WORKING PAPER Nº IDB-WP-1840

Keeping Heads Above Water:

Evidence from a Labor Retention Scheme in Response to Floods in Brazil

Ricardo Campante-Vale
Luiz Felipe Fontes
Stefanie Roost
Raquel Tebaldi

Inter-American Development Bank
Department of Research and Chief Economist

July 2026

Keeping Heads Above Water:

Evidence from a Labor Retention Scheme in Response to Floods in Brazil

Ricardo Campante-Vale*
Luiz Felipe Fontes**
Stefanie Roost***
Raquel Tebaldi****

* Insper Institute of Research and Education and Institute for Applied Economic Research
** Insper Institute of Research and Education
*** United Nations University and German Institute of Development and Sustainability
**** United Nations University

Inter-American Development Bank
Department of Research and Chief Economist

July 2026

Abstract*

We study the labor market effects of a major flood disaster in Brazil and whether a temporary labor-retention scheme—Programa Emergencial de Apoio Financeiro (PEAF)—mitigated these effects by providing wage subsidies while requiring firms to retain covered workers. Drawing on administrative data, we use a matched event-study design that compares workers in flood-affected establishments with and without PEAF coverage to matched workers in non-affected establishments. Direct flood exposure reduced formal employment among unprotected workers by about 2 percentage points and increased job switching, indicating rapid reallocation to other formal employers. PEAF increased employment by 2.4 percentage points relative to the counterfactual of flood exposure without program protection. It also reduced reliance on unemployment insurance, consistent with its effect on employment retention. Earnings effects are more nuanced: PEAF mitigated unconditional earnings losses through employment preservation, but earnings among retained workers declined, consistent with downward compensation adjustment within continuing jobs. Effects are concentrated in very small establishments and in sectors more exposed to disruption. The results show that labor-retention schemes can preserve employment after climate disasters, but that need not imply full income protection, a difference that is central to the design of policies aimed at mitigating the economic consequences of climate shocks.

Keywords: Labor Market, Floods, Labor Retention Schemes, Wage Subsidies, Climate Change.

JEL Codes: H25, J38, J65, J68, Q54.

* *We thank Craig McIntosh, Diego Vera-Cossio, and Rodrigo R. Soares for their thoughtful comments and suggestions. We also thank seminar participants at the Inter-American Development Bank, Insper, the German Institute of Development and Sustainability (IDOS), the University of Bonn, and the Institute for Applied Economic Research (IPEA) for valuable feedback and discussions. We are grateful to IPEA for providing access to the data used in this study, and especially to Rafael Pereira, Lucas Mation, and Brunu Amorim for facilitating data access. All remaining errors are our own. This paper was financed with the support of the Latin American and the Caribbean Research Network of the Inter-American Development Bank. The views expressed in this publication are those of the authors and do not necessarily reflect those of the Inter-American Development Bank, its Board of Directors, or the countries they represent.*

1 Introduction

The world is currently witnessing intensified climate change and global warming, driven by continued increases in carbon emissions. Emissions between 2010 and 2019 accounted for more than 40% of total emissions accumulated since 1850 (IPCC, 2023). Rising temperatures disrupt the atmosphere, oceans, and biosphere, increasing the frequency and severity of extreme weather events (Rodell & Li, 2023). Consistent with this trend, 2023 to 2025 were the three most disaster-intensive years on record (CRED & USAID, 2025; Delforge et al., 2026). Climate disasters generate severe economic impacts, particularly in developing countries, reducing GDP levels and growth and acting as negative labor market shocks (Ferreira, 2024). Employment and income losses are typical consequences of destructive extreme weather events, and evidence from Latin America shows that informality, income, and sector of employment shape workers' vulnerability to disasters (Otero-Cortés & Bohorquez-Penuela, 2020; Wagner, 2025; Xie, 2024).

As extreme weather events become more frequent and severe, understanding the resilience of labor markets to sudden disruptions has become an increasingly urgent research question. Central to this debate are the magnitude and persistence of employment and income losses caused by climate shocks, and whether policy interventions can mitigate these losses, particularly among economically vulnerable workers. One possible response is the use of labor-retention schemes (LRSs)—such as furloughs, short-time work arrangements, and wage subsidies—which aim to preserve employment relationships during temporary covariate shocks. These policies expanded substantially during the COVID-19 pandemic, especially in Western Europe (Giupponi, Landais, & Lapeyre, 2022). Yet credible evidence on their effectiveness after climate disasters remains limited.

This paper addresses this gap by studying a major flood disaster in Brazil followed by the adoption of a labor-retention policy. Starting in late April 2024, Rio Grande do Sul, Brazil's southernmost state, experienced floods of unprecedented intensity, the largest disaster in the state's recorded history. Extreme rainfall caused widespread destruction of homes, businesses, and industrial facilities and forced approximately 600,000 people into displacement or temporary shelters (Tebaldi, 2025). Immediate economic losses were estimated at USD 17.3 billion (IDB, 2024). In response, the federal government launched the Emergency Program for Financial Assistance (Programa Emergencial de Apoio Financeiro, or PEAF), a two-month program requiring employers to retain covered workers for four months—two months during which a wage subsidy was paid and the subsequent two months.

We investigate both the labor market consequences of direct flood exposure and the extent to which PEAF mitigated these impacts by preserving employment relationships and workers' earnings. To do so, we combine Brazil's matched employer-employee dataset with geospatial flood maps, PEAF administrative records, and social-protection registries. These data allow us to construct a monthly panel tracking workers through December 2024. We implement a matched event-study design that partitions workers into three groups: workers in non-affected establishments, workers in flood-affected establishments not covered by PEAF, and workers in flood-affected establishments covered by PEAF. We use coarsened exact matching to balance these groups on a rich set of baseline worker and establishment covariates. This design allows us to estimate the effect of direct flood exposure among unprotected workers and the extent to which PEAF mitigated that effect within a unified empirical framework. The internal validity of our research design is supported by flat pre-trends in the event-study specifications across all outcomes we examine, both for affected untreated workers and for PEAF-treated workers relative to their matched non-affected controls. The results are also robust to specifications that absorb time-varying shocks associated with the baseline covariate profiles used in the matching procedure and robust to the exclusion of comparison workers who may have been indirectly affected by the floods through local spillovers or general-equilibrium effects.

We find that direct flood exposure generated persistent employment losses among unprotected workers. By the end of 2024, formal employment had fallen by about 2.2 percentage points among workers not covered by PEAF. This net employment loss masks a larger disruption to workers' baseline employment relationships. Job switching increased by about 2 percentage points by the end of the period, implying that separations from baseline employers increased by roughly 4 percentage points. Thus, a substantial share of the disruption to original worker-firm matches was absorbed by rapid reallocation to other formal employers. PEAF largely offset the remaining employment loss. Workers in PEAF-covered establishments experienced no average decline in formal employment, and the estimated program effect is 2.4 percentage points relative to the counterfactual of being flood exposed without program protection. This pattern is consistent with the design of the program, which conditioned transfers on the preservation of covered employment relationships.

The earnings results are more nuanced. Among unprotected workers, direct flood exposure reduced unconditional earnings by 2.1% on average. This decline appears to be driven primarily by the net loss of formal employment documented above. Conditional on positive earnings, the effect is much smaller, at 0.8%, indicating that workers who remained formally employed or who rapidly reallocated to another formal job experienced only modest earnings losses on average. PEAF only partially mitigated the earnings losses by preserving employment. Relative to the counterfactual of being flood exposed without program protection, it increased unconditional earnings by about 1.1%. However, the program did not fully protect pay among workers who remained employed. Conditional on positive earnings, PEAF-treated workers experienced larger declines, with earnings falling by 1.3% on average and by around 3% in some post-flood months. This pattern suggests that covered establishments maintained formal job links while adjusting compensation downward among retained workers, likely through variable components of pay.

The floods also increased workers' reliance on the social-protection system, although the effects on noncontributory benefits are modest. Among unprotected affected workers, registration in CadÚnico, Brazil's national registry for low-income households, increased by 0.2 percentage points, while transfers from Bolsa Família, the country's flagship conditional cash transfer program, increased by R$1.12. Both effects correspond to about 4% of the respective baseline control means. Similar effects for PEAF-treated workers imply that PEAF did not meaningfully affect this margin, which likely reflects the broader postdisaster mobilization to register affected households in social programs. The effects on unemployment insurance are larger and more directly connected to the employment losses. Unprotected affected workers became 1.1 percentage points more likely to receive unemployment insurance and received R$19.43 more in monthly benefits, both roughly 28% of the post-flood control mean. PEAF moved these outcomes in the opposite direction: Relative to unprotected affected workers, the program reduced unemployment insurance receipt by 1.9 percentage points and benefits by R$34.17.

A back-of-the-envelope calculation helps benchmark these magnitudes against the scale of the intervention. PEAF paid two minimum-wage transfers, totaling R$2,824 per covered worker. Our estimates imply that the program reduced unemployment-insurance payments by about R$34 per worker-month. In addition, using the estimated 1.1% effect on unconditional earnings and average monthly earnings in the control group, PEAF preserved roughly R$32 in labor earnings per worker-month. Cumulated over the April–December window, these directly measured short-run gains amount to about R$600 per covered worker, well below the transfer cost. This comparison is necessarily partial, as it excludes potentially important benefits from preserving worker-firm matches, avoiding search and training costs, and preserving the tax base. It also reflects only the short-run horizon observed in this version of the paper.

Finally, we show that average effects mask substantial heterogeneity across establishments. Employment losses are especially large among workers attached to very small establishments: In establishments with four or fewer employees, flood exposure without PEAF reduced employment by 6.7 percentage points. PEAF was also most protective in this group, implying a program effect of about 8 percentage points relative to unprotected affected workers. We also find meaningful heterogeneity across sectors, with larger untreated employment losses in construction, manufacturing, and commerce than in services, and PEAF mitigated losses across these sectors. By contrast, heterogeneity across worker characteristics is less systematic. While some differences appear across wage and education groups, they do not map cleanly into a single worker-level vulnerability gradient. Overall, the incidence of employment losses and the protective effects of PEAF appear to be shaped primarily by the characteristics of affected establishments, especially firm size and sector.

Taken together, the results point to a more nuanced view of postdisaster labor market adjustment. Even among unprotected workers, the employment losses caused by the floods were partly offset by rapid reallocation to other formal employers. This adjustment was not enough to prevent a persistent decline in employment, but it indicates that local labor markets absorbed part of the shock through worker mobility. PEAF further reduced these short-run employment losses, with the strongest effects in the firms and sectors in which untreated workers were most exposed. At the same time, preserving employment relationships did not fully insure workers against earnings losses. PEAF mitigated unconditional earnings losses by keeping workers formally employed, but earnings among retained workers still declined. This suggests an important policy lesson: Labor-retention schemes can be effective at preventing separations after climate disasters, but their design must also consider whether firms can adjust compensation along dimensions not directly covered by retention requirements. Our evidence is short run; a longer horizon will be important to assess whether unprotected workers continue to recover through reallocation and whether preserved matches translate into more persistent gains.

The remainder of this article is organized as follows. Section 2 reviews the literature on the labor market effects of natural disasters and the role of labor-retention schemes. Section 3 provides institutional background on the 2024 Rio Grande do Sul floods and the PEAF program. Section 4 describes the data and empirical strategy. Section 5 presents the main results, discusses robustness checks, and reports heterogeneity analyses. Section 6 concludes.

2 Literature Review

2.1 The Impacts of Disasters on Workers

The short-term economic harms of disasters take four main forms: deaths, destruction of infrastructure and assets, displacement, and economic disruption. The extent of each type varies with the level of economic development of the impacted areas. For instance, while fatalities are less common in more developed economies, these economies tend to suffer greater destruction of physical capital (Deryugina, 2022).

Most of the evidence on natural disasters' effects on workers' labor market outcomes focuses on income and employment and finds losses, at least in the short term. But while labor market participation might be reduced by large-scale displacement, labor engagement might increase if workers boost their working hours or engage in multiple jobs to compensate for lost assets. Effects also often vary by sector. For instance, tourism to impacted areas might fall, but postdisaster construction might experience a spike in demand (Deryugina, 2022).

Evidence from Latin America generally shows negative impacts of extreme weather events on labor market outcomes, and in several cases it underscores the role of sectors in mediating these effects. Heat extremes are linked to negative impacts in the formal labor market in Brazil, particularly manufacturing sector layoffs. Effects are more severe for workers performing routine manual activities (Xie, 2024). Heat waves in Brazil are also linked to greater labor market attrition, as they accelerate retirement among higher-income workers (Hernandez-Cortes & Mathes, 2024). In Colombia, episodes of excessive rainfall are associated with negative effects in the formal labor market in both agricultural and non-agricultural sectors in rural areas (Otero-Cortés & Bohorquez-Penuela, 2020). In Mexico, storms are generally associated with decreases in wages, working hours, and probability of employment (Acevedo, Castellani, Lopez de la Cerda, Lotti, & Székely, 2023).

Turning to floods specifically, evidence from Brazil suggests that labor informality and sector of work are key explanatory factors to understand their differential impacts across income groups. Using data from 2012 to 2022, Wagner (2025) finds that flash floods are associated with immediate negative impacts on the incomes of the working poor, with labor informality increasing the vulnerability of these workers, and these effects persist for more than nine months after the disaster. Wagner also finds that specific sectors increase the vulnerability of workers, such as agricultural-sector work in rural areas and domestic, retail, and some manufacturing work in urban areas. Another study finds that businesses affected by the 2008 flash floods in the southern state of Santa Catarina were more likely to close as a result of the disaster. Workers laid off in the aftermath were less likely to obtain another formal job, with effects persisting more than five years after the disaster (Alves, Ehrl, & Carvalho, 2024).

Gender is an important dimension of the relationship between disasters and labor market outcomes. Disasters tend to cause shifts in the type of labor market participation among female workers, whether that involves entering into unemployment, reentering the labor market, or transitioning into self-employment. These shifts are at least partially explained by increased domestic labor needs and occupational segregation in the aftermath of disasters, as the construction and rehabilitation sectors tend to rely more on male workers (Erman, De Vries Robbe, Thies, Kabir, & Maruo, 2021). In Mexico, women were less likely to be employed and reduce their hours worked after storms and floods, which could be because their employment was concentrated in particularly vulnerable sectors (Acevedo et al., 2023).

2.2 Labor-Retention Schemes in Response to Economic Downturns

LRSs are policies designed to preserve employment relationships when negative shocks hit the economy. The three main types are short-time work arrangements, furlough schemes, and wage subsidies. Under short-time work arrangements, firms reduce working hours and the state compensates workers for the hours not worked. Furlough schemes place workers on temporary paid leave, typically financed through unemployment insurance systems. Wage-subsidy programs support firms directly by covering part of their wage bill (Eichhorst, Marx, Rinne, & Brunner, 2022).

These policies share the objective of providing temporary support to prevent inefficient job destruction during transitory downturns. They are justified in multiple ways. From an individual perspective, LRSs help preserve income and welfare during crises. From an efficiency standpoint, they may reduce future search, matching, and rehiring costs by preventing separations that would otherwise destroy jobs that are profitable in the long term but temporarily nonviable because of liquidity constraints. From a fiscal perspective, LRSs may reduce public expenditures on unemployment insurance during short-lived downturns. Broader social motivations have also been highlighted, including the potential to mitigate social dislocation and associated externalities (Cahuc, 2024).

Despite these potential benefits, LRSs also raise important concerns. Beyond public finance considerations, such programs may generate economic inefficiencies. First, retention schemes can delay the destruction of unproductive jobs, hindering efficient labor reallocation. Second, they may be wasteful if they merely postpone unavoidable layoffs or firm closures. At a more aggregate level, LRSs may disproportionately benefit low-productivity firms or sectors, slowing structural adjustment. In addition, windfall effects may arise if subsidies accrue to firms that would have retained workers and met their obligations even in the absence of public support (Cahuc, 2024). In such cases, expanding unemployment benefits could dominate LRSs from a welfare perspective.

Notwithstanding these concerns, LRSs have become increasingly common since their early adoption in Germany during the Great Depression. Their use expanded substantially during the Global Financial Crisis and reached unprecedented scale during the

COVID-19 pandemic (Eichhorst et al., 2022). In the United States, a closely related policy, the Paycheck Protection Program, disbursed approximately USD 800 billion in forgivable loans to small businesses conditional on employment retention (Autor et al., 2022).

While LRS frameworks were largely institutionalized in advanced economies prior to the pandemic, many developing countries introduced or expanded such schemes only after the COVID-19 shock (Eichhorst et al., 2022). Countries including Egypt, Peru, the Philippines, Serbia, South Africa, and Vietnam adopted newly designed short-time work arrangements. Brazil followed a similar path by launching the Emergency Benefit for Employment and Income Maintenance, which supported both short-time work and furlough arrangements, conditional on agreements between employers and employees and commitments to job retention for the duration of the benefit (up to four months) (Brasil. Ministério do Trabalho e Emprego, 2024b). Empirical evidence suggests positive effects of LRS on job retention, reduced unemployment duration, and re-employment probabilities following the COVID-19 shock span beyond higher-income settings (Chatri & Tahir, 2024; Köhler, Bhorat, & Hill, 2023).

Labor-retention schemes have also been used in response to large-scale nature-related disasters. Chile adopted a six-month wage-subsidy program following its 2010 earthquake and tsunami. Australia implemented targeted wage assistance after the Queensland floods and cyclones of 2010–11. New Zealand introduced six-week wage subsidies after the Canterbury earthquakes in 2010 and 2011, while Japan and Turkey relied on short-time work and furlough schemes following major earthquakes in 2011 (Venn, 2012). And, in France, pre-established short-time work schemes have been increasingly used in response to climate-related disasters (Calavrezo, Walkowiak, Terrasse, & Noël, 2025).

Drawing on these international experiences and the precedent set by the Emergency Benefit for Employment and Income Maintenance, the Brazilian Ministry of Labor established PEAF in response to the 2024 floods in Rio Grande do Sul. The net welfare impact of this program remains an open question, and a comprehensive causal assessment of the preceding program is still largely absent from the literature.

As emphasized by Cahuc, Kramarz, and Nevoux (2021), the efficiency of labor-retention schemes is highly sensitive to program design. When such programs subsidize firms that would have retained workers even in the absence of support, the fiscal cost per job saved can become substantial. This is particularly likely when eligibility is extended to large firms, which are generally better equipped to cope with shocks without experiencing binding liquidity constraints. This pattern characterized the French short-time work program following the 2008–9 crisis, whose windfall effects were large and cost per saved job consequently high. To address these efficiency considerations, Cahuc et al. (2021) suggest it is important to analyze policy impacts across heterogeneous groups of firms and workers while accounting for potential fiscal offsets. Heterogeneity analyses allow researchers to identify the specific segments in which the program effectively preserved employment versus those in which restricting eligibility would improve policy targeting. Evaluating fiscal offsets allows researchers to quantify the direct impact of the program on alternative social expenditures, such as reduced unemployment insurance outlays, mitigated poverty, and lower subsequent enrollment in noncontributory social assistance programs.

At the opposite extreme, poorly designed schemes may induce adverse selection by attracting low-productivity firms, thereby preserving inefficient matches and hindering reallocation in the medium and long run, even when short-run employment effects are sizable. This was the case with the Italian short-time work program after the Great Recession: Although positive effects on workers' employment and earnings were initially observed, these gains dissipated within three years, and general-equilibrium effects reduced employment inflow rates and total factor productivity in the most heavily treated local labor markets (Giupponi & Landais, 2023).

Despite the policy relevance of labor-retention schemes, credible causal evidence remains limited. To the best of our knowledge, the studies by Cahuc et al. (2021) and Giupponi and Landais (2023), together with the Swiss evaluation by Kopp and Siegenthaler (2021), were among the first to leverage administrative data to overcome long-standing identification challenges in the evaluation of short-time work programs. All three studies focus on European countries during the Global Financial Crisis.

For wage-subsidy programs, as in our case, the body of evidence based on credible causal identification is even more limited and remains largely absent for the Global South and climate-related disaster contexts. The closest study is Bruhn (2020), which uses industry-level data to evaluate Mexico's eight-month wage-subsidy program implemented during the Global Financial Crisis and documents sizable positive employment effects in eligible industries.

Our study provides worker-level evidence on a wage-subsidy program using administrative data. PEAF's eligibility criterion is mainly geographical and coincides with the flooded areas, thus allowing us to isolate treatment effects among firms and workers directly exposed to the shock.

3 Historical and Institutional Background

3.1 The Rio Grande do Sul 2024 Floods

Rio Grande do Sul is Brazil's southernmost state, bordering Argentina and Uruguay. The state is less economically vulnerable compared to other states because of its higher average income and lower labor informality (IBGE, 2025), but it is very exposed to climate shocks, particularly floods, which have recurred with increasing frequency in recent years.

Beginning on April 28, 2024, precipitation reached unprecedented levels in the state, causing the most devastating floods in its history. Between May 1 and 13, climatological stations registered more than 500 mm of rainfall in several municipalities. According to the National Institute of Meteorology, the state's capital, Porto Alegre, registered 564.8 mm of rainfall that month, compared with a historical average of 112.8 mm (Brasil. Instituto Nacional de Meteorologia, 2025). This led to the destruction of thousands of houses, commercial establishments, and industrial plants. The accumulated rainfall caused approximately 600,000 people to be displaced or accommodated in shelters.

IDB (2024) highlights the magnitude of the immediate economic losses, estimating total destroyed or displaced assets at BRL 88.9 billion (circa USD 17.3 billion). In particular, productive sectors accounted for 61% of the total damages and 89% of the losses, while infrastructure suffered 11% of the damages and 3% of the losses, concentrated in transportation and electricity. IDB also emphasizes the crucial contribution of the postdisaster responses by the various levels of government and the resilient private sector to a speedy recovery. There was a net loss of 22,000 formal jobs in the state,[1] in contrast to the typically positive levels for that month (except during the COVID-19 pandemic).

Despite the wide geographical reach of the floods across the state's fluvial basins, affecting 416 of 497 municipalities (Pereira, Herszenhut, Ferreira, Mation, Cunha, & Stivali, 2024), their direct incidence on firms was relatively limited. An estimated 23,300 establishments, or 9.5% of the total, were located within the flooded areas (Pereira, Herszenhut, Ferreira, Mation, Stivali, & Cunha, 2024). Firms operating in these areas experienced losses of productive assets, weakening their competitive capacity, while firms outside the flooded zones were more exposed to indirect effects arising from disruptions to the state's productive infrastructure and logistics. In total, 334,600 workers, or 13.7% of the state's workforce, were employed in directly affected establishments and thus faced a higher risk of employment and welfare losses.

[1] Statistics from Novo Caged. Accessed on May 24, 2026.

3.2 Policy Responses

To prevent potential employment losses, the federal government enacted PEAF, the wage subsidy program targeting establishments in flood-affected areas as defined by federal flood maps. The program provided two payment installments equivalent to the minimum wage per formally employed worker to assist with employment maintenance. The benefit was scheduled to be paid in July and August 2024 (Brasil. Ministério do Trabalho e Emprego, 2024a) as compensation for June and July payrolls, respectively. The transfers were directly made to benefited workers' accounts, and firms could thus discount the benefit amount from their own payments to workers, although their tax payments and social contributions remained the same. In return, firms were mandated to hold the benefited employees for the subsequent two months.

Eligibility required establishments to be located within federal flood-mapping limits, firms to report decreased revenues or operational constraints due to floods, and firms to have no open debts with the social security system or tax authorities. Starting one month after the disaster, PEAF reached 9,847 establishments and 112,779 workers, with transfers totaling BRL 287.6 million (circa USD 57.1 million at the May 2026 exchange rate) for the government (Bischler, Ferreira, Campante Vale, & Schwarzer, 2026). In addition to formal workers in establishments within the flooded areas (92% of the benefited), the program targeted domestic workers whose houses flooded, as well as artisanal fishers.

Despite the relative ease of getting the subsidies, only about 42% of eligible establishments—covering approximately 33.7% of eligible employees—enrolled in PEAF. Reporting that an establishment was affected was straightforward and required only the completion of a simple form. Unlike previous programs of this type, PEAF did not require employers to reduce employees' working hours. Several factors may have hindered take-up. Either an establishment carried prior liabilities with public authorities, or disruptions to internet, energy, and road access impeded registration, or the chaotic situation and physical damages made the commitment of four months of employment too risky for managers. Information asymmetry could also have played a role, but the seemingly even spatial distribution of take-up makes this possibility less plausible. In later sections, we discuss how these factors may affect our identification strategy.

In parallel to PEAF, both federal and state governments implemented a broad set of policies to mitigate the economic impacts of the floods at the household level. The measures enacted by the National Social Security System included authorized withdrawals from Fundo de Garantia do Tempo de Serviço savings accounts,[2] as well as two extra

[2]Fundo de Garantia do Tempo de Serviço is a mandatory savings account for registered employees working in Brazil with a shared contribution by employers and workers. Generally, workers can use their saved money to access real estate credit or withdraw it when they are dismissed or when they retire.

installments of unemployment insurance for eligible unemployed workers (Silva, 2026). Actions targeting noncontributing workers included activation of the state-level Programa Volta por Cima, expansion of coverage, and temporary anticipation of dates for payments from Brazil's flagship cash transfer program, Bolsa Familia (BF) (Bischler et al., 2026; Tebaldi, 2025).

PEAF was the only policy program to adopt the federal flood maps as an eligibility criterion. Most other programs had broader geographic scope, potentially reaching people indirectly affected throughout the state. Fundo de Garantia do Tempo de Serviço withdrawals, which injected the most liquidity into the local economy, were open to any account holder residing in one of the roughly 400 municipalities under a federal emergency or calamity declaration. BF payment anticipation covered all municipalities in the state, while Programa Volta por Cima used state government identification of flooded areas to deliver a transfer focused on households in the informal economy.

4 Data and Methodology

We combine matched employer-employee records with geospatial flood maps and administrative registries on PEAF participation, social protection, and unemployment insurance to follow formal workers monthly before and after the floods. We then define flood exposure and PEAF treatment at the establishment level, construct matched comparison groups using coarsened exact matching, and estimate matched event-study specifications.

4.1 Data

Employment data. Our primary data source for workers' labor market outcomes is the Relação Anual de Informações Sociais (RAIS), Brazil's matched employer-employee administrative registry. RAIS covers the universe of formal employment relationships in Brazil and contains detailed information on workers and establishments, ranging from demographics to contractual arrangements. Although RAIS is released annually, employment histories allow us to reconstruct workers' trajectories in a monthly panel.

Flood maps and geospatial data. We have access to shapefiles delineating the geographic polygons of the impacted areas and access to the geographic coordinates of establishments, which we cross with the RAIS data to identify jobs physically exposed to the shock. The mapping of affected areas comes from official federal government data produced through a technical assessment that combined remote sensing, hydrological modeling, and on-site field validation (Namikawa et al., 2024). Flooded areas are defined not only as locations where water levels exceeded ground level, but also as areas affected by landslides and mudflows. This exposure map was used by the government to verify eligibility for PEAF, as established by Ministry of Labor Ordinance No. 991 of 2024.[3] Firms located within these areas were identified through a complementary geocoding effort based on establishments' registered addresses in eSocial, Brazil's digital system for recording tax, social security, and labor obligations. To maximize precision in identifying the coordinates of each corporate establishment, Pereira, Herszenhut, Ferreira, Mation, Stivali, and Cunha (2024) use a four-step algorithm that takes advantage of advanced GIS software, private and public address registries, and the standard geocoding API. This procedure geocodes establishments accounting for 88.4% of registered formal jobs at a satisfactory level of spatial precision.

PEAF data. Information on PEAF participation is drawn from Brazil's administrative registry of unemployment insurance. We have access to the identified version of these data, which allows us to observe the amount received by each worker and the firm responsible for maintaining the employment relationship after transfers were resumed.

Social-protection data. We have access to the identified national social-protection registry (CadÚnico), which contains administrative microdata for the families in Brazil verified to have monthly income lower than three minimum wages in total or less than half the minimum wage per capita. CadÚnico is mainly used to verify the eligibility and operate BF. We also have the monthly payroll data of BF, which keeps records of the transfers made to each family head. Finally, we have data on the national unemployment insurance program (Seguro-Desemprego) and its registry of benefit payments.

4.2 Analytical Sample

Baseline sample. Our baseline sample comprises all workers employed in the private sector on March 1, 2024, the first day of the month preceding the onset of the floods. We define flood and PEAF exposure at the establishment level. We avoid assigning PEAF receipt at the worker level because take-up occurred after the shock, in May and June. A worker-level definition would mechanically condition treatment status on workers remaining employed at the same establishment after the floods, biasing employment estimates upward. Our establishment-level definition avoids this post-treatment selection and is conservative. If anything, it should attenuate the estimated effect of PEAF.

At baseline, we classify workers into three groups according to whether their establishment was exposed to the floods (affected) and whether it took up PEAF (treated). Of the 204,505 active establishments in the state, 19,476 were located within flooded

[3] Portaria Nº 991, de 19 de junho de 2024

areas (9.5%). Among affected establishments, 6,375 took up PEAF (32.7%). In terms of workers, 274,129 were employed in affected establishments at baseline, of whom 87,340 were attached to establishments that took up PEAF (31.9%).[4]

Matching. A potential concern for our empirical strategy is that the three groups may differ in baseline characteristics associated with differential trends after the floods. In that case, our estimates could confound the effects of flood exposure or PEAF with other contemporaneous shocks or with preexisting differences in firms' ability to recover. For example, if affected establishments were disproportionately concentrated in sectors experiencing favorable demand shocks after the floods, we might underestimate the negative effects of flood exposure. Similarly, if establishments that take up PEAF differ systematically from other affected establishments—for instance, because they employ more workers who can perform tasks remotely—estimated treatment effects could partly reflect differential resilience rather than the program itself.

To mitigate these concerns, we use coarsened exact matching (CEM) to balance the three groups on baseline worker and establishment characteristics. The matching variables include gender, race, age, educational attainment, hourly wages one year before baseline, establishment size, tenure at the baseline job, economic sector, and whether the worker held an open-ended contract at baseline. This approach follows recent applications of CEM in labor settings (e.g., Arnold, 2022; Goldschmidt & Schmieder, 2017; Jaravel, Petkova, & Bell, 2018; Smith, Yagan, Zidar, & Zwick, 2019).

Before matching, the three groups differ along some observable dimensions (Table A.1). Treated workers are less likely to be female, more likely to be non-white, and less likely to hold a college degree, and they differ in sectoral composition relative to both affected untreated workers and unaffected workers. CEM reduces baseline imbalances while preserving most of the sample. Table 1 shows that the matched sample retains all treated workers, 90.0% of affected untreated workers, and 93.4% of unaffected workers. It also shows that the three groups are highly comparable on baseline covariates. The average age is approximately 38.8 years in all groups; the female share is 38.5%; and average tenure at the baseline job is around 3.8 years. Sectoral composition is also closely aligned: Services account for about 45% of workers in all groups, while commerce and manufacturing account for roughly 24% and 23% of employment, respectively.[5] Figure 2b summarizes covariate balance more systematically by plotting standardized mean differ-

[4]We exclude 826 residual establishments located outside the affected area that nevertheless received PEAF transfers. For many of these establishments, payments were made only in October or November and likely reflect administrative or judicial appeals.

[5]In the Appendix, we also report balance on establishment-level characteristics not directly included in matching (Table A.3). Remaining differences are small relative to the dispersion of the underlying distributions.

ences for a broad set of baseline characteristics before and after matching. While several differences are visible in the unmatched sample, the adjusted differences collapse toward zero after CEM, indicating a high degree of balance between treated workers, affected untreated workers, and unaffected workers.

Spatial distribution of PEAF. A remaining concern is that PEAF take-up may be correlated with the severity of flood exposure. If less damaged establishments were more likely to take up the program, treated establishments would have had better recovery prospects even absent PEAF. We examine this possibility in two ways. First, Appendix Figure A.1 compares, for each municipality, its share of all flood-affected jobs with its share of PEAF-treated jobs. The points lie close to the 45-degree line, indicating that municipalities with larger shares of affected jobs also account for similar shares of PEAF-treated jobs. This pattern provides no evidence that PEAF take-up was disproportionately concentrated in municipalities less exposed to the floods. Second, Figure 3 zooms in on Porto Alegre (the state capital) and its metropolitan area, where a large share of affected establishments is concentrated. The figure plots establishment locations and distinguishes affected establishments that took up PEAF from affected establishments that did not. Treated and untreated establishments appear spatially interspersed within the flooded area, rather than concentrated in distinct clusters.

4.3 Research Design

Overview of the design. We estimate the effects of the 2024 floods on individual economic outcomes, as well as whether PEAF mitigated these effects, using a matched event-study design. We examine workers' employment, job switching, and wages, as well as receipt of BF cash transfers and contributory unemployment insurance benefits. As described in Section 4.2, the baseline sample comprises all formal workers employed in Rio Grande do Sul on March 1, 2024, whom we follow monthly. Event time is centered in April 2024, when rainfall began to exceed normal levels and the floods started. We classify workers into three mutually exclusive groups based on their baseline establishment's flood exposure and PEAF take-up: workers in establishments outside flood-exposed areas non-affected); workers in flood-exposed establishments that did not take up PEAF (affected untreated); and workers in flood-exposed establishments that took up PEAF (affected treated).

We estimate two difference-in-differences contrasts using the matched non-affected workers as the comparison group. First, we compare flood-exposed workers in establishments that did not take up PEAF with their matched non-affected controls. This contrast identifies the effect of flood exposure in the absence of program protection. Second, we compare flood-exposed workers in PEAF-treated establishments with their own matched non-affected controls. This contrast captures the combined effect of flood exposure and PEAF protection among treated workers. The mitigating effect of PEAF is then given by the difference between these two estimates. It measures how outcomes changed for PEAF-treated workers relative to the counterfactual in which they were exposed to the floods but not protected by the program. In unreported results, we obtain very similar estimates when restricting the sample to affected workers only and directly comparing workers in PEAF-treated establishments with workers in affected establishments that did not take up the program.

Estimating equation. We estimate the following event-study specification on the matched sample:

$$y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq -1} \beta_\tau \mathbb{1}\{r_t = \tau\} \times F_i + \sum_{\tau \neq -1} \gamma_\tau \mathbb{1}\{r_t = \tau\} \times G_i + \varepsilon_{it} \tag{1}$$

y_{it} is the outcome of worker i in month t, α_i are worker fixed effects, and λ_t are month fixed effects. The variable r_t denotes event time in months relative to April 2024. We omit March 2024, $r_t = -1$, as the reference period. The indicator F_i identifies workers employed at baseline in flood-exposed establishments that did not take up PEAF, while G_i identifies workers employed at baseline in flood-exposed establishments that took up PEAF. Workers in non-affected establishments form the omitted comparison group. The coefficients $\{\beta_\tau\}$ trace the evolution of outcomes for affected untreated workers relative to their matched non-affected controls, capturing the effect of flood exposure in the absence of PEAF protection. The coefficients $\{\gamma_\tau\}$ trace the evolution of outcomes for affected treated workers relative to their matched non-affected controls, capturing the combined effect of flood exposure and PEAF protection. The mitigating effect of PEAF at event time τ is given by $\gamma_\tau - \beta_\tau$. We compute standard errors for this difference using the delta method, based on the full variance-covariance matrix of the estimated coefficients. Standard errors are clustered at the baseline establishment level.

All specifications are weighted using the matching weights produced by CEM. These weights reweight workers in the comparison groups so that their distribution across the coarsened baseline covariate cells mirrors that of PEAF-treated workers. Treated workers receive weight one, while comparison workers receive higher weights in cells where they are underrepresented relative to treated workers and lower weights in cells in which they are overrepresented. The weights are normalized so that the relative size of the three analytical groups is preserved in the matched sample.

Identification. The key identifying assumption is conditional parallel trends—that is, absent the floods and PEAF, affected treated workers and matched affected untreated workers would have followed the same outcome paths as comparable workers in non-affected establishments. The event-study design allows us to examine the pre-period analog of this assumption. Reassuringly, the estimated pre-treatment coefficients are systematically close to zero both when comparing affected untreated workers with non-affected controls and when comparing affected treated workers with non-affected controls. These flat pre-trends provide strong support for the identifying assumption, although they cannot rule out differential shocks that emerge only after the floods. Our matching procedure is designed to reduce the scope for such concerns by making comparisons local to workers and establishments with similar pre-shock characteristics, reducing the scope for violations of parallel trends driven by differences in baseline observable covariates. In Section 5.4, we go further and estimate a more demanding specification with matching-stratum-by-month fixed effects. This specification nonparametrically absorbs any time-varying shock associated with the full set of covariates used in the matching procedure. The estimates remain very similar, suggesting that our results are not driven by covariate-specific post-shock trends.

Despite these checks, we cannot rule out all forms of selection on unobservables. This limitation is especially relevant for the estimated effects of PEAF, whose take-up may depend on unobserved establishment traits. For example, establishments that applied to the program may differ in administrative ability, risk preferences, or expected recovery prospects. If these traits also predict employment retention after the floods even in the absence of PEAF, our estimates of the program's mitigating effect may partly reflect selection into treatment. We cannot fully rule out this concern.

Another threat to identification is spillovers across groups. Workers in non-affected establishments may still have been indirectly exposed to the disaster through disruptions to transportation, communication infrastructure, supply chains, or local demand. In this case, the comparison group would also be affected by the floods, attenuating the estimated effect of direct flood exposure. Conversely, PEAF may have generated spillovers within affected areas. By preserving employment and local demand in treated establishments, the program may also have benefited nearby affected establishments that did not receive transfers. This would make the affected untreated group a partially treated counterfactual and attenuate the estimated mitigating effect of PEAF. To assess the first concern, we re-estimate our main specifications excluding comparison workers employed in municipalities with at least one affected establishment, as well as workers employed within 5 km of the flooded areas. The results remain very similar. The second concern is harder to eliminate empirically, so we interpret the PEAF estimates as direct effects that may be conservative in the presence of local spillovers.

Finally, the floods may have triggered several policy responses beyond PEAF. We therefore interpret our flood-exposure estimates as net effects of direct exposure in the postdisaster policy environment, rather than as the effect of the physical shock alone (Deryugina, 2022). We believe this is less problematic for identification because the main policy responses we are aware of were broadly targeted across the state, rather than differentially targeted to our analytical groups. In contrast, PEAF was tied to the mapped affected areas and targeted a clearly delimited set of workers and establishments.

5 Results

5.1 Effects on Labor Market Outcomes

We begin by examining the direct effects of flood exposure and the mitigating role of PEAF on formal employment, job mobility, and earnings. Figure 4 reports the event-study estimates, Table 2 summarizes average post-event effects, while Figure A.2 in the Appendix depicts the estimated program's time-varying impacts on the flood-affected workers.[6] Across outcomes, the pre-event coefficients are close to zero, providing no evidence of differential pre-trends between affected workers and their matched non-affected controls.

Panel A of Figure 4 shows that flood exposure reduced formal employment among workers in establishments that did not take up PEAF. The employment effect becomes increasingly negative after the onset of the floods and stabilizes at around 2.2 percentage points by the end of 2024. The average post-event effect is -2.0 percentage points (Table 2, column 1). Although modest in absolute terms, this decline is economically meaningful when benchmarked against the essentially flat employment trajectory of matched non-affected workers over the same period.

This decline is accompanied by increased job mobility. Figure 4 Panel B shows that affected workers without PEAF became more likely to switch to a different establishment. The average post-event effect is 0.9 percentage points (Table 2, column 2), but the event-study estimates show a growing effect over time, reaching about 2 percentage points by the end of the period. Taken together, the employment and mobility effects imply that separations from baseline employers increased by roughly 4 percentage points by the end of 2024 among nonprotected workers, with about half of this increase absorbed by reallocation to other formal jobs.

[6] The estimated program impacts are computed as differences between coefficients γ_τ and β_τ in equation 1 for all months $t \in$ [2023m4, 2024m12].

PEAF substantially mitigated these employment losses. Workers in PEAF-treated establishments do not experience a statistically significant decline in employment on average, and the point estimates become positive a few months after the shock (Figure 4, Panel A). This pattern is consistent with the program's mechanism, which conditioned transfers on the preservation of employment relationships. Relative to the counterfactual of being flood exposed without program protection, PEAF increased employment by 2.4 percentage points (Table 2, column 1). The program also reduced job switching relative to affected untreated workers, although this estimate is not statistically significant on average (Table 2, column 2). Figure A.2 (Panels A and B) illustrates the dynamic mitigating effects of the program on employment variables. Overall, the evidence indicates that PEAF largely achieved its stated goal of preserving employment relationships after the floods.

We next turn to earnings. We measure earnings in logs and examine both an unconditional measure, which incorporates changes along the extensive and intensive margins, and a conditional measure that focuses on workers with positive earnings. For the unconditional measure, we keep workers with zero earnings and estimate effects using PPML, following Chen and Roth (2024). Panel C of Figure 4 shows that unconditional earnings fell among affected workers in establishments that did not take up PEAF. The decline occurs gradually after the floods and becomes more pronounced in the second half of the post-period, mirroring the employment losses documented above. On average, unconditional earnings fell by 2.1% for this group (Table 2, column 3). Figure 4 Panel D shows a smaller but still negative effect among affected untreated workers with positive earnings. The average conditional earnings effect is −0.8% (Table 2, column 4), indicating that the earnings losses are not driven only by exits from formal employment.

For workers in treated establishments, the event studies reveal an important difference between employment preservation and earnings protection. Panel C of Figure 4 shows that unconditional earnings also decline for treated workers in the months after the floods, especially around the period in which the program was implemented, before partially converging back toward zero by the end of the period. The average unconditional earnings effect is −1.0%, implying that PEAF preserved 1.1% of earnings relative to the counterfactual of being flood exposed without program protection (Table 2, column 3). Panel D of Figure 4, however, shows a sharper decline in earnings conditional on positive earnings among PEAF-treated workers, with effects reaching around −3% in some post-event months. The average conditional earnings effect is −1.3%, and the estimated program impact on this margin is −0.5% (Table 2, column 4). Thus, while PEAF preserved formal employment relationships, it did not fully protect earnings among workers who remained employed. Figure 5 (Panels C and D) presents dynamic evidence of that. A plausible interpretation is that treated establishments maintained formal job links while adjusting compensation downward among retained workers, likely through variable components of pay.

5.2 Effects on Access to Social Protection and Unemployment Benefits

We next examine whether the floods and PEAF affected workers' access to the social-protection system, focusing on noncontributory transfers through BF and contributory unemployment insurance. Figure 5 reports the event-study estimates, and Figure A.3 shows the program's estimated impacts on benefit amounts, while Table 3 summarizes average post-event effects on both benefit receipt and benefit amounts.

Flood exposure modestly increased access to noncontributory social protection. Among affected workers in establishments that did not take up PEAF, CadÚnico registration increased by 0.2 percentage points, about 4% relative to the control mean of 4.8% (Table 3, column 1). Monthly BF transfers increased by R$1.12, about 4% relative to the baseline control mean of roughly R$30 (Table 3, column 2). Panel A of Figure 5 shows that transfers rise gradually after the floods and remain above the pre-period level through the end of 2024. A similar pattern is observed for PEAF-treated workers, so the estimated program effects on CadÚnico registration and BF transfers are small and statistically indistinguishable from zero. These results may partly capture a deterioration in household economic conditions after the disaster, affecting both PEAF and non-PEAF workers. A more direct explanation, however, is that the postdisaster mobilization to register affected households in social programs increased registration among formal workers close to the eligibility margin (Bischler et al., 2026). In any case, the magnitudes are very modest and the program estimated effect on the affected is statistically indistinguishable from zero (Figure A.3, Panel A).

The results for unemployment insurance are more directly connected to the labor market effects documented above. Panel B of Figure 5 shows that unemployment insurance benefits increased three months after the floods, consistent with the gradual employment impact. The effects peak at around R$25, and then partially recede by year-end. On average, these workers became 1.1 percentage points more likely to receive unemployment insurance and received R$19.43 more in monthly benefits, with both effects corresponding to roughly 28% of the post-flood control mean (Table 3, columns 3 and 4). PEAF moved unemployment insurance outcomes in the opposite direction. Workers in PEAF-treated establishments became 0.8 percentage points less likely to receive unemployment insurance and received $14.70 less in monthly benefits relative to non-affected controls. Relative to affected untreated workers, the program gradually reduced unemployment benefits at least until two months after the expiration of the nondismissal mandate (Figure A.3). On average, it reduced unemployment insurance receipt by 1.9 percentage points and benefits by R$34.17. These results reinforce the interpretation that PEAF mitigated formal job separations and reduced workers' reliance on contributory unemployment benefits after the floods.

5.3 PEAF Costs and Directly Measured Benefits

A back-of-the-envelope calculation helps put the estimated effects of PEAF in perspective. The program paid two amounts equal to the minimum wage, totaling R$2,824 per covered worker. Against this fiscal outlay, our estimates identify two directly measured short-run benefits. First, PEAF reduced unemployment-insurance payments by R$34.17 per worker-month. Second, given an estimated 1.1% effect on unconditional earnings and average monthly earnings in the control group, the program preserved roughly R$32 in labor earnings per worker-month.

Cumulated over the April–December window, these two directly measured gains amount to about R$600 per covered worker, substantially below the transfer cost. This comparison should not be interpreted as a full welfare analysis. It excludes potentially important benefits from preserving worker-firm matches, avoiding search and training costs, limiting scarring effects from job loss, supporting firm survival, and preserving the tax base. It also abstracts from the value of the transfers themselves to liquidity-constrained workers in the immediate aftermath of the disaster.

This exercise nevertheless provides a useful benchmark. PEAF was effective at preserving formal employment relationships and reducing reliance on unemployment insurance, but the directly measured short-run gains are modest relative to the size of the transfers. The policy value of labor-retention schemes in disaster settings therefore depends critically on program design, including the transfer amount, eligibility rules, and duration of the retention requirement.

5.4 Robustness

We now assess the robustness of our results to two checks motivated by the identification concerns discussed in Section 4.3. The first check tests whether the results are robust to allowing for month effects specific to each matching stratum. This specification is substantially more demanding because it nonparametrically absorbs time-varying shocks that differentially affect workers with different baseline covariate profiles. The second check addresses potential spillovers or local general-equilibrium effects of the floods on the comparison group. We exclude unaffected workers employed in municipalities with at least one affected establishment, as well as unaffected workers employed within 5 km of the flooded areas. In this specification, we recompute the CEM weights after imposing the sample restriction.

Table 4 presents the results for our two main outcomes: formal employment and unconditional earnings. The results are very stable across specifications. For employment, the estimated effect of flood exposure among workers in establishments without PEAF is -2.0 percentage points in the baseline specification, -2.0 percentage points with matching-stratum-by-month fixed effects, and -1.8 percentage points when excluding potentially exposed controls. The estimated effect for PEAF-treated workers remains close to zero in all three specifications. As a result, the estimated mitigating effect of PEAF is virtually unchanged, ranging from 2.3 to 2.4 percentage points. The unconditional earnings results are similarly robust. The estimated effect of flood exposure among affected untreated workers is -2.1% in the baseline specification, -2.1% with matching-stratum-by-month fixed effects, and -2.0% in the restricted-control sample. For PEAF-treated workers, the corresponding estimates range from -0.9% to -1.0%. The implied program effect remains positive and statistically significant across specifications, ranging from 1.05% to 1.1%.

Overall, these checks suggest that the main findings are not driven by unobserved differential trends associated with baseline worker and establishment covariates, nor by the inclusion of comparison workers who may have been indirectly affected by the floods.

5.5 Heterogeneity Analysis

The aggregate effects can mask substantial variation in the incidence of employment losses and in the protective effect of PEAF. We therefore estimate heterogeneous effects by splitting the sample according to baseline worker and establishment characteristics. We focus on formal employment, the outcome most directly targeted by the program. Figure 6 summarizes the heterogeneous effects according to age, economic sector, establishment size, race, education, and gender. Appendix Table A.4 reports the corresponding estimates and presents additional splits, including baseline wage groups and finer bins for the characteristics shown in the figure.

The most pronounced heterogeneity is by establishment size. Workers in very small establishments are by far the most affected by the floods. Among workers in establishments with four or fewer employees (roughly 12% of the sample), flood exposure without PEAF reduced employment by 6.7 percentage points. Importantly, this is also where PEAF has the strongest mitigating effect. For workers in PEAF-treated establishments with four or fewer employees, employment is 1.5 percentage points higher than among comparable workers in non-affected establishments, implying a program effect of about 8 percentage points relative to affected untreated workers. For larger establishments, employment losses are much smaller. Among workers in establishments with 5 to 19 employees, flood exposure without PEAF reduced employment by 1.2 percentage points, while the estimated effect is close to zero for establishments with 20 to 99 employees. Losses increase again for larger establishments, reaching about 2.2 percentage points among establishments with 100 to 499 employees and among those with 500 or more employees. PEAF again offsets these losses, although the implied program effects are smaller than among the smallest establishments.

We also find meaningful heterogeneity across sectors. Among affected untreated workers, employment losses are largest in construction, in which flood exposure reduced employment by 3.8 percentage points. Manufacturing and commerce also experienced sizable losses, both around 2.2 percentage points, while the effect in services is smaller, at 1.5 percentage points. PEAF mitigates these losses across sectors. In construction, PEAF-treated workers experience a positive but statistically imprecise employment effect of 1.3 percentage points, implying a program effect of about 5 percentage points relative to affected untreated workers. The corresponding program effects are about 2.8 percentage points in manufacturing, 2.3 percentage points in commerce, and 1.7 percentage points in services. In summary, the sectoral pattern suggests that PEAF was particularly relevant in activities more exposed to physical disruption and local demand shocks.

Beyond establishment characteristics, heterogeneity across worker characteristics is more muted and less systematic. Gender, race, and age show relatively similar patterns: Affected untreated workers experience employment losses across groups, and PEAF generally brings the estimates closer to zero or into positive territory. Baseline wages show a somewhat clearer pattern at the bottom of the distribution. Among workers earning at most one minimum wage one year before the floods, flood exposure without PEAF reduces employment by 2.3 percentage points, while the corresponding estimate for PEAF-treated workers is close to zero, implying a program effect of roughly 2.7 percentage points. For higher wage bins, however, the pattern is less monotonic, and several estimates are imprecise. Education also displays some variation, with larger untreated losses among workers with less than elementary education, middle school education, and college education, but again without a clear gradient. Overall, the worker-level heterogeneity does not point to a single demographic mechanism.

Taken together, the heterogeneity results suggest that the floods primarily disrupted employment relationships in more exposed or fragile establishments. Very small firms and certain sectors experienced the largest untreated employment losses, and PEAF was most protective in those settings. Worker characteristics matter less systematically, suggesting that the incidence of employment losses was shaped more by the nature of affected establishments than by workers' demographic profiles.

6 Conclusion

This paper studies how a major climate disaster affected formal workers and whether a temporary labor-retention scheme mitigated these effects. We examine the 2024 floods in Rio Grande do Sul, Brazil, and the emergency wage subsidy implemented in response, PEAF. By combining matched employer-employee records, geospatial flood maps, and administrative data on PEAF and social-protection programs, we follow workers before and after the disaster and compare affected workers with and without program coverage to matched workers in non-affected establishments.

The floods generated persistent employment losses among unprotected workers. By the end of 2024, formal employment had fallen by about 2.2 percentage points among workers in affected establishments not covered by PEAF. This net decline masks a larger disruption to baseline employment relationships: Job switching increased by about 2 percentage points, implying that many workers separated from their original employers but rapidly reallocated to other formal jobs. PEAF was important to offset the remaining employment loss. Covered workers experienced no average decline in formal employment, and the estimated program effect was 2.4 percentage points relative to the counterfactual of being flood exposed without program protection. Very small firms experienced the largest employment losses, and PEAF was most protective for this group. Sectoral differences were also meaningful, with larger losses in construction, manufacturing, and commerce than in services. In contrast, heterogeneity across worker characteristics was less systematic. The incidence of employment losses and the protective effects of PEAF appear to have been shaped primarily by where workers were employed before the disaster.

The program was less successful at protecting earnings. PEAF mitigated unconditional earnings losses by keeping workers formally employed, but earnings among retained workers still declined. This suggests that covered establishments preserved formal job links while adjusting compensation downward, likely through variable components of pay. PEAF also reduced reliance on unemployment insurance, consistent with its employment-preserving effects. However, a back-of-the-envelope calculation suggests that the directly measured short-run benefits were modest relative to the fiscal cost of the transfers. PEAF paid R$2,824 per covered worker, while the estimated reduction in unemployment-insurance payments and preserved labor earnings over the April–December window amount to roughly R$600 per worker. This comparison excludes broader benefits from preserving worker-firm matches, avoiding search and training costs, supporting firm survival, and preserving the tax base.

Overall, the results suggest that local labor markets can absorb part of a climate shock through rapid worker reallocation but that this adjustment is incomplete. Temporary labor-retention schemes can help preserve employment relationships after disasters, especially in more fragile firms, but preserving jobs does not necessarily imply full income protection if firms can adjust compensation among retained workers. Our evidence is short run, through December 2024. A longer-run follow-up will be important to assess whether unprotected workers continue to recover through reallocation, whether preserved matches generate persistent gains, and how the broader benefits of labor-retention schemes compare with their fiscal costs.

References

Acevedo, I., Castellani, F., Lopez de la Cerda, C., Lotti, G., & Székely, M. (2023). *Natural disasters and labor market outcomes in Mexico* (Tech. Rep.). IDB Working paper series.

Alves, P. J., Ehrl, P., & Carvalho, R. (2024). *The impact of flash floods on the spatial distribution of businesses and workers* (Tech. Rep.). Tech. rep. Working paper. url: https://sbe. org. br/anais/45EBE ....

Arnold, D. (2022). The impact of privatization of state-owned enterprises on workers. *American economic journal: applied economics*, *14*(4), 343–380.

Autor, D., Cho, D., Crane, L. D., Goldar, M., Lutz, B., Montes, J., ... Yildirmaz, A. (2022). The 800 billion paycheck protection program: where did the money go and why did it go there? *Journal of Economic Perspectives*, *36*(2), 55–80.

Bischler, J., Ferreira, G., Campante Vale, R., & Schwarzer, H. (2026). Institutionalising social protection responses to disasters in brazil: Lessons from the 2024 floods in rio grande do sul. *IPCid Working Paper*.

Brasil. Instituto Nacional de Meteorologia. (2025). *Inundação histórica no Rio Grande do Sul completa um ano.* Retrieved from https://portal.inmet.gov.br/noticias/inunda%C3%A7%C3%A3o-hist%C3%B3rica-no-rio-grande-do-sul-completa-um-ano (Accessed: Jan. 8, 2026)

Brasil. Ministério do Trabalho e Emprego. (2024a). *Empresas gaúchas já podem aderir ao programa emergencial de Apoio Financeiro a trabalhadores de municípios em situação de calamidade no RS.* Retrieved from https://www.gov.br/trabalho-e-emprego/pt-br/noticias-e-conteudo/2024/Junho/empresas-gauchas-ja-podem-aderir-ao-programa-emergencial-de-apoio-financeiro-a-trabalhadores-de-municipios-em-situacao-de-calamidade-no-rs (Accessed: Jan. 8, 2026)

Brasil. Ministério do Trabalho e Emprego. (2024b). *Portal do Benefício Emergencial (BEm).* Retrieved from https://servicos.mte.gov.br/bem/ (Accessed: Jan. 7, 2026)

Bruhn, M. (2020). Can wage subsidies boost employment in the wake of an economic crisis? Evidence from Mexico. *The Journal of Development Studies*, *56*(8), 1558–1577.

Cahuc, P. (2024). The micro and macro economics of short-time work. In *Handbook of labor economics* (Vol. 5, pp. 385–433). Elsevier.

Cahuc, P., Kramarz, F., & Nevoux, S. (2021). The heterogeneous impact of short-time work: From saved jobs to windfall effects. *IZA Discussion Paper Series*, *14381*.

Calavrezo, O., Walkowiak, E., Terrasse, S., & Noël, K. (2025). Is short-time work a climate-adapted legacy system? Evidence from natural disasters in France between 2015 and 2022. *French Unemployment Insurance Fund Working Paper*.

Chatri, A., & Tahir, N. (2024). Temporary wage subsidies and post-COVID re-employment in Morocco: a regression discontinuity approach. *International Review of Applied Economics*, *38*(3), 357–375.

Chen, J., & Roth, J. (2024). Logs with zeros? some problems and solutions. *The Quarterly Journal of Economics*, *139*(2), 891–936.

CRED & USAID. (2025). *Disaster year in review 2024* (Tech. Rep. No. 78). Brussels: Centre for Research on the Epidemiology of Disasters. Retrieved from https://files.emdat.be/2025/05/CredCrunch78.pdf

Delforge, D., Below, R., Wathelet, V., Tonnelier, M., Alonso, A., & Speybroeck, N. (2026). *2025 disasters in numbers* (Tech. Rep.). Brussels, Belgium: Centre for Research on the Epidemiology of Disasters (CRED). Retrieved from https://files.emdat.be/reports/2025_EMDAT_report.pdf

Deryugina, T. (2022). Economic effects of natural disasters. *IZA World of Labor*.

Eichhorst, W., Marx, P., Rinne, U., & Brunner, J. (2022). Job retention schemes during COVID-19: A review of policy responses. *IZA Policy Paper Series*, *187*.

Erman, A., De Vries Robbe, S. A., Thies, S. F., Kabir, K., & Maruo, M. (2021). Gender dimensions of disaster risk and resilience: Existing evidence.

Ferreira, S. (2024). Extreme weather events and climate change: Economic impacts and adaptation policies. *Annual Review of Resource Economics*, *16*(1), 207–231.

Giupponi, G., & Landais, C. (2023). Subsidizing labour hoarding in recessions: The employment and welfare effects of short-time work. *The Review of economic studies*, *90*(4), 1963–2005.

Giupponi, G., Landais, C., & Lapeyre, A. (2022). Should we insure workers or jobs during recessions? *Journal of Economic Perspectives*, *36*(2), 29–54.

Goldschmidt, D., & Schmieder, J. F. (2017). The rise of domestic outsourcing and the evolution of the German wage structure. *The Quarterly Journal of Economics*, *132*(3), 1165–1217.

Hernandez-Cortes, D., & Mathes, S. (2024). The effects of environmental distress on labor markets: Evidence from Brazil. *Available at SSRN 4749053*.

IBGE. (2025). *Síntese de Indicadores Sociais* (Tech. Rep.). Brasilia: Author.

IDB. (2024). *Avaliação dos efeitos e impactos das inundações no Rio Grande do Sul* (Tech. Rep.).

IPCC. (2023). Summary for policymakers. In H. Lee & J. Romero (Eds.), *Climate change 2023: Synthesis report. contribution of working groups I, II and III to the sixth assessment report of the intergovernmental panel on climate change* (pp. 1–34). Geneva, Switzerland: IPCC. (Core Writing Team, H. Lee and J. Romero (eds.)) doi: 10.59327/IPCC/AR6-9789291691647.001

Jaravel, X., Petkova, N., & Bell, A. (2018). Team-specific capital and innovation. *Amer-*

ican Economic Review, *108*(4-5), 1034–1073.
Köhler, T., Bhorat, H., & Hill, R. (2023). *The effect of wage subsidies on job retention in a developing country: Evidence from South Africa* (No. 2023/114). WIDER Working Paper.
Kopp, D., & Siegenthaler, M. (2021). Short-time work in and after the great recession. *Journal of the European Economic Association*.
Namikawa, L. M., Ferreira, K. R., Körting, T. S., de Queiroz, G. R., Boscolo, H. K., de Mello, A. J. H., ... Araújo, J. P. d. C. (2024). *Metodologia da Produção do Mapa de Inundações e Movimentos de Massa do Desastre do RS em Maio de 2024* (Tech. Rep.). Instituto Nacional de Pesquisas Espaciais.
Otero-Cortés, A., & Bohorquez-Penuela, C. (2020). Blame it on the rain: the effects of weather shocks on formal rural employment in Colombia. *Documento de Trabajo sobre Economía Regional y Urbana; No. 292*.
Pereira, R. H. M., Herszenhut, D., Ferreira, P. C. G., Mation, L., Stivali, M., & Cunha, A. (2024). *Uma estimativa de empresas e postos de trabalho diretamente atingidos pelas enchentes do Rio Grande do Sul em 2024* (Tech. Rep.). Retrieved 2025-09-04, from https://repositorio.ipea.gov.br/handle/11058/14186 (Publisher: Instituto de Pesquisa Econômica Aplicada (Ipea))
Pereira, R. H. M., Herszenhut, D., Ferreira, P. C. G., Mation, L. F., Cunha, A., & Stivali, M. (2024). *Uma estimativa da população atingida pelas enchentes do Rio Grande do Sul em 2024* (Tech. Rep.). Retrieved 2025-09-04, from https://repositorio.ipea.gov.br/handle/11058/14337 (Publisher: Instituto de Pesquisa Econômica Aplicada (Ipea))
Rodell, M., & Li, B. (2023). Changing intensity of hydroclimatic extreme events revealed by grace and grace-fo. *Nature Water*, *1*(3), 241–248.
Silva, S. P. (2026). Ações do ministério do trabalho e emprego diante da calamidade climática no rio grande do sul em 2024: O papel dos fundos trabalhistas (fat e fgts). *Mercado de Trabalho: Conjuntura e Análise. Brasília, DF: Ipea*, *32*(81).
Smith, M., Yagan, D., Zidar, O., & Zwick, E. (2019). Capitalists in the twenty-first century. *The Quarterly Journal of Economics*, *134*(4), 1675–1745.
Tebaldi, R. (2025). Social protection and the climate crisis: The case of Brazil's emergency responses to the 2024 Rio Grande do Sul floods. *International Social Security Review*, *78*(2-3), 123–144. doi: 10.1111/issr.70005
Venn, D. (2012). *Helping displaced workers back into jobs after a natural disaster: recent experiences in OECD countries* (Tech. Rep.). OECD.
Wagner, P. (2025). The hole dug deeper: Flash floods, income disparities, and labor informality in Brazil. *Ecological Economics*, *237*, 108689.
Xie, V. W. (2024). Labor market adjustment to extreme heat shocks: Evidence from

Brazil. *Journal of Economic Behavior & Organization*, *222*, 266–283.

Tables and Figures

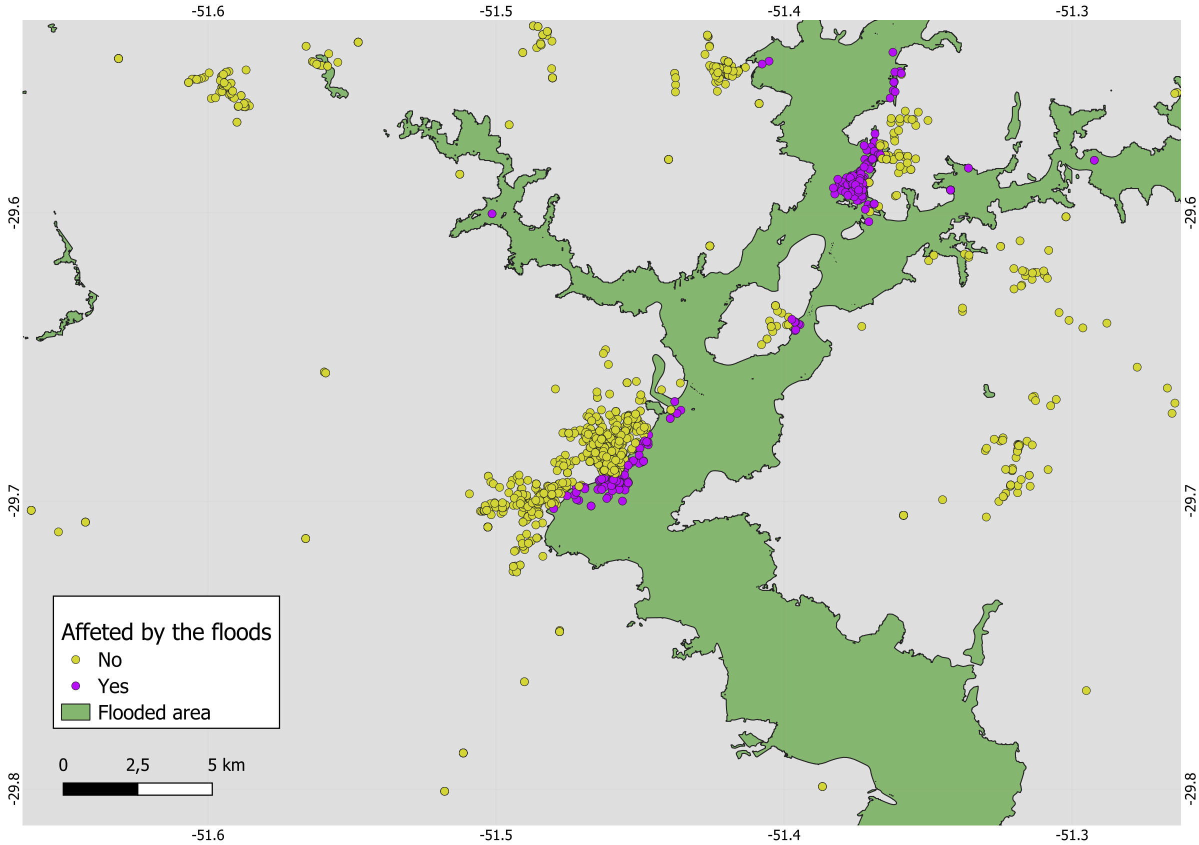

Figure 1: The assignment of affected units by the map of flooded areas

Notes: The green areas map flooded lands (characterized by land slides, water above ground level, and mudflows), while gray areas map the non-flooded areas. The purple points are the locations of establishments within the flooded areas, which we designate as affected (or impacted) throughout the paper. Yellow points locate establishments outside the flooded area. Even though we acknowledge the latter establishments suffered indirect consequences of the floods such as interrupted energy and internet supply or blocked roads, we refer to them as non-affected units. Empirically, we identify the marginal effects of being directly affected.

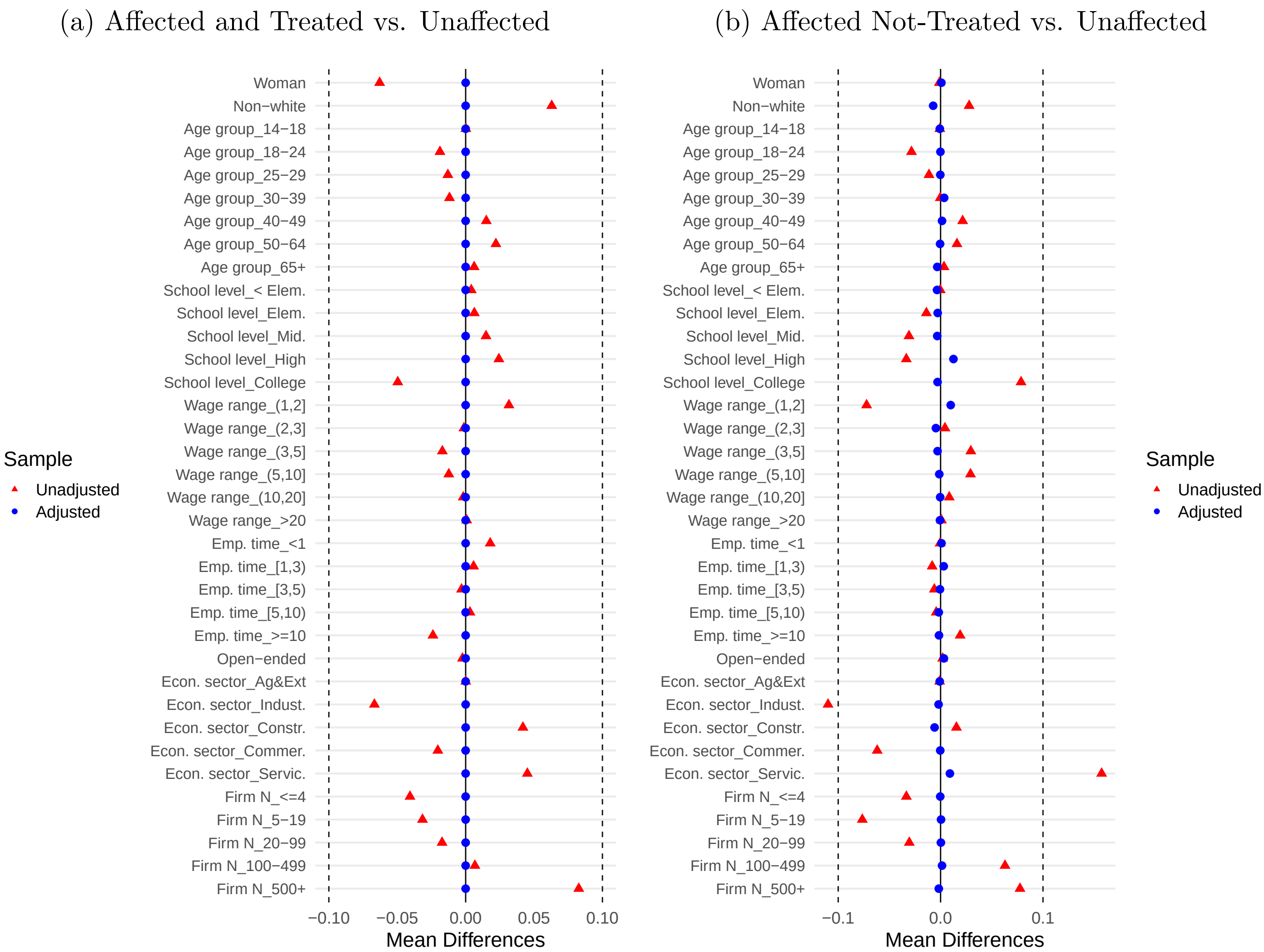

Figure 2: Matching balance assessment

Notes: Differences in covariate means between matched groups, computed before (red markers) and after (blue markers) CEM matching. The post-means apply the matching weights produced by CEM. Left panel: Affected and Treated (AT) versus Unaffected (UN). Right panel: Affected Not-Treated (AN) versus Unaffected (UN). Matching covariates include sex, race, age range, school attainment level, contracted hourly wage group scaled to minimum wage, years-in-the-job range, economic sector, and firm-size range in number of employees.

Figure 3: The spatial distribution of PEAF

Notes: The green areas map the flooded territory (characterized by landslides, water above ground level, and mudflows), while gray areas map non-flooded lands. Blue points have the coordinates of establishments that were impacted and took up PEAF, while red points are the locations of establishments that were impacted but did not take up the program. The gray points represent non-impacted firms.

Figure 4: Labor market: Estimated effects

Notes: These graphs report coefficients β_τ (green) and γ_τ (purple) estimated from equation 1 for all months $t \in$ [2023m4, 2024m12] using the listed labor market variables as outcomes. The omitted period is $t = -1$ (2024m3), so all results are relative to it. Treatment groups are defined at the establishment level based on the federal flood maps and PEAF participation. Panel (c) estimates effects on unconditional log-earnings via PPML; all other panels use OLS. Shaded areas are 95% confidence intervals; standard errors are clustered at the establishment level. The vertical dashed gray line marks the onset of the flooding period. The vertical red dotted lines indicate the beginning and conclusion of PEAF's standard operation, respectively.

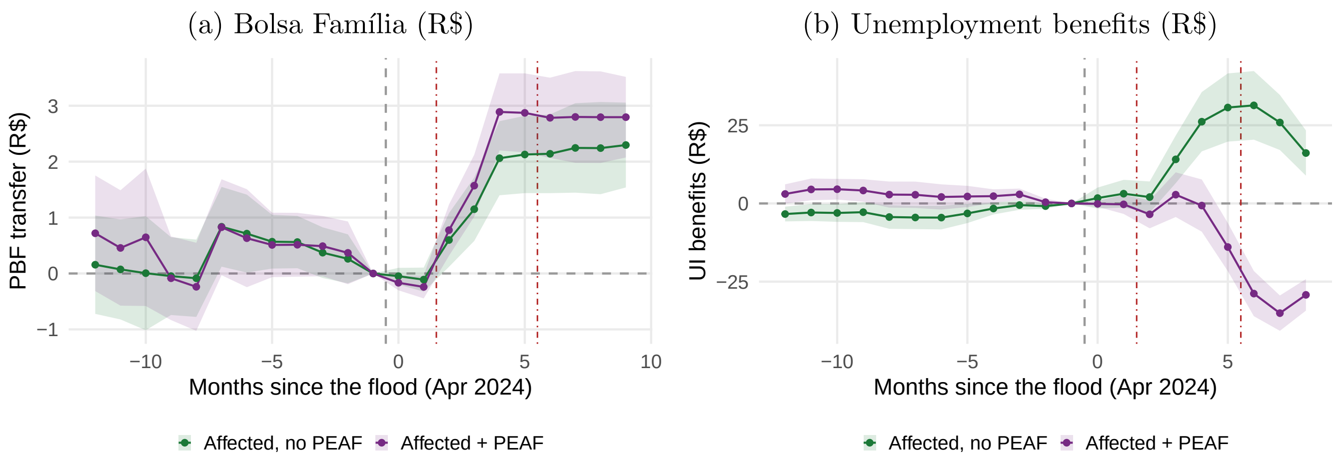

Figure 5: Social transfers: Estimated effects

Notes: These graphs report coefficients β_τ (green) and γ_τ (purple) estimated from equation 1 for all months $t \in$ [2023m4, 2024m12] using Bolsa Família transfers and unemployment insurance benefits as outcomes, respectively. The omitted period is $t = -1$ (2024m3), so all the results are relative to it. Treatment groups are defined at the establishment level based on the federal flood maps and PEAF participation. Shaded areas are 95% confidence intervals; standard errors are clustered at the establishment level. The vertical dashed gray line marks the onset of the flooding period. The vertical dotted red lines indicate the beginning and conclusion of PEAF's standard operation.

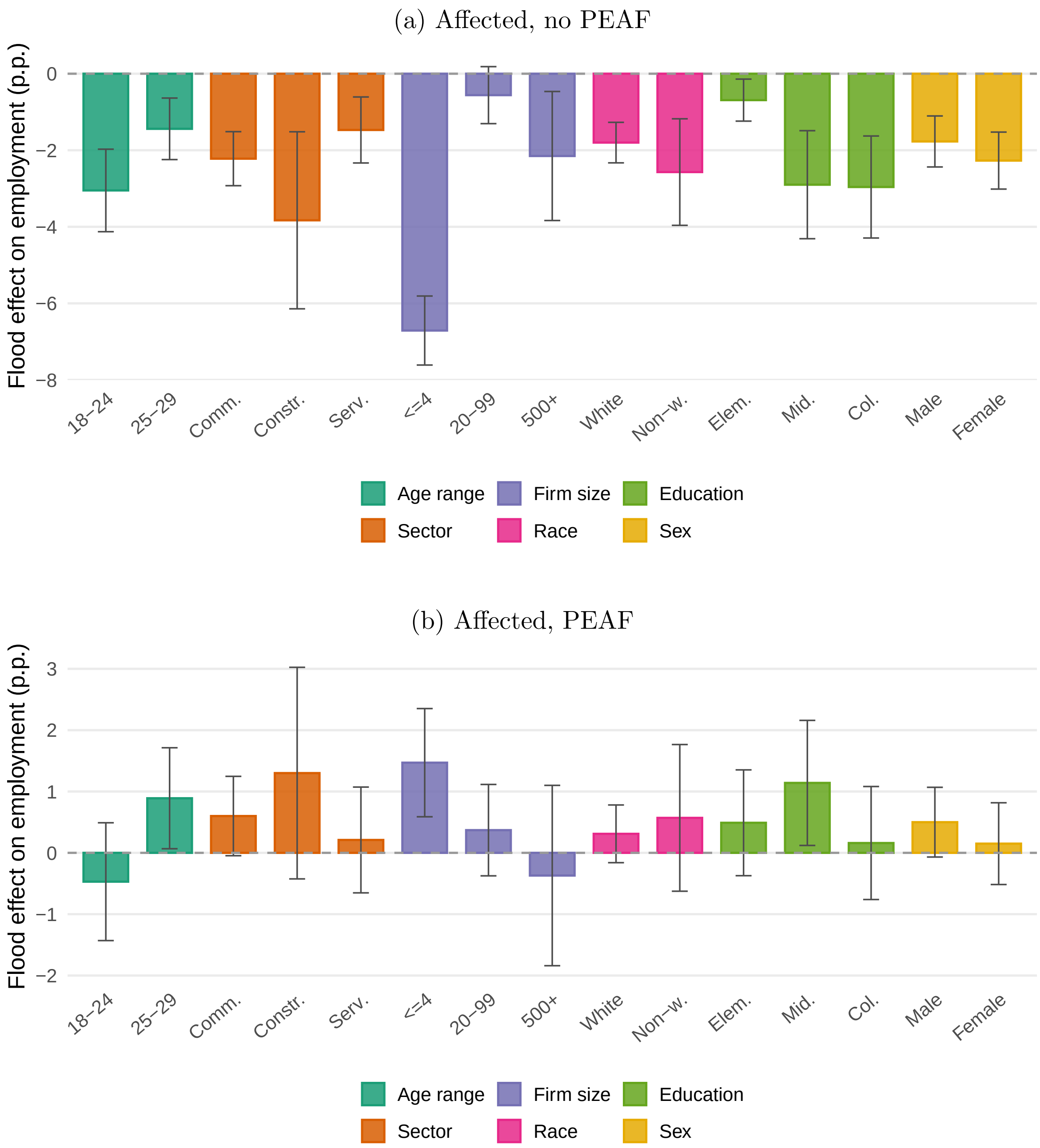

Figure 6: Heterogeneous estimated effects on employment

Notes: Average post-flood treatment effects β_τ (Panel (a)) and γ_τ (Panel (b)) on formal employment probability, estimated separately for each subgroup from equation 1. Post-event is defined for all the months $t \in [2024m4, 2024m12]$, and treatment effects are estimated relative to the omitted pre-event period, and over individual fixed effects and common post-event trend. Error bars represent 95% confidence intervals; standard errors are clustered at the establishment level. Panel (a): effects for workers at flood-exposed establishments without PEAF coverage (*Affected, no PEAF*). Panel (b): effects for workers at flood-exposed establishments with PEAF coverage (*Affected, PEAF*).

Table 1: Post-matching summary of covariates

Variable	Unaffected	Affected, not treated	Affected, treated
N. Observations	1,612,867	166,933	86,717
From full sample (%)	93.427	89.956	100.000
Woman	0.385	0.386	0.385
Non-white	0.200	0.193	0.200
Age	38.803	38.787	38.857
	(11.765)	(11.666)	(11.748)
School attainment			
< Elementary	0.014	0.011	0.014
Elementary	0.071	0.068	0.071
Middle school	0.169	0.166	0.169
High school	0.597	0.610	0.597
College	0.149	0.146	0.149
Earnings Mar24	3290.474	3313.679	3262.941
	(2923.234)	(3273.499)	(3511.668)
Tenure	3.849	3.799	3.790
	(5.326)	(5.266)	(5.094)
Open-ended contract	0.986	0.990	0.986
Economic sector			
Agri. & Extractive	0.001	0.000	0.001
Manufacturing	0.228	0.226	0.228
Constr. & Utilities	0.078	0.072	0.078
Commerce	0.239	0.238	0.239
Services	0.454	0.463	0.454

Notes: The table reports means of worker-level characteristics for each group after coarsened exact matching (CEM). Standard deviations appear in parentheses for continuous variables. The sample comprises all formal private sector workers formally employed in Rio Grande do Sul on March 1, 2024, retained in the matched analytical sample. *Affected, not treated* denotes workers at establishments within the federally mapped flood zone that did not participate in PEAF; *Affected, treated* denotes workers at flood-zone establishments that participated in PEAF; *Unaffected* denotes workers at establishments outside the flood zone retained as controls. *From full-sample (%)* is the share of each pre-matching group retained after CEM. Earnings are in Brazilian reais (R$) measured at the March 2024 baseline.

Table 2: Employment and earnings results

Group	Employment & Mobility: Employment (pp)	Employment & Mobility: Job switch (pp)	Earnings (R$): log Earnings unconditional	Earnings (R$): log Earnings if employed
Affected, no PEAF	-0.020^{***}	$+0.009^{**}$	-0.021^{***}	-0.008^{***}
	(0.003)	(0.004)	(0.003)	(0.002)
Affected, PEAF	$+0.004$	$+0.003$	-0.010^{**}	-0.013^{***}
	(0.003)	(0.004)	(0.004)	(0.002)
Program impact	$+0.024^{***}$	-0.006	$+0.011^{**}$	-0.005^{**}
(PEAF − no PEAF)	(0.004)	(0.005)	(0.005)	(0.003)
Control mean (post-flood)	0.90	0.06	3,094	3,445
R^2	0.343	0.365		0.870
Adj. Pseudo R^2			0.809	
Observations	38,302,992	35,347,002	37,560,603	34,341,675

Notes: Average post-flood treatment effects (β_τ and γ_τ) estimated from equation 1 for the aggregation of the months $t \in [2024m4, 2024m12]$ as post-event using listed labor market variables as outcomes. Post-event is estimated relative to the omitted pre-event period, and over individual fixed effects as well as common post-period effect. Treatment groups are defined at the establishment level based on the federal flood mapping and PEAF participation. The *Program impact* row reports the difference (PEAF − no PEAF) with standard errors from the delta method applied to the variance-covariance matrix. Unconditional log-earnings are estimated via PPML, conditional log-earnings and the other outcomes via OLS. Control means refer to the unaffected group at the post-event, before any log transformation. Standard errors are clustered at the establishment level and reported in parentheses. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

Table 3: Social-protection benefit receipt results

Group	Cash transfers		Unemployment Insurance	
	CadUnico registration (pp)	**Bolsa Família (R$)**	**UI receipt (pp)**	**UI benefits (R$)**
Affected, no PEAF	$+0.002^{***}$	$+1.120^{***}$	$+0.011^{***}$	$+19.430^{***}$
	(0.000)	(0.286)	(0.002)	(3.390)
Affected, PEAF	$+0.003^{***}$	$+1.389^{***}$	-0.008^{***}	-14.740^{***}
	(0.000)	(0.307)	(0.001)	(2.298)
Program impact	$+0.001$	$+0.269$	-0.019^{***}	-34.172^{***}
(PEAF − no PEAF)	(0.001)	(0.392)	(0.002)	(3.768)
Control mean (post-flood)	0.048	28.810	0.040	64.180
R^2	0.881	0.850	0.197	0.200
Observations	39,196,626	39,196,626	39,196,626	39,196,626

Notes: Average post-flood effects (β_τ and γ_τ) estimated from equation 1 for the aggregation of the months $t \in [2024m4, 2024m12]$ as post-event using listed social-protection variables as outcomes. Post-event treatment effects are estimated relative to the omitted pre-event period and over individual fixed effects and common post-event effect. Treatment groups are defined at the establishment level based on the federal flood maps and PEAF participation. The *Program impact* row reports the difference (PEAF − no PEAF) with standard errors from the delta method applied to the variance-covariance matrix. Unconditional log-earnings are estimated via PPML, conditional log-earnings and the outcomes via OLS. Control means refer to the unaffected group at the post-event, before any log transformation. Standard errors are clustered at the establishment level and reported in parentheses. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

Table 4: Robustness checks

	Employment			**Earnings (Poisson)**		
Group	**(1)**	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**
Affected, no PEAF	-0.020^{***}	-0.020^{***}	-0.018^{***}	-0.0211^{***}	-0.0208^{***}	-0.0198^{***}
	(0.003)	(0.003)	(0.005)	(0.0035)	(0.0033)	(0.0052)
Affected, PEAF	+0.004	+0.004	+0.006	-0.0101^{***}	-0.0103^{***}	-0.0091^{*}
	(0.003)	(0.002)	(0.004)	(0.0038)	(0.0037)	(0.0054)
Program impact	$+0.024^{***}$	$+0.024^{***}$	$+0.023^{***}$	$+0.0110^{**}$	$+0.0105^{**}$	$+0.0107^{**}$
(PEAF − no PEAF)	(0.004)	(0.003)	(0.004)	(0.0048)	(0.0046)	(0.0047)
Fixed effects						
Individual	✓	✓	✓	✓	✓	✓
Time	✓	×	✓	✓	×	✓
Time strata	×	✓	×	×	✓	×
Sample						
Excludes neighboring firms and affected municipalities	×	×	✓	×	×	✓
R^2	0.343	0.383	0.339	–	–	–
Pseudo-R^2	–	–	–	0.809	0.813	0.937
Observations	38,302,992	38,302,992	7,319,382	37,560,603	37,560,596	7,205,762

Notes: This table reports robustness-check estimates. Specification (1): Follows Table 2, with specification from equation 1 and full matched sample. Specification (2): Follows specification (1), except for changing the time fixed effect for interacted time per CEM strata fixed effects. Specification (3): follows specification (1) but restricts control units to those outside flood-affected municipalities and more than 5 km (by Euclidean distance) from flooded areas in the matched sample. Flood-affected municipalities are defined as those having one or more affected establishment. For specification (3), CEM weights are calibrated again, taking into account the new ratios between groups and the number of observations within strata. Treatment groups are defined at the establishment level based on the federal flood maps and PEAF participation. The *Program impact* row reports the difference (PEAF − no PEAF) with standard errors from the delta method. Standard errors are clustered at the establishment level and reported in parentheses. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

A Appendix

Table A.1: Pre-Matching Summary of Covariates

Variable	Unaffected	Affected, not treated	Affected, treated
N	1,726,331	185,572	86,717
Woman	0.448	0.446	0.385
Nonwhite	0.137	0.165	0.200
Age	37.652	38.865	38.857
	(11.529)	(11.531)	(11.748)
School attainment			
< Elementary	0.010	0.010	0.014
Elementary	0.065	0.051	0.071
Middle school	0.154	0.123	0.169
High school	0.573	0.539	0.597
College	0.198	0.277	0.149
Earnings Mar24	3477.741	4438.118	3262.941
	(3236.767)	(5362.255)	(3511.668)
Tenure	4.212	4.636	3.790
	(5.696)	(6.543)	(5.094)
Open-ended contract	0.989	0.991	0.986
Economic sector			
Agri. & Extrative	0.001	0.000	0.001
Manufacturing	0.295	0.185	0.228
Constr. & Utilities	0.036	0.051	0.078
Commerce	0.259	0.197	0.239
Services	0.409	0.566	0.454

Notes: The table reports means of worker-level characteristics for each group before coarsened exact matching (CEM). Standard deviations appear in parentheses for continuous variables. The sample comprises all formal private-sector workers formally employed in Rio Grande do Sul on 1 March 2024. *Affected, not treated* denotes workers at establishments within the federally mapped flood zone that did not participate in PEAF; *Affected, treated* denotes workers at flood-zone establishments that participated in PEAF; *Unaffected* denotes workers at establishments outside the flood zone retained as controls. Earnings are in Brazilian reais (R$) measured at the March 2024 baseline.

Table A.2: Pre-Matching Summary of Observable Firm Characteristics

Variable	Unaffected	Affected, not treated	Affected, treated
N. Observations	1,726,331	185,572	86,717
N. employees Mar24	371.613	406.339	575.648
	(1004.599)	(659.962)	(1097.413)
Payroll Mar24	1,852,393.22	2,682,041.72	1,612,324.05
	(6,655,602.22)	(7,581,966.02)	(2,838,000.69)
Share Afro-Br employees	0.170	0.198	0.221
	(0.151)	(0.149)	(0.155)
Share women	0.456	0.464	0.397
	(0.295)	(0.266)	(0.273)
Share young	0.345	0.299	0.310
	(0.195)	(0.187)	(0.175)
Share college-degree	0.244	0.322	0.180
	(0.259)	(0.317)	(0.203)
N. employees Mar23	346.377	391.428	518.844
	(931.162)	(657.450)	(970.378)
Payroll Mar23	1,660,463.90	2,490,914.56	1,355,723.05
	(5,929,874.73)	(7,111,900.77)	(2,297,435.01)
Turnover rate	0.368	0.360	0.384
	(0.250)	(0.249)	(0.222)

Notes: The table reports means of establishment-level characteristics for each group prior to coarsened exact matching. The sample comprises all formal private-sector workers formally employed in Rio Grande do Sul on 1 March 2024. *Affected, not treated* denotes workers at establishments within the federally mapped flood zone that did not participate in PEAF; *Affected, treated* denotes workers at flood-zone establishments that participated in PEAF; *Unaffected* denotes workers at establishments outside the flood zone retained as controls. *Share Afro-Br employees*, *Share women*, *Share young*, and *Share college-degree* are the fractions of establishment employees who are Afro-Brazilian, female, aged 18–24, and holding a college-degree, respectively. Payroll is in Brazilian reais (R$). *Turnover rate* is the inter-annual employment turnover rate from March 2023 to March 2024. Payroll values are in Brazilian reais (R$) measured at the March 2024 baseline.

Table A.3: Summary of Observable Firm-level Characteristics After the Matching

Variable	Unaffected	Affected, not treated	Affected, treated
N	1,612,867	166,933	86,717
N. employees Mar24	459.518	345.508	575.648
	(1,086.892)	(556.431)	(1,097.413)
Payroll Mar24	2,013,844.10	1,395,197.00	1,612,324.05
	(6,357,343.72)	(3,613,537.00)	(2,838,000.69)
Share Afro-Br employees	0.192	0.210	0.221
	(0.165)	(0.157)	(0.155)
Share women	0.435	0.442	0.397
	(0.296)	(0.277)	(0.273)
Share young	0.329	0.310	0.310
	(0.185)	(0.183)	(0.175)
Share college-degree	0.218	0.231	0.180
	(0.237)	(0.254)	(0.203)
N. employees Mar23	410.007	326.026	518.844
	(997.294)	(539.757)	(970.378)
Payroll Mar23	1,776,928.97	1,270,764.00	1,355,723.05
	(5,744,646.34)	(3,389,181.00)	(2,297,435.01)
Turnover rate	0.377	0.383	0.384
	(0.244)	(0.247)	(0.222)

Notes: The table reports means of establishment-level characteristics for each group after coarsened exact matching (CEM). Standard deviations appear in parentheses. These variables were not included in the CEM algorithm. *Affected, not treated* denotes workers at establishments within the federally mapped flood zone that did not participate in PEAF; *Affected, treated* denotes workers at flood-zone establishments that participated in PEAF; *Unaffected* denotes workers at establishments outside the flood zone retained as controls. *Share Afro-Br employees*, *Share women*, *Share young*, and *Share college-degree* are the fractions of establishment employees who are Afro-Brazilian, female, aged 18–24, and holding a college-degree, respectively. Payroll is in Brazilian reais (R$). *Turnover rate* is the inter-annual employment turnover rate from March 2023 to March 2024.

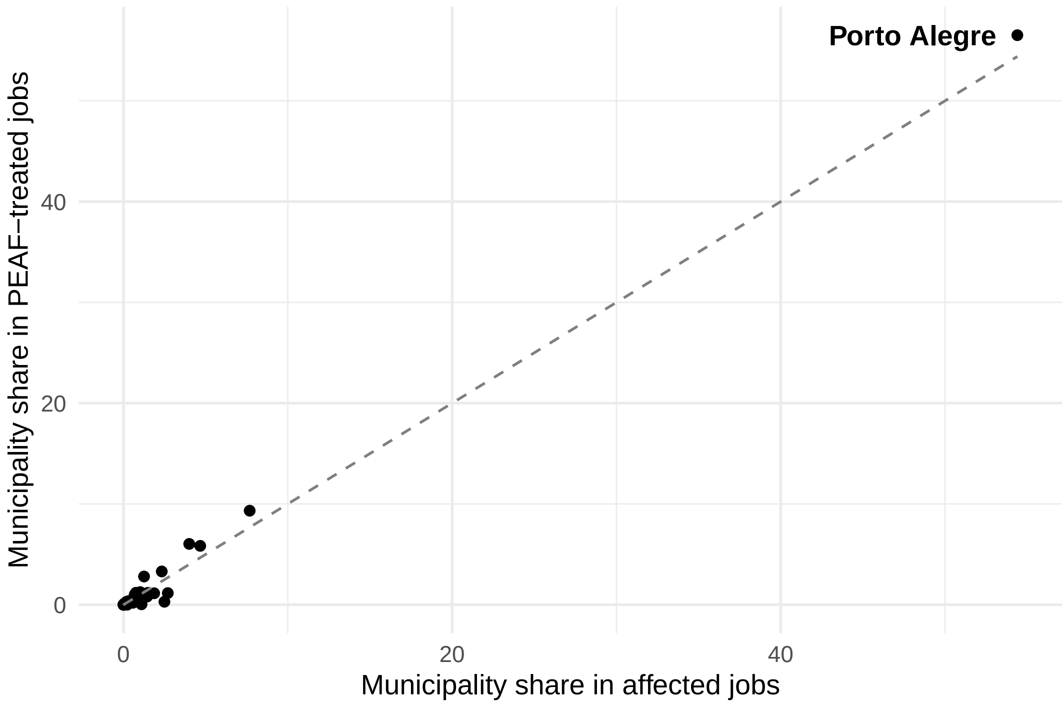

Figure A.1: Municipalities proportion of affected jobs compared with proportion of PEAF take-up

Notes: Each dot represents a municipality in Rio Grande do Sul with at least one flood-affected formal establishment. The horizontal axis measures the share of formal workers in the municipality employed at flood-affected establishments, according to the April 2024 federal flood mapping. The vertical axis measures the share of workers in the municipality employed at PEAF benefited establishments according to the administrative data of the program.

Figure A.2: Labor market estimated program impact

Notes: These graphs report the respective differences between coefficients γ_τ and coefficients β_τ estimated from equation 1 for all months $t \in$ [2023m4, 2024m12] using the listed labor-market variables as outcomes. The omitted period is $t = -1$ (2024m3), so all the results are relative to that. Treatment groups are defined at the establishment-level based on the federal flood mapping and PEAF participation. This is equivalent to estimate the event-study retaining only flood affected units, and rewriting the specification to assign PEAF beneficiaries as the single treated group, while non-PEAF becomes the control one. Panel (c) estimates effects on unconditional log-earnings via PPML; all other panels use OLS. Shaded areas are 95% confidence intervals; standard errors clustered at the establishment-level. The vertical dashed gray line marks the onset of the flooding period. The vertical red dotted lines indicate the entry into force and the conclusion of PEAF's standard operation, respectively.

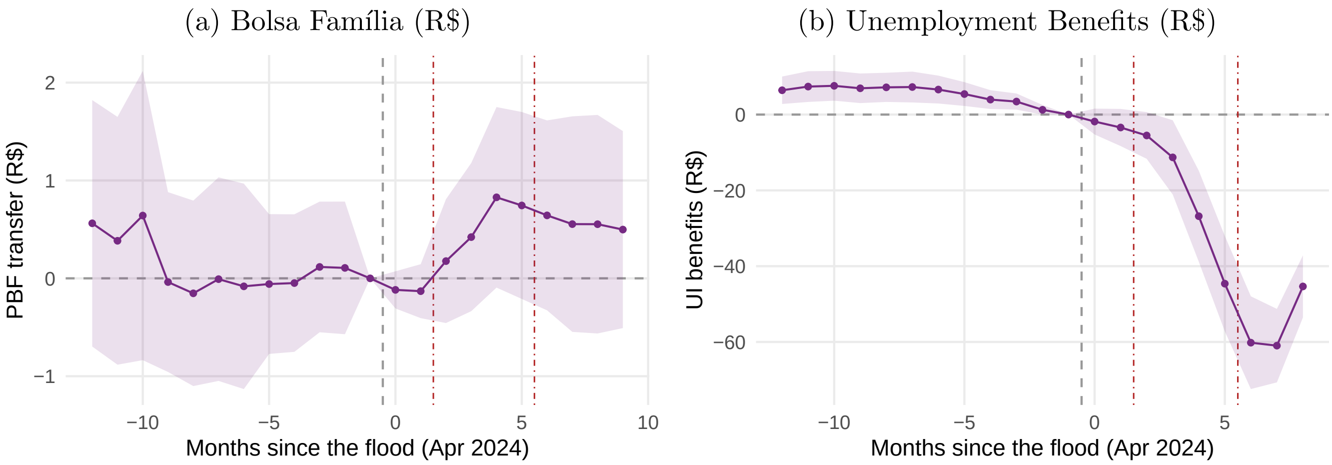

Figure A.3: Social transfers estimated program impact

Notes: These graphs report the respective differences between coefficients γ_τ and coefficients β_τ estimated from equation 1 for for all months $t \in$ [2023m4, 2024m12] using Bolsa Familia transfers and Unemployment Insurance benefits as outcomes, respectively. The omitted period is $t = -1$ (2024m3), so all the results are relative to that. Treatment groups are defined at the establishment-level based on the federal flood mapping and PEAF participation. This is equivalent to estimate the event-study retaining only flood affected units, and rewriting the specification to assign PEAF beneficiaries as the single treated group, while non-PEAF becomes the control one. Shaded areas are 95% confidence intervals; standard errors clustered at the establishment-level. The vertical dashed gray line marks the onset of the flooding period. The vertical dotted red lines indicate the entry into force and the conclusion of PEAF's standard operation, respectively.

Table A.4: Heterogeneous effects on formal employment by subgroup

Dimension	Subgroup	Affected, no PEAF	Affected, PEAF	Obs.	R^2
Woman (Gender)					
	No (Men)	-0.018***	0.005*	21,218,085	0.350
		(0.003)	(0.003)		
	Yes (Women)	-0.023***	0.002	17,013,675	0.334
		(0.004)	(0.003)		
Non-white (Race)					
	No (White)	-0.018***	0.003	32,862,396	0.341
		(0.003)	(0.002)		
	Yes (Non-white)	-0.026***	0.006	5,369,364	0.344
		(0.007)	(0.006)		
Age Range					
	14-18	-0.059	-0.030	26,229	0.272
		(0.079)	(0.053)		
	18-24	-0.031***	-0.005	4,893,525	0.309
		(0.006)	(0.005)		
	25-29	-0.014***	0.009**	5,683,650	0.329
		(0.004)	(0.004)		
	30-39	-0.018***	0.005	11,302,284	0.340
		(0.004)	(0.003)		
	40-49	-0.019***	0.004	9,587,844	0.349
		(0.003)	(0.003)		
	50-64	-0.020***	0.003	6,369,678	0.349
		(0.005)	(0.003)		
	65+	-0.015	0.003	368,550	0.354
		(0.011)	(0.010)		
School Attainment					
	< Elem.	-0.031**	0.017	386,211	0.371
		(0.013)	(0.011)		
	Elem.	-0.007**	0.005	7,815,486	0.322
		(0.003)	(0.004)		
	Mid.	-0.029***	0.011**	2,426,508	0.374
		(0.007)	(0.005)		
	High	-0.019***	0.003	21,823,557	0.338
		(0.003)	(0.003)		
	College	-0.030***	0.002	5,779,998	0.354
		(0.007)	(0.005)		
Wages one year before (Minimum wages)					
	≤ 1	-0.023***	0.004	27,154,365	0.347
		(0.004)	(0.003)		
	(1,2]	0.010	-0.012	230,349	0.315
		(0.011)	(0.015)		
	(2,3]	-0.010**	0.002	6,198,969	0.329
		(0.005)	(0.007)		
	(3,5]	-0.005	0.009	3,241,098	0.325
		(0.007)	(0.011)		
	(5,10]	0.001	0.001	1,368,801	0.305
		(0.007)	(0.008)		
	(10,20]	-0.050	-0.002	38,178	0.280
		(0.035)	(0.029)		
Firm Size					
	≤ 4	-0.067***	0.015***	4,594,464	0.334
		(0.005)	(0.005)		
	5-19	-0.012*	0.007	7,878,822	0.341
		(0.007)	(0.006)		
	20-99	-0.006	0.004	10,039,722	0.342
		(0.004)	(0.004)		
	100-499	-0.022***	0.004	9,211,818	0.346
		(0.005)	(0.003)		
	500+	-0.022**	-0.004	6,506,934	0.345
		(0.009)	(0.008)		
Economic Sector					
	Ag&Ext	0.070	0.021	39,648	0.323
		(0.055)	(0.039)		
	Indust.	-0.022***	0.006*	9,655,695	0.333
		(0.004)	(0.003)		
	Constr.	-0.038***	0.013	1,444,296	0.363
		(0.012)	(0.009)		
	Commer.	-0.022***	0.001	10,850,742	0.339
		(0.007)	(0.003)		
	Servic.	-0.015***	0.002	16,241,379	0.344
		(0.004)	(0.004)		
Open-ended Contract					
	No (Fixed term)	-0.005	-0.024	334,152	0.361
		(0.024)	(0.018)		
	Yes (Open)	-0.020***	0.004	37,897,608	0.332
		(0.003)	(0.003)		

Clustered (at firm-level) standard-errors in parentheses.
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Each row reports the average post-flood treatment effects on formal employment probability for workers in the specified subgroup. Columns "Affected, no PEAF" and column "Affected, PEAF" report estimated β_τ and γ_τ, respectively, from equation 1, with time aggregated as post-flood if $t \in [2024m4, 2024m12]$ and pre-flood otherwise. Standard-errors are clustered at the establishment-level and reported in parentheses. Signifiance codes: ***: 0.01, **: 0.05, *: 0.1.