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RODRIGO ULIAN MEGALE

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

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RIBEIRÃO PRETO
2024

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Dissertação apresentada ao Programa de Pós-Graduação em Economia – Área: Economia Aplicada da Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto da Universidade de São Paulo, para obtenção do título de Mestre em Ciências. Versão Original.

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RESUMO

MEGALE, Rodrigo Ulian. *Transferências de renda e oferta de trabalho: efeitos de um programa de emergência em larga escala no Brasil.* 2024. 63f. Manual – Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto, Universidade de São Paulo, Ribeirão Preto, 2024.

Programas de transferência de renda tipicamente têm efeitos nulos ou pequenos na oferta de trabalho dos beneficiários. Este estudo contribui para a literatura ao examinar um programa de transferência de renda em larga escala no Brasil durante a pandemia de COVID-19: o Auxílio Emergencial (AE). Este cenário poderia ser propício para que os beneficiários saíssem do mercado de trabalho como forma de proteção contra a pandemia. Nossos resultados para a força de trabalho agregada estão alinhados com a literatura, mostrando consistentemente nenhum efeito da transferência na chance de estar ativo no mercado de trabalho. No entanto, ao desagregar esses efeitos para aqueles inicialmente ativos e inativos no mercado de trabalho no período base, identificou-se efeitos compensatórios para cada grupo, levando ao efeito nulo geral. O benefício impactou positivamente os inativos, primeiro para a informalidade ou desemprego, mas positivamente para a formalidade após 4 meses de exposição. A análise de efeitos heterogêneos revela que esse efeito é ainda mais forte para minorias, como mulheres, não brancos e residentes das regiões Norte e Nordeste. Isso sugere que aqueles fora do mercado de trabalho podem ter usado a ajuda para adquirir ferramentas para trabalho informal ou para buscar empregos melhores a curto prazo. Em oposição, o Auxílio Emergencial reduziu a oferta de trabalho daqueles que já estavam ativos no período base, especialmente entre trabalhadores informais ou desempregados, sendo esse efeito mais pronunciado para mulheres, possivelmente ligado a mães solteiras que receberam o dobro do benefício, e para brancos, provavelmente devido à maior renda total associada a esse grupo. Assim, o programa parece oferecer alguma proteção contra o vírus para este segmento da população, o que é visto como um resultado "positivo"neste contexto.

Palavras-chave: Transferências de renda, Programas de Bem Estar, Oferta de trabalho ,COVID-19
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ABSTRACT

MEGALE, Rodrigo Ulian. Cash transfer and labor supply: effects of a large-scale emergency program in Brazil. 2024. 66f. Manual – Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto, Universidade de São Paulo, Ribeirão Preto, 2024.

Cash transfer programs typically have null or minor effects on beneficiaries' labor supply. This study adds to the literature by examining a large-scale income transfer program in Brazil during the COVID-19 pandemic: the *Auxílio Emergencial* (AE). This scenario could arguably be most conducive for beneficiaries to exit the labor market as a means of pandemic protection. Our findings for the aggregate labor force align with the literature consistently showing no effect of the transfer on the chance of being active in the labor market. Yet, when disaggregating these effects for those initially active and inactive in the labor market during the baseline period, it was identified offsetting effects for each group, leading to the overall null effect. The benefit positively impacted the inactive, first towards informality or unemployment, but positively towards formality after 4 months of exposure. This suggests that those outside the labor market might have used the aid to acquire tools for informal work or to seek better jobs in the short term. Heterogeneous effect analysis reveals this effect is even stronger for minorities, such as women, non-whites, and residents of the North and Northeast regions. Conversely, the *Auxílio Emergencial* reduced the labor supply of those who were already active in the baseline period, particularly among informal workers or the unemployed. This effect is also more pronounced for women, likely linked to single mothers who received double the benefit, and for whites, probably due to the higher total income associated with this group. Thus, the program seems to provide some protection against the virus for this segment of the population, which is seem as a "positive"result in this context.

Keywords:Cash Transfers, Welfare Programs, Labor Supply,COVID-19
JEL: J08 J22 I38

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1 INTRODUCTION

Cash transfer programs have a wide range of impacts in developing countries, such as poverty reduction (Perova; Vakis, 2012; AIR, 2014), improvement of children's nutritional and health levels (Ferré; Sharif, 2014), enhanced food security (Braido; Olinto; Perrone, 2012; Perova; Vakis, 2012; AIR, 2014), and increased school attendance (Perova; Vakis, 2012; AIR, 2014; Brauw et al., 2015b). However, a common concern about cash transfer programs is the possibility of generating "lazy-welfare recipients" (Banerjee et al., 2017) by incentivizing workers to withdraw from the labor force[1].

In the canonical labor market model, cash transfer programs can reduce workers' labor supply through two mechanisms. First, through an income effect. If we consider leisure as a normal good, part of the income increase stemming from the program might lead workers to demand more leisure and, thus, supply less labor. Second, cash transfers might discourage labor supply if they are perceived as a tax on earnings, creating a substitution effect. This is particularly true if workers anticipate that earning more will make them ineligible for these benefits (Banerjee et al., 2017; Araujo et al., 2017; Baird; McKenzie; Özler, 2018). Conversely, in more sophisticated labor market models, cash transfers may positively impact labor supply as they can help families escape the poverty trap by allowing workers to improve their productivity (Dasgupta; Ray, 1986). This monetary injection can also alleviate credit constraints, allowing workers to start their businesses and finance riskier investments, which in turn can increase the labor supply (Baird; McKenzie; Özler, 2018). Moreover, spillover effects can occur, where higher incomes in poorer regions boost sales and, consequently, increase employment (Gerard; Naritomi; Silva, 2021).

Workers may also respond to cash transfers by substituting between the formal and informal sectors in developing countries. Earnings from informal labor are less visible, so workers may be inclined to work longer hours in informality, especially if they perceive that having a formal job would make them ineligible for welfare payments (Araujo et al., 2017). Indeed, studies from developing countries suggest that cash transfer programs do not affect labor supply or total hours worked (Alzúa; Cruces; Ripani, 2013; Brauw et al., 2015a; Banerjee et al., 2017; Araujo et al., 2017; Salehi-Isfahani; Mostafavi-Dehzooei, 2018), but there might be a shift from the formal to the informal sector, or a disincentive for formalization among beneficiaries (Brauw et al., 2015a; Garganta; Gasparini, 2015; Araujo et al., 2017; Bergolo; Cruces, 2021; Gerard; Naritomi; Silva, 2021).

This study contributes to this literature 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. The program aimed to bridge the gap in social protection for informal workers and individual micro-entrepreneurs, potentially covering 117.5

[1] See Bastagli et al. (2016) for a general literature review and Baird, McKenzie and Özler (2018) for a specific review on labor market outcomes.

million Brazilians[2](Hecksher; Foguel, 2022). Eligible recipients were granted monthly cash payments starting from US$120, which corresponds to approximately 66% of Brazil's median income (Levy; Filho, 2022), extending up to US$240 for special groups[3]. In comparison, the world's largest conditional cash transfer program (Gerard; Naritomi; Silva, 2021), the *Programa Bolsa Família* (PBF), supported in 2019 approximately 41.8 million beneficiaries (13.4 million families) with an average benefit[4] of about US$36 (Ministério do Desenvolvimento Social e Agrário, 2019). Thus, AE represents an unprecedented expansion of direct transfers, reaching more individuals than the usual groups targeted by other cash transfer policies and granting a considerably larger income boost to its beneficiaries.

However, not all beneficiaries received 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 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, including 700,000 from PBF, 21.6 million from *Cadastro Único*, and 20.4 million application-based requests (Marins et al., 2021). These factors, combined with the monthly schedule of payments, resulted in a process with a staggered entrance of beneficiaries within the program.

Using data from a large representative national survey (*PnadCovid-19*) that followed every month more than 190,000 households from May to November 2020, we exploit the staggered entrance of beneficiaries into the program to estimate the causal impact of cash transfers on labor supply using the staggered Difference in Differences 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. Our data and research design allow us to estimate the overall program effect on labor supply in the short run and also to disentangle this main effect for the different sub-samples of individuals, including active workers (either formal, informal, or looking for a job), and on individuals that were initially out of the labor force.

Overall, we found a null effect of AE on labor supply. But this does not mean no dynamics were occurring in the labor market. The program led to a considerable reduction in the labor supply of those initially active in the labor market. Conversely, we observed a positive

[2] The authors estimate this coverage by considering whether an individual belongs to a household in which the person themselves or another household member is potentially eligible to receive the benefit. If we consider at the individual level, the authors estimate coverage of 59.2 million individuals.

[3] The transfers began on April 2020, and the benefit's value was cut by half starting in September 2020, with payments continuing until December 2020

[4] PBF transfers value depends on conditionalities as children and adolescents in the households.

effect among those who were initially inactive. These effects in opposite directions explain the absence of any significant overall effect.

Thus, these findings are associated with the ambiguous theoretical effect of transfers on work, suggesting that both the sign and magnitude depend on the program design and prevailing economic conditions (Banerjee et al., 2017). Indeed, our results might be explained by Dasgupta and Ray (1986) poverty trap model. The program appears to have provided sufficient resources, even in the short term, for those inactive in the labor market to start offering labor. Disentangling the results, it's observed that initially inactive individuals predominantly moved into the informal sector or unemployment. Moreover, a significant shift towards formal employment was also noted for those who accessed the program earlier. This could be due to the financial support from the program, which might have allowed them to invest in tools or resources, increasing productivity. Furthermore, the resource might have enabled individuals to search for better jobs in the formal market, creating a job-search effect (Baird; McKenzie; Özler, 2018). In opposition, AE negatively impacted the labor supply for those already in the labor market. Breaking down the results, this effect was notably stronger for those initially unemployed or in the informal sector. Within the poverty trap framework, these individuals may have experienced an increase in their reservation wage, leading them to exit the labor market. While this finding is not commonly reported in the literature, context should also be taken into account. Policymakers, previously concerned about cash transfers potentially discouraging work, intentionally leveraged these disincentives during the pandemic response to keep people out of the labor market (Gentilini, 2022). Therefore, the benefit may have provided this segment of the population with the necessary financial relief to leave the labor force and protect themselves during the sanitary crisis.

A heterogeneity analysis was carried out to evaluate the AE effects on specific demographic groups within the population. Our core results are uniform across gender, ethnicity, and regional disparities. Nevertheless, in terms of productivity gains for labor market entry among the initially inactive, women, non-whites, and residents of Brazil's poorer regions appear to have benefited more from the transfers. Concurrently, actively employed women also exhibited a higher withdrawal from the labor force, likely 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 wage.

Several robustness checks were conducted. The main models were re-estimated considering the never treated as the control group. However, the data suggests systematic differences between individuals eventually treated and those who never participated in the program, indicating non-parallel trends. This issue is addressed using the method developed by Rambachan and Roth (2023), assuming that post-treatment violations are not larger than pre-treatment. This choice relies on the fact that the COVID-19 earliest shocks were in March and April, thus, there is no reason to believe that differences in trends would be higher in late periods. The core results remain consistent, though they approach the threshold of significance when adjusting for the maximum pre-treatment trend. Similarly, estimates were made considering the "treatment memory", that is, once treated, individuals remain treated. Our results remain but with smaller magnitudes. Models exclusively for those initially self-employed were also made. This particular test is interesting because it avoids reverse causality problems, as self-employed workers are not subject to dismissal. Despite insignificant effects on hours worked, a larger negative effect was found for this group of the population on labor force participation. Lastly, to better understand if there is any particular effect for the group treated in May - which is the "always treated" group in our main models - we estimate AE impact using data from the first quarter of 2019 and the first quarter of 2020. The problems with these estimates are the long temporal space for the first case, which could result in long-term labor market dynamics, and the small sample size in the second case. When considering these individuals, our results are insignificant regardless of the initial labor market status, although the signs of the coefficients are consistent.

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

Standard economic models predict that cash transfers decrease work and earnings due to the income-leisure trade-off. As leisure is a normal good, cash transfer recipients demand more leisure, reducing labor supply (Baird; McKenzie; Özler, 2018). In the seminal approach proposed by Becker (1965), time allocated to non-working activities is incorporated into agents' decisions. Households combine time and market goods to produce commodities that enter directly into their utility in such a way as to maximize this utility subject to a budget constraint. Time allocated to consumption results in a "forgone income", creating indirect production costs for these commodities. Thus, total marginal costs are determined by marginal direct costs - the marginal costs of market goods producing commodities- and indirect costs, represented by the marginal costs of the time spent on production. With an increase in other money income sources - in our case, a cash transfer- time allocated to consumption increases for most commodities, leading to a reduction in hours worked. The only case when hours worked could increase with a rise in non-working income is when relatively time-intensive commodities were "sufficiently inferior[1]".

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).

In low-income countries, different constraints are in play. Dasgupta and Ray (1986) elaborated a poverty trap theory, linking involuntary unemployment with malnutrition incidence and consequently to production and income distribution. Involuntary unemployment occurs because even without missing markets, market clearing in the labor market may not happen. Land and labor power, both owned by workers, are inputs in production. Labor power is endogenously

[1] By reducing consumption of time-intensive goods, the time previously allocated to these goods becomes available, allowing individuals to increase their labor supply in order to acquire more desirable goods.

determined in the model, being increasingly dependent on consumption; that is, a positive relationship exists between an individual's consumption and productivity. The "efficiency piece rate" is then calculated based on the wage-labor power ratio, determining the minimum wage per unit of labor power for each individual[2]. People with no assets don't get income from property or investments and struggle more in the labor market. While wealthier individuals - those with more assets - might not be employed due to a high reservation wage, leading to an efficiency piece rate above market value, people without assets have a higher efficiency piece rate not due to a lack of willingness to work but as a direct consequence of their reduced food intake and diminished productivity. Hence, the most impoverished individuals might not fulfill the labor quality demanded by the market or, despite being capable, find themselves without employment due to the scarcity of jobs, resulting in malnutrition[3]. Transfers increasing non-wage income emerge as an intervention to elevate productivity by allowing workers to escape the poverty trap and significantly mitigate involuntary unemployment and malnutrition rates[4].

Incorporating missing markets into the analysis, Baird, McKenzie and Özler (2018) uncover additional mechanisms that cash transfers can positively impact labor supply, particularly for those with limited credit access. They highlight the self-employment liquidity effect, which enables recipients to start or grow businesses. Additionally, the insurance effect encourages taking on riskier, potentially more lucrative activities by lowering failure fears. Lastly, the labor search effect supports extended job searches for better matches, possibly reducing employment in the short term but leading to long-term gains. This suggests cash transfers can significantly influence economic behavior and outcomes by easing liquidity constraints.

The importance of these programs is on the rise in less developed regions. A global review by World Bank (2015) reveals that 130 nations have implemented some form of unconditional cash transfer (UCT) program, with 64 adopting conditional cash transfer programs (CCT). Such social safety nets are now the most common program in the developing world, despite not serving the largest number of beneficiaries[5]. Most empirical literature documents insignificant or negligible effects of cash transfer programs on the likelihood of employment or hours worked in these nations. Banerjee et al. (2017) re-analyzed five CCT[6] and two UCT programs[7]. Their results highlighted that only the program with the least generous transfer[8] showed a significant change—a 3% decrease in employment probability at the 10 percent significance level. In

2 Conditional on the constraint that he is willing to work for that wage

3 The model hypothesizes that landless individuals (the asset in the model) do not die of starvation, they experience malnutrition if they are unable to find employment.

4 See Dasgupta and Ray (1987) for policy applications.

5 Together, UCT and CCT benefit approximately 718 million individuals; however, in-kind assistance and fee waivers have the largest group of beneficiaries.

6 These programs were: PRAF II (Programa de Asignación Familiar - Phase II) in Honduras, Progresa in Mexico, the Pantawid Pamilyang Pilipino Program (PPPP) in the Philippines, Program Keluarga Harapan (PKH) in Indonesia, and the Red de Protección Social (RPS) in Nicaragua

7 Programa de Apoyo Alimentario (PAL), in Mexico and the Tayssir program in Morocco, which included two possible treatments: a CCT and a "labeled" cash transfer, with conditions suggested but not strictly enforced

8 Honduran PRAF II

contrast, no significant impact was observed on weekly hours worked in any of the programs[9]. Similarly, Alzúa, Cruces and Ripani (2013) analyzed the effects of three of these CCT programs[10] on households' labor supply. Their findings also indicate that these programs generally had a small and statistically insignificant impact on the adult labor supply. However, they provide additional insights, as PROGRESA in Mexico led to a modest increase in work hours for female beneficiaries, notable wage growth for male beneficiaries, and an increase in household labor income after two years and decreased female labor-force participation in ineligible households within the program's localities. In the poverty trap framework, this does not necessarily mean there are no labor market movements. Assetless individuals might be escaping the poverty trap. Still, at the same time, some wealthier workers who receive the benefit could be leaving the labor market due to a higher reservation wage.

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 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[11].

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. (2015a) 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

[9] Indonesian PKH did not have available information on hours worked.

[10] PROGRESA, RPS, and PRAF

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

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.

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.

AE was approved at the end of March 2020[2] and swiftly 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).

The Brazilian government adopted criteria 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, with the exception of 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 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.

A critical characteristic of the AE in this analysis is the program's staggered entry. The fast implementation led to many registration difficulties (Marins et al., 2021) that can explain this

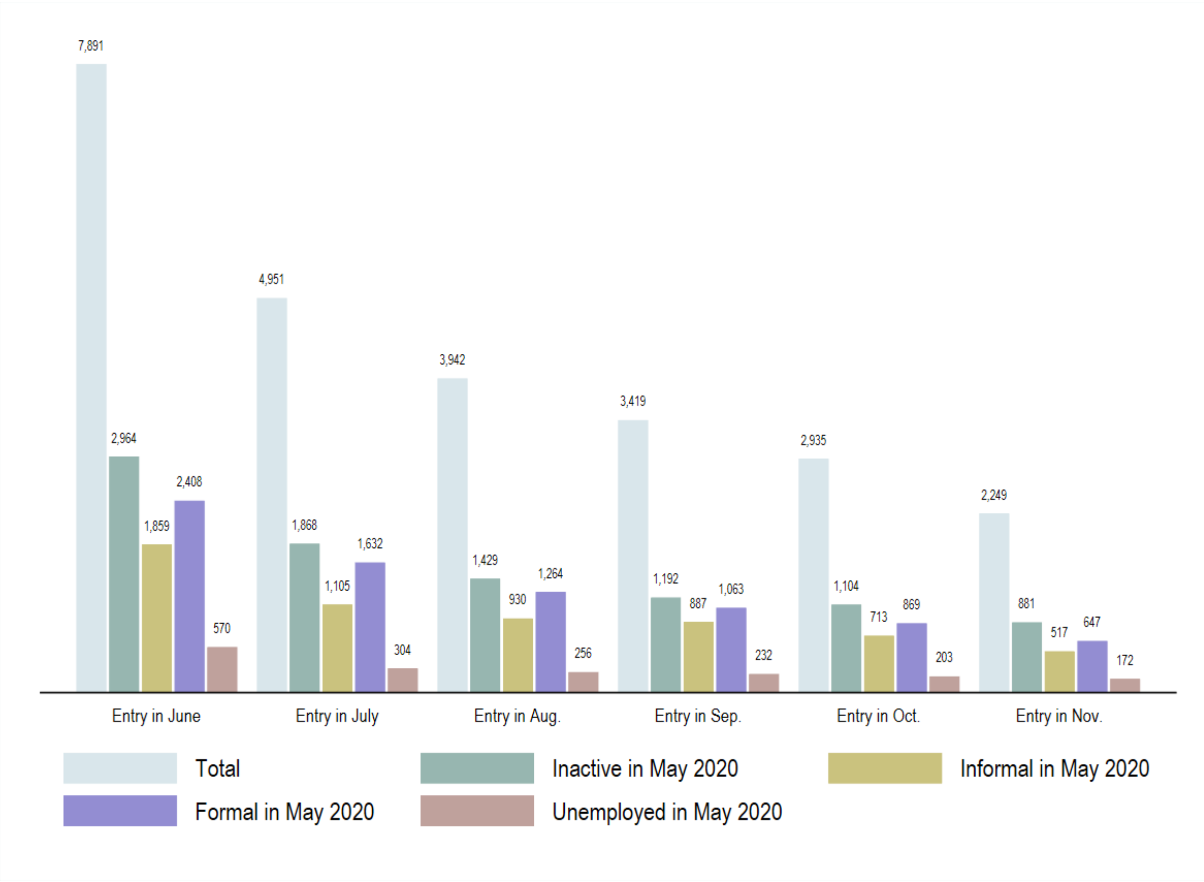
[1] The World Health Organization (WHO) reported approximately 700,000 deaths from COVID-19 in the country by the end of 2022.

[2] Source: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

variation in treatment timing. Figure 1 shows new entries in the program for each work status group in the initial period. Despite the downward trend over the months of 2020, we observe that every month, many individuals were added as new beneficiaries.

Figure 1 – Staggered entries into the AE by initial work status



Note: The figure displays the staggered entries of individuals over time. The sample selection and definitions of formality, informality, and unemployment will be presented in the next section.

A recent study by Menezes-Filho, Komatsu and Rosa (2021) shows the program's 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.

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[1] 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 Labor Organization (ILO) definition of informality [2], 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.

Evidence suggests that the employment dynamics during this period were notably affected by the pandemic, as illustrated in Table 1. A significant portion of the population exited the labor force, leading to a fall in unemployment from 2019 first quarter to May 2020, similar to Menezes-Filho, Komatsu and Rosa (2021) findings[3]. One plausible explanation for this could be that wealthier individuals opted to cease working to comply with pandemic safety guidelines. On the other hand, given the economic downturn, the rise in the number of people leaving the workforce could be attributed to diminished expectations of finding employment, causing many to stop seeking work. Additionally, we observe that the share of workers in the informal sector[4] experienced a decline at the pandemic onset. This could be indicative that informal jobs, typically associated with worse working conditions (Williams; Horodnic, 2019), might be more vulnerable to the initial economic and sanitary crisis compared to formal employment. Furthermore, the data shows stable gender, ethnicity, and household head proportions, as expected. Age and education levels saw marginal increases, consistent with tracking individuals over time.

Figure 2 presents the participation rates in AE, disaggregated by employment categories in the labor market. The graph highlights that the highest participation rates are observed in categories traditionally associated with informal employment—family care or production for personal consumption, domestic workers, self-employed, and non-remunerated workers. These

[1] Regular PNADC has a quarterly frequency

[2] *"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"*

[3] The differences in magnitude likely occur because the authors consider all individuals over 14 years of age.

[4] See that our informality proxy is pretty close to IBGE (2020) indicators.

Table 1 – Adult population summary statistics by period

	1° quarter 2019	May 2020	June 2020	July 2020	Aug. 2020	Sep. 2020	Oct. 2020	Nov.2020
Out of Labor Force	0.26	0.33	0.33	0.34	0.33	0.32	0.31	0.31
Unemployed	0.13	0.10	0.12	0.12	0.13	0.13	0.13	0.13
Formal	0.58	0.64	0.64	0.65	0.65	0.64	0.64	0.64
Informal	0.42	0.36	0.36	0.35	0.35	0.36	0.36	0.36
Woman	0.51	0.51	0.51	0.51	0.51	0.51	0.51	0.51
White	0.42	0.43	0.43	0.43	0.43	0.43	0.43	0.43
Age	38.73	38.99	39.00	39.03	39.05	39.06	39.08	39.09
No schooling	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Incomplete Elementary School	0.25	0.20	0.20	0.20	0.20	0.20	0.20	0.20
Complete Elementary School	0.08	0.08	0.08	0.07	0.07	0.07	0.07	0.07
Incomplete High School	0.07	0.09	0.09	0.09	0.09	0.09	0.09	0.09
Complete High School	0.33	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Incomplete Graduation	0.07	0.09	0.08	0.09	0.09	0.09	0.09	0.09
Complete Graduation or more	0.16	0.18	0.18	0.18	0.19	0.18	0.18	0.18
Householder	0.43	0.41	0.41	0.41	0.41	0.41	0.41	0.41
N	357,461	229,027	249,372	250,785	251,973	252,362	248,056	248,785
Sum of Weights	137,179,136	138,258,240	138,420,960	138,384,144	138,471,312	138,520,480	138,671,712	138,797,552

The table displays descriptive statistics for the adult population aged 18 to 65 years, using weights to ensure representativenes.
Variables include employment status and demographic characteristics, among others.
Employees in the private sector without a formal contract, domestic employees without a formal contract are classified as informal.
Employers or self-employed without social security contributions derived from work, and working family assistants are also considered as informal workers.
Source: National Household Sample Survey (PNAD) and the PNAD COVID-19.

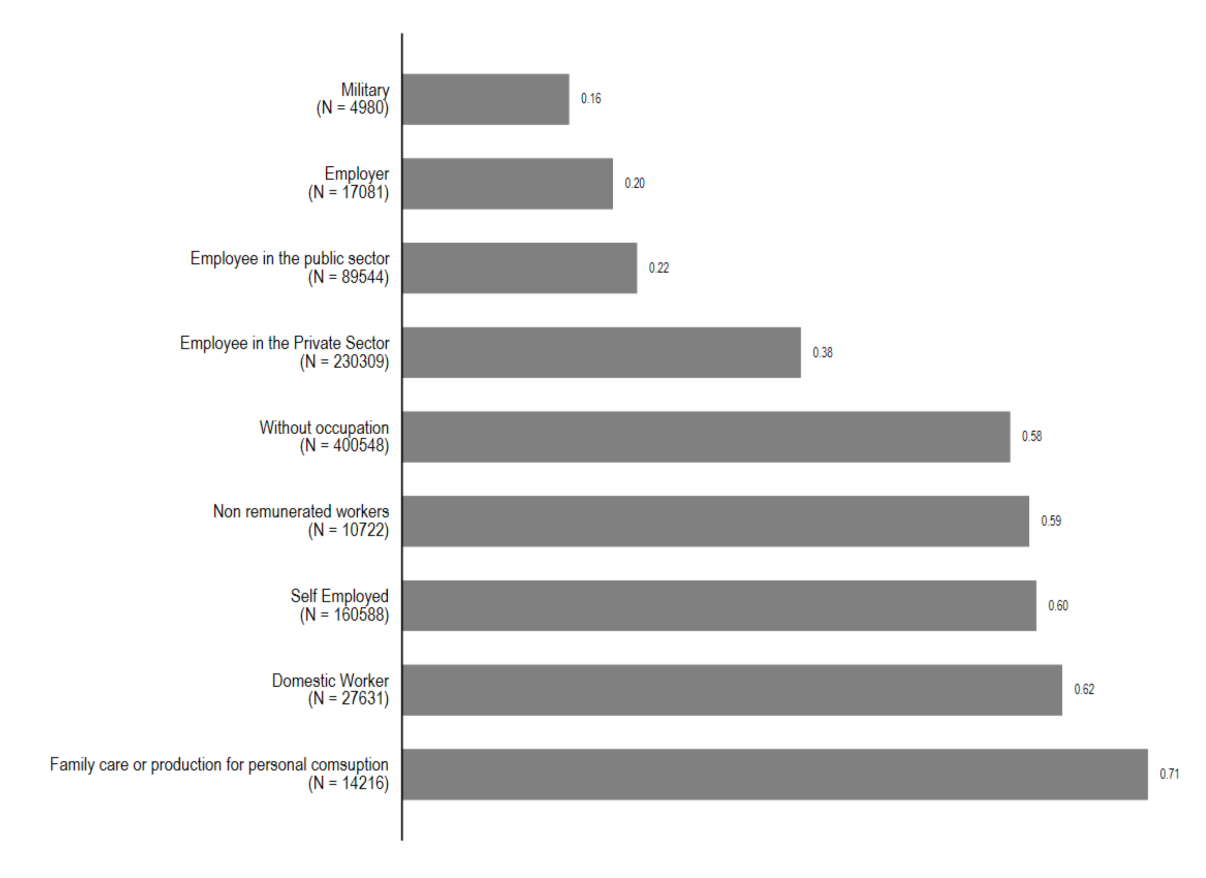
categories represent the target demographic that AE aims to support, which aligns with the program's objective to bridge the social protection gap for informal workers and individual micro-entrepreneurs. However, more formal employment categories, such as employees in the private sector and public sector, showed considerable participation rates, which is explained, in theory, by the fact that AE transfers were reported at the household level.

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[5]. Third, we further exclude individuals

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

Figure 2 – AE participation by work category



Note: The figure displays the participation in the program by general occupation categories. The receive of the benefit is reported at household level. Data from *PNAD-Covid19*.

who received the benefit but stopped it at some point[6]. 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[7].

The summary statistics of our final sample are presented in Table 2. Systematic differences are observed between the never-treated group and the treated groups. These differences are evident in both labor market variables – with the never-treated group having a lower proportion of people out of the labor force, unemployed, or in informal employment – and socioeconomic variables, as there is a higher percentage of white individuals, higher education levels, and income in the never-treated group. However, it appears that groups treated at different times are relatively similar, which might indicate that variations in treatment timing are not related to these covariates.

This argument gains further support from the means test available in Table 3. The first column shows the mean differences between the eventually-treated and the never-treated groups. The subsequent columns display the differences between each treated group - defined by their entry month - and the other groups of eventually treated individuals. It is noted that there are

[6] This exclusion led to the removal of about 21,892 individuals, approximately 16% of the sample.
[7] 3 individuals were dropped due to missing information on ethnicity.

differences for all covariates between the never-treated group and the eventually-treated group. This is not necessarily a problem, as we only require parallel trends (not identical ones) for identification. However, it is evident that these individuals may not be an ideal counterfactual for the treatment groups. On the other hand, it is observed that the eventually treated groups are considerably similar to each other. The group with the most significant differences in covariates was the June group, however, these values are considerably small, indicating that groups treated at different times can be used to produce reasonable counterfactuals.

Table 2 – Descriptive Statistics by group

	(1) Never Treated	(2) June entry	(3) July entry	(4) Aug. entry	(5) Sep. entry	(6) Oct. entry	(7) Nov.entry
Out of Labor Force	0.31	0.39	0.38	0.35	0.35	0.37	0.37
	(0.463)	(0.487)	(0.484)	(0.477)	(0.476)	(0.483)	(0.483)
Unemployed	0.05	0.15	0.14	0.15	0.14	0.15	0.14
	(0.216)	(0.360)	(0.344)	(0.359)	(0.346)	(0.353)	(0.343)
Informal	0.20	0.48	0.43	0.42	0.43	0.37	0.37
	(0.398)	(0.500)	(0.496)	(0.493)	(0.495)	(0.483)	(0.484)
Woman	0.53	0.53	0.53	0.51	0.52	0.54	0.53
	(0.499)	(0.499)	(0.499)	(0.500)	(0.500)	(0.499)	(0.499)
White	0.55	0.40	0.41	0.46	0.42	0.48	0.48
	(0.497)	(0.490)	(0.492)	(0.498)	(0.494)	(0.500)	(0.500)
Age	43.58	40.38	40.56	40.73	41.17	41.26	41.50
	(13.54)	(13.60)	(13.79)	(13.49)	(13.78)	(14.16)	(14.26)
No schooling	0.02	0.02	0.02	0.03	0.03	0.03	0.03
	(0.142)	(0.152)	(0.137)	(0.158)	(0.162)	(0.167)	(0.164)
Incomplete Elementary School	0.16	0.25	0.25	0.22	0.25	0.24	0.25
	(0.367)	(0.435)	(0.432)	(0.416)	(0.433)	(0.427)	(0.434)
Complete Elementary School	0.06	0.09	0.09	0.09	0.07	0.08	0.07
	(0.245)	(0.288)	(0.280)	(0.286)	(0.263)	(0.275)	(0.254)
Incomplete High School	0.06	0.11	0.09	0.09	0.10	0.08	0.10
	(0.228)	(0.308)	(0.293)	(0.285)	(0.294)	(0.270)	(0.295)
Complete High School	0.29	0.34	0.35	0.37	0.32	0.32	0.32
	(0.454)	(0.473)	(0.476)	(0.482)	(0.468)	(0.465)	(0.466)
Incomplete Graduation	0.09	0.08	0.09	0.09	0.08	0.11	0.08
	(0.292)	(0.276)	(0.285)	(0.280)	(0.277)	(0.318)	(0.265)
Complete Graduation or more	0.32	0.10	0.12	0.12	0.14	0.14	0.16
	(0.465)	(0.306)	(0.319)	(0.326)	(0.351)	(0.348)	(0.367)
Householder	0.46	0.40	0.39	0.40	0.40	0.40	0.40
	(0.498)	(0.490)	(0.488)	(0.491)	(0.491)	(0.490)	(0.490)
Total Income	3446.99	1367.08	1569.61	1556.22	1921.04	1853.35	1869.37
	(4170.3)	(1665.5)	(1906.9)	(1894.3)	(3094.5)	(1787.9)	(1855.3)
Number of individuals	55,757	5,759	3,032	1,630	1,028	809	515

The table displays descriptive statistics for the groups in the sample.
The group is defined by the month of entry into the program or, in the case of the never-treated group, by not entering.
Informal definition is the same as in Table 1
Source: National Household Sample Survey (PNAD) and PNAD COVID-19.

Table 3 – Mean Differences Test by Group

	ET-NT	June-ET	July-ET	Aug.- ET	Sep.- ET	Oct.- ET	Nov - ET
Woman	**-0.01**	0.01	-0.00	**-0.02**	-0.01	0.01	0.00
White	**-0.13**	**-0.04**	**-0.01**	**0.05**	0.01	**0.06**	**0.06**
Age	**-2.95**	**-0.46**	-0.10	0.11	**0.59**	**0.68**	**0.91**
No schooling	**0.00**	0.00	**-0.01**	0.00	0.00	0.01	0.00
Incomplete Elementary School	**0.09**	**0.01**	0.00	**-0.03**	0.00	-0.01	0.00
Complete Elementary School	**0.02**	**0.01**	-0.00	0.00	**-0.01**	-0.01	**-0.02**
Incomplete High School	**0.04**	**0.01**	-0.00	**-0.01**	-0.00	**-0.02**	-0.00
Complete High School	**0.05**	-0.01	**0.01**	**0.03**	**-0.02**	**-0.03**	**-0.02**
Incomplete Graduation	**-0.01**	**-0.01**	0.00	-0.00	-0.00	**0.03**	-0.01
Complete Graduation or more	**-0.20**	**-0.02**	-0.00	0.00	**0.03**	**0.03**	**0.04**
Householder	**-0.06**	0.00	-0.01	0.00	0.00	-0.00	0.00
Total Income	**-1912.06**	**-305.66**	**45.47**	24.41	**419.91**	**339.96**	**348.49**
N	479710	89411	89411	89411	89411	89411	89411

The table displays t-tests for differences in means for the socioeconomic covariates.

The group is defined by the month of entry into the program or, in the case of the never-treated group, by not entering.

NT stands for “Never-Treated” and ET for “Eventually Treated”.

Bold numbers indicate significance at the 5% level or higher.

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 seen as a natural approach to identify 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[1] (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).

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 group-time average treatment effect, which represents the average treatment effect for group g at time t, where a group is defined by the first treatment period.

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.

Now 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 $\bar{g} = \max_{i=1,...,\tau} G_i$, 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

[1] 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.

$$Y_{it} = Y_{it}(0) + \sum_{g=2}^{\tau}(Y_{it}(g) - Y_{it}(0)) \cdot G_{i,g} \tag{5.1}$$

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

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1] \tag{5.2}$$

To identify 5.2, 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[2]. 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[3] $t \geq g$, 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 $t \leq s < \bar{g}$[4], 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,

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

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 fol-

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

lowed 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[5]. 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[6] (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[7].

5.1 Estimation

With these assumptions fulfilled, the parameters in (5.2) 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 correctly specified model 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. If irreversibility of treatment, random sampling, no anticipation, conditional parallel trends based on not yet treated groups, and overlap assumptions

[5] Some evidence of that is also presented in Table 3
[6] This is because we are assuming $\delta = 0$
[7] See Table 2

hold, then[8]

$$\text{ATT}_{dr}(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}}{\mathbb{E}\left[\frac{p_{g,t}(X)(1-D_t)(1-G_g)}{1-p_{g,t}(X)}\right]}\right)(Y_t - Y_{g-1} - m_{g,t}(X))\right] \tag{5.3}$$

$$= \text{ATT}(g,t)$$

To calculate these estimates, two steps are necessary. 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[9].

In our analysis, the covariate set X comprises mostly time-invariant characteristics. This set includes gender, race (white indicator), age, and educational attainment[10], 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 5.2. A notable feature of our methodology is its capacity to generate 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 [11]

$$\theta_{es}(e) = \sum_{g \in \mathbb{G}} \mathbf{1}\{g + e \leq \tau\} \Pr(G = g | G + e \leq \tau) \text{ATT}(g, g+e) \tag{5.4}$$

[8] Here we are already considering $\delta = 0$. See Callaway and Sant'Anna (2021) Theorem 1.

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

[10] 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.

[11] Note in (3) that the immediate effect of the treatment occurs for $e = 0$.

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[12]. 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

$$\theta_{sel}(g) = \frac{1}{\tau - g + 1} \sum_{t=g}^{\tau} \mathrm{ATT}(g, t) \tag{5.5}$$

Then, the single average effect is calculated by

$$\theta_{sel}^{O} = \sum_{g \in \mathbb{G}} \theta_{sel}(g) \Pr(G = g | G \leq \tau) \tag{5.6}$$

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.

[12] For more details, see sections 3.1.1 and 3.2 in Callaway and Sant'Anna (2021)

6 RESULTS

Our main result is illustrated in Figure 3, which shows AE effect on the likelihood of being active[1] in the workforce by length of exposure for the entire sample, as well as segmented by initial labor force status[2]. As previously discussed, our main identification strategy relies on 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 3, 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 3 confirms no pre-treatment labor supply behavior differences between treatment and control groups, reinforcing our identification approach[3]. Also, this provides support for the no anticipation hypothesis.

Our findings across the entire sample align with recent studies on cash transfers. As depicted in Figure 3, 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 participation rate by 5.2% at the maximum observed after two exposure periods. Conversely, for individuals already employed, the program negatively affected their active labor market participation, with a peak reduction of 3.2% after two months in the program. Our findings show that both impacts diminish over time, although the accuracy of the estimates decreases. This is probably linked to the halving of the transfer value from the fifth month onward in the sample, resulting in a new amount that may not be sufficient for inactive individuals to enter the labor market or for active individuals to find it compelling enough to withdraw the labor supply.

This result is further supported in Table 11, which shows the group aggregated effect of the AE (equation 5.6) on labor market participation across each sample. The program did not impact the overall labor supply. However, it increased the probability of workforce entry by 3.91% for those initially outside the labor market. In contrast, for individuals already in the labor market, there was a decrease in participation by 1.46%, though this finding is on the boundaries of statistical significance.

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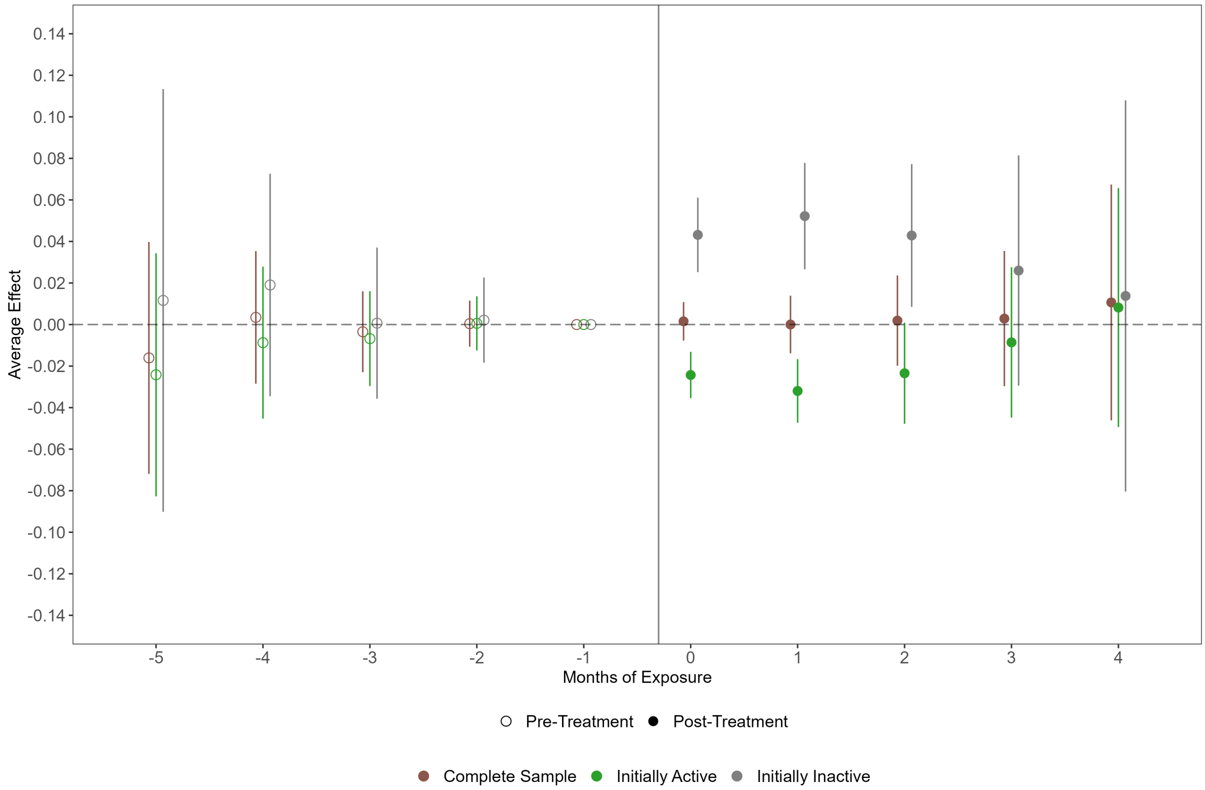
[1] Individuals looking for a job or employed are considered as active in the labor force.

[2] See Table 11 in the appendix for detailed results.

[3] We chose the period before treatment as the fixed baseline to get an interpretation similar to the TWFE event study (Roth, 2024).

Figure 3 – 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*.

Table 4 – Group 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	0.0057	0.0072	[-0.0084, 0.0198]
Initially Active	-0.0146	0.0078	[-0.0299, 0.0006]
Initially Inactive	0.0391	0.0112	[0.0172, 0.0611]

The table displays the aggregated group parameter in Equation 5.6.

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

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

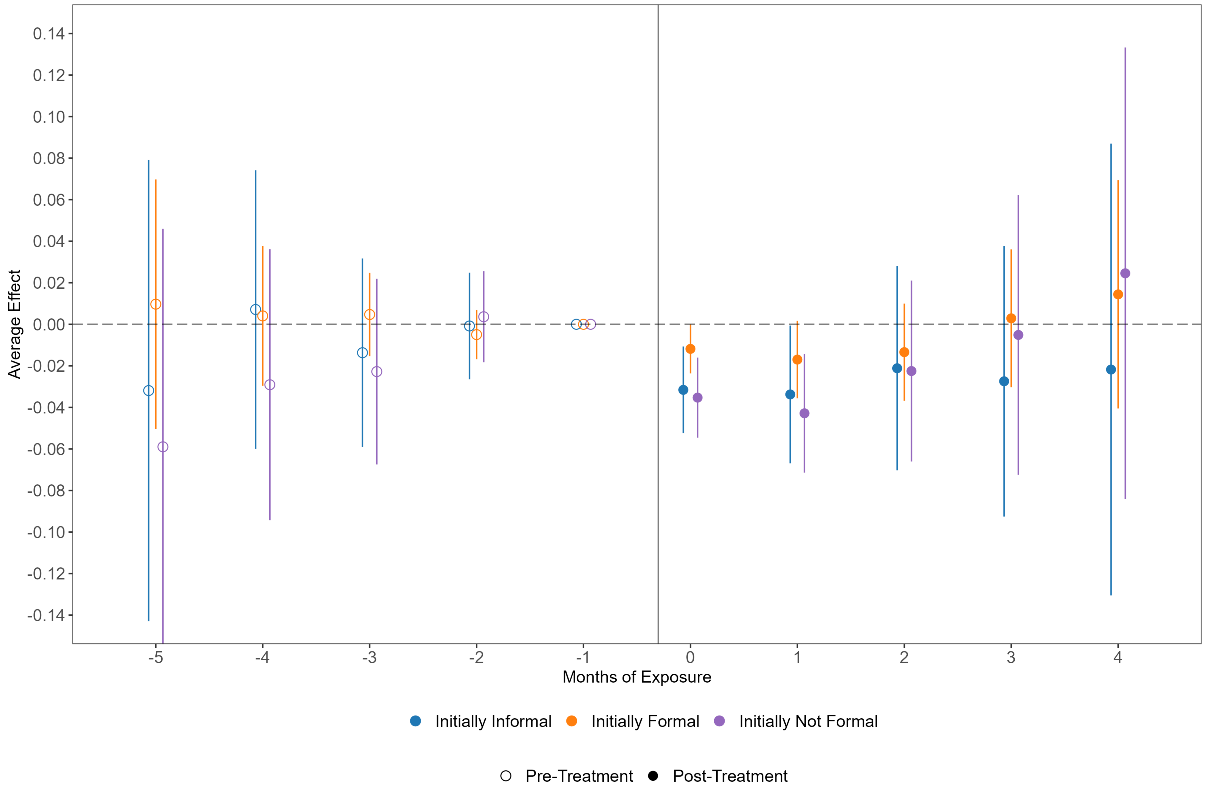
Source: PNAD COVID-19.

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 4, 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, 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 12 of the appendix, but our statistical power significantly decreased due to the smaller sample size. Our findings indicate that the AE had small effects for initially formally employed individuals. The only significant effect detected was an immediate reduction of 1.2%. For informal workers, the effect remained significant across two exposure periods, peaking at a reduction of 3.4%. Our estimates are even higher for non-formal individuals, with a negative impact up to 4.3% after two exposure periods. The aggregated effects were not significant for any of the cases. However, our estimates were still higher for the informal worker sample, where significance was only achieved at a 10% level, as available in Table 5.

If we had reverse causality, with workers applying for aid after losing their jobs, one would expect our estimates not to be significantly higher for non-formal workers, as informal workers are mainly self-employed[4]. This effect on non-formals might be related to the occupations in this sector, especially for low-skilled occupations associated with lower income - which might be the case for informal workers in our sample, as the AE is an income means-tested program- that could face higher risks from working due to the virus.

[4] In our baseline period, approximately 48% of initially informal workers were self-employed.

Figure 4 – 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*.

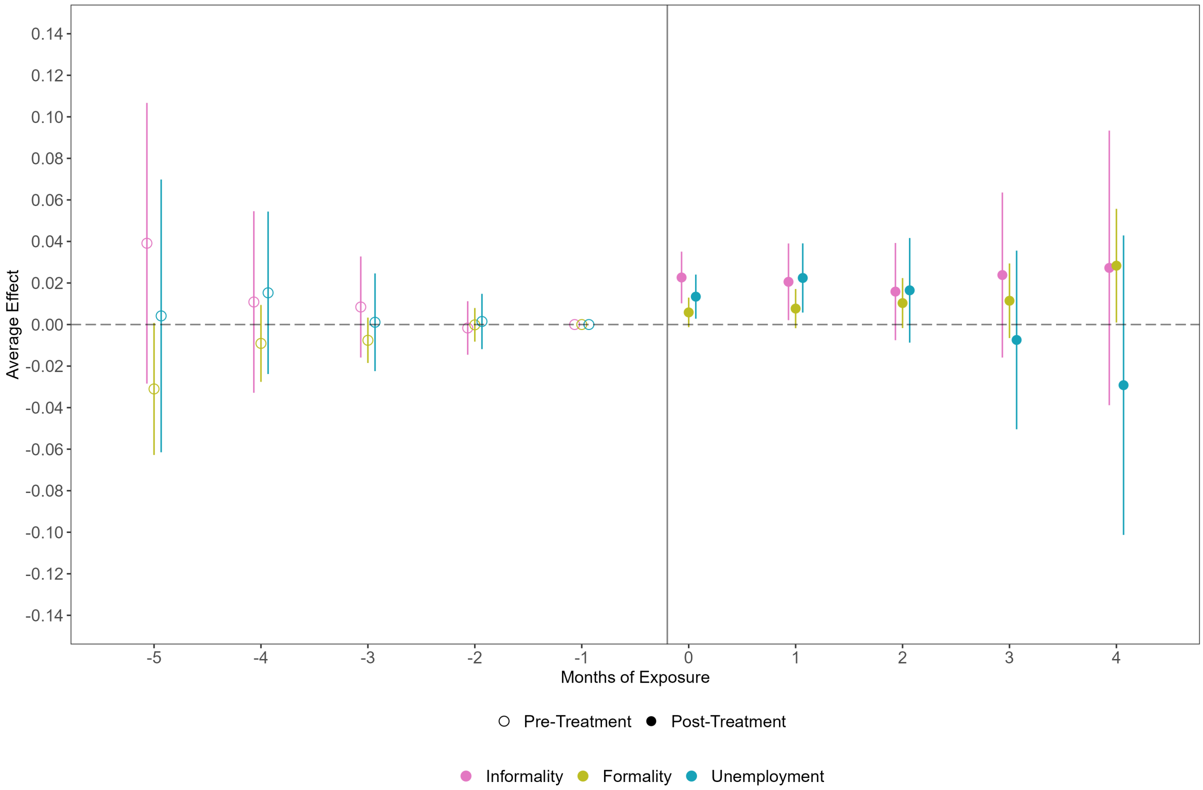
Table 5 – Group aggregated effect of the AE on the likelihood of offering work for groups of initially active individuals

Sample	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Formal	-0.0035	0.008	[-0.0193, 0.0122]
Informal	-0.024	0.014	[-0.0514, 0.0035]
Not Formal	-0.0191	0.0135	[-0.0456, 0.0074]

The table displays the aggregated group parameter in Equation 5.6.
Dependent Variable: Labor offer (employment or job seeking).
Estimates for initial formal, informal, and unemployed individuals
Source: *PNAD Covid-19*.

Similarly, in Figure 5, we examine AE impact on labor market transitions for those previously outside the labor force, highlighting shifts into informal work, formal employment, and unemployment[5]. Our estimates show that the AE prompted a 2.27% rise in informal employment at the first exposure to the program. Moreover, the program has positively influenced job search activities, with an initial increase up to 2.24%. Despite this pattern that might imply that the transfers initially facilitated access primarily to less stable employment options, we identify a compelling trend among early participants (those who joined in June 2020), showing a 2.83% effect to move toward formal employment after five exposure periods. This observation is supported by aggregated results in Table 6, documenting a 2.34% increase in informality and a 1.1% increase in formal employment without affecting aggregated unemployment.

Figure 5 – 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*.

[5] See Table 13 of the appendix

Table 6 – Group aggregated effect of the AE on each labor market transitions for initially inactive individuals

Dependent Variable	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Informality	0.0234	0.0076	[0.0084, 0.0383]
Formality	0.011	0.0037	[0.0037, 0.0182]
Unemployment	0.0066	0.0088	[-0.0107, 0.0238]

The table displays the aggregated group parameter in Equation 5.6.

Dependent Variables: Informal work, Formal work or Unemployment.

Estimates for individuals initially out of labor force.

Source: *PNAD Covid-19*.

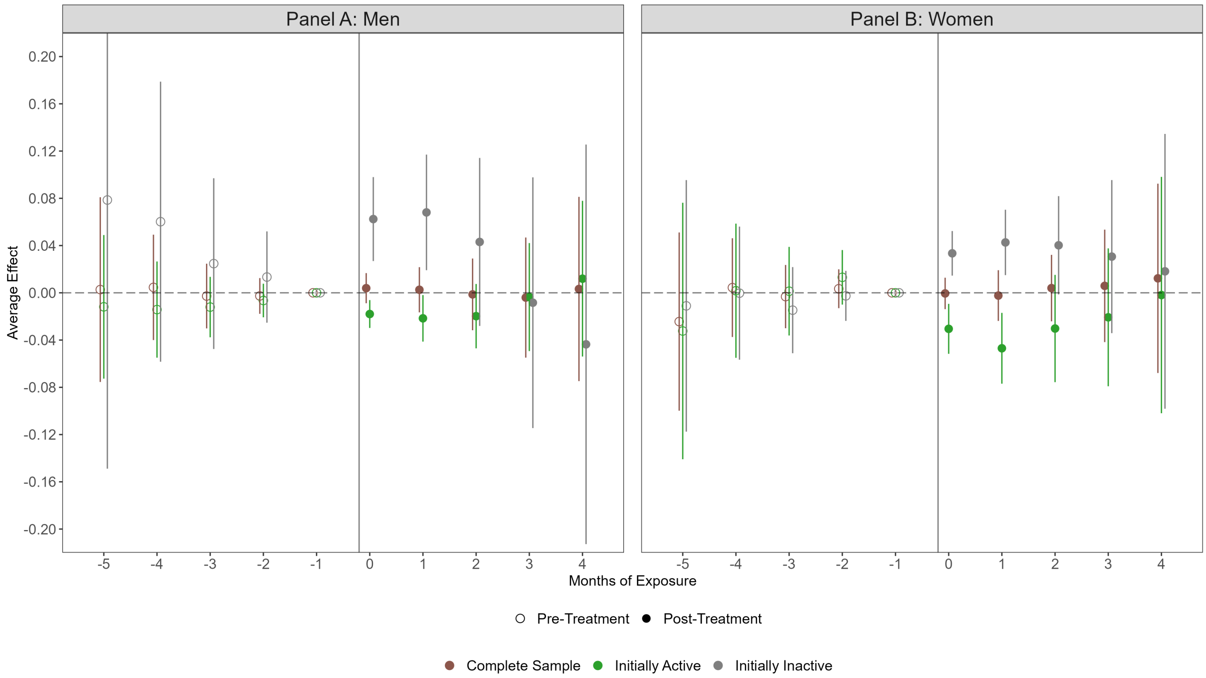
A possible explanation for these results lies in the poverty trap theory proposed by Dasgupta and Ray (1986). Individuals initially outside the labor market may have decided to stop seeking employment during our baseline period because they could not meet the market's quality demands or, even if qualified, 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. For those already in the labor market, this 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.

6.1 Heterogeneous effects

Under the specific conditions of the pandemic and the AE design, its impact on labor market participation likely varied across different groups. Particularly, single mothers received twice the standard AE benefit, which potentially altered their labor market behavior. 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[6], as illustrated in Figure 6. Yet, we note specific variations. Among initially active workers, women saw up to a 4.7% reduction in their likelihood to offer work, in contrast to a maximum reduction of 2.2% observed for men. Additionally, for those initially inactive, the increase in labor force participation was 4.3% for women compared to 6.8% for men. However, inactive women were affected by one more exposure month compared to their male counterparts (three versus two event times). This is reflected in the aggregated parameters, available in Table 7. The only significant aggregated effect was found among active women, who saw a 3.72% increase in their likelihood of offering work.

6 See Table 14 of the appendix.

Figure 6 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Men vs Women



Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts disaggregated by gender and initial labor market status for each event-time period. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market and gender 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 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*.

Table 7 – Group aggregated effect of the AE on the likelihood of offering work by gender and initial labor force status

Sample	**ATT**	**Std. Error**	**[95% Conf. Int.]**
		Men	
Complete Sample	0.0017	0.0103	[-0.0185, 0.0218]
Initially Active	-0.0117	0.0084	[-0.028, 0.0047]
Initially Inactive	0.0325	0.0241	[-0.0147, 0.0797]
		Women	
Complete Sample	0.0083	0.0103	[-0.0119, 0.0285]
Initially Active	-0.0194	0.0138	[-0.0465, 0.0077]
Initially Inactive	0.0372	0.0133	[0.0111, 0.0633]

The table displays the aggregated group parameter in Equation 5.6.

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

Estimates for genders for the complete sample and different initial statuses in the labor market

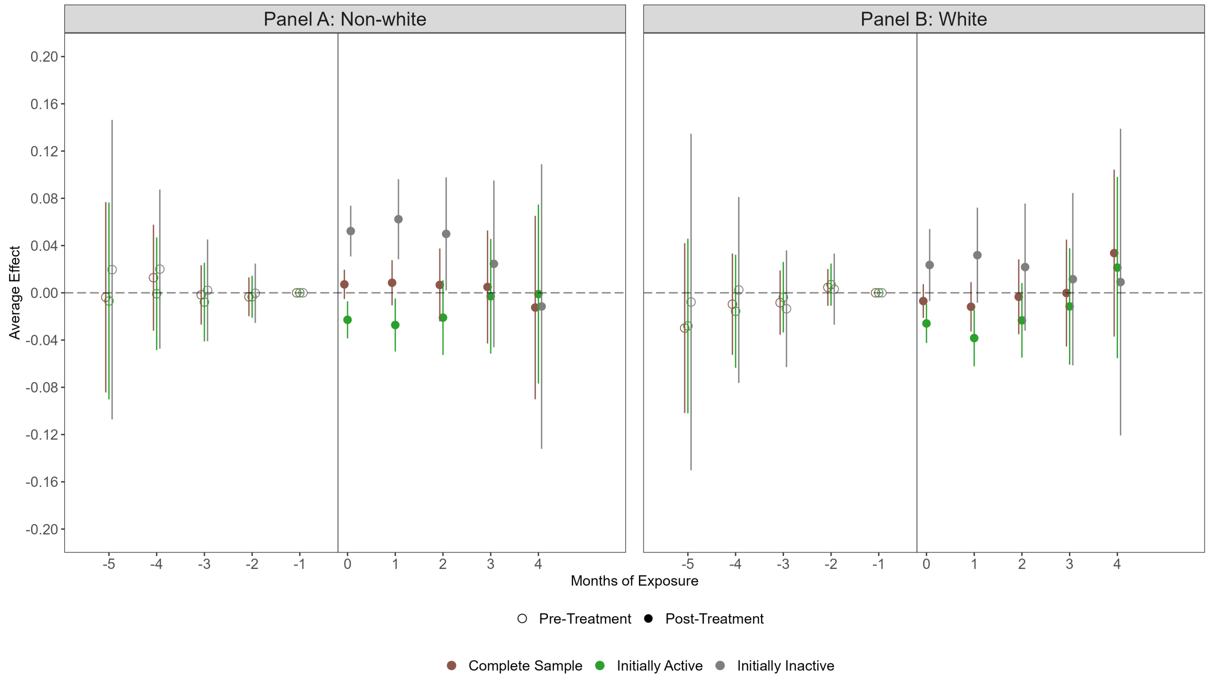
Source: *PNAD Covid-19*.

This diversity in the labor market response to the program is also observed among different minority groups. Figure 7 shows AE's effect broken down between non-white and white individuals[7]. Our findings[8] again revealed no significant effects on the overall and negative impact for initially active individuals in the labor market across ethnic groups. Notably, the decrease was more pronounced for active whites, with a magnitude of up to 4%, compared to non-whites, for whom the reduction was up to 2.7%. Interestingly, non-white who were initially inactive experienced a significant positive effect on their likelihood of working for three event times, peaking at 6.2%, while no significant effects were observed for whites across any event times. Overall, inactive non-whites increased their labor participation by 4.11%, as demonstrated in Table 8.

[7] In our sample, non-white individuals had an average income of around US$423, and white individuals about US$686.26

[8] See Table 15

Figure 7 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Non-white vs. White



Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts disaggregated by ethnic groups and initial labor market status for each event-time period. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market and ethnic group 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, 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*.

Table 8 – Group aggregated effect of the AE on the likelihood of offering work by ethnic groups and initial labor force status

Sample	**ATT**	**Std. Error**	**[95% Conf. Int.]**
		Non-white	
Complete Sample	0.0069	0.0091	[-0.011, 0.0248]
Initially Active	-0.0136	0.0107	[-0.0347, 0.0074]
Initially Inactive	0.0411	0.0159	[0.0099, 0.0723]
		White	
Complete Sample	0.0024	0.0087	[-0.0146, 0.0195]
Initially Active	-0.0159	0.0098	[-0.0352, 0.0034]
Initially Inactive	0.0262	0.0166	[-0.0063, 0.0586]

The table displays the aggregated group parameter in Equation 5.6.

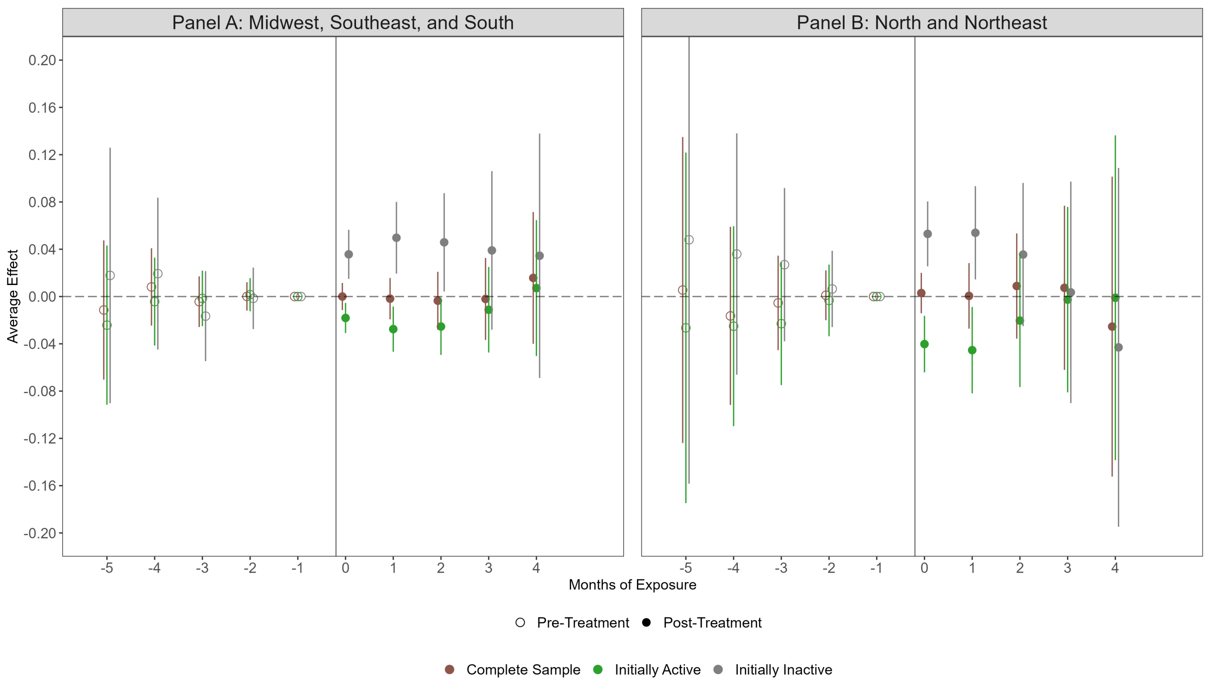
Dependent Variable: Labor offer (employment or job seeking).

Estimates for ethnic groups for the complete sample and different initial statuses in the labor market

Source: PNAD COVID-19.

Figure 8 shows the main results by geographic region, contrasting the North and Northeast against the South, Southeast, and Midwest. This distinction is made based on the understanding that the North and Northeast represent the poorest regions in Brazil, with a larger informal sector[9]. The analysis explores how regional economic disparities relate to cash transfers and labor market decisions. The main results were consistent across the disaggregated regions[10], yet there were larger magnitudes observed, both for the positive effect on inactive individuals (5% vs 5.4%) and the negative effect on active ones (2.8% vs 4.5%) in the North and Northeast. The aggregated single parameter in Table 9 shows similar results across regions. However, for both, it was on the edge of significance.

Figure 8 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: Midwest, Southeast, and South vs. North and Northeast



Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts disaggregated by regional groups and initial labor market status for each event-time period. Control groups are not-yet-treated beneficiaries from the corresponding initial labor market and regional group 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*.

[9] Our proxy for informality is 39.51% for the North and Northeast, with an average income of approximately US$405, in contrast to an informality rate of 24.23% and average income of US$624 for the South, Southeast, and Midwest regions.

[10] See Appendix Table 16 for detailed results.

Table 9 – Group aggregated effect of the AE on the likelihood of offering work by residence regions and initial labor force status

Sample	ATT	Std. Error	[95% Conf. Int.]
South, Southeast, and Midwest			
Complete Sample	0.0037	0.0077	[-0.0115, 0.0208]
Initially Active	-0.0126	0.0081	[-0.0285, 0.0032]
Initially Inactive	0.0395	0.0125	[0.0151, 0.0639]
North and Northeast			
Complete Sample	0.0037	0.0143	[-0.0242, 0.0317]
Initially Active	-0.0222	0.0166	[-0.0547, 0.0104]
Initially Inactive	0.0329	0.019	[-0.0043, 0.0702]

The table displays the aggregated group parameter in Equation 5.6.

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

Estimates for regions of residence for the complete sample and different initial statuses in the labor market

Source: PNAD COVID-19.

Together, our heterogeneous findings suggest that AE may have primarily benefited minorities for inactive individuals in terms of productivity gains. Women, non-white individuals, and people from the country's poorer regions who were initially inactive seem to have gained the most from the program in terms of acquiring the necessary resources to escape the poverty trap. This outcome is indeed expected in this framework, as transfers to lower-income individuals tend to have a greater effect on consumption, thereby potentially increasing these individuals' productivity.

This observation is also valid for individuals who were initially active in the labor market. Women, including single mothers who received twice the standard benefit amount, might have had an increased opportunity to withdraw from work and focus on domestic and childcare responsibilities during the pandemic. Additionally, the program led to a more substantial reduction in labor force participation among individuals typically engaged in low-skilled informal work, particularly in the North and Northeast regions. Conversely, the exit from the labor force was more pronounced among whites, likely due to their higher incomes, which corresponds with higher reservation wages.

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

$$\Delta(M) = \{\delta : \forall t \geq 0, |\delta_{t+1} - \delta_t| \leq M \cdot \max_{j<0} |\delta_{j+1} - \delta_j|\}. \tag{6.1}$$

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 $\theta^{lb}_e(\beta, \Delta)$ and upper bounds $\theta^{ub}_e(\beta, \Delta)$ given by[11].

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

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

Our sensitivity analysis results are illustrated in Figure 9. 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 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.

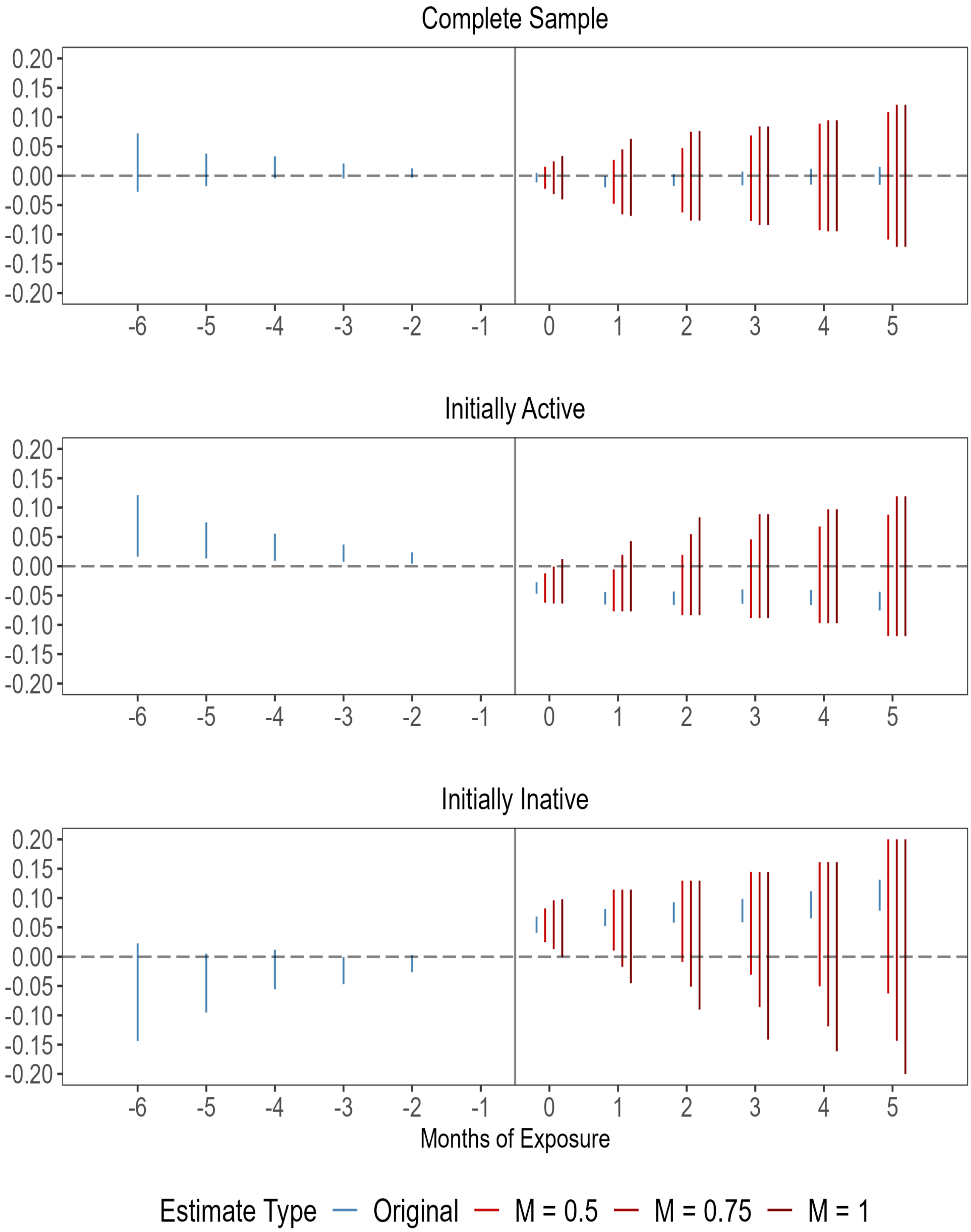
The selection of M reflects the specific circumstances under investigation. Recognizing that the initial impact of the pandemic was most pronounced during March and April and tended to decrease in the following months, it is assumed $M \in \{0.5, 0.75, 1\}$, presupposing that pre-treatment shocks did not exceed post-treatment shocks[12]. The results for the complete sample consistently show a clear absence of general AE impacts on the labor force. Additionally, we

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

[12] For detailed analysis on the varying values of M, see Tables 17, 18, and 19.

observe that at the time of exposure, the respective positive and negative effects of the AE on the likelihood of entering the labor market for inactive and active individuals were maintained, albeit marginally significant when assuming $M = 1$ is considered, representing the maximal anticipated shock.

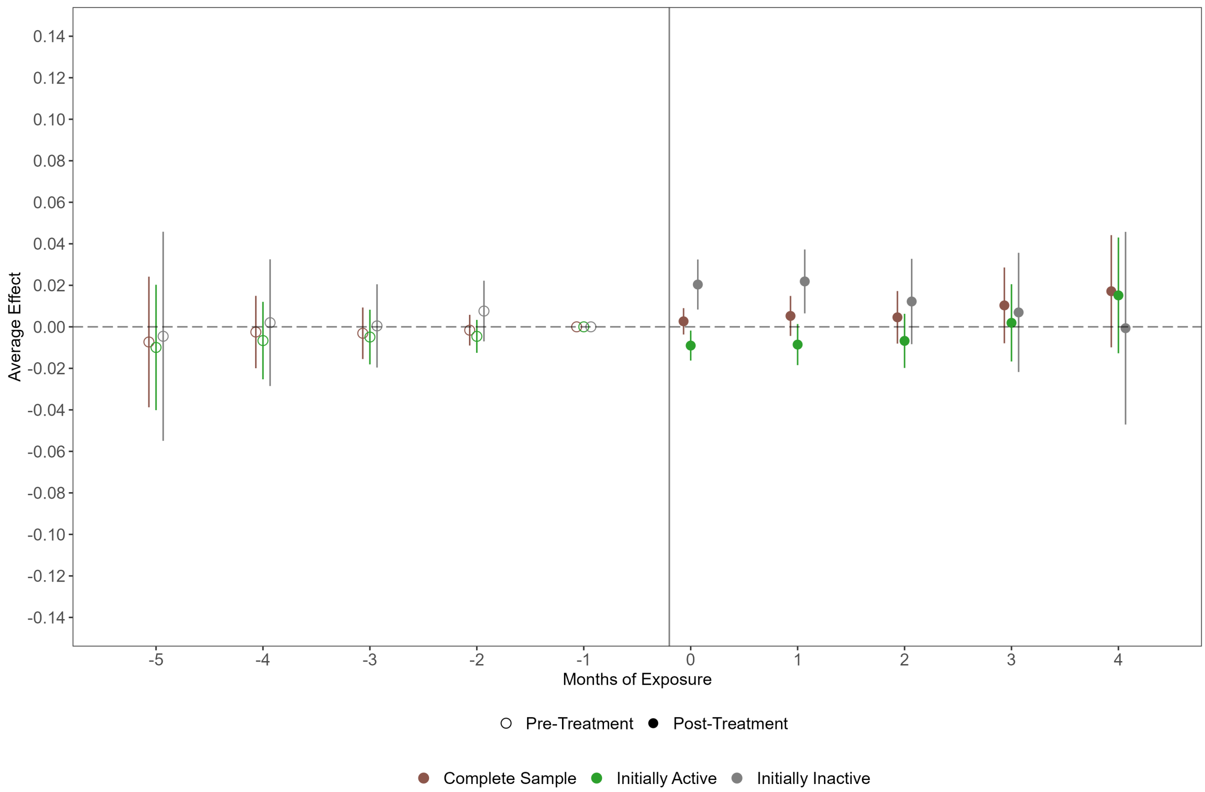
Figure 9 – Sensitivity analysis: Never-treated as control group



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 10, our principal findings hold even when including these individuals[13]. However, we identify weaker effects for both inactive and active individuals. Our calculations suggest an immediate reduction of about 1% for the initially active and an increase after two exposure periods of up to 2.3% for the inactive.

Figure 10 – 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*.

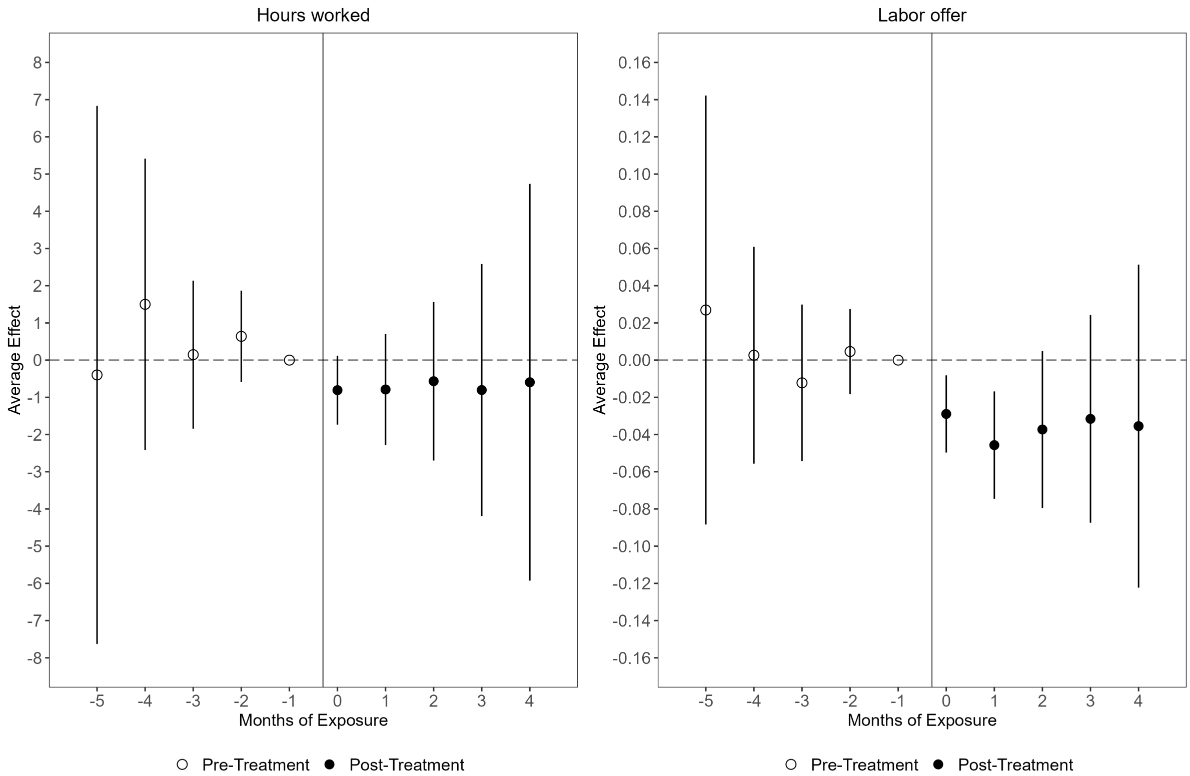
Third, we turn our attention to how initially self-employed individuals responded to the AE in terms of labor supply. Self-employed workers are particularly noteworthy due to their generally less restrictive work contracts and greater flexibility and are less likely to lose their jobs to receive the benefit, so we avoid reverse causality. Figure 11 illustrates AE's impact on both the chance of being active in the labor force and the number of hours worked weekly[14]. Although the negatives estimates on hours worked were at significance margin, we identified an

[13] See Table 20

[14] See Table 21.

immediate 2.8% up to 4.5% impact on labor force participation. When aggregating for a single parameter, we estimate a 3.07% reduction in self-employed labor offer. Thus, this finding asserts the robustness of our results for initially active individuals.

Figure 11 – AE effect on the likelihood of offering work by months of exposure for initially self-employed individuals: Hours worked and Labor offer



Notes: Each circle shows the estimated ATT, respectively, for hours worked and labor market participation averaged across treatment adoption cohorts specifically for initially self-employed workers. 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 self-employed workers. 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*.

Table 10 – Group aggregated effect of the AE on hours worked and the likelihood of offering work for self-employed workers.

Dependent Variable	**ATT**	**Std. Error**	**[95% Conf. Int.]**
Hours worked	-0.9247	0.6781	[-2.2536, 0.4043]
Labor offer	-0.0307	0.0123	[-0.0547, -0.0066]

The table displays the aggregated group parameter in Equation 5.6.

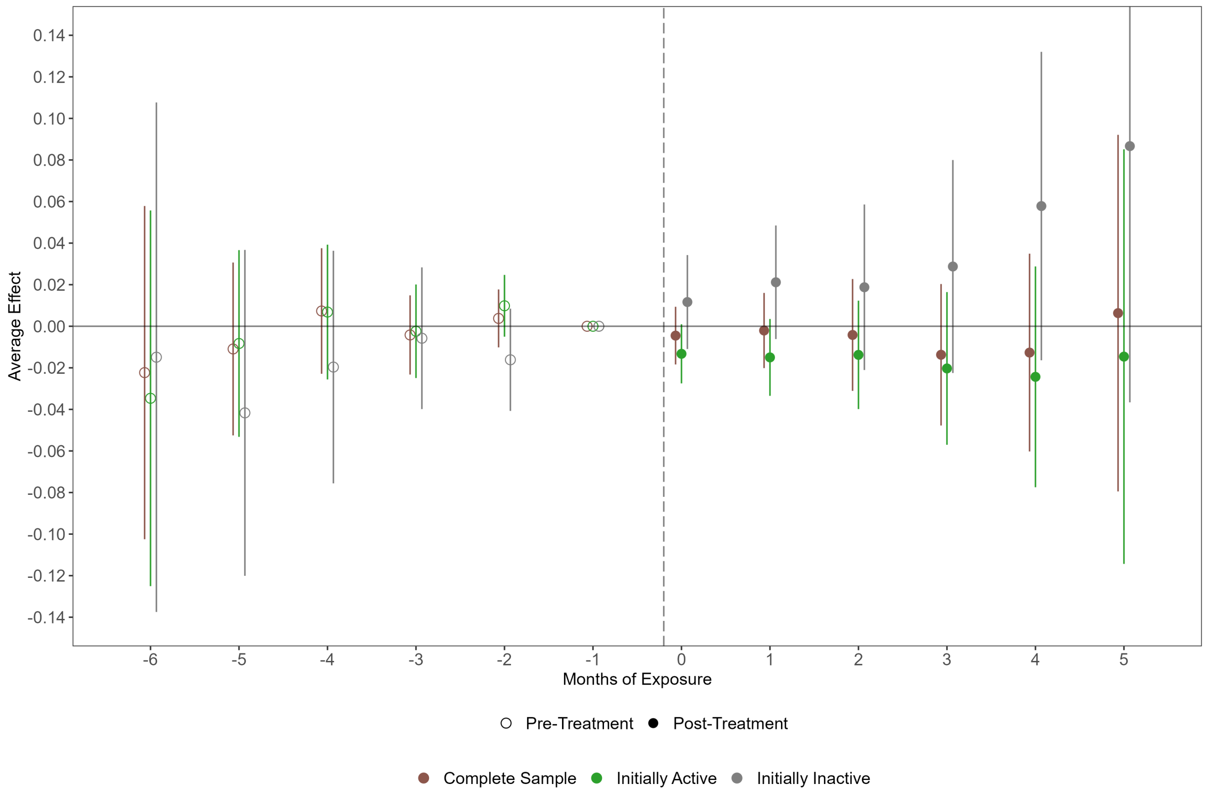
Dependent Variables: Hours actually worked and Labor offer (employment or job seeking).

Estimates for individuals initially self-employed

Source: PNAD COVID-19.

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 through two approaches. The first utilizes 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. The estimates in Figure 12 show that our results' directions stay consistent, though they are at the threshold of significance.[15].

Figure 12 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: First quarter 2019 PNADC data



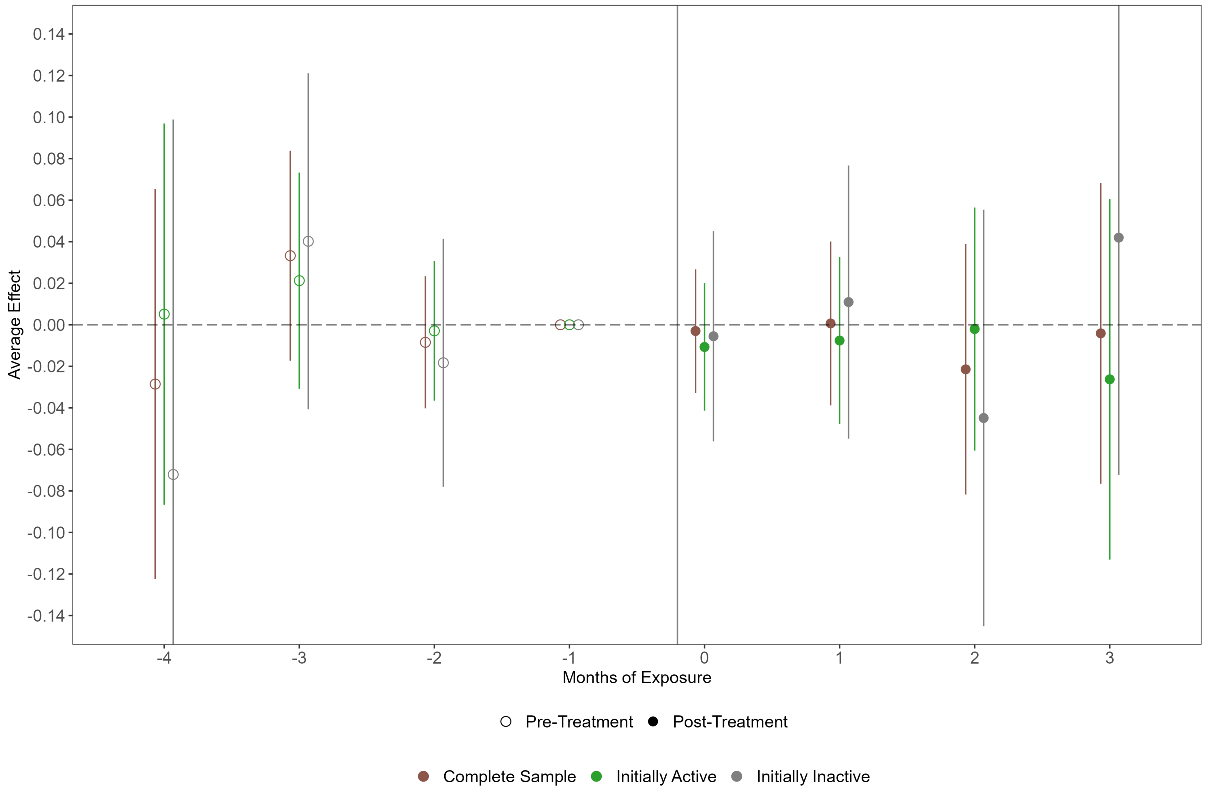
Notes: Each circle shows the estimated ATT for labor market participation averaged across treatment adoption cohorts and initial labor market status for each event-time period. The baseline period in these estimates is the first quarter of 2019, allowing for the estimation of treatment for those who 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*.

Similarly, the same models were estimated considering only participants identified in the first quarter of 2020 PNADC data. As the PNADC has a rotating feature, where each household remains in the sample for five consecutive quarters, identification is only achievable for those who had their first interview in the first quarter of 2019. Consequently, although we avoid long-term labor market dynamics, a drawback of this method is the considerable sample loss. Indeed, only 105 individuals are treated in the last month (November 2020). Therefore, we limit our

[15] See Table 22 for details.

model estimation to include data only until August 2020 (the benefit was halved in the following month), where the September, October, and November groups are considered as "never treated", similar to the approach we applied to the November group before. The results[16], did not show any significant coefficient, with patterns we hadn't seen before, as illustrated in Figure 13. This likely reflects the attrition present in the sample, making these results inconclusive.

Figure 13 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: First quarter 2020 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 2020, allowing for the estimation of treatment for those who entered the program in May 2020. The sample period was shortened only until August 2020 due to the low number of identified controls. This specific month was chosen because, in theory, the transfers were reduced by half starting from September 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*.

[16] See Table 23

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, 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. However, this effect is null in the latter months, probably linked to the value of the transfer that was halved, which no longer allowed workers to cease working.

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. However, we also observe a positive shift towards formal employment among inactive beneficiaries exposed to the program for more months. 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 have emerged. Women who were initially active in the workforce exhibited a greater tendency to withdraw than their male counterparts, likely linked to the higher payments provided to single mothers. This reveals that the program likely incentivized women, especially single mothers, to remain at home during the pandemic, allowing them to do domestic work and childcare. However, upon assessing the impact on the initially inactive population, it was observed that although the effect sizes for men were more pronounced in the initial months following exposure, the effects for women persisted significantly over a longer duration. Notably, there was a discernible shift in the pattern for men in the later months of exposure, although our estimates are less precise.

Among ethnic groups, initially active white participants showed a more marked 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 provoked a larger decline in work participation due to a higher reservation wage. In contrast, for those initially out of the labor force, the effects were inverse in both magnitude and sign. The program had strong and consistent effects on labor force entry for non-whites and non-significant effects for whites. Lower-income groups tend to consume more with income boosts, which can lead to productivity gains in the poverty trap framework and thus enhance the employability of this segment of the population.

Regional differences in responses to the *Auxílio Emergencial* were also evident. Higher magnitudes, particularly regarding labor force withdrawal among initially active individuals, were estimated for residents of the North and Northeast regions of Brazil, where there is a higher prevalence of low-paying informal work. Therefore, the program may have also provided greater protection from the virus for workers in precarious occupations who would likely 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 and the first quarter of 2020. No significant effects were found, though estimates with the 2019 PNADC data showed signs consistent with those previously estimated, and the 2020 PNADC data were noisy.

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

Table 11 – 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
-5	-0.016	-0.024	0.012
	(0.021)	(0.022)	(0.037)
-4	0.003	-0.009	0.019
	(0.012)	(0.013)	(0.020)
-3	-0.003	-0.007	0.001
	(0.007)	(0.009)	(0.013)
-2	0.000	0.001	0.002
	(0.004)	(0.005)	(0.007)
-1			
0	0.002	-0.024*	0.043*
	(0.004)	(0.004)	(0.006)
1	0.000	-0.032*	0.052*
	(0.005)	(0.006)	(0.010)
2	0.002	-0.023	0.043*
	(0.008)	(0.009)	(0.013)
3	0.003	-0.009	0.026
	(0.012)	(0.014)	(0.020)
4	0.011	0.008	0.014
	(0.022)	(0.023)	(0.037)

The table displays the event study parameters in Equation 5.4.

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 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 for groups of initially active individuals

Event Time	Formal	Informal	Not Formal	Unemployed
-5	0.010	-0.032	-0.059	-0.118
	(0.023)	(0.042)	(0.037)	(0.090)
-4	0.004	0.007	-0.029	-0.103
	(0.012)	(0.023)	(0.023)	(0.061)
-3	0.005	-0.014	-0.023	-0.035
	(0.008)	(0.016)	(0.015)	(0.040)
-2	-0.005	-0.001	0.004	0.025
	(0.005)	(0.010)	(0.009)	(0.022)
-1				
0	-0.012*	-0.032*	-0.035*	-0.046
	(0.004)	(0.007)	(0.007)	(0.017)
1	-0.017	-0.034*	-0.043*	-0.063
	(0.007)	(0.013)	(0.010)	(0.024)
2	-0.013	-0.021	-0.023	-0.021
	(0.009)	(0.020)	(0.016)	(0.034)
3	0.003	-0.027	-0.005	-0.012
	(0.012)	(0.026)	(0.026)	(0.058)
4	0.014	-0.022	0.025	0.114
	(0.020)	(0.038)	(0.042)	(0.086)

The table displays the event study parameters in Equation 5.4

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

Estimates for the initially formal, informal, "not formal", and unemployed

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 13 – AE effect on the likelihood of each labor market transitions by months of exposure for initially inactive individuals.

Event Time	Informality	Formality	Unemployment
-5	0.0391	-0.0310*	0.0042
	(0.0267)	(0.0118)	(0.0241)
-4	0.0109	-0.0091	0.0153
	(0.0155)	(0.0074)	(0.0139)
-3	0.0084	-0.0076	0.0011
	(0.0088)	(0.0046)	(0.0087)
-2	-0.0017	-0.0001	0.0015
	(0.0052)	(0.0033)	(0.0049)
-1			
0	0.0227*	0.0058	0.0134*
	(0.0044)	(0.0028)	(0.0043)
1	0.0206*	0.0077	0.0224*
	(0.0062)	(0.0040)	(0.0060)
2	0.0159	0.0103	0.0165
	(0.0091)	(0.0048)	(0.0095)
3	0.0238	0.0114	-0.0074
	(0.0139)	(0.0071)	(0.0168)
4	0.0273	0.0283*	-0.0292
	(0.0227)	(0.0105)	(0.0297)

The table displays the event study parameters in Equation 5.4.

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 14 – 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
-5	0.003	-0.012	0.079	-0.024	-0.032	-0.011
	(0.030)	(0.024)	(0.080)	(0.029)	(0.038)	(0.039)
-4	0.005	-0.014	0.060	0.004	0.002	-0.000
	(0.017)	(0.014)	(0.045)	(0.015)	(0.020)	(0.021)
-3	-0.003	-0.012	0.025	-0.003	0.001	-0.015
	(0.010)	(0.010)	(0.027)	(0.010)	(0.013)	(0.014)
-2	-0.003	-0.007	0.013	0.003	0.013	-0.003
	(0.006)	(0.006)	(0.015)	(0.006)	(0.009)	(0.008)
-1						
0	0.004	-0.018*	0.062*	-0.001	-0.030*	0.033*
	(0.005)	(0.004)	(0.013)	(0.005)	(0.008)	(0.007)
1	0.003	-0.022*	0.068*	-0.002	-0.047*	0.043*
	(0.008)	(0.007)	(0.018)	(0.008)	(0.011)	(0.010)
2	-0.001	-0.020	0.043	0.004	-0.030	0.040*
	(0.012)	(0.010)	(0.026)	(0.011)	(0.015)	(0.015)
3	-0.004	-0.004	-0.008	0.006	-0.021	0.031
	(0.019)	(0.016)	(0.036)	(0.018)	(0.023)	(0.023)
4	0.003	0.012	-0.044	0.012	-0.002	0.018
	(0.031)	(0.026)	(0.061)	(0.030)	(0.038)	(0.042)

The table displays the event study parameters in Equation 5.4.

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 15 – 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
-5	-0.004	-0.007	0.020	-0.030	-0.028	-0.004
	(0.030)	(0.034)	(0.045)	(0.029)	(0.027)	(0.052)
-4	0.013	-0.001	0.020	-0.010	-0.015	0.008
	(0.017)	(0.019)	(0.026)	(0.016)	(0.017)	(0.029)
-3	-0.002	-0.008	0.002	-0.008	-0.004	-0.010
	(0.010)	(0.012)	(0.016)	(0.010)	(0.010)	(0.017)
-2	-0.003	-0.003	-0.000	0.005	0.006	0.004
	(0.006)	(0.007)	(0.009)	(0.006)	(0.006)	(0.011)
-1						
0	0.007	-0.023*	0.052*	-0.007	-0.027*	0.028
	(0.005)	(0.006)	(0.008)	(0.005)	(0.006)	(0.011)
1	0.009	-0.027*	0.062*	-0.012	-0.040*	0.037
	(0.007)	(0.009)	(0.011)	(0.008)	(0.008)	(0.014)
2	0.007	-0.021	0.050*	-0.003	-0.025	0.027
	(0.011)	(0.012)	(0.017)	(0.011)	(0.013)	(0.020)
3	0.005	-0.003	0.025	-0.000	-0.014	0.016
	(0.018)	(0.020)	(0.027)	(0.016)	(0.019)	(0.028)
4	-0.012	-0.001	-0.012	0.034	0.018	0.014
	(0.030)	(0.028)	(0.044)	(0.025)	(0.027)	(0.044)

The table displays the event study parameters in Equation 5.4.

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 16 – 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
-5	-0.011	-0.024	0.018	0.006	-0.027	0.048
	(0.024)	(0.025)	(0.040)	(0.048)	(0.058)	(0.073)
-4	0.008	-0.004	0.019	-0.016	-0.025	0.036
	(0.013)	(0.014)	(0.025)	(0.025)	(0.032)	(0.040)
-3	-0.004	-0.002	-0.017	-0.005	-0.023	0.027
	(0.008)	(0.009)	(0.016)	(0.014)	(0.019)	(0.026)
-2	0.000	0.002	-0.002	0.001	-0.003	0.006
	(0.004)	(0.005)	(0.009)	(0.008)	(0.011)	(0.013)
-1						
0	0.000	-0.018*	0.036*	0.003	-0.040*	0.053*
	(0.004)	(0.005)	(0.008)	(0.007)	(0.009)	(0.010)
1	-0.002	-0.028*	0.050*	0.001	-0.045*	0.054*
	(0.006)	(0.007)	(0.012)	(0.010)	(0.013)	(0.013)
2	-0.004	-0.025*	0.046	0.009	-0.020	0.036
	(0.009)	(0.009)	(0.017)	(0.017)	(0.021)	(0.026)
3	-0.002	-0.011	0.039	0.007	-0.003	0.004
	(0.013)	(0.013)	(0.026)	(0.026)	(0.030)	(0.036)
4	0.016	0.007	0.034	-0.025	-0.001	-0.043
	(0.021)	(0.023)	(0.039)	(0.043)	(0.051)	(0.056)

The table displays the event study parameters in Equation 5.4.

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

Event time	Original	M = 0.5	M = 0.75	M = 1
-6	[-0.026 , 0.071]			
-5	[-0.019 , 0.039]			
-4	[-0.006 , 0.035]			
-3	[-0.006 , 0.022]			
-2	[-0.003 , 0.013]			
-1				
0	[-0.0113 , 0.0049]	[-0.0222 , 0.0154]	[-0.0313 , 0.0245]	[-0.0404 , 0.0336]
1	[-0.0203 , -1e-04]	[-0.0477 , 0.027]	[-0.0657 , 0.045]	[-0.0685 , 0.0629]
2	[-0.0179 , 0.003]	[-0.0626 , 0.0472]	[-0.0765 , 0.075]	[-0.0765 , 0.0765]
3	[-0.0158 , 0.0067]	[-0.0773 , 0.0688]	[-0.084 , 0.084]	[-0.084 , 0.084]
4	[-0.0148 , 0.0117]	[-0.0928 , 0.089]	[-0.0947 , 0.0947]	[-0.0947 , 0.0947]
5	[-0.0176 , 0.0178]	[-0.1088 , 0.1088]	[-0.121 , 0.121]	[-0.121 , 0.121]

The table displays the set identified parameters in Equation 6.1.

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

Event time	Original	M = 0.5	M = 0.75	M = 1
-6	[0.0163 , 0.1215]			
-5	[0.0132 , 0.0749]			
-4	[0.0093 , 0.0555]			
-3	[0.0074 , 0.037]			
-2	[0.0042 , 0.0237]			
-1				
0	[-0.0468 , -0.0272]	[-0.0623 , -0.0122]	[-0.0636 , -6e-04]	[-0.0636 , 0.0122]
1	[-0.0652 , -0.0439]	[-0.077 , -0.0054]	[-0.077 , 0.0194]	[-0.077 , 0.0428]
2	[-0.0661 , -0.0431]	[-0.0834 , 0.0194]	[-0.0834 , 0.0548]	[-0.0834 , 0.0834]
3	[-0.0642 , -0.0396]	[-0.0887 , 0.0457]	[-0.0887 , 0.0887]	[-0.0887 , 0.0887]
4	[-0.0664 , -0.0405]	[-0.0972 , 0.0678]	[-0.0972 , 0.0972]	[-0.0972 , 0.0972]
5	[-0.0754 , -0.0435]	[-0.1192 , 0.0879]	[-0.1192 , 0.1192]	[-0.1192 , 0.1192]

The table displays the set identified parameters in Equation 6.1.

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

Event time	Original	M = 0.5	M = 0.75	M = 1
-6	[-0.1417 , 0.0205]			
-5	[-0.0917 , 0.0015]			
-4	[-0.0533 , 0.0099]			
-3	[-0.047 , -4e-04]			
-2	[-0.0259 , 0.0019]			
-1				
0	[0.0405 , 0.0683]	[0.0248 , 0.0822]	[0.0129 , 0.0961]	[-0.001 , 0.098]
1	[0.051 , 0.0822]	[0.0104 , 0.1144]	[-0.0173 , 0.1144]	[-0.0451 , 0.1144]
2	[0.0584 , 0.0923]	[-0.0092 , 0.1295]	[-0.051 , 0.1295]	[-0.0902 , 0.1295]
3	[0.0582 , 0.099]	[-0.0306 , 0.1444]	[-0.0861 , 0.1444]	[-0.1415 , 0.1444]
4	[0.0648 , 0.1123]	[-0.0505 , 0.1612]	[-0.1189 , 0.1612]	[-0.1612 , 0.1612]
5	[0.078 , 0.1316]	[-0.0627 , 0.2001]	[-0.1435 , 0.2001]	[-0.2001 , 0.2001]

The table displays the set identified parameters in Equation 6.1.

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 20 – 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
-5	-0.007	-0.011	-0.006
	(0.011)	(0.011)	(0.020)
-4	-0.002	-0.007	0.002
	(0.007)	(0.008)	(0.011)
-3	-0.003	-0.005	-0.000
	(0.004)	(0.005)	(0.008)
-2	-0.002	-0.005	0.008
	(0.003)	(0.003)	(0.005)
-1			
0	0.003	-0.009*	0.021*
	(0.002)	(0.003)	(0.005)
1	0.005	-0.008	0.023*
	(0.003)	(0.004)	(0.006)
2	0.004	-0.007	0.014
	(0.005)	(0.005)	(0.008)
3	0.010	0.002	0.010
	(0.006)	(0.007)	(0.011)
4	0.017	0.016	0.003
	(0.010)	(0.010)	(0.016)

The table displays the event study parameters in Equation 5.4.

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 21 – AE effect on the likelihood of offering work by months of exposure for initially Self Employed individuals: Hours worked and Labor offer

Event Time	Hours worked	Labor offer
-5	-0.470	0.024
	(2.657)	(0.038)
-4	1.427	0.001
	(1.402)	(0.020)
-3	0.054	-0.013
	(0.845)	(0.015)
-2	0.596	0.004
	(0.489)	(0.009)
-1		
0	-0.822	-0.028*
	(0.337)	(0.007)
1	-0.799	-0.045*
	(0.576)	(0.012)
2	-0.487	-0.037
	(0.895)	(0.016)
3	-0.637	-0.032
	(1.369)	(0.022)
4	-1.370	-0.045
	(2.134)	(0.032)

The table displays the event study parameters in Equation 5.4.

Dependent Variable: Hours actually worked and Labor offer (employment or job seeking).

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

Estimates for initially self-employed individuals.

Standard Deviations in parenthesis.

* indicates statistical significance at the 5% level

Source: PNAD COVID-19.

Table 22 – 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
-6	-0.0223	-0.0347	-0.0149
	(0.0277)	(0.0317)	(0.0435)
-5	-0.0109	-0.0083	-0.0417
	(0.0158)	(0.0167)	(0.0288)
-4	0.0073	0.0068	-0.0196
	(0.0104)	(0.0106)	(0.0178)
-3	-0.0042	-0.0024	-0.0058
	(0.0069)	(0.0078)	(0.0126)
-2	0.0038	0.0098	-0.0161
	(0.0055)	(0.0055)	(0.0086)
-1			
0	-0.0045	-0.0133	0.0116
	(0.0053)	(0.0050)	(0.0077)
1	-0.0021	-0.0150	0.0212
	(0.0071)	(0.0067)	(0.0110)
2	-0.0042	-0.0138	0.0188
	(0.0094)	(0.0097)	(0.0149)
3	-0.0137	-0.0203	0.0287
	(0.0122)	(0.0121)	(0.0188)
4	-0.0127	-0.0243	0.0578
	(0.0163)	(0.0176)	(0.0258)
5	0.0063	-0.0146	0.0867
	(0.0294)	(0.0318)	(0.0476)

The table displays the event study parameters in Equation 5.4.

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.

Table 23 – AE effect on the likelihood of offering work by months of exposure and initial labor force status: PNADC first quarter 2020.

Event Time	Complete Sample	Initially Active	Initially Inactive
-5	-0.0562	-0.0642	-0.0805
	(0.0477)	(0.0463)	(0.0857)
-4	-0.0255	-0.0043	-0.0564
	(0.0273)	(0.0280)	(0.0492)
-3	0.0346	0.0252	0.0462
	(0.0156)	(0.0168)	(0.0299)
-2	-0.0056	-0.0004	-0.0124
	(0.0119)	(0.0117)	(0.0206)
-1			
0	-0.0028	-0.0098	-0.0055
	(0.0104)	(0.0116)	(0.0167)
1	0.0004	-0.0065	0.0081
	(0.0154)	(0.0148)	(0.0251)
2	-0.0184	0.0000	-0.0405
	(0.0216)	(0.0214)	(0.0333)
3	0.0001	-0.0195	0.0408
	(0.0256)	(0.0293)	(0.0387)
4	0.0963	0.0810	0.0760
	(0.0442)	(0.0507)	(0.0423)

The table displays the event study parameters in Equation 5.4.

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.