Cash transfer and labor supply: effects of a large-scale emergency program in Brazil

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ABSTRACT

This study assesses the short-term labor supply effects of Brazil's *Auxílio Emergencial* program, which distributed cash transfers equal to 66% of the median household income to over 70 million individuals. Utilizing the staggered rollout of the program, we analyze its impact on beneficiaries' labor supply. Our findings reveal no overall change in labor supply; however, a disaggregated analysis shows significant offsetting effects among different groups. Individuals previously inactive in the labor market began to seek employment, predominantly as self-employed and in the informal sector, shortly after receiving the transfer. Conversely, those already employed tended to reduce their labor input. The effects were particularly pronounced among women, non-whites, and residents of Brazil's North and Northeast regions. These results underscore the importance of considering heterogeneous dynamics in assessing the impact of cash transfer programs on labor supply, which often appear negligible in broader analyses.

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1.Introduction

Before the COVID-19 pandemic, most cash transfer programs were designed to balance social assistance while limiting unintended behavioral effects, particularly labor supply disincentives (Banerjee et al., 2017). However, during the pandemic, these programs saw an unprecedented expansion, reaching one-sixth of the world's population— approximately 1.36 billion individuals. Additionally, the profile of beneficiaries extended well beyond the traditional groups of vulnerable recipients (Gentilini, 2022). The consequences of this historical scale up are not yet fully understood, particularly with respect to its unintended labor supply impacts.

In the canonical labor market model, a cash transfer reduces labor supply as the additional income boosts the individuals' demand for leisure(Becker, 1965). Empirical evidence supporting this outcome initially emerged from the first income transfer programs in the wealthiest countries. Developed countries began implementing social welfare programs that included income transfers in the late 1960s and early 1970s, particularly with the American experiments of the Negative Income Tax (NIT) program. Under the NIT scheme, a set cash transfer was guaranteed as a basic income. For every dollar earned, the transfer was reduced by a tax, diminishing the aid as income rose (Hum; Simpson, 1993; Marinescu, 2018). The first randomized control trial occurred in urban New Jersey and Pennsylvania from 1968 to 1972, involving 1,216 people. These were followed by experiments in rural Iowa and North Carolina (1970-1972, involving 809 participants), Gary, Indiana (1971-1974, with 1,799 participants), and a more extensive experiment in Seattle, Washington (1970-1978, involving 4,800 participants), later expanded to Denver, Colorado, which is also known as SIME/DIME experiment (Marinescu, 2018). Despite criticisms related to the friction collected in the sample (Hausman; Wise, 1979; Ashenfelter; Plant, 1990), studies at the time indicated that these experiments encouraged individuals to offer less labor and work fewer hours, but with estimates consistently significant only for SIME/DIME, which was the most generous of the experiments (Robins, 1985).

However, the canonical model predictions may not hold in settings where individuals face involuntary unemployment due to credit constraints. In these cases, cash transfers may positively impact labor supply by allowing workers to improve their productivity (Dasgupta; Ray, 1986), invest on their own businesses and finance riskier investments (Baird; McKenzie; Özler, 2018). Moreover, spillover effects can occur, so higher incomes in poorer regions can boost sales and, consequently, increase employment (Gerard; Naritomi; Silva, 2021). Recent empirical literature mostly supports these extended model predictions, in contrast to the canonical model. Most evidence suggests that cash transfer programs do not discourage work in most settings and for most segments of the population (Bastagli et al., 2016; Alzúa; Cruces; Ripani, 2013; Banerjee et al., 2017). However, this evidence is based on pre-COVID-19 cash transfer programs, which were narrower and typically targeted very specific vulnerable segments of society.

This study contributes to this debate by examining the short-term effects on labor supply of a large-scale direct transfer program aimed at mitigating the economic impacts of COVID-19 in Brazil: the *Auxílio Emergencial* (AE) program, which provided social protection for up to 117.5 million Brazilians(Hecksher; Foguel, 2022), more than half of the country's population. Eligible recipients were granted monthly cash payments starting from US$120, which corresponds to approximately 66% of Brazil's median income (Levy; Filho, 2022). In comparison, *Bolsa Família*, the world's largest conditional cash transfer program before AE (Gerard; Naritomi; Silva, 2021) supported, in 2019, approximately 41.8 million beneficiaries with an average benefit of US$36 (Ministério do Desenvolvimento Social e Agrário, 2019). Thus, AE represented an unprecedented expansion of direct transfers in Brazil, reaching more individuals and granting a considerably larger income boost to its beneficiaries.

Not all beneficiaries received the AE transfers immediately. As the program required swift implementation due to the sanitary crisis, many challenges were faced. The most significant was reaching over 29 million eligible individuals not enrolled in *Cadastro Único*, the Brazilian government's social welfare records (Cardoso, 2020). A complex operation was needed to create these records, leading to registration difficulties (Schymura, 2020; Marins et al., 2021). Delays in evaluations, the need for familiarity with smartphones and websites, internet access, lack of support for registration solutions, and the absence of public transparency regarding approvals and rejections were some hurdles faced by individuals to participate in the program (Marins et al., 2021). Furthermore, in the first implementation month, severe inaccuracies in updating records led to the denial of AE for approximately 43 million people (Marins et al., 2021). These factors, combined with the monthly schedule of payments, resulted in a process with a staggered entrance of beneficiaries.

Using data from a large representative national survey (*PnadCovid-19*) that followed more than 190,000 households from May to November 2020, we exploit the staggered entrance of individuals into AE to estimate the causal impact of its cash transfers on labor supply with the framework proposed by Callaway and Sant'Anna (2021). In this method, late adopters are used as the control group to early entrants, as they all meet the program eligibility criteria. This research design allow us to estimate the overall program effect on labor supply in the short run and also to exlplore specific effects for different sub-samples of individuals.

Overall, we found a null average effect of AE on labor supply, a result in line with most of the pre-COVID-19 empirical literature on the topic. However, this null average result hides interesting dynamics occurring in the labor market as a consequence of the cash transfer boost from AE for different subgroups. On one hand, the program led to a considerable reduction in the labor supply of those initially active in the labor market. Conversely, we observed a positive effect among those who were initially inactive. These results add to the literature that finds evidence contrary to the predictions of the canonical labor market model. The AE program appears to have provided sufficient resources, even in the short term, for inactive individuals to start offering labor or enabling individuals to search for better jobs, creating a job-search effect (Baird; McKenzie; Özler, 2018). More specifically, initially inactive individuals predominantly moved into the informal sector or to unemployment, although a significant shift towards formal employment was also noted. In opposition, AE negatively impacted the labor supply for those already in the labor market, indicating that for these individuals already outside the involuntary unemployment trap, the predictions from the canonical model were correct. This effect was notably stronger for those initially searching for a job or who were working in the informal sector.

We also evaluated the heterogeneity of effects on specific demographic groups. The null overall average result was uniform across gender, ethnicity, and region. Nevertheless, with respect to effects by initial employment status, women, non-whites, and residents of Brazil's poorer regions appear to have been more impacted by AE in terms of starting suppling work. Concurrently, actively employed women also exhibited a higher withdrawal from the labor force, possibly due to the increased amount allocated to single mothers. Nonetheless, estimates indicate that it was predominantly the white active population that exited the workforce, which is likely linked to the higher income level of this group and consequently greater reservation wages.

The remainder of this article is structured as follows. In the next section, we present a literature review on the effect of cash transfers on labor supply. Sections three and four describe the context and data utilized in this study. In section five, we detail the methodology employed. Section six outlines the results and robustness checks. Finally, we present our discussion in section seven.

2.Literature review of the effect of cash transfers on labor supply in Brazil

In Brazil, studies on cash transfer programs mainly evaluate PBF, which, like the AE, employs an income means test for eligibility. Identification strategies often hinge on the selection process for PBF recipients, where municipalities are assigned quotas based on poverty maps. Households exceeding the per capita income threshold set by their municipality's quota are not chosen. Since selection odds vary by municipality, identical households in different locations can have different statuses on program participation. Brauw et al. (2015) analyzed PBF's effects on household labor supply, finding no overall significant impact. However, a shift from formal to informal employment was noted, interpreted as a response to income eligibility. Differently, Gerard, Naritomi and Silva (2021) assesses a policy change increasing municipal PBF quotas and identifies a positive impact through multiplier effects on local formal labor markets, predominantly driven by low-skilled jobs. Their results are confirmed by consistently impacting individuals who never participated in the program, larger geographic aggregations (micro-regions vs. municipalities), and local GDP and tax collection. Yet, using individual-level data, they find disincentives for formalization among program beneficiaries, indicating that the multiplier effect is even larger than estimated.

However, the COVID-19 pandemic brought new challenges and highlighted the importance of emergency cash transfer programs. A recent study by Menezes-Filho, Komatsu and Rosa (2021) shows *Auxílio Emergencial* program importance in many dimensions. The authors conducted a descriptive analysis and simulated poverty, extreme poverty rates, inequality indices, and labor market indicators with and without the presence of the AE. They illustrated that poverty was reduced from 12% in 2019 to 9% in May 2020, and without that transfer, poverty would have been around 19% if the agents did not change their behavior. Extreme poverty also saw a substantial decline, from 3.5% in 2019 to values between 1% and 2% in 2020, and without the AE, it would have hovered around 7% and 8%. In both cases, the difference is even higher for minorities, such as black, indigenous, and less educated people. Regarding inequality, their descriptive analysis indicates that the AE may have contributed to a 10% reduction in the Gini index of per capita household income from May to September 2020, under the same assumption that agents would maintain consistent behavior in the absence of the AE. For labor market indicators, their descriptive analysis suggests that the pandemic led to a decrease in labor force participation, which in turn may explain the observed reduction in the unemployment rate.

To the best of our knowledge, two recent studies evaluated labor market responses to the AE. Levy and Filho (2022), estimated the program impact on women's labor supply, finding small but negative effects. Nevertheless, their analysis was limited to the group treated in May 2020, which might introduce contamination issues if later-treated individuals were included in the control group or lead to information loss since groups treated in later months could react differently to the aid. Building on this, Nazareno and Galvao (2023) further estimated the program impact, finding statistically significant but economically insignificant effects on household labor supply. However, the authors applied a dynamic fixed-effects model, which faces potential issues from negative weights due to the multiplicity in treatment timing (Goodman-Bacon, 2021), and can introduce correlation with unobserved heterogeneity over time, potentially leading to inconsistent estimators (Cameron; Trivedi, 2005). Thus, we aim to contribute to this literature by addressing these gaps, utilizing a methodology robust to the multiplicity in treatment timing and including all individuals in the population.

3.Context: The *Auxílio Emergencial* program

As in many developing countries, the COVID-19 pandemic led to humanitarian costs in Brazil[1]. The shock from the sanitary crisis further impacted the country's economy, which had already been experiencing downturns and modest growths since 2014. One of the emergency responses to this situation was the AE launch, which aimed to provide a minimum income for Brazilians in vulnerable situations throughout the COVID-19 pandemic.

The criteria adopted by the government were designed to target the economically vulnerable segments of the population during the COVID-19 pandemic. Eligible individuals were required to be over 18, not engaged in formal or public employment, and without access to other federal social benefits, except for PBF. The income threshold for eligibility was set at a per capita family income below half a minimum wage or a total family income of less than three minimum wages. Furthermore, candidates should not have declared an annual taxable income exceeding

[1] The World Health Organization (WHO) reported approximately 700,000 deaths from COVID-19 in the country by the end of 2022.

R$28,559.70 (about US$5700) in 2018. Additionally, the program was accessible to individuals who were Individual Micro-Entrepreneurs, individual contributors to the General Social Security System, informal workers (whether employed or self-employed) or unemployed, registered in the *Cadastro Único*, or who, through self-declaration, meet the income requirements (Hecksher; Foguel, 2022). The program stipulated a monthly benefit of R$600 (about US$120), equivalent to 66% of median wages in Brazil (Levy; Filho, 2022). This amount was doubled for female heads of single-parent families and limited to two members per family. The initial intention of the program was to last only three months, however, it continued in the following months but with the value reduced by half starting from September 2020.

The benefit was approved at the end of March 2020[2] and implemented in the following months, using pre-existing institutional arrangements from other social programs (Cardoso, 2020). Two groups of beneficiaries were defined in the program (Menezes-Filho; Komatsu; Rosa, 2021). The first group comprised individuals already registered in the *Cadastro Único*, a census-based database for the poorest, managed by the *Ministério da Cidadania*, which also played a key role in administering the AE. For PBF participants, the benefit was automatically replaced if it was more advantageous. Those not enrolled in the PBF had to meet the eligibility criteria, albeit without the need for registration. The second group consisted of those who met the program's criteria but were not registered in the *Cadastro Único*. These individuals had to self-declare to validate their eligibility for the benefit through the app or website of the *Caixa Econômica Federal*[3], which was not automatic. Estimates indicate that about half of the target population belonged to this second group (Cardoso, 2020).

Due to the social costs caused by the COVID-19 pandemic, the program required swift implementation. Less than 20 days passed between its approval and the distribution of the first benefits[4]. However, this urgency resulted in numerous registration difficulties, particularly for those not registered in *Cadastro Único* (Marins et al., 2021), leading to a staggered entry into the program.

Table 1 shows new AE beneficiaries for each type of possible beneficiary for each month available in *Auxílio Emergencial* administrative data. Most of the staggered entries over time occur among individuals who are not registered in the *Cadastro Único*. Therefore, unlike other evaluations of cash transfer programs in Brazil, which mostly assess a poorer segment of the population (Brauw et al., 2015; Gerard; Naritomi; Silva, 2021), we contribute to the literature by investigating the effects of transfers on those with higher income levels.

One aspect of AE is the income mean test, which is different from most Latin American cash transfer programs (Gerard; Naritomi; Silva, 2021). This means that program eligibility criteria are based on declared income. This might lead to more substantial work disincentives (Gerard; Naritomi; Silva, 2021) and also to a substitution effect, where workers shift to informal

2 <https://www12.senado.leg.br/noticias/materias/2020/03/30/coronavirus-senado-aprova-auxilio-emergencial-de-r-600>
3 The largest state-owned bank in Brazil responsible for operating AE and other social benefit payments
4 <https://www.mds.gov.br/webarquivos/sala_de_imprensa/boletins/boletim_bolsa_familia/2020/abril/boletim_BFInforma709.html>

Table 1 – Monthly Entries in AE for Each Type of Beneficiary

Month	AE value	PNAD Covid	PBF	Cadunico (not PBF)	Out of Cadunico
Apr	regular	no	19,221,231	9,284,812	18,137,921
May	regular	yes	96,160	325	9,036,866
June	regular	yes	24,326	427	5,215,061
July	regular	yes	56,668	862	1,079,508
August	regular	yes	32,810	310	1,258,514
September	halved	yes	22,994	0	310,284
October	halved	yes	12,260	166	427,805
November	halved	yes	1,221	112	197,566
December	halved	no	6	474	87,705

The table displays total entries in *Auxílio Emergencial* program by month and type of beneficiary.
Individuals whose payments were blocked, canceled, or returned to the Union were not considered in the calculations.
Source: Auxílio Emergencial Administrative Data.

labor to conceal their actual income (Araujo et al., 2017). For instance, Garganta and Gasparini (2015) analyzed the impact of the Universal Child Allowance program- a monthly cash transfer benefit- on Argentina's formal work. Although there is no effect to transit from formality to informality, their results point to a considerable disincentive to the labor market formalization of the program beneficiaries, especially among self-employed workers, informal salaried employees, and the unemployed, with more substantial effects for poor workers in large households and with children of young age. In Uruguay, Bergolo and Cruces (2021) estimated the impact of the Family Allowance Assistance Program on formal labor force participation using eligibility rules around a poverty score threshold. They find reductions in formal labor force participation among all beneficiaries with higher intensity for single mothers[5].

4.Data and final sample selection

Our main dataset is a special version of the Brazilian government's largest home survey - the Continuous National Household Sample Survey (PNADC)- developed by the Brazilian Institute of Geography and Statistics(IBGE) during 2020, the *PNAD Covid19*. Households interviewed in PNADC first quarter of 2019[6] were followed in a monthly frequency from May to November 2020. As in the case of PNADC, it's reasonable to admit that the sample is big enough to make inference on typical estimations domains. Crossing information with many public datasets, IBGE found at least one phone number by household for about 92% of the original sample (193,662 households).

We first define our informality measure, facing a limitation due to the *PNAD Covid19* lack of information on registered companies or self-employed workers. Following the International

5 See Araujo et al. (2017) for similar results for *Bono Solidario* in Ecuador and Bosch and Campos-Vazquez (2014) for *Seguro Popular* in Mexico.

6 Regular PNADC has a quarterly frequency

Labor Organization (ILO) definition of informality [7], our informality proxy tries to capture workers that are not beneficiaries in government social security systems. So, employees in the private sector without a formal contract, domestic employees without a formal contract, employers or self-employed without social security contributions derived from work, and working family assistants are classified as informal.

Although the unit of analysis is households, we can identify individuals from the PNADC first quarter of 2019 sample in the *PNAD Covid19* data. However, to avoid long-term labor market dynamics, our main models use only *PNAD Covid19* information. We identify individuals with the same approach as Menezes-Filho, Komatsu and Rosa (2021), using the household identifier added to the date of birth and sex. Some problems may arise with this strategy. First, we can't identify people who didn't answer their birth date, representing 6.32% of the sample (202.493 observations). Second, it's not possible to identify same sex twins that live in the same household, as our identifier would assign the same value for both. However, it is a minor problem as twins were only 0.43% of the sample(12.908 observations). The sample is restricted to individuals who were at least 18 years old at some point between May and November 2020 (the minimum age to receive the benefit) and to those who were under 65 years old at the same period, in accordance with retirement rules in Brazil.

To establish our identification strategy, we initially kept in our data only those individuals identified in every period, as attrition makes it difficult to ascertain when a person enters the program or if any particular dynamic happened. This method enabled us to include 136,520 individuals, comprising 58% of our monthly sample. Second, as in our estimates it will be explored with a longitudinal analysis, "always treated" individuals —those who were already receiving the benefit as of May 2020- were excluded[8]. Third, we further exclude individuals who received the benefit but stopped it at some point[9]. We also restrict our analysis until August 2020, given the policy change in September 2020. Our final dataset is a balanced monthly panel data set from May to November 2020 consisting of 68,530 adults who never received or started receiving AE between June to November 2020 and were observed in all months of the *PNAD Covid19* survey[10].

The summary statistics of our final sample are presented in Table 2. Bold values indicate statistically significant differences at the 5% level compared to our early treated group (individuals treated between June and August 2020), shown in column 4. Systematic differences are observed between the never-treated group and the treated groups. The never-treated group shows a higher proportion of individuals active in the labor force, a lower unemployment rate, and a significantly lower proportion of individuals in informal employment. Additionally, this

[7] *"All remunerative work (i.e. both self-employment and wage employment) that is not registered, regulated or protected by existing legal or regulatory frameworks, as well as non-remunerative work undertaken in an income-producing enterprise. Informal workers do not have secure employment contracts, workers' benefits, social protection or workers' representation"*

[8] This step resulted in the removal of 46,095 individuals, constituting around 34% of the identified sample

[9] This exclusion led to the removal of about 21,892 individuals, approximately 16% of the sample.

[10] 3 individuals were dropped due to missing information on ethnicity.

group exhibits higher percentages of white individuals, higher education levels, and greater total income. These differences suggest that the never-treated group may not be an ideal counterfactual for the treated groups due to their more advantageous socioeconomic position. However, our identification strategy relies on parallel trends rather than identical covariates. Importantly, the groups treated at different times appear relatively similar, suggesting that variations in treatment timing are not strongly associated with these observed characteristics.

Table 2 – Descriptive Statistics by group

	(1) June entry	(2) July entry	(3) Aug. entry	(4) June-Aug. entry	(5) Sept or later entry	(6) Never Treated
Active in Labor Force	0.61	0.62	0.63	0.61	0.63	**0.69**
	(0.489)	(0.486)	(0.482)	(0.487)	(0.482)	(0.464)
Unemployed	**0.13**	**0.09**	0.11	0.11	0.12	**0.04**
	(0.332)	(0.292)	(0.317)	(0.318)	(0.323)	(0.200)
Informal	**0.48**	0.43	0.42	0.46	**0.38**	**0.20**
	(0.500)	(0.496)	(0.494)	(0.498)	(0.487)	(0.402)
Woman	0.53	0.53	0.51	0.53	0.53	0.53
	(0.499)	(0.499)	(0.500)	(0.499)	(0.499)	(0.499)
White	**0.40**	0.41	**0.46**	0.41	0.45	**0.55**
	(0.489)	(0.491)	(0.498)	(0.492)	(0.498)	(0.497)
Age	40.12	40.31	40.47	40.23	41.03	**43.33**
	(13.59)	(13.80)	(13.49)	(13.63)	(14.01)	(13.53)
Complete High School or more	**0.53**	0.55	0.57	0.54	0.56	**0.70**
	(0.499)	(0.498)	(0.495)	(0.498)	(0.497)	(0.459)
Householder	0.40	0.39	0.40	0.40	0.40	**0.46**
	(0.490)	(0.488)	(0.490)	(0.489)	(0.490)	(0.498)
Total Income	**1400.24**	**1663.04**	1575.06	1504.04	**1899.48**	**3363.38**
	(1763.6)	(2267.5)	(1910.1)	(1949.0)	(2518.8)	(4184.6)
Number of individuals	5759	3032	1630	10421	2352	55757

The table displays descriptive statistics for the groups in the sample in the baseline period (May 2020).
The group is defined by the month of entry into the program or, in the case of the never-treated group, by not entering.
Bold numbers indicate significant differences at the 5% level or higher related to June to August entry group (column 4).
Source: National Household Sample Survey (PNAD) and PNAD COVID-19.

5.Methodology

Given the sample longitudinal data, a two-way fixed effects (TWFE) specification may be a natural approach to identify the AE effect on labor supply. However, the Difference-in-Differences Decomposition Theorem states that the policy estimator is a weighted average of all pairwise difference-in-differences estimators[11] (Goodman-Bacon, 2021). The weights assigned during this estimation are proportional to the size of each group over time and to the variance of the treatment dummy variable for each group. Notably, this can lead to negative weights, especially when the average treatment effects vary over time (Goodman-Bacon, 2021).

[11] For instance, in a scenario spanning three periods where one group is treated at t=1, another group is treated at t=2, and a third group is never treated, a pairwise comparison of these three groups is conducted.

This problem may be relevant to our case. Individuals might behave differently depending on when and for how long they receive the benefit. An elegant solution to address this issue was proposed by Callaway and Sant'Anna (2021). Their approach allows for estimation and inference on interpretable causal parameters, allowing for treatment effect heterogeneity and dynamic effects. These disaggregated causal parameters are defined as the group-time average treatment effect, representing the average treatment effect for group g at time t, where a group is defined by the first treatment period.

Define G as the first period when an individual first becomes treated. Define G_g as a dummy variable equal to one if the individual is first treated in period g and define C to be a dummy variable equal to one if an individual does not participate in the program in any period. Let $\bar{g} = \max_{i=1,\ldots,\tau} G_i$, that is, the maximum G and let $\mathbb{G} = \sup(G) \setminus \{\bar{g}\}$ be the support of G less $\bar{g}$. Denote $Y_{it}(0)$ individual i untreated potential outcome at time t if they remain untreated every period. Similarly, let $Y_{it}(g)$ denote the potential outcome that individual i would have at time t if they were first treated in time g. The observed and potential outcomes for each individual are then related by

$$Y_{it} = Y_{it}(0) + \sum_{g=2}^{\tau}(Y_{it}(g) - Y_{it}(0)) \cdot G_{i,g} \tag{1}$$

Individuals who never undergo the treatment exhibit potential outcomes corresponding to a lack of treatment across all periods. Conversely, observed outcomes for units that undergo treatment are the unit-specific potential outcomes corresponding to the particular period when that unit adopts the treatment (Callaway; Sant'Anna, 2021). We then use the author's generalization of average treatment effect on treated(ATT) for individuals who are members of a particular group g at a particular time t given by

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1] \tag{2}$$

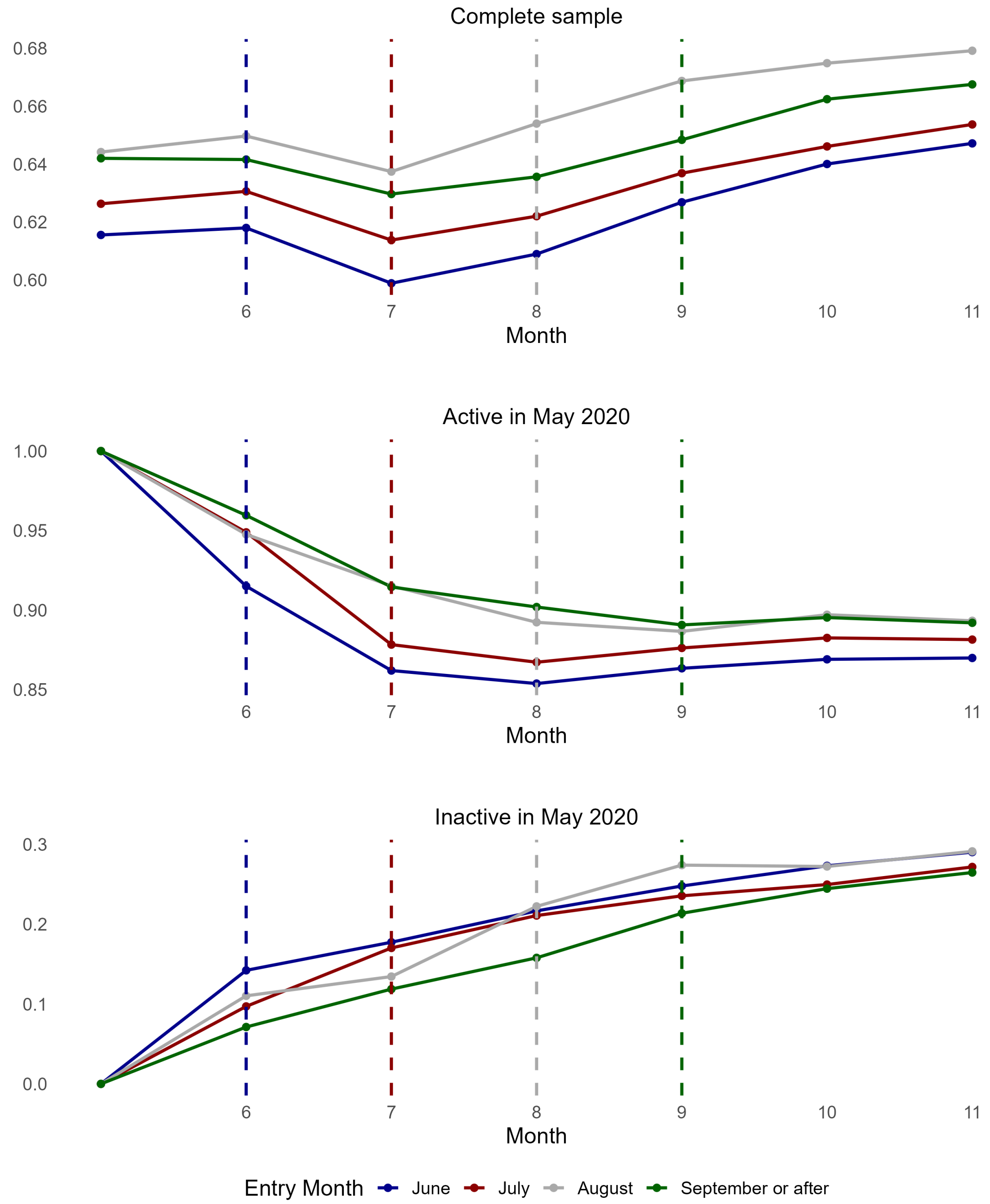
Our main identification hypothesis[12] is similar to the classical parallel trends condition on the classic difference in differences setup. The conditional parallel trends based on "not-yet-treated" groups impose parallel trends between the group g and those that are not yet treated at the respective time of estimation.

Some evidence for the validity of this hypothesis is presented in Figure 1. Despite the limited number of pre-treatment periods—there is only May 2020 for the group entering the program in June, for example—similar trajectories are observed among the groups not yet treated. For the complete sample, the program entry does not seem to have affected individuals' behavior, as the trajectories remain considerably similar even after each month. Considering those initially active, different patterns emerge. Since all were initially active in May 2020, this proportion is equal to 1 in the first period. However, by June, there is already a divergence in the trajectory of the group starting to receive the benefit that month and the same occurs in July,

12 For all identification hypotheses, see the Mathematical Appendix.

indicating a negative effect on the chance of offering work. In contrast, none of those initially inactive were in the labor market during the initial period. There is a greater initial deviation in June for those treated in that period and similarly for the months of July and August. Thus, this indicates that the AE may have had a positive effect on the labor supply for this segment of the population.

Figure 1 – Percentage of active individuals by entry month and initial labor force status.



Notes: In each graph, the lines represent the trajectory of the percentage of individuals active in the labor force by group. From top to bottom, the first graph considers the complete sample, the second only those who were active in May 2020, and the third those who were inactive in May 2020. Data from *PNAD-Covid19*.

5.1 Estimation

With all identification hypotheses fulfilled, the parameters in (2) can be identified using ordinary regression (OR), inverse probability weighting (IPW), or doubly robust (DR) estimations. The OR approach necessitates a correctly specified model for the evaluated outcome. The IPW method requires a model that is correctly specified for the propensity score. The DR estimation combines both methods and only requires that either the model for the outcome or the propensity score be correctly specified, but not necessarily both. Therefore, we conducted our estimations using the DR method, which offers greater robustness compared to OR and IPW (Callaway; Sant'Anna, 2021).

Define $m_{g,t}(X) = \mathbb{E}[Y_t - Y_{g-1}|X, D_t = 0, G_g = 0]$ as the population outcome regressions for the not yet treated group by time t and $p_{g,s}(X) = \Pr(G_g = 1|X, G_g = 1 + (1 - D_s)(1 - G_g) = 1)$ as the generalized propensity score of the probability of participating in the AE for the first time at g or being in the not-yet-treated group at time s , where D_{it} is a dummy treatment variable for individual i in period t. Then, we can express the DR estimator of 2 as

$$\begin{aligned} \text{ATT}_{dr}(g,t) &= \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}}{\mathbb{E}\left[\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}\right]}\right)(Y_t - Y_{g-1} - m_{g,t}(X))\right] \\ &= \text{ATT}(g,t) \end{aligned} \tag{3}$$

Two steps are necessary to calculate these estimates. First, we compute $p_{g,t}(X)$ and $m_{g,t}(X)$. The next step is to use these fitted values in the sample analog of the ATT(g, t) in question. Moreover, with these estimates, it becomes possible to calculate the aggregated effects based on the principle of the sample analogue[13].

In our analysis, the covariate set X is comprised mostly of time-invariant characteristics. This set includes gender, race (white indicator), age, and educational attainment[14], along with indicators for being the household head, residence regions, and total initial period income. It is also worth mentioning that our variable of interest, the receipt of the AE, is reported at the household level. Thus, we avoid having individuals in both control and treatment groups within the same household.

For the robustness of our estimates, we accounted for clustering at the individual level to adjust for within-person correlation over time. Additionally, we employed bootstrapping with 1000 iterations to ensure the reliability of our standard errors.

5.2 Aggregated Parameters

As we have many groups and periods, it's challenging to interpret many average treatment effects represented in equation 2. A notable feature of our methodology is its capacity to gen-

[13] See section 4 in Callaway and Sant'Anna (2021) for more details

[14] Our educational categories are: No schooling or incomplete elementary school, complete elementary school, incomplete high school, complete high school, incomplete graduation, complete graduation, or more.

erate summary parameters and explore various aggregation schemes. These approaches can effectively reveal diverse sources of treatment effect heterogeneity across different groups and periods.

One aggregation of particular interest is investigating how the program's effect varies with the length of exposure to the treatment. Our method allows us to estimate these event-study regressions, avoiding typical problems associated with dynamic TWFE specification (Callaway; Sant'Anna, 2021). Let $e = t - g$ denote the event time. We calculate the average effect of participating in the program e periods after treatment was adopted across all groups that are ever observed to have participated in the program for e periods by [15]

$$\theta_{es}(e) = \sum_{g \in \mathbb{G}} \mathbf{1}\{g + e \leq \tau\} \Pr(G = g | G + e \leq \tau) \text{ATT}(g, g + e) \tag{4}$$

Another parameter of particular interest is aggregating group time average treatment effects into a single mean effect of participating in the treatment. There are several specific ways to calculate this parameter. However, in many of these estimates, we may encounter issues such as disproportionate weights given to groups participating in the treatment for longer periods or composition group issues[16]. We follow the authors' suggestion to compute this parameter, avoiding weights. First, we estimate the average treatment effect for each group g across all their post-treatment periods, given by

$$\theta_{sel}(g) = \frac{1}{\tau - g + 1} \sum_{t=g}^{\tau} \text{ATT}(g, t) \tag{5}$$

Then, the single average effect is calculated by

$$\theta_{sel}^{O} = \sum_{g \in \mathbb{G}} \theta_{sel}(g) \Pr(G = g | G \leq \tau) \tag{6}$$

Hence, θ_{sel}^{O} represents the average effect of participating in the treatment experienced by all individuals who participated in the program. A favorable characteristic of this parameter is the similar interpretation to the ATT in the standard difference in differences framework. This is highly beneficial for summarizing the total impact of treatment involvement, especially in situations involving numerous periods and varying treatment schedules, as in the AE setting.

6.Results

Our main result is illustrated in Figure 2, which shows AE effect on the likelihood of being active[17] in the workforce by length of exposure for the entire sample, as well as segmented by initial labor force status[18]. As previously discussed, our main identification strategy relies on

[15] Note in (3) that the immediate effect of the treatment occurs for $e = 0$.
[16] For more details, see sections 3.1.1 and 3.2 in Callaway and Sant'Anna (2021)
[17] Individuals looking for a job or employed are considered as active in the labor force.
[18] See Table 6 in the appendix for detailed results.

the fact that the AE was a large-scale program requiring swift implementation, which posed challenges for registering all beneficiaries. Despite the lack of extensive pre-treatment periods for most treated individuals—our data begins in May, and treatment starts in June—we find empirical support for our hypothesis. As shown in Table 2, there are no systematic differences in observed covariates for eventually treated groups. This indicates that these groups are good counterfactuals for each other, and thus, there would be no reason to believe in differences in trends. Furthermore, Figure 2 confirms no pre-treatment labor supply behavior differences between treatment and control groups, reinforcing our identification approach[19].

Our findings across the entire sample align with recent studies on cash transfers. As depicted in Figure 2, the AE program had no significant effect on active participation in the labor market across every exposure length. However, it also unveils underlying labor market dynamics at play. The program positively affected the entry of previously inactive individuals into the labor force, increasing their chance to offer work up to 4.5%. Conversely, for individuals already employed, the program negatively affected their active labor market participation, with a peak reduction of 3.8% after two months in the program.

This result is further supported in Table 3, which shows the single aggregated effect of the AE (equation 6) on labor market participation across each sample. No overall effect of the program on labor supply was identified. However, it increased the aggregated probability of workforce entry by 4.28% for those initially outside the labor market. In contrast, for individuals already in the labor market, there was a decrease in participation by 3.09%. Therefore, these compensatory effects are likely to explain the absence of an aggregate null effect.

Table 3 – Single aggregated effect of the AE on the likelihood of offering work by initial labor force status

Sample	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Complete Sample	-0.0033	0.0060	[-0.015, 0.0084]
Initially Active	-0.0309	0.0065	[-0.0437,-0.0182]
Initially Inactive	0.0428	0.0097	[0.0097, 0.0618]

The table displays the aggregated group parameter in Equation 6.

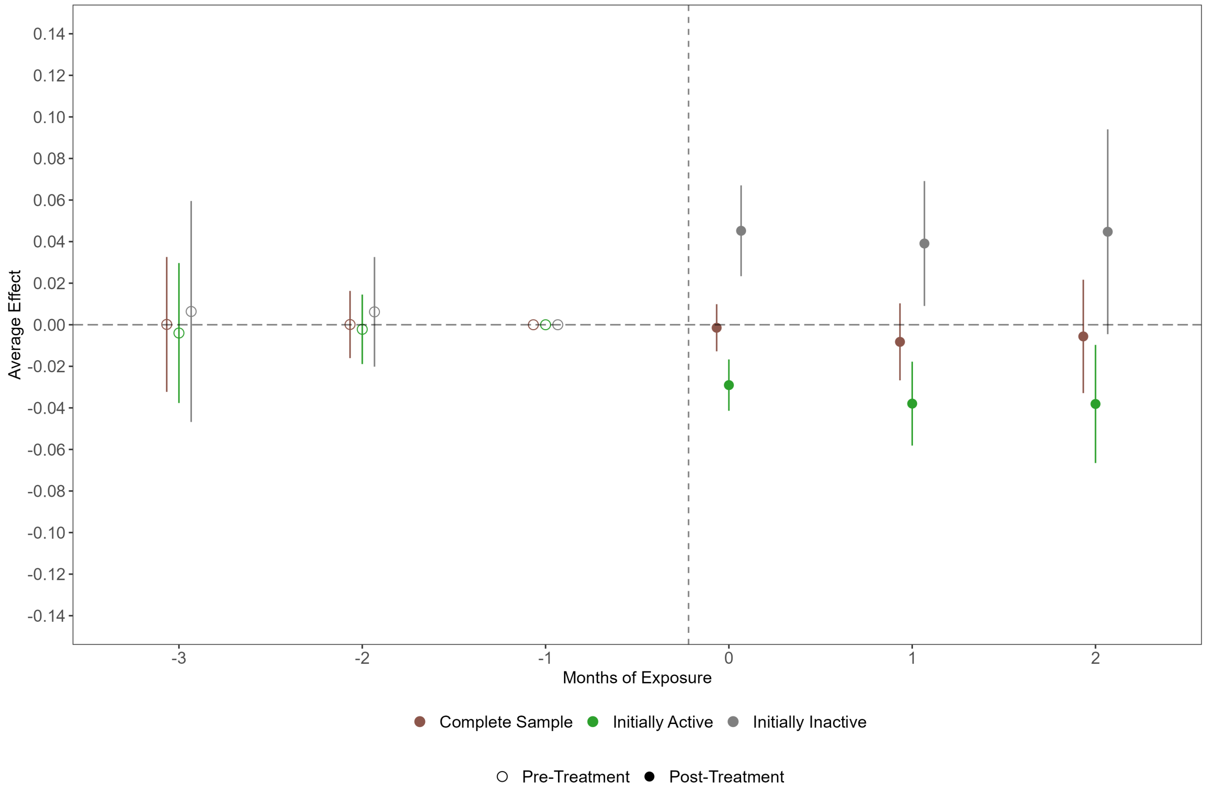
Dependent Variable: Labor offer (employment or job seeking).

Estimates for the complete sample, and by initial status on the labor market.

Source: PNAD COVID-19.

[19] We chose the period before treatment as the fixed baseline to get an interpretation similar to the TWFE event study (Roth, 2024). Also, this provides support for the no anticipation hypothesis.

Figure 2 – AE effect on the likelihood of offering work by months of exposure and initial labor force status

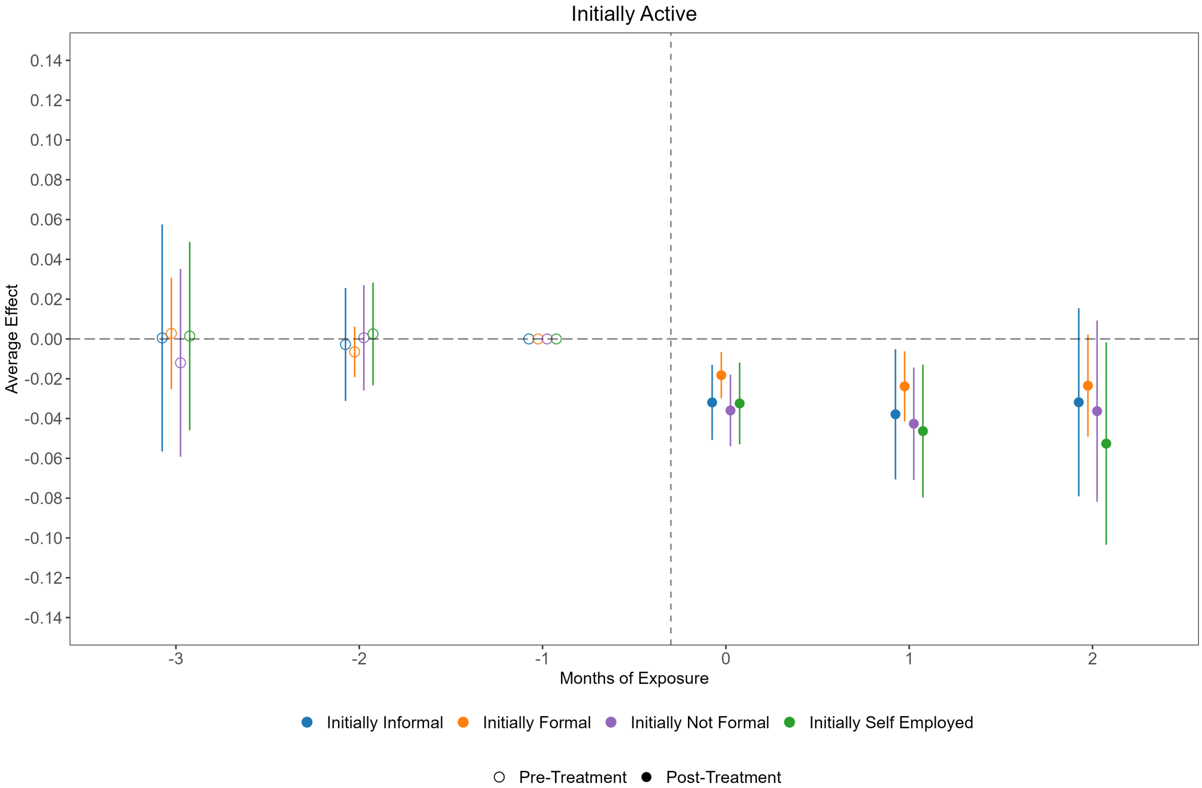


Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts and initial labor market status for each event-time period. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market categories. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown for $t < 0$. Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

Considering the impact on those initially active in the labor market, one may question whether a reverse dynamic is at play. Individuals might be losing their jobs and then applying for emergency aid rather than the other way around. Evidence suggesting this is not the case is presented in Figure 3, which displays the AE's effects on the likelihood of offering work, segmented by active individuals' initial labor market status. We categorize these between formal, informal, self-employed, and "non-formal", including the unemployed and informal workers. We also ran our estimates only for those initially unemployed, which are available with all other estimates in Table 7 of the appendix, but our statistical power significantly decreased due to the smaller sample size. Our findings indicate that the AE had smaller effects for initially formally employed individuals, which is confirmed in the single aggregate effect in Table 4.

Overall, we find a 2.14% reduction for those classified as formal in May 2020, 3% for only informal workers, and 3.36% when considering those initially in informality or actively seeking work. Self-employed workers are particularly noteworthy due to their generally less restrictive work contracts and greater flexibility, making them less likely to lose their jobs to receive the benefit, thereby avoiding reverse causality. Our estimates show a 3.8% reduction in the likelihood of offering labor for self-employed individuals in May 2020.

Figure 3 – AE effect on the likelihood of offering work by months of exposure and groups of initially active individuals



Notes: Each circle shows the estimated ATT for labor market participation for groups of initially active individuals (employed or actively looking for a job) averaged across treatment adoption cohorts for each event-time period. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market categories. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown for $t < 0$. Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

For those already in the labor market, the canonical model (Becker, 1965), seems like a plausible explanation. The increase in income could have raised their reservation wage, leading to a reduction in labor supply. This might be particularly true in the COVID-19 context, where individuals with the option not to offer labor would do it to survive the pandemic.

Table 4 – Single aggregated effect of the AE on the likelihood of offering work for groups of initially active individuals

Sample	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Formal	-0.0214	0.0058	[-0.0327, -0.0100]
Informal	-0.0300	0.0103	[-0.0501, -0.0099]
Not Formal	-0.0336	0.0092	[-0.0517, -0.0155]
Self Employed	-0.0380	0.0104	[-0.0583, -0.0176]

The table displays the aggregated group parameter in Equation 6.

Dependent Variable: Labor offer (employment or job seeking).

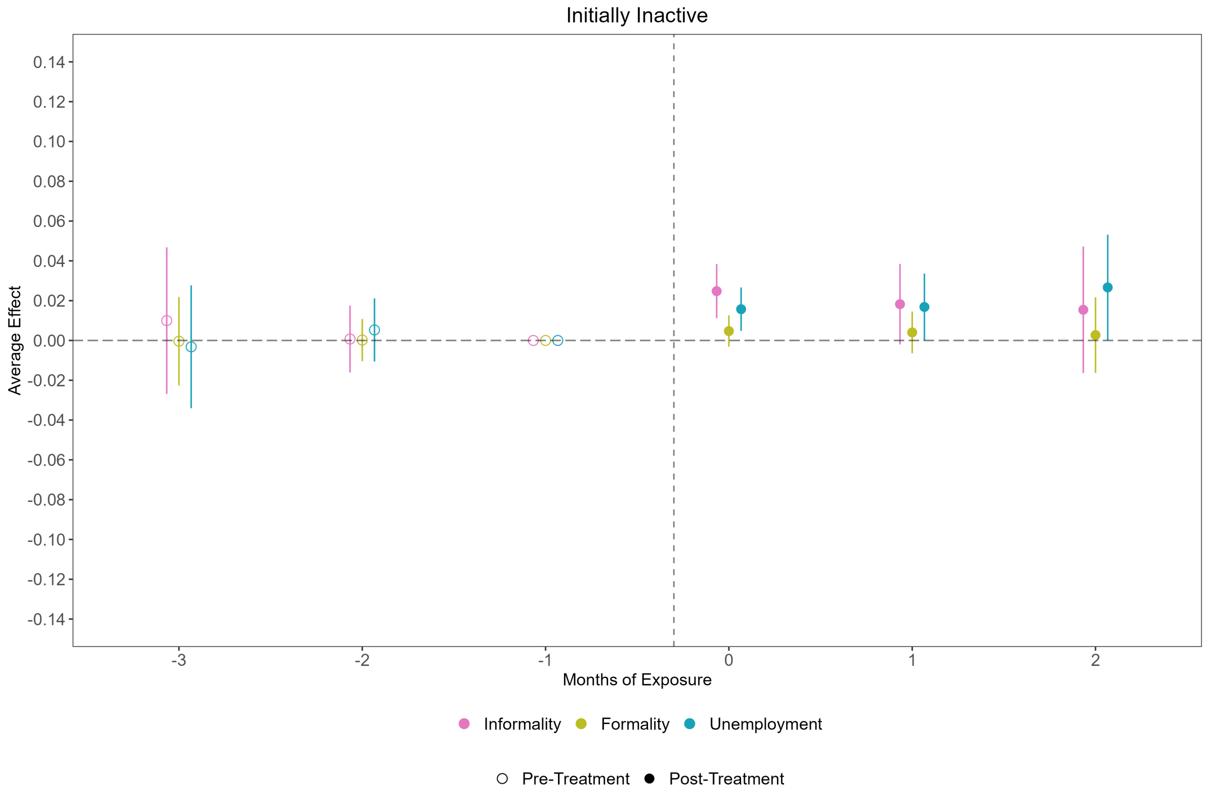
Estimates for initial formal, informal, self-employed, and "not formal" individuals

Source: *PNAD Covid-19*.

Alternatively, in Figure 4, we examine the AE impact on labor market transitions for individuals previously outside the labor force, highlighting shifts into informal work, formal employment, and unemployment[20]. Our estimates show that the AE prompted a 2.25% rise in informal employment at the first exposure to the program. Furthermore, the program has positively influenced job search activities, with an initial increase of up to 1.7% with a more persistent effect over time, with a 2.7% positive effect after two months of exposure. This pattern suggests that the transfers initially facilitated access primarily to less stable employment options and might have allowed individuals to search for better jobs. This observation is supported by aggregated results in Table 5, which document a 1.96% increase in the likelihood of moving into informal employment and a 1.78% increase in the probability of transitioning to unemployment, without impacting overall formal employment.

[20] See Table 8 of the appendix

Figure 4 – AE effect on the likelihood of each labor market transitions by months of exposure for initially inactive individuals



Notes: Each circle shows the estimated ATT for transitions to formality, informality or job seeking for initially inactive individuals (not employed or actively looking for a job) averaged across treatment adoption cohorts for each event-time period. Control groups are not-yet-treated beneficiaries initially inactive in the labor market. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown for $t < 0$, with the exception of the first pre-treatment estimation for formality, which is marginally significant. Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

Table 5 – Single aggregated effect of the AE on each labor market transitions for initially inactive individuals

Dependent Variable	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Informality	0.0196	0.0063	[0.0073, 0.032]
Formality	0.0054	0.0036	[-0.0017 , 0.0124]
Unemployment	0.0178	0.0057	[0.0067, 0.0289]

The table displays the aggregated group parameter in Equation 6.
Dependent Variables: Informal work, Formal work or Unemployment.
Estimates for individuals initially out of labor force.
Source: *PNAD Covid-19*.

A possible explanation for these results lies in the poverty trap theory proposed by Dasgupta and Ray (1986). Individuals initially outside the labor market may have decided to stop seeking employment during our baseline period because they could not meet the market's quality demands or, even if qualified, gave up because they were simply unlucky in finding a job. With the financial boost, these individuals might have gained the necessary means to increase their productivity even in the short term. For example, individuals could have financed investments such as transportation means—like bicycles or cars—to increase their labor supply in the informal sector. Additionally, individuals with prolonged access to the benefit might have leveraged it to gain skills and be unemployed, with a job search effect (Baird; McKenzie; Özler, 2018) for better opportunities in the formal sector.

6.1 Heterogeneous effects

Under the specific conditions of the pandemic and the AE design, the impact on labor market participation likely varied across different groups. Figure 5 shows the value of the aggregated unique parameter, estimated in equation 6, of the program's effect on labor supply likelihood, with variations by gender, race, and region[21].

Single mothers might be a particular group of interest as they received twice the standard AE benefit, which potentially altered their labor market behavior. However, due to data limitations, we couldn't precisely identify single mothers, so our analysis shifted focus to gender disparities. Our primary results hold for both genders[22]. Yet, there are specific gender variations in the labor market response to the AE. The program apparently had a more pronounced effect on inactive men, with a 6.38% increase in the likelihood of offering work among beneficiaries, and on active women, with an aggregated 4.48% reduction in the likelihood of being active in the labor market. Women might have had an increased opportunity to withdraw from work and focus on domestic and childcare responsibilities during the pandemic. While for men, the benefit might have provided new work opportunities, although it was not sufficient to live on during the pandemic.

[21] See Table 9.
[22] See Table 10 of the appendix for the event study results by gender.

This diversity in the labor market response to the program is also observed among different minority groups and regions[23]. Non-white individuals faced greater economic and social difficulties compared to their white counterparts. For instance, in our sample, non-white individuals had an average income of around US$423, while white individuals had an average income of approximately US$686. Our main findings still hold for ethnic desegregation. However, white initially active individuals had a higher propensity to leave the labor force, with an aggregated reduction of 3.67% compared to a negative effect of 2.68% among non-whites. White individuals, generally having higher incomes, might have benefited more from the AE as a means of shielding themselves from the pandemic. Conversely, the opposite is observed among those who were initially inactive. There was a higher tendency for initially inactive non-whites to enter the labor force (4.92%) compared to whites (3.48%). Therefore, this more vulnerable population may have also gained the necessary resources to offer labor during the pandemic.

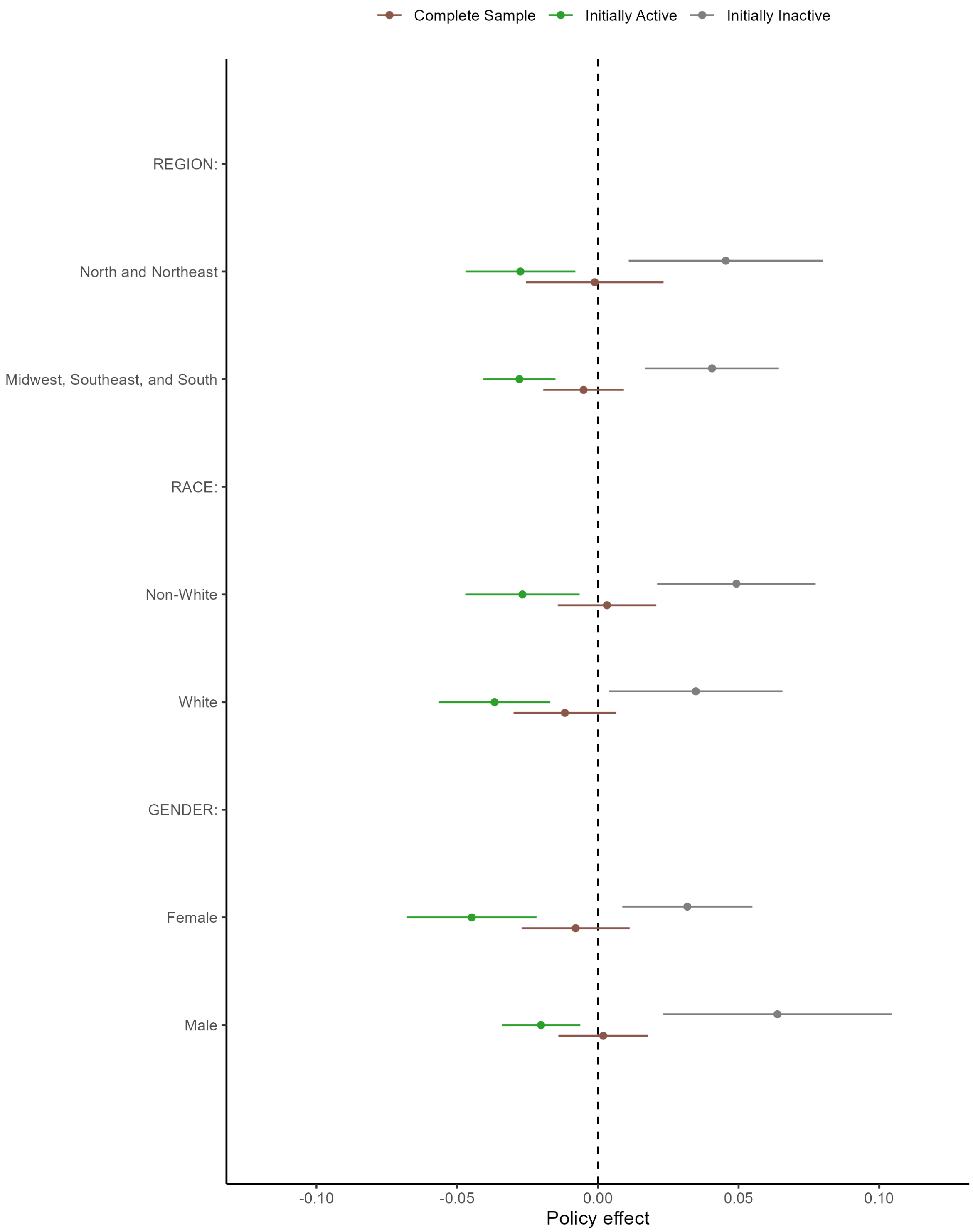
Regional economic disparities further illustrate the varied impact of AE on labor market decisions. The North and Northeast, the poorest regions in Brazil, have a larger informal sector. Our proxy for informality is 39.51% for the North and Northeast, with an average income of approximately US$405, compared to an informality rate of 24.23% and an average income of US$624 for the South, Southeast, and Midwest regions. While the main results were consistent across these regions[24], the effect of AE on the propensity to offer labor was more pronounced in the North and Northeast for initially inactive individuals (4.55% vs. 4.06%). Conversely, a greater exit from the labor force was observed among active individuals in the Midwest, Southeast, and South (2.79% vs. 3.96%).

Our analysis underscores that AE may have primarily benefited minorities and inactive individuals in terms of productivity gains. Men, non-white individuals, and residents of the poorer regions of Brazil who were initially inactive seem to have gained the most from the program, acquiring the necessary resources to escape the poverty trap. This outcome aligns with the expectation that transfers to lower-income individuals tend to have a greater effect on consumption, thereby potentially increasing productivity with greater potential gains. In contrast, for initially active women, particularly single mothers who received increased benefits, the program might have worked as a relief, allowing for protection and childcare during the pandemic. However, this relief seems to have varied across different segments of the population. The exit from the labor force was more pronounced among whites, likely due to their higher incomes and associated higher reservation wages, and among those in more formal labor markets, as in the Midwest, Southeast, and South.

[23] See Table 11 for the event study's results by race.

[24] See Appendix Table 12 for detailed results of the event study by region.

Figure 5 – Single aggregated effect of the AE on the likelihood of offering work by initial work status and social groups.



Notes: Each circle shows the estimated single parameter in equation 6 for labor market participation for different social groups. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market categories. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs. non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level (disregarding the respective variable in case of perfect collinearity). Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

6.2 Robustness

We conduct several exercises to verify the robustness of the results. First, the main models were estimated considering the never treated as control. This group is quite different from eventually treated groups with higher income and educational levels. To address the concern of non-parallel trends, we applied an approach outlined by Rambachan and Roth (2023) to partially identify our interest parameters. Rather than assuming that trends for control and treated groups would have followed the same path without treatment, we allowed for the post-treatment trend to differ, restricted by actual pre-treatment violations. We opted for relative magnitudes restrictions on trend differences. Although smoothness restrictions may seen as ideal for job market evolution, which typically does not experience abrupt or unanticipated changes over short periods, we are dealing in a context with an external negative shock, the COVID-19 pandemic.

Formally, define the event study coefficients as $\hat{\beta} = (\hat{\beta}'_{pre}, \hat{\beta}'_{post})$. Under the no anticipation hypothesis, it's assumed that the vector of coefficients to be estimated can be partitioned as

$$\beta = \begin{pmatrix} 0 \\ \tau_{post} \end{pmatrix} + \begin{pmatrix} \delta_{pre} \\ \delta_{post} \end{pmatrix}$$

Where τ_{post} is the interest parameter vector and $\delta = (\delta_{pre}, \delta_{post})$ as a vector of trend deviations, partitioned into pre (δ_{pre}) and post (δ_{post}) treatment violations. Thus, instead of assuming $\delta_{post} = 0$ testing $\delta_{pre} = 0$, we consider $\delta \in \Delta(M)$, with the set of restrictions $\Delta(M)$ specified as

$$\Delta(M) = \{\delta : \forall t \geq 0, |\delta_{t+1} - \delta_t| \leq M \cdot \max_{j<0} |\delta_{j+1} - \delta_j|\}. \tag{7}$$

Where $t = 0$ represents the first treatment time. Then is partially identified the parameters $\theta_e = l'_e \tau_{post}$, where l'_e represents the weight vector. We vary this weight for each event study e, where the e-th entry assumes a value of 1 if we are conducting the sensitivity analysis for the specific event time in question and 0 otherwise.

Typically, the parameters θ_e are set identified. Specifically, if we assume $\Delta(M)$ to be a closed and convex set, the θ_e identified sets are intervals in the real numbers with lower bounds $\theta_e^{lb}(\beta, \Delta)$ and upper bounds $\theta_e^{ub}(\beta, \Delta)$ given by[25].

$$\theta_e^{lb}(\beta, \Delta) = l'_e \beta_{post} - \left(\max_{\delta} l'_e \delta_{post},\ \text{s.t. } \delta \in \Delta(M), \delta_{pre} = \beta_{pre} \right)$$

$$\theta_e^{ub}(\beta, \Delta) = l'_e \beta_{post} - \left(\min_{\delta} l'_e \delta_{post},\ \text{s.t. } \delta \in \Delta(M), \delta_{pre} = \beta_{pre} \right)$$

Our sensitivity analysis results are illustrated in Figure 6. There are no significant pre-treatment differences for the full sample, which might lead one to believe that a sensitivity analysis isn't required. However, when we look at the disaggregated results, clear trend differences emerge between the never-treated group and the eventually-treated ones. For the active group, pre-treatment trend differences seem to decrease over time, while the opposite is true

[25] See Lemma 2.1 of Rambachan and Roth (2023)

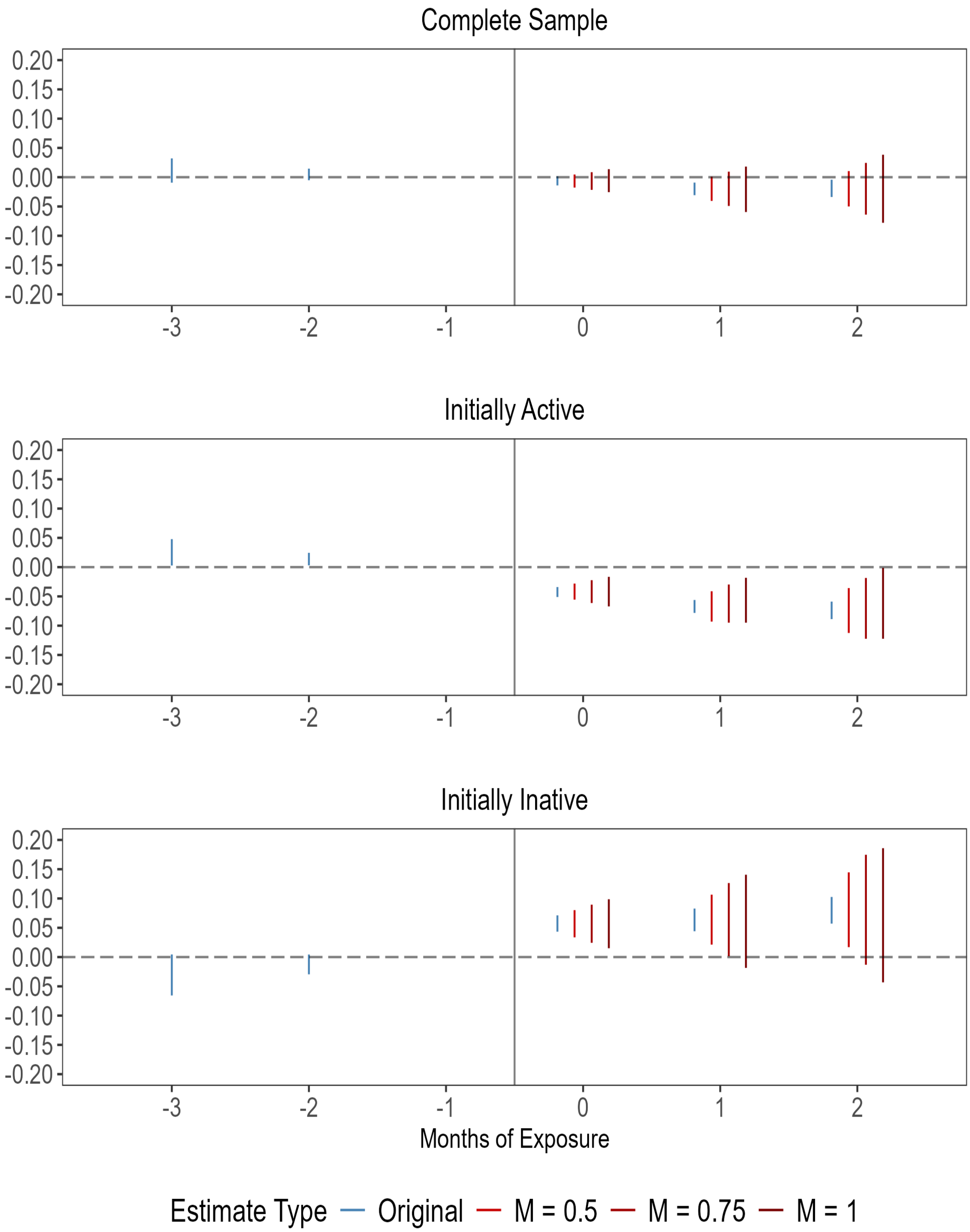
for the inactive group. This type of trend is the most challenging in our scenario, as the negative effects on the active could stem from this decreasing trend, while the positive effect on the inactive might arise from the increasing trend.

The selection of M reflects the specific circumstances under investigation. Recognizing that the initial impact of the pandemic was most pronounced during March and April and tended to decrease in the following months, it is assumed $M \in \{0.5, 0.75, 1\}$. With these values, we are implicitly presupposing that pre-treatment shocks did not exceed post-treatment shocks[26].

The results highlight the importance of considering trend differences when using the never-treated group as controls. Without accounting for trends, our estimates would suggest a negative effect on the complete sample after one and two exposure months. However, when we control for pre-trend differences, no significant effect was found for any value of M considered. Our main results also hold when considering labor market segmentation. For initially active individuals, a clear difference in pre-treatment trends occurs. However, for every value of M considered, we detected a positive effect of AE on the likelihood of entering the labor market, albeit marginally significant when assuming the maximal anticipated shock ($M = 1$). Similarly, for initially inactive individuals, positive effects are observed for all exposure months, with a few exceptions for larger values of M.

[26] For detailed analysis on the varying values of M, see Tables 13, 14, and 15.

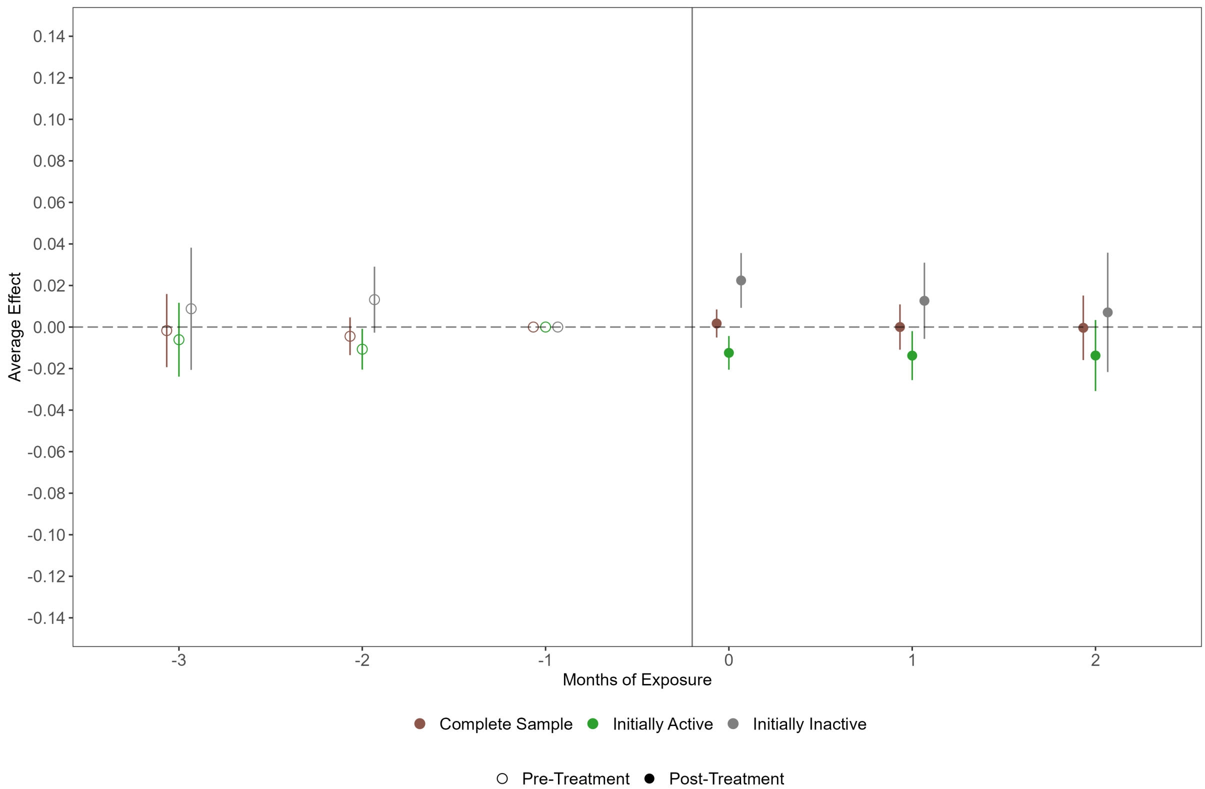
Figure 6 – Sensitivity analysis: Never-treated as control group



Notes: Each bar shows the estimated sensitivity analysis for labor market participation averaged across treatment adoption cohorts and initial labor market status for each event-time period. Control groups are never-treated individuals from the corresponding initial labor market categories. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level. Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

Second, we re-estimate the main results using the concept that individuals, once treated, do not "forget" the treatment (Callaway; Sant'Anna, 2021). That is, we now assume that individuals, once treated, remain treated. As shown in Figure 7, our principal findings hold even when including these individuals[27]. However, we identify weaker effects for both inactive and active individuals. Our calculations suggest an immediate reduction of about 1.2% for the initially active (and by 1.4% after one exposure month) and an increase up to 2.3% for the inactive.

Figure 7 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Individuals that don't forget treatment



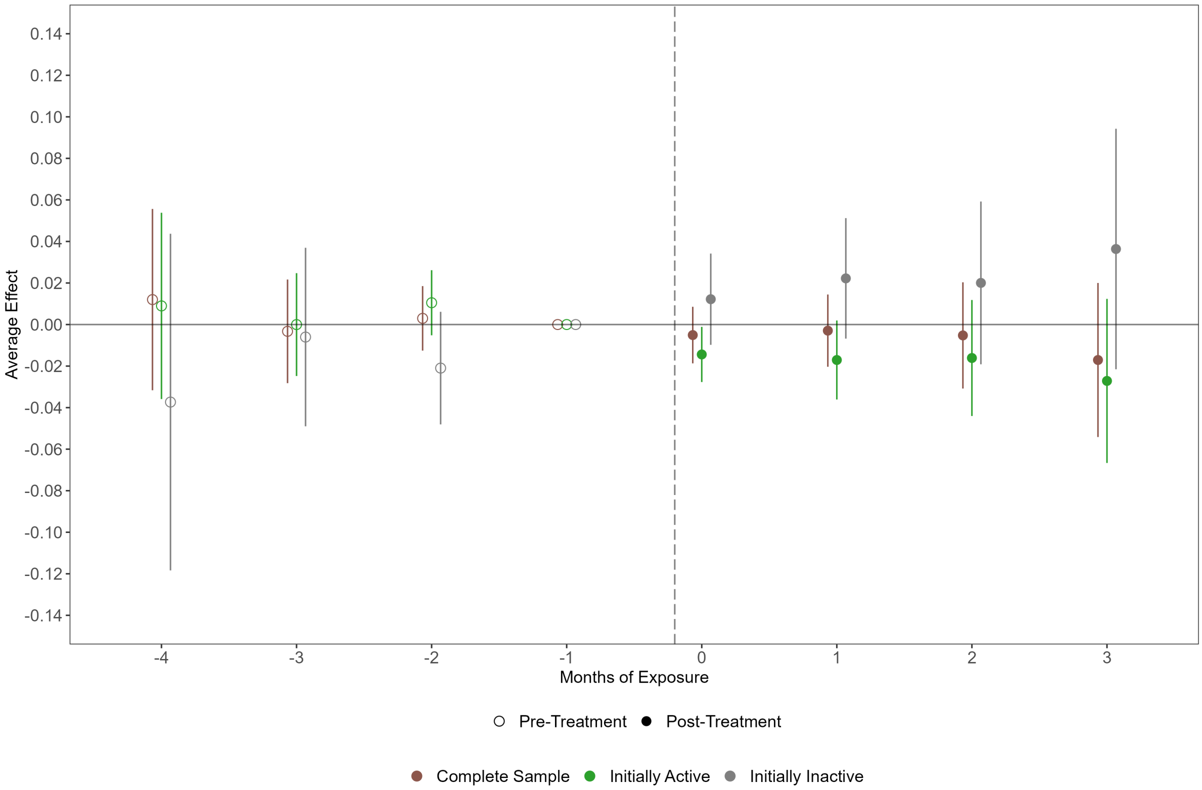
Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts and initial labor market status for each event-time period. It is considered that once individuals are treated, they remain treated in these estimates. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market categories. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups, age, educational levels, householder dummy, and pre-treatment income level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown for $t < 0$. Treatment status is defined by receipt of AE at the household level. Data from *PNAD-Covid19*.

Lastly, we also investigate if those who before were "always treated", meaning those already treated by May 2020, had a different labor market response to the program. We used PNADC data from the first quarter of 2019 as the pre-treatment period since all households in our main dataset participated in this interview. A potential drawback of this approach is the lengthy time gap between the pre-treatment and post-treatment periods, which may allow for possible labor market dynamics to occur and make interpreting the parameters of interest more challenging.

[27] See Table 16

The estimates in Figure 8 show that our results' directions stay consistent, though they are at the threshold of significance[28].

Figure 8 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: First quarter 2019 PNADC data



Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts and initial labor market status for each event-time period. The baseline period in these estimates is the first quarter of 2019, allowing for the estimation of treatment for those who entered the program in May 2020. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market categories. The vertical bars represent the 95% confidence intervals for each point estimate. Standard errors are computed using a multiplier bootstrap and clustered at the individual level. The model includes time invariant controls for gender, ethnic groups (white vs non-whites), age, educational levels, householder dummy, regional indicators, and pre-treatment income level. Pre-policy adoption estimates that are statistically indistinguishable from zero are shown for $t < 0$. Treatment status is defined by receipt of AE at the household level. Data from first quarter 2019 PNADC and *PNAD-Covid19*.

7.Discussion and Conclusions

Our overall results align with existing cash transfers literature, showing no effect of the *Auxílio Emergencial* program on labor supply. While cash transfer studies often find no impact on labor offering likelihood, it is noteworthy to observe this during the COVID-19 pandemic. Given the health restrictions and exposure risks in various jobs, one might expect a reduced willingness to supply labor.

Yet, this effect indeed materializes for a segment of the population already in the labor market, with stronger effects for informal workers and the unemployed. Unlike in other contexts,

[28] See Table 17 for details.

this outcome can be viewed as "positive". The financial transfer might have provided individuals in more vulnerable situations, such as informality and unemployment, with the necessary conditions to stop offering labor in the short term, reducing further spread and fatalities.

In contrast, for those previously outside the labor market, we observe a reverse effect mostly towards the informal sector but also to formal jobs for early participants. With the transfers, individuals previously outside the labor force might have accessed resources to escape poverty traps and begin offering labor. The initial shift towards informality suggests that beneficiaries used the aid to purchase tools for informal work, like bicycles, computers, or cars, facilitating access to previously unreachable jobs. Coupled with our findings on unemployment, this implies that beneficiaries could afford to wait for better short-term job opportunities, achieving better employment after some months, indicating a job search effect. Furthermore, with extended financial support, beneficiaries might invest in long-term opportunities, such as training or educational courses, enhancing their productivity.

In the analysis of heterogeneous effects, distinct patterns emerged. Women who were initially active in the workforce exhibited a greater tendency to withdraw than their male counterparts, likely due to the higher payments provided to single mothers. This suggests that the program likely incentivized women, especially single mothers, to remain at home during the pandemic, allowing them to focus on domestic work and childcare. However, for the initially inactive population, the impact was more pronounced among men. This indicates that the benefit was not sufficient to completely deter men from offering labor, but it provided them with the necessary means to escape the poverty trap.

Among ethnic groups, initially active white participants showed a more pronounced exit from the labor force compared to their non-white counterparts. This trend could be attributed to the typically higher income levels among whites, where increased financial assistance likely led to a larger decline in work participation due to a higher reservation wage. Conversely, for those initially out of the labor force, the effects were opposite in both magnitude and direction. The program had strong and consistent effects on labor force entry for non-whites and marginally significant effects for whites. Lower-income groups tend to consume more when their income increases, which can lead to productivity gains within the poverty trap framework and enhance the employability of this segment of the population.

Regional differences in responses to the *Auxílio Emergencial* also exist but were less evident. Higher magnitudes, particularly regarding labor force entry among initially inactive individuals, were observed for residents of the North and Northeast regions of Brazil, where there is a higher prevalence of low-paying informal work. Consequently, the program may have provided greater protection from the virus for workers in precarious occupations, who were likely to face a higher risk of infection.

The robustness tests confirm the main findings. Considering the never-treated as the control group revealed clear trend differences with eventually treated groups. This issue was addressed by assuming that pre-treatment trends are not larger than post-treatment trends, a reasonable hypothesis given that the initial economic shocks of COVID-19 occurred in the early months of the sample. Even when controlling for these trends, our results remained consistent or hovered at the threshold of significance with the maximum trend. Additionally, we estimated the main models considering that individuals, once treated, would remain treated. The results also pointed towards effects in the same direction but with diminished magnitudes, contrary to expectations since participants who eventually stopped receiving the aid might have done so upon securing employment. In analyzing the program's impact specifically on self-employed individuals, we observed an even greater tendency to exit the labor market compared to the general results for inactive and informal workers transitioning out of the workforce. This adds further robustness to our findings, given that self-employed individuals are less susceptible to dismissal. Lastly, we examined the effects using PNADC data from the first quarter of 2019. No significant effects were found, though estimates with the 2019 PNADC data showed signs consistent with those previously estimated.

As discussed by Banerjee et al. (2017), policymakers often harbor concerns that income transfer programs might create "lazy welfare recipients", meaning individuals who choose not to work and live solely off benefits. If this concern were valid, the context analyzed in this study would be the most conducive for such an outcome. That is, for occupations with a high risk of contagion, ceasing to offer labor is perceived positively, specifically within the COVID-19 context. Yet, even in this scenario, we find no evidence that the overall labor supply was affected, with the direction of the effect depending on the initial economic situation of the beneficiaries. Thus, it is expected to contribute to the cash transfer literature as a benchmark. In essence, if, in this most likely scenario, cash transfers did not affect labor supply, the concept of the lazy welfare recipient seems indeed to be a myth.

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APPENDIX

Tables

Table 6 – AE effect on the likelihood of offering work by months of exposure and initial labor force status.

Event Time	Complete Sample	Initially Active	Initially Inactive
-3	0.0001	-0.0040	0.0064
	(0.0133)	(0.0132)	(0.0224)
-2	0.0001	-0.0022	0.0062
	(0.0064)	(0.0061)	(0.0101)
-1			
0	-0.0015	-0.0290*	0.0452*
	(0.0045)	(0.0051)	(0.0074)
1	-0.0082	-0.0380*	0.0391*
	(0.0072)	(0.0078)	(0.0117)
2	-0.0056	-0.0381*	0.0447
	(0.0109)	(0.0116)	(0.0184)

The table displays the event study parameters in Equation 4.
Dependent Variable: Labor offer (employment or job seeking).
Estimates for the complete sample, initially active and initially inactive in the labor market.
Standard Deviations in parentheses.
* indicates statistical significance at the 5% level
Source: PNAD COVID-19.

Table 7 – AE effect on the likelihood of offering work by months of exposure for groups of initially active individuals

Event Time	Formal	Informal	Not Formal	Self Employed	Unemployed
-3	0.003	0.001	-0.012	-0.042	0.001
	(0.012)	(0.022)	(0.019)	(0.050)	(0.018)
-2	-0.006	-0.003	0.001	0.017	0.003
	(0.005)	(0.011)	(0.011)	(0.026)	(0.010)
-1					
0	-0.018*	-0.032*	-0.036*	-0.052*	-0.032*
	(0.005)	(0.007)	(0.007)	(0.018)	(0.008)
1	-0.024*	-0.038*	-0.043*	-0.059*	-0.046*
	(0.007)	(0.013)	(0.011)	(0.027)	(0.013)
2	-0.023	-0.032	-0.036	-0.049*	-0.053*
	(0.011)	(0.018)	(0.018)	(0.039)	(0.020)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

Estimates for the initially formal, informal, "not formal", self-employed, and unemployed groups.

Standard Deviations in parentheses.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 8 – AE effect on the likelihood of each labor market transition by months of exposure for initially inactive individuals.

Event Time	Informality	Formality	Unemployment
-3	0.010	-0.000	-0.003
	(0.014)	(0.009)	(0.013)
-2	0.001	0.000	0.005
	(0.006)	(0.004)	(0.007)
-1			
0	0.025*	0.005	0.016*
	(0.005)	(0.003)	(0.005)
1	0.018	0.004	0.017
	(0.008)	(0.004)	(0.007)
2	0.015	0.003	0.027*
	(0.013)	(0.007)	(0.011)

The table displays the event study parameters in Equation 4.

Dependent Variables: Informal work, Formal work or Unemployment.

Estimates for individuals initially inactive.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 9 – Estimates by Gender, Race, and Region

Group	Estimate	Sample	CI Lower	CI Upper
Gender				
Men	0.002	Complete Sample	-0.013	0.017
	-0.020	Initially Active	-0.035	-0.005
	0.064	Initially Inactive	0.023	0.105
Women	-0.008	Complete Sample	-0.027	0.012
	-0.045	Initially Active	-0.068	-0.021
	0.032	Initially Inactive	0.009	0.054
Race				
Non-whites	0.003	Complete Sample	-0.013	0.020
	-0.027	Initially Active	-0.045	-0.009
	0.049	Initially Inactive	0.022	0.077
Whites	-0.012	Complete Sample	-0.028	0.004
	-0.037	Initially Active	-0.055	-0.018
	0.035	Initially Inactive	0.004	0.065
Region				
Midwest, Southeast, and South	-0.005	Complete Sample	-0.018	0.008
	-0.028	Initially Active	-0.042	-0.013
	0.041	Initially Inactive	0.015	0.066
North and Northeast	-0.001	Complete Sample	-0.026	0.024
	-0.028	Initially Active	-0.047	-0.008
	0.045	Initially Inactive	0.013	0.078

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 10 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Men vs Women

	Men			Women		
Event Time	Complete Sample	Active	Inactive	Complete Sample	Active	Inactive
-3	-0.003	-0.010	0.021	0.004	0.001	0.002
	(0.016)	(0.013)	(0.039)	(0.015)	(0.020)	(0.021)
-2	-0.005	-0.009	0.011	0.005	0.008	0.004
	(0.007)	(0.006)	(0.018)	(0.007)	(0.010)	(0.009)
-1						
0	0.004	-0.019*	0.070*	-0.006	-0.041*	0.033*
	(0.005)	(0.005)	(0.015)	(0.006)	(0.008)	(0.009)
1	-0.000	-0.023*	0.062*	-0.015	-0.057*	0.028
	(0.009)	(0.008)	(0.021)	(0.009)	(0.013)	(0.012)
2	-0.003	-0.028*	0.053	-0.008	-0.052*	0.036
	(0.014)	(0.012)	(0.031)	(0.014)	(0.018)	(0.020)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

The results are disaggregated by initial labor status and gender.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 11 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Non-white vs White

	Non White			White		
Event Time	Complete Sample	Active	Inactive	Complete Sample	Active	Inactive
-3	-0.002	-0.016	0.028	-0.003	-0.011	-0.028
	(0.015)	(0.017)	(0.026)	(0.015)	(0.014)	(0.032)
-2	-0.006	-0.013	0.007	0.008	0.010	-0.001
	(0.007)	(0.008)	(0.012)	(0.007)	(0.008)	(0.014)
-1						
0	0.004	-0.028*	0.052*	-0.008	-0.031*	0.034*
	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)	(0.011)
1	0.002	-0.030*	0.048*	-0.022*	-0.050*	0.030
	(0.009)	(0.010)	(0.014)	(0.009)	(0.011)	(0.016)
2	0.002	-0.031	0.048	-0.016	-0.048	0.042
	(0.014)	(0.015)	(0.024)	(0.013)	(0.015)	(0.022)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

The results are disaggregated by initial labor status and ethnic group.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 12 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: South, Southeast and Midwest vs North and Northeast

	South, Southeast and Midwest			North and Northeast		
Event Time	Complete Sample	Active	Inactive	Complete Sample	Active	Inactive
-3	-0.008	0.001	-0.033	-0.015	-0.020	0.053
	(0.012)	(0.012)	(0.025)	(0.021)	(0.024)	(0.034)
-2	-0.002	-0.000	-0.004	0.005	-0.007	0.019
	(0.006)	(0.006)	(0.012)	(0.010)	(0.012)	(0.016)
-1						
0	-0.004	-0.023*	0.035*	0.002	-0.045*	0.059*
	(0.004)	(0.005)	(0.010)	(0.007)	(0.009)	(0.011)
1	-0.009	-0.035*	0.042*	-0.008	-0.046*	0.035
	(0.007)	(0.008)	(0.014)	(0.011)	(0.015)	(0.017)
2	-0.005	-0.038*	0.058*	-0.009	-0.038	0.032
	(0.011)	(0.011)	(0.020)	(0.019)	(0.022)	(0.027)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

The results are disaggregated by initial labor status and residence.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 13 – Sensitivity analysis: Never-treated as control group (Complete Sample)

Event time	Original	M = 0.5	M = 0.75	M = 1
-3	[-0.0093 , 0.0321]			
-2	[-0.0052 , 0.0146]			
-1				
0	[-0.014 , 0.0015]	[-0.0177 , 0.0046]	[-0.0216 , 0.0085]	[-0.0255 , 0.0138]
1	[-0.0307 , -0.0092]	[-0.0405 , 9e-04]	[-0.0491 , 0.0095]	[-0.0594 , 0.0181]
2	[-0.0338 , -0.0043]	[-0.05 , 0.0105]	[-0.0639 , 0.0244]	[-0.0778 , 0.0383]

The table displays the set identified parameters in Equation 7.

Dependent Variable: Labor offer (employment or job seeking).

Estimates for the complete sample.

Each column represents the confidence intervals calculated by varying maximum trends.

The first column displays the original result (M = 0), while the subsequent ones are for $M = 0.5$, $M = 0.75$, and $M = 1$.

Source: PNAD COVID-19.

Table 14 – Sensitivity analysis: Never-treated as control group (Initially Active)

Event time	Original	M = 0.5	M = 0.75	M = 1
-3	[-0.0656 , 0.0044]			
-2	[-0.0295 , 0.0043]			
-1				
0	[0.0433 , 0.0712]	[0.0337 , 0.0801]	[0.0244 , 0.0894]	[0.0151 , 0.0987]
1	[0.0441 , 0.0828]	[0.0213 , 0.1065]	[0.0014 , 0.1264]	[-0.0185 , 0.1406]
2	[0.0571 , 0.1026]	[0.0169 , 0.1446]	[-0.0131 , 0.1746]	[-0.0432 , 0.1859]

The table displays the set identified parameters in Equation 7.

Dependent Variable: Labor offer (employment or job seeking).

Estimates for individuals initially active in labor market.

Each column represents the confidence intervals calculated by varying maximum trends.

The first column displays the original result (M = 0), while the subsequent ones are for $M = 0.5$, $M = 0.75$, and $M = 1$.

Source: PNAD COVID-19.

Table 15 – Sensitivity analysis: Never-treated as control group (Initially Inactive)

Event time	Original	M = 0.5	M = 0.75	M = 1
-3	[0.0028 , 0.0478]			
-2	[0.0031 , 0.0244]			
-1				
0	[-0.051 , -0.034]	[-0.0555 , -0.0281]	[-0.0613 , -0.0223]	[-0.067 , -0.0166]
1	[-0.0782 , -0.0561]	[-0.093 , -0.0412]	[-0.0949 , -0.0297]	[-0.0949 , -0.0182]
2	[-0.0888 , -0.0589]	[-0.1124 , -0.0358]	[-0.1223 , -0.0185	[-0.1223 , -0.0012]

The table displays the set identified parameters in Equation 7.

Dependent Variable: Labor offer (employment or job seeking).

Estimates for individuals initially inactive in labor market.

Each column represents the confidence intervals calculated by varying maximum trends.

The first column displays the original result (M = 0), while the subsequent ones are for $M = 0.5$, $M = 0.75$, and $M = 1$.

Source: PNAD COVID-19.

Table 16 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Memory of treatment

Event Time	Complete Sample	Initially Active	Initially Inactive
-3	-0.002	-0.006	-0.009
	(0.007)	(0.007)	(0.002)
-2	-0.004	-0.001*	0.013
	(0.004)	(0.004)	(0.007)
-1			
0	0.002	-0.012*	0.025*
	(0.003)	(0.003)	(0.005)
1	0.000	-0.014*	0.013
	(0.004)	(0.004)	(0.007)
2	-0.000	-0.014	0.007
	(0.006)	(0.006)	(0.010)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

These estimates assume that once treated, an individual remains treated.

Estimates for the complete sample, initially active and initially inactive in the labor market.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 17 – AE effect on the likelihood of offering work by months of exposure and initial labor force status:PNADC first quarter 2019.

Event Time	Complete Sample	Initially Active	Initially Inactive
-4	0.012	0.009	-0.037
	(0.016)	(0.018)	(0.028)
-3	-0.003	0.000	-0.006
	(0.009)	(0.010)	(0.016)
-2	0.003	0.011	-0.021
	(0.006)	(0.007)	(0.010)
-1			
0	-0.005	-0.014	0.012
	(0.005)	(0.005)	(0.008)
1	-0.003	-0.017	0.022
	(0.007)	(0.007)	(0.011)
2	-0.005	-0.016	0.020
	(0.011)	(0.010)	(0.016)
3	-0.017	-0.027	0.036
	(0.015)	(0.015)	(0.021)

The table displays the event study parameters in Equation 4.

Dependent Variable: Labor offer (employment or job seeking).

Estimates including identified individuals in first quarter 2019 PNADC.

Estimates for the complete sample, initially active and initially inactive in the labor market.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: National Household Sample Survey (PNAD) and the PNAD COVID-19.

Mathematical Appendix

To identify 2, the first hypothesis that must be considered is the irreversibility of treatment. Define D_{it} as a dummy treatment variable for individual i in period t. Then, for the irreversibility of treatment hypothesis to be satisfied, we must have that for all i, $D_{i1} = 0$ almost surely, and for $t = 2, ..., \tau$ $D_{i(t-1)} = 1$ implies that $D_{it} = 1$ almost surely. This assumption states two key premises. First, it posits that no individual receives treatment during the first period. We followed the author's suggestion and dropped all always treated individuals of our main sample. The second premise asserts that once an individual receives the AE benefit, they remain to receive the benefit in the succeeding periods, which is satisfied, as we also dropped these observations. Importantly, we run robustness checks, considering that individuals have a "memory" of the treatment, to ensure the validity of our findings.

The second assumption required for identification is random sampling. This is automatically satisfied by the sample design made by IBGE.

Also, one also must take into account the limited treatment anticipation assumption. This requires that for a X vector of observable variables, there is a known $\delta \geq 0$ such that $\mathbb{E}[Y_t(g)|X, G_g = 1] = \mathbb{E}[Y_t(0)|X, G_g = 1]$ almost surely for all $g \in \mathbb{G}$ and $t \in \{1, ..., \tau\}$ such that $t < g - \delta$. This means that individuals cannot change their behavior based on the expectation of receiving the treatment in the future[29]. We believe this assumption is satisfied simply because people can survive on the AE transfers after actually receiving them, not due to the expectation of receiving them in the future. With this in mind, we implicitly assume the no-anticipation hypothesis, that is, $\delta = 0$.

The fourth and fifth assumptions in the Callaway and Sant'Anna (2021) framework are similar to the classical parallel trends condition on the classic difference in differences setup. The conditional parallel trends based on the never-treated group states that for each treatment group $g \in G$ and each period $t \in \{2, ..., \tau\}$ satisfying[30] $t \geq g$, the expected difference in potential outcomes between two adjacent periods, given the covariates X, should be the same for the "never-treated" and the treatment groups. Mathematically,

$$E[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0)|X, C = 1] \textit{ almost surely}$$

Furthermore, the conditional parallel trends based on "not-yet-treated" groups extend this by stating that for each $g \in G$ and each $(s, t) \in \{2, ..., \tau\} \times \{2, ..., \tau\}$ satisfying $t \geq g$ and $t \leq s < \bar{g}$[31], the expected difference in potential outcomes between two adjacent periods, given the covariates X, should be the same for the "not-yet-treated" and the treatment groups. Formally,

$$E[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0)|X, D_s = 0, G_g = 0] \textit{ almost surely}$$

[29] Actually, if δ is known, it's possible to incorporate anticipation.
[30] Here we are already considering $\delta = 0$. Generically, we could write $t \geq g - \delta$
[31] Similarly, we could generically express it as $t \geq g - \delta$ and $t + \delta \leq s < \bar{g}$.

The fourth hypothesis asserts that conditional on control variables, the average outcome for the group first treated in the respective period, g, and for the never treated group would have followed parallel trends without the program. Similarly, the fifth hypothesis implies parallel paths, conditional on covariates, between the group that entered the program in time g and groups not yet treated by time t. Despite our sample being mainly composed of never-treated individuals, we consider that those are not similar enough to eventually treated groups. AE had specific rules on income, occupations, and household characteristics. Thus, our main estimations consider groups that have not yet been treated as controls. One potential drawback of this choice is that we need to restrict pre-treatment trends across groups, while conditional parallel trends with the never-treated group do not[32] (Callaway; Sant'Anna, 2021). However, considering the monthly frequency of our data and the fact that the major impact of the pandemic shock was concentrated in March and April 2020, we believe that the economic environment did not undergo drastic changes between May and November 2020. Therefore, imposing restrictions on pre-treatment parallel trends should not pose a significant issue in our analysis.

Now let the generalized propensity score be $p_{g,s}(X) = \Pr(G_g = 1|X, G_g = 1 + (1 - D_s)(1 - G_g) = 1)$, where $p_{g,s}$ represents the probability of participating in the AE for the first time at g, conditional on covariates X and depending on being in group g ($G_g = 1$) or being in the not-yet-treated group at time s ($(1 - D_s)(1 - G_g) = 1$). Then, the final condition necessary for identification is the *overlap* hypothesis. This assumption extends the overlap assumption to a setup with multiple groups and periods, ensuring that a positive fraction of the population initiates treatment in period g, and for every g and t, the generalized propensity score is uniformly bounded away from one. Formally, for each $t \in \{2, ..., \tau\}$, $g \in G$, exists some $\epsilon > 0$ such that $\Pr(G_g = 1) > \epsilon$ and $p_{g,t}(X) < 1 - \epsilon$ almost surely. This assumption is crucial to rule out irregular identification scenarios, and it's satisfied given the similar composition of treatment and control groups.

[32] This is because we are assuming $\delta = 0$