessment of the strength in the correlation between these two variables on various segments of the income distribution, i.e., when comparing lower and higher levels of socioeconomic mobility. Unlike an OLS analysis, this approach offers more-comprehensive understanding of the relationship between two variables because it does not presume a constant effect throughout all parts of the dependent variable distribution. Therefore, it properly allows evaluation of whether income and stigma go hand in hand, according to the strength of their association at different income levels.

Moreover, quantile regressions are an attractive alternative estimation when OLS models are not the best linear unbiased estimator, i.e., when OLS models

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[17] Note that the stigmatization process does not vary according to participation in the training program, so it is expected to affect both the treated and control groups comparably. Thus, these analyses included the entire sample of participants.

suffer from non-normality of residuals and heteroscedasticity. This is precisely the case in this study (see Online Appendix 7 for more details). Following Koenker and Bassett (1978: 38), the quantile regression minimizes the following equation with respect to β:

$$\min_{\beta \in \mathbb{R}^K} \left[\sum_{i \in \{Income_i \geq Favela'_i \beta + X'_i \gamma\}}^{N} q \left| Income_i - Favela'_i \beta - X'_i \gamma \right| + \sum_{i \in \{Income_i < Favela'_i \beta + X'_i \gamma\}}^{N} (1-q) \left| Income_i - Favela'_i \beta - X'_i \gamma \right| \right]$$

where $Income_i$ is the dependent variable of the model measuring the income per capita of the individual i at the endline (survey t_1); $Favela_i$ is the explanatory variable favela stigma of the individual i at the endline (survey t_1); X_i is a vector of control variables including job allocation at the endline (t_1), income level at the baseline (t_0), and demographic characteristics at the baseline (t_0); and q is the estimated quantile. The main models consider each income decile ($q10 \leq q \leq q90$).

The models were estimated both with the favela stigma variable and with its factors isolated, i.e., the two statements composing the favela stigma construct. Therefore, in Table 7 and Figure 5, Panel (A) evaluates the relationship between favela stigma in t_1 and income per capita in t_1; Panel (B) analyzes this relationship using favela stigma's first factor, which states, ''I often suffer from prejudice because I live in a favela'' in t_1; and Panel (C) considers this relationship using favela stigma's second factor, which states, ''I believe I have a harder time getting a job because I live in a favela'' in t_1. These individual factors help to determine whether income gains and favela stigma go hand in hand because of participants' experiences of prejudice (Panel B) or participants' difficulties in successfully finding work (Panel C).

Table 7, Panel (A) demonstrates that the favela stigma in t_1 significantly differs among lower- versus higher-income deciles. From the tenth income decile ($q10$) to the median income decile ($q50$), the favela stigma is approximately zero or slightly negative and non-significant. By the sixth income decile ($q60$), the favela stigma coefficient is 3.22 but still non-significant. At the seventh income decile ($q70$), the favela stigma coefficient is 8.87 ($p < 0.1$), and by the eighth income decile ($q80$) the coefficient is 13.41 ($p < 0.1$). Finally, the highest income decile ($q90$) has a coefficient of 21.63 ($p < 0.05$), which is approximately 13 times higher than Panel (A)'s OLS coefficient. Figure 5, Panel (A) graphically illustrates these results.

The results become even more striking when the favela stigma construct is decomposed along its factors. Comparing Table 7, Panels (B) and (C), we observe that both indicators increase in magnitude as we move to higher-income deciles. However, only Panel (B), which measures experiences of prejudice more directly, reveals significant increases with higher magnitudes. Specifically, Panel (B) shows that at the seventh income decile ($q70$) the coeficient equals 7.38 ($p < 0.1$), at the eighth income decile ($q80$) the coefficient is 10.66 ($p < 0.1$), and at the highest income decile ($q90$) the coefficient equals 23.54 ($p < 0.05$), which is approximately six times higher than
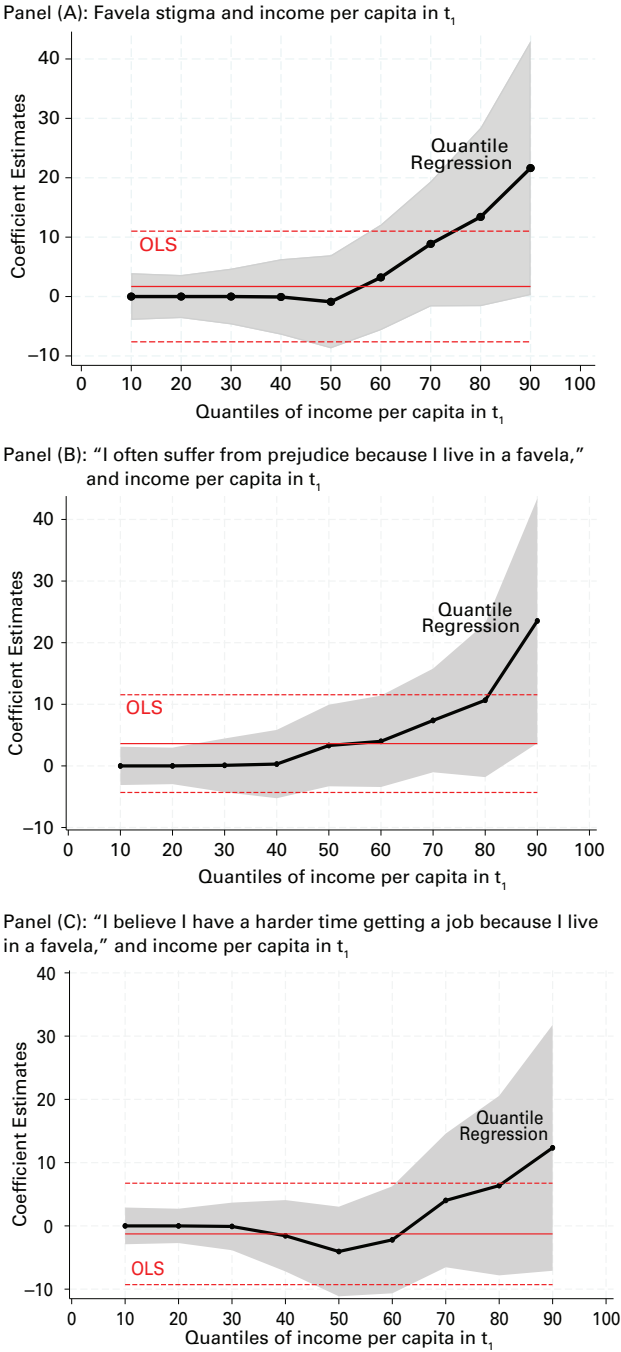
Table 7. OLS and Quantile Regression Between Favela Stigma in t_1 and Income Per Capita in t_1 (R$)*

DV =	(0) OLS	(1) q10	(2) q20	(3) q30	(4) q40	(5) q50	(6) q60	(7) q70	(8) q80	(9) q90
					Income Per Capita in t_1 (R$)					
Panel (A): Favela stigma in t_1	1.693	0	-0.00122	-0.00181	-0.0690	-0.884	3.223	8.872[+]	13.41[+]	21.63•
	(4.641)	(1.995)	(1.837)	(2.381)	(3.223)	(3.975)	(4.508)	(5.342)	(7.621)	(10.84)
Constant	175.6[+]	0	2.387	18.72	42.33	101.0	117.5	138.8	148.8	234.4
	(94.64)	(51.16)	(54.05)	(64.44)	(78.39)	(90.32)	(99.42)	(118.3)	(169.3)	(247.5)
Panel (B): "I often suffer from prejudice because I live in a favela" in t_1	3.622	0	0.00405	0.0928	0.298	3.314	3.991	7.378[+]	10.66[+]	23.54•
	(4.087)	(1.575)	(1.502)	(2.210)	(2.810)	(3.356)	(3.759)	(4.265)	(6.324)	(10.07)
Constant	166.8[+]	0	2.030	16.73	35.86	76.68	120.8	157.3	204.5	162.2
	(94.24)	(50.03)	(59.28)	(73.13)	(86.68)	(95.04)	(104.7)	(120.7)	(165.7)	(255.0)
Panel (C): "I believe I have a harder time getting a job because I live in a favela" in t_1	-1.267	0	0	-0.0848	-1.560	-4.049	-2.204	4.025	6.371	12.35
	(3.904)	(1.471)	(1.375)	(1.908)	(2.858)	(3.590)	(4.287)	(5.356)	(7.195)	(9.855)
Constant	191.8•	0	0	19.84	61.00	117.1	143.4	134.2	169.6	245.4
	(95.99)	(53.10)	(56.84)	(74.69)	(83.48)	(93.40)	(107.0)	(128.6)	(186.3)	(245.4)
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	207	207	207	207	207	207	207	207	207	207
Bootstrap replications	N/A	500	500	500	500	500	500	500	500	500
Descriptive statistics										
DV average by quantile (in R$)	R$214	R$ 0	R$ 0	R$45	R$97	R$160	R$216	R$297	R$422	R$733
Job allocation by quantile in t_1										
No income, no job (in %)	49.8%	100.0%	100.0%	62.5%	65.2%	26.1%	37.5%	31.8%	8.0%	4.8%
Informal job (in %)	30.4%	0.0%	0.0%	37.5%	26.1%	56.5%	54.2%	36.4%	36.0%	38.1%
Formal job (in %)	13.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.1%	22.8%	44.0%	42.9%
Entrepreneur (in %)	6.8%	0.0%	0.0%	0.0%	8.7%	17.4%	0.0%	9.1%	12.0%	14.3%
Number of individuals in each quantile	207	27	26	16	23	23	24	22	25	21

[+] $p < .10$; • $p < .05$.

* Controls include individual-level characteristics, namely job allocation at the endline (survey t_1), income level at the baseline (survey t_0), and demographic characteristics at the baseline (survey t_0). Socio-psychological and personality trait baseline controls were not included to avoid bad-control issues, given their strong intertemporal correlation. Models were estimated using 500 bootstrap replications. The first and second quantiles represent all individuals with zero income per capita in t_1 (53 in total). Panel (C) had 206 respondents at the endline (survey t_1). In the lower part of the table, darker shades indicate higher values. Robust standard errors are in parentheses.

Figure 5. OLS and Quantile Regression Between Favela Stigma in t_1 and Income Per Capita in t_1 (R\$)*

Panel (A): Favela stigma and income per capita in t_1

Panel (B): "I often suffer from prejudice because I live in a favela," and income per capita in t_1

Panel (C): "I believe I have a harder time getting a job because I live in a favela," and income per capita in t_1

* Point estimates (solid lines) and 95% C.I. (shaded area and dashed lines). OLS is represented with thin lines, quantile regression is represented with thick lines and shaded areas. All results are controlled by individual-level characteristics, namely job allocation at the endline (survey t_1), income level at the baseline (survey t_0), and demographic characteristics at the baseline (survey t_0). Given their strong intertemporal correlation, the socio-psychological and personality trait baseline controls were not included to avoid bad-control issues. Panel (C) had 206 respondents at the endline (survey t_1). Models estimated using 500 bootstrap replications.

Panel (B)'s OLS coefficient. Figure 5, Panels (B) and (C) graphically illustrate these results.

Note that in higher-income deciles, individuals perform jobs that usually involve more interactions with peers from outside favelas. At the bottom of Table 7, we see that at the seventh decile ($q70$), 31.9 percent of individuals are either formally employed (22.8 percent) or developing small enterprises (9.1 percent). At the eighth income decile ($q80$) this joint percentage increases to 56.0 percent, and at the highest income decile ($q90$) this joint proportion equals 57.2 percent, the highest amount among all income deciles. In other words, the higher the socioeconomic mobility (higher quantiles), the more likely individuals are to interact with external peers of relatively higher status.[18] These results support the second explanation, suggesting that individuals going outside favelas for employment opportunities or searching for suppliers have higher income levels, and as income levels increase, participants' experiences of discrimination also increase.

Qualitative Evidence

Qualitative data were collected in July 2019, seven months after the program ended, through 14 semi-structured recorded interviews with participants and community leaders, as well as meetings and roundtables with enumerators, CRAS employees, Rio de Janeiro Social Assistance and Human Rights social workers and administration, and Banco da Providência managers. In total, the data amount to 764 minutes of audio recordings. The informants were selected by convenience (Strauss and Corbin, 1990). These interviews, meetings, and roundtables were complemented by two field visits, one in each favela in the sample, during which I visited some of the local enterprises developed by participants.

Data triangulation between the previous quantitative results and qualitative information validates the second explanation regarding the multifaceted nature of socioeconomic mobility. Informants provided several illustrative cases and descriptions supporting this explanation. One enumerator mentioned the case of a participant in the final phases of a formal employment recruitment process, at which point her application was declined after she notified the potential employer of her address inside Cidade de Deus. Similarly, during one of the field visits, an entrepreneur from Pavuna reported having difficulties purchasing and receiving supplies when describing the location of her store inside the favela, a phenomenon that I documented in earlier research (Pongeluppe, 2022). Several informants described the process of searching for employment and suppliers outside favelas as ''getting out of the ghetto.'' During an interview, when asked directly about possible prejudice toward favela dwellers, another entrepreneur stated,

> Almost nobody wants to invest in a community [favela], nobody looks at a community [favela] and says ''wow, good things will arise from there.'' (Program participant and entrepreneur from Cidade de Deus, personal interview)

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[18] Interestingly, the favela stigma is the only stigma that significantly soared with income gains. Online Appendix 8, Panel (B) shows the results of quantile regressions on alternative types of stigma, namely poverty, gender, and race. None of these alternative stigmas significantly change for higher-income quantiles.

Finally, a community leader from Cidade de Deus gave her interpretation of why the prejudice against favela populations is so fierce:

> The violence hinders [favela residents] a lot, so when people say they feel discriminated against because they live in the favela, it is precisely for this reason. When you say: ''I live in a favela,'' people already look down on you. Because automatically, those living in a favela are either drug dealers or gang members. To people from outside the favela, there is no possibility that you are just a resident. However, the truth is that we coexist but do not collude [with the gangs]. We can't do anything to change this situation except to resist. (Community leader from Cidade de Deus, personal interview)

The Allegory of the Favela

An ''allegory'' is frequently described as a story within a story. Inspired by Plato's allegory of the cave in *The Republic* (Bloom, 1968), the allegory of the favela provides a symbolic narrative of the manifest and latent experiences of favela dwellers as they navigate societal perceptions in their quest for socioeconomic mobility. Initially, individuals residing within these stigmatized neighborhoods lacked external social references, leaving them largely unaware of the extent of their devalued societal status and the depth of their stigmatization. Their interactions, primarily confined to the favela, established a sociopsychological comfort zone while restricting opportunities for broader educational and socioeconomic advancement.

Post-training, these individuals emerged equipped with enhanced skills, ready to enter the labor force. However, due to their devalued social status, their endeavors often exposed them to the harsh realities of societal prejudice, juxtaposing the gains of economic mobility against the challenges of discrimination. This bittersweet process forced many individuals to pivot toward entrepreneurship within their communities. This shift can foster their economic progress and position them as agents of change, potentially liberating others within their community from economic disenfranchisement while partially safeguarding them from external discriminatory systems.

The allegory of the favela shows that socioeconomic mobility can change how much individuals earn but not where they come from or how society reacts to them. Similar to the ancient allegory of Plato (Bloom, 1968), the process experienced when getting out of the favela is generalizable to other contexts and situations. Examples include immigrants and ethnic groups who are economically integrated but discriminated against due to their country of origin or religion (Marinoni, 2023), first-generation students and first-generation white-collar workers who develop their academic and labor skills but are underappreciated due to their working-class backgrounds and lack of ''pedigree'' (Rivera, 2015: 3), and gender and racial minorities who advance in their professional careers but are deemed unfit and unable (Smith, 2005; Kanter, 2008). The allegory of the favela symbolically illustrates and generalizes the multifaceted process of socioeconomic mobility experienced by individuals of devalued social status, who, despite prospering economically, realize through their inclusion in social systems that those systems are biased and discriminatory against them (see Online Appendix 9 for more details).

IMPLICATIONS FOR THEORY AND PRACTICE

The findings of this study have several implications for the literature on labor market bias and discrimination and on social comparison, inclusion, and belonging.

Labor Market Bias and Discrimination

In the same vein as Hwang (2022) and Hwang and Phillips (2023), this study demonstrates that entrepreneurship can be a potential career path for individuals facing labor market discrimination. Although the individuals in this study did not have criminal records, their place of residence in the favelas affected their likelihood of entering formal labor markets in a way similar to the experiences of formerly incarcerated individuals (Hwang, 2022; Hwang and Phillips, 2023). In other words, discrimination from potential employers, who classified favela dwellers as potentially dangerous and associated with criminal activities, hurt these individuals' chances of entering the labor market as formal employees, whose jobs usually represent more-stable, less-risky, and higher-paid careers. The results reinforce the idea that necessity entrepreneurship (Dencker et al., 2021) can be an appealing alternative career path given its effects on socioeconomic mobility. Disenfranchised individuals might considerably increase their income by engaging in these small-scale enterprises. Also, these endeavors might contribute to the economic development of stigmatized neighborhoods (Ropert and Di Masso, 2021; Born, 2023), fostering market activities in communities that have traditionally been underserved by conventional firms (Pongeluppe, 2022).

The findings in this study are also theoretically significant for the literature by highlighting the concept of location-based discrimination, an often-understudied type of discrimination. Studies in labor market discrimination traditionally have focused on individuals' observable characteristics, such as gender and race, when evaluating bias and discrimination in hiring and promotion practices (Goldin and Rouse, 2000; Kang et al., 2016; He, Kang, and Lacetera, 2021; Rider et al., 2023). In line with studies that consider non-observable characteristics such as social class (Rivera, 2015) and ethnic background (Marinoni, 2023) as potential sources of labor market discrimination, this study enlarges this set of possibilities by considering another non-observable characteristic: place of residence. To my knowledge, this is the first study to experimentally evaluate the effect of individuals' place of residence within segregated and stigmatized neighborhoods (Massey and Denton, 1988; Charles, 2003; Ropert and Di Masso, 2021; Born, 2023) on their likelihood of entering the labor market.

This study's findings are also theoretically significant for the literature on training programs dedicated to disenfranchised people. Training programs have been extensively deemed one of the best tools to promote socioeconomic mobility. By intervening on the supply side of the market, training programs provide individuals with knowledge and skills that enable them to improve their economic conditions (Attanasio, Kugler, and Meghir, 2011; de Mel, McKenzie, and Woodruff, 2012; McKenzie and Woodruff, 2014; Bulte, Lensink, and Vu, 2017; Carlson and Hager, 2021). This study confirms the adequacy of training programs to promote economic gains for disenfranchised individuals, but these interventions do not affect the demand side of the market, i.e., how societal

systems will receive and include the newly trained individuals. The study shows that socio-psychological outcomes might be mixed. Therefore, interventions focused on changing the supply-side behavior might more effectively promote liberations (Freire, 1970) if such interventions are informed by more-extensive consideration of demand-side perceptions and responses.

Social Comparison, Inclusion, and Belonging

The study also has interesting implications for the social comparison literature by reinforcing well-known mechanisms described in this literature. Individuals change their social comparison set by interacting with higher-status peers (Merton, 1968; Frank, 1985). In changing their reference group for comparison, individuals (and organizations) are more aware of their relative social position and status and update their evaluation of themselves based on their peers' perceptions (Tan and Rider, 2023). This literature has conventionally focused on high-end settings such as Nobel laureates and immediately close peers (Merton, 1968), law firms and legal services (Phillips and Zuckerman, 2001; Rider and Tan, 2015; Tan and Rider, 2017), venture capital funds and financial advice (Phillips and Zuckerman, 2001; Rider, 2009), and private colleges and universities (Askin and Bothner, 2016). Thus, this article also contributes by presenting evidence that similar mechanisms operate in a low-end setting. As Freire (1970: 63) suggested, ''Self-depreciation is another characteristic of the oppressed, which derives from their internalization of the opinion the oppressors hold of them.''

This study's findings are of interest to the literature on inclusion and feelings of belonging. Similarly to Turco (2010), this study shows that discriminatory practices are subject to local cultural context and contingencies. As described in the quote above by the Cidade de Deus community leader, violence associated with favelas—which is stereotypically pictured even in movies—hinders favela populations' opportunities for development. This process happens in two ways. First, this association neglects favela dwellers' access to educational, cultural, and financial support systems. Second, among potential employers and suppliers outside favelas, it fosters discriminatory views about these populations and their communities. This cultural context of prejudice against favelas limits people's entry into the labor market, causes socio-psychological distress due to their stigmatization, and may foster ''impostor syndrome'' feelings among them (Tewfik, 2022: 1011). As Kanter (2008: 9) described, ''The loss of a job, or the right to pursue a profession, or the opportunity to continue one's education, may be far more hurtful than a term in jail.''

Finally, this study demonstrates the multifaceted nature of disenfranchised people's socioeconomic mobility. By showing that income gains go hand in hand with stigmatization processes, the study unveils the complexities of socioeconomic mobility, which may cause both pleasure in terms of economic gains and pain due to socio-psychological distress. These findings are consistent with Merton's (1957) arguments, according to which social phenomena may have intended (or manifest) and unintended (or latent) consequences, and scholars must consider both aspects. The allegory of the favela summarizes how this bittersweet process occurs and suggests its potential generalizability to individuals experiencing socioeconomic mobility in other contexts.

Practical Implications

In addition to the theoretical contributions described, the results of this study also have two salient practical implications. First, the study presents evidence on the importance of including a new set of outcomes in evaluations about the effectiveness of training programs and policies dedicated to disenfranchised individuals. This is of utmost importance since economic gains, which are prevalently measured, might mask socio-psychological distress, which is rarely measured and has consequences for individuals' mental health and well-being. A broader evaluation of these programs that considers individuals more holistically, not only in terms of their economic production, would be beneficial.

Second, the results have essential implications for entrepreneurship among disenfranchised individuals. This career path seems to be a viable option for these individuals, as it provides further opportunities for social integration beyond the formal labor market. Moreover, these activities enable them to have an impact on their communities, helping their local peers and, consequently, their neighborhoods to develop. Entrepreneurship brings these individuals an additional sense of responsibility and fosters market and economic development in areas that the public and private sectors often neglect. Policies aiming to lower the cost of capital or even develop specific credit lines and funding for individuals living in segregated and stigmatized neighborhoods might be an opportune way forward. Such actions may contribute to the survival and scale of local enterprises and, by extension, to the economy in their communities.

LIMITATIONS, ROBUSTNESS CHECKS, AND FUTURE RESEARCH

Several limitations deserve to be mentioned. First, as is conventionally known, generalizability is an issue with field experiments. Particularly in this case, as the randomization was performed among people interested in a spot in the training program, the results are generalizable to the extent that individuals are interested in receiving training to enter the labor force. This limitation is of theoretical importance since it is unclear whether the patterns observed in this group would be similarly present in other favela dwellers who are not actively looking for labor market opportunities or among residents of other segregated and stereotyped neighborhoods that are not Brazilian favelas.

Second, the small sample size (207 individuals) prevents additional investigations of heterogeneous treatment effects conditional on participants' job type allocations at the endline (survey t_1). Despite efforts to address these issues (see Online Appendices 10 and 11), the lack of statistical power in the test makes the heterogeneous treatment effect results suggestive rather than definitive. Despite acknowledging it as a limitation, I believe this feature offers an opportunity for future studies to investigate how different job allocations affect economic and socio-psychological outcomes, particularly stigmatization.

Third, the limited sample size might also affect the quantile regression results. Despite using bootstrap techniques to estimate the relationship between income and stigma on each decile, the constrained set of individuals present in each decile (on average 23 individuals per decile) might raise concerns. Aiming to address this limitation, Online Appendix 12 presents an

additional set of quantile regression tests using quartiles (i.e., *q25*, *q50*, and *q75*), which have, on average, a more extensive set of individuals per block (on average 69 individuals per quartile). Despite slight changes in significance and magnitude, the pattern of results holds. This result is of conceptual importance to validate that income and experiences of prejudice go hand in hand as upward socioeconomic mobility occurs.

Finally, the measures of stigma and discrimination were collected through a self-reported questionnaire (Online Appendix 3). Therefore, they may represent real or perceived discrimination. While acknowledging this as a limitation, I point out that the favela stigma is the only stigma type that significantly increased after the treatment (see Online Appendix 4). Similarly, the favela stigma is the only type that significantly soared among higher-income deciles (see Online Appendix 8, Panel B). These results potentially serve as falsification tests since the actual or perceived instances of discrimination appear to have impacted exclusively the residential domain and not others. These results are also conceptually important since they suggest that in this context, non-observable status characteristics, such as residence location, seem to be a more salient source of labor market discrimination than are more easily observable characteristics, such as gender or race.

Future Research

This study presents opportunities for theoretical and empirical developments in future research. First, to the best of my knowledge this is the first study to consider both economic and socio-psychological measures of well-being among disenfranchised people living in stigmatized neighborhoods, testing the effects of an intervention on both sets of outcomes. This practice should be incorporated into future scientific studies and public policies to help stakeholders holistically comprehend the consequences of similar interventions. Moreover, this study presents the possibility of considering other socio-psychological outcome measures not measured here, such as creativity, faith, resilience, sturdiness, and happiness.

Second, future research may build on this study by aiming to fully engage and understand how and why entrepreneurship might be a good career option for disenfranchised individuals, such as favela populations, formerly incarcerated people (Hwang and Phillips, 2023), immigrants and ethnic minorities (Marinoni, 2023), and women in specific institutional settings (Thébaud, 2015), among others. Moreover, future research could directly explore how entrepreneurship as a career choice might contribute to socioeconomic development in stigmatized neighborhoods. Similarly, future studies could investigate to what extent public interventions might spark entrepreneurial activities in disenfranchised communities, consequently contributing to their development.

Finally, and perhaps most important, this study highlights the advantages of using abduction as a methodological approach in theory development, particularly in the context of pre-registered randomized field experiments. While pre-registration promotes transparency and accountability, it also potentially limits further exploration and theorization of unexpected findings. This study offers insights for researchers grappling with these constraints. It demonstrates how abduction allows for adherence to pre-registration protocols—maintaining the

integrity of the original description, data, and tests—while also facilitating the investigation of unanticipated findings and intricate relationships. Future research on the complex dimensions of socioeconomic mobility could benefit from this approach. For example, a study might pre-register a hypothesis that increased socioeconomic mobility is associated with higher socio-psychological distress. Through abduction, researchers could explore underlying reasons for this coexistence, identify behaviors indicative of socio-psychological unrest, understand their manifestations, and examine factors that may mitigate or exacerbate this relationship.

Conclusion

Investigating the causal effect of a training program on the economic and socio-psychological outcomes of disenfranchised individuals subject to residential segregation, this article engages an essential topic for the achievement of the United Nations Sustainable Development Goals, specifically the goals related to poverty eradication (SDG #1), inequality reduction (SDG #10), and sustainable cities and communities (SDG #11). The study calls for change in societal mindset when programs and policies attempt to include disenfranchised individuals in socioeconomic systems. It advocates for a more holistic and humane approach that adequately supports individuals' liberation (Freire, 1970), allowing them to achieve their full potential and moving us closer to a society in which prosperity is an attainable reality for all.

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