# The Burden of Care: Labor Market Spillovers from a Mental Health Reform in Brazil

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**Abstract**

The psychiatric reform movement of the 1990s promoted a shift away from psychiatric hospitalization and toward community based mental health care. In this context, Brazil developed several mental health reforms, including the expansion of Psychosocial Care Centers (CAPS) after 2002. Using administrative RAIS data for 2002–2018 and a Stacked Difference-in-Differences framework, the analysis exploits the staggered rollout of CAPS across Brazilian municipalities. Both direct effects, through employment in mental health related occupations, and indirect effects, through caregiving responsibilities, are considered. The direct effect analysis shows an increase in psychiatrist employment, particularly an increase of 0.0236 in the number of active psychiatrists and 0.024 in the number of active psychiatrist jobs per 10,000 inhabitants. The main indirect result is a reduction of 2.2% in the number of active female formal workers after CAPS opening. There is little evidence of significant changes in hours worked or part-time employment, suggesting that the adjustment occurs mainly through the extensive margin rather than through changes in working time. For men, the results show a smaller reduction in earnings of approximately 0.5%, with no clear evidence of adjustment through hours worked. Overall, the results indicate that mental health reforms may generate broader labor market spillovers, particularly when community based care shifts part of the care burden from institutions to households.

## 1 Introduction

Mental disorders constitute a major global public health challenge and have become even more salient since the COVID-19 pandemic. According to the World Health Organization (2025), more than one billion people worldwide live with a diagnosable mental disorder. To put this in perspective, Chisholm et al. (2016) estimate that depression and anxiety lead to the loss of 12 billion working days annually worldwide, representing an economic

cost of nearly 1 trillion US dollars. This highlights the importance of mental health policies not only for health outcomes, but also for broader social and economic consequences.

During the 1990s, mental health reforms in several countries promoted the deinstitutionalization of the existing system of psychiatric care. The objective was to replace the hospital centered model with community based services that preserved patients' relationships with their families and local communities, facilitating their daily life outside psychiatric hospitals. Unexpectedly, these novel care systems can generate new unintended consequences if the public services are not enough to cover patients' care needs. This translates into burdens for families, with a potential gender bias since women are more likely to bear this responsibility.

Schmitz and Westphal (2017); Maestas et al. (2024); Brito and Contreras (2025) exhibits that responses to relatives' health shocks are unequal, as women are the ones who experience reductions in earnings and labor supply, widening the gender gap. Furthermore, health shocks affect not only economic outcomes, but also generate broader family responses through stress, mental health, and changes in household responsibilities (Schmitz and Westphal, 2015; Glaser and Pruckner, 2023; López Artero et al., 2025). In this context, it is important to acknowledge the caregiver burden, closely related to this result, which refers to a deterioration in physical, emotional, and psychological health caused by the strain of taking care of an ill family member.

At the same time, the relationship between mental health and economic outcomes is bidirectional and may apply to both patients and caregivers. Poor mental health is associated with higher absenteeism and presenteeism (Stewart et al., 2003; Evans-Lacko and Knapp, 2016), higher worker turnover (Andersen et al., 2024), and lower earnings (Biasi et al., 2021; Germinario et al., 2022). In turn, poverty, unemployment, and economic insecurity may worsen mental wellbeing, perpetuating poverty (Ridley et al., 2020; Haushofer and Fehr, 2014). In this case, these effects may play out through two groups, patients and caregivers, placing already vulnerable households at even greater risk.

This paper studies the labor market consequences of the expansion of Psychosocial Care Centers (CAPS) in Brazil. CAPS are the main community based mental health centers created under the Brazilian reform, and their staggered expansion across municipalities allows us to analyze their local labor market effects. The paper first analyzes the direct effects of CAPS openings through mental health related professionals as a validation exercise. Nonetheless, the main focus is on the indirect effects that may arise due to household caregiving responsibilities. By studying employment, hours worked, part-time employment, and earnings separately for women and men, the analysis examines whether a reform designed to improve mental health care may also generate unintended labor market spillovers.

This paper contributes to three strands of literature. First, it contributes to the literature on the economic and household consequences of health shocks (Schmitz and Westphal,

2017; López Artero et al., 2025). Second, it relates to the literature on caregiving burdens and gendered responses of labor market to family care responsibilities (Maestas et al., 2024; Brito and Contreras, 2025). Third, it contributes to the evaluation of mental health policies. Existing evidence on the Brazilian reform shows that CAPS reduced psychiatric hospitalizations and increased the number of mental health providers (Dias and Fontes, 2024). This paper complements that evidence by studying whether the same reform also generated broader labor market spillovers.

Using administrative RAIS data for 2002-2018 and a Stacked Difference-in-Differences framework, the analysis estimates the labor market effects of CAPS openings across Brazilian municipalities. The results indicate that CAPS openings are associated with an increase in the number of active psychiatrists by 0.0236 workers per 10,000 inhabitants. The main indirect result is a reduction of 2.2% in the number of active female formal workers after CAPS opening, suggesting a decline in women's attachment to active formal employment. There is little evidence of significant changes in hours worked or part-time employment which points to an adjustment which occurs mainly through the extensive margin rather than through changes in working time. For men, there is a reduction of approximately 0.5% in total earnings, with no clear evidence of adjustment through hours worked.

The rest of the paper is organized as follows. Section 2 explains the institutional context. Section 3 describes the data used and the construction of the main variables. Section 4 presents the identification strategy and empirical specification. Section 5 analyzes the Brazilian labor market in the years of the policy through a descriptive analysis. Section 6 shows the main results, distinguishing between direct and indirect effects. Section 7 presents the robustness checks. Section 8 outlines the limitations and possible further research. Lastly, section 9 discusses the results and draws the conclusions.

# 2 Context

Brazil, like many Latin American countries, endured a military dictatorship from 1964 to 1985, during which public social services were limited and health governance remained highly centralized.

The 1988 Constitution guaranteed the universal right to health and defined it as a responsibility of the State. Title VIII, Chapter II, Section II, Articles 196-200 established the basis for a decentralized, universal, and unified public health system (Brazil, 1988). This framework led to the creation of the *Sistema Único de Saúde* (SUS).

Mental health became a central concern within this broader process of health system reform. A key regional milestone was the 1990 Caracas Declaration, adopted at the Regional Conference for the Restructuring of Psychiatric Care in Latin America, organized by the Pan American Health Organization (PAHO), the regional office of the World Health

Organization (WHO) for the Americas. The declaration promoted a change from the hospital centered psychiatric model and toward community based care, with the aim of humanizing treatment and reducing the isolation of people with mental illness from family and community life (Tenório, 2002; Mari, 2014).

In Brazil, this transition was particularly relevant given the strong reliance on psychiatric hospitalization. In 1995, 95.5% of the mental health budget was allocated to psychiatric admissions, highlighting the limited development of community based and outpatient care (Andreoli et al., 2007). The reform was later formalized through Law 10.216/2001 and the creation of Psychosocial Care Centers (*Centros de Atenção Psicossocial*, CAPS) under Portaria 336/2002. The network was subsequently reorganized within the broader *Rede de Atenção Psicossocial* (RAPS), reflecting the continued relevance of mental health policy within SUS.

CAPS are community based public mental health services designed to provide psychiatric care outside the hospital setting while maintaining users' bonds with family and community life. They consist of multidisciplinary teams composed of psychiatrists, psychologists, occupational therapists, social workers, nurses, and other health professionals. There are three main types of CAPS according to the population of the municipality: CAPS I (15,000-70,000 inhabitants), CAPS II (70,000-150,000 inhabitants), and CAPS III (for more than 150,000 inhabitants and open 24 hours). Additionally, there are two other types which do not conflict with having other CAPS: CAPS AD (I or III, depending on the population) for substance and alcohol abuse and CAPS i, specialized in the treatment of children and teenagers.

The expansion of CAPS has been associated with a substantial reduction in psychiatric hospitalizations, with effects more pronounced in log stay admissions (Dias and Fontes, 2024). Between 2000 and 2014, the overall rate of patients hospitalized for mental and behavioral disorders by SUS fell from 188.5 to 94.4 per 100,000 inhabitants (Rocha et al., 2021). According to Dias and Fontes (2024), this reduction was mainly driven by patients with schizophrenia. This is relevant for the mechanism considered, since schizophrenia is a severe and persistent mental disorder that often requires long term support and supervision. If community based services do not fully substitute for inpatient care, part of the care burden may shift to households. Given that informal caregiving responsibilities are disproportionately borne by women, this institutional change may have different implications for women and men in the formal labor market.

Despite the institutional expansion of community mental health care, the system continued to face persistent funding constraints. These constraints became particularly important with the fiscal austerity measures introduced in the mid 2010s. In 2016, Constitutional Amendment 95 capped federal primary expenditure in real terms, allowing spending to grow only with inflation and placing additional pressure on an already constrained mental health system.

# 3 Data

All data come from Brazilian administrative sources. The expansion of CAPS is implemented at the municipality level, while the outcomes of interest are individual labor market outcomes collapsed at the municipality level. Therefore, the main unit of analysis is the municipality-year.

The main labor market outcomes are constructed from the *Relação Anual de Informações Sociais* (RAIS), a matched employer-employee dataset covering the Brazilian formal labor market. We use RAIS from 2002 to 2018 which provides yearly information on workers' municipality of employment, gender, earnings, contracted weekly hours, occupation, and other employment characteristics.

Additionally, to obtain pretreatment municipality characteristics and a descriptive analysis of the Brazilian labor market, including informal work, we use microdata from the Brazilian Population Census for the years 2000 and 2010. The Census includes anonymized individual level information on age, education, municipality, occupation, hours worked, earnings, and household characteristics, such as access to water, electricity, telephone, washing machine, and other assets. Although the Census is not available annually, it allows us to analyze the labor market at two benchmark moments: 2000, before the main expansion of CAPS, and 2010, during the expansion period, when CAPS had already been introduced in many municipalities.

The key treatment variable, $CAPS_{mt}$, is the opening of Psychosocial Care Centers in municipality $m$ and year $t$. Data on CAPS openings are provided by the Brazilian Ministry of Health and compiled by Dias and Fontes (2024). The dataset identifies the year in which each CAPS began operating, as well as its type, and covers the period 2002-2018. For each municipality, the treatment year is defined as the first year in which a CAPS is observed.

Labor market outcomes are constructed using RAIS microdata. For the main analysis, employment relationships are first aggregated at the worker-year level, so that each individual is counted only once per year. If a worker has jobs in different municipalities, the worker-year observation is assigned to the municipality of the main job. The main job is defined as the one contributing the most to the worker's annual earnings, taking into account the number of months during which the relationship was active. In the event of a tie, we prioritize the employment relationship with the longest duration and then the one with the greatest number of contracted weekly hours. Then, the data are aggregated separately by gender, producing a municipality-year-gender panel.

The main extensive margin outcomes are the log number of formal workers, the log number of active formal workers, and the share of workers active at the end of the year. Active workers are defined as those with at least one active formal employment relationship on December 31. The difference between the first two variables is that the number of

formal workers includes all individuals observed in RAIS during the year, even if they were no longer active at the end of the year. This constitutes a broader measure of the participation in the formal market including sporadic or short term formal jobs. In contrast, the number of active formal workers only counts workers who still had an active formal employment relationship at the end of the year and it is intended to quantify the stock of workers remaining in the formal labor market at that point, enabling us to compare this stock across years. We distinguish between the two variables to assess whether the change in formal employment is generalized or is concentrated in only one of these dimensions. It could be, for example, that the number of individuals in the formal market remains unchanged, but that the jobs do not last until the end of the year, then, the number of workers active at the end of the year is fewer. Moreover, the share of active formal workers is defined as the number of active formal workers divided by the total number of formal workers in the same municipality, year and gender. This variable helps us identify which dimension is driving the change, if there is one, or if both dimensions move in the same direction, indicating a contraction or expansion of the formal labor market.

The intensive margin outcomes include total weekly hours, part-time employment, and multiple-job holding. Additionally, we also study earnings and hourly wage as monetary compensation. These outcomes are constructed among workers with active formal employment at the end of the year, using those jobs that were active at that time. Hourly wages are constructed by dividing monthly earnings by weekly hours multiplied by 4.33, the number of weeks included in a month. Part-time employment is defined as working 25 hours per week or less according to the Consolidação das Leis do Trabalho, article 58A. Since the legal definition of part-time work changed in 2017, increasing the threshold from 25 to 30 weekly hours, we also analyze an alternative definition which considers part-time as working 30 hours per week or less. Furthermore, for the part-time and multiple-job outcomes, the denominator is the number of active formal workers in the corresponding municipality, year and gender.

Gender specific outcomes are estimated separately for women and men. We also construct gender gap outcomes, defined as the female outcome minus the male outcome within each municipality-year. A negative coefficient in the gender gap specification, therefore, indicates a relative decline in the female outcome compared to the male outcome.

For the occupation level analysis, we focus on five mental health occupations directly related to CAPS implementation: psychiatrists, clinical psychologists, social psychologists, occupational therapists and social workers. We also construct two broader categories: psychologists and all mental health professionals. The occupation analysis starts in 2003 because of changes in occupational codes around 2002, which make it difficult to build a consistent occupation panel for all years. The final occupation panel covers 2003-2018.

For each occupation, we construct four outcomes. We first calculate the number

of unique formal workers and formal jobs observed at any point during the year. We then calculate the number of workers and jobs active on December 31. Each measure is expressed per 10,000 inhabitants because the number of mental health professionals is relatively small, making changes easier to observe with this measure.

To account for municipality characteristics, we use GDP data and other variables from the Brazilian Statistical Office (IBGE). We also include municipal expenditure on the social program *Bolsa Família* (PBF), obtained from the Ministry of Social Development. From the Instituto de Pesquisa Econômica Aplicada (IPEA), we retrieve geographical variables such as distance to the state capital. From DATASUS, the administrative data platform of the Brazilian Ministry of Health, we obtain information from the *Pesquisa Assistência Médico Sanitária* in 1999 about the number of psychologists, psychiatrists, social workers and the number of psychiatric outpatient establishments. Lastly, from the 2000 Population Census we could also construct municipality variables for the year 2000 as for example the illiteracy rate or rural share. Our preferred labor market specifications include three baseline controls interacted with year indicators: log population, the poverty rate, and an indicator for the presence of a psychiatric outpatient establishment.

# 4 Identification and Empirical Strategy

## 4.1 Methodology

The identification strategy exploits the staggered introduction of CAPS across Brazilian municipalities after 2002. Since municipalities received their first CAPS in different years, treatment varies across both municipalities and time, as shown in Figure 1. The main objective is to estimate whether CAPS affected labor market outcomes differently by gender. To do so, we estimate effects separately for women and men and also construct municipality-year gender gaps as additional outcomes. This allows us to assess both gender specific effects and whether CAPS changed the relative position of women compared with men.

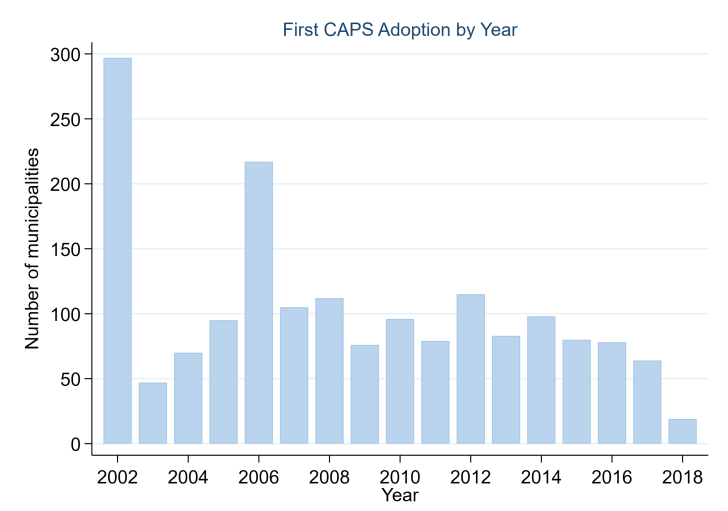



Figure 1: Staggered rollout CAPS

Because CAPS adoption is staggered across municipalities, standard Two-Way Fixed Effects estimators may be biased in the presence of heterogeneous treatment effects. In particular, they combine comparisons across treatment cohorts and may use already treated municipalities as controls for later-treated municipalities (De Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). To address this concern, we use a Stacked Difference-in-Differences (Stacked DiD) design (Wing et al., 2024), which compares treated municipalities only with municipalities that are not treated within the relevant event window.

The key identifying assumptions are no anticipation and conditional common trends. No anticipation requires that municipalities do not adjust labor market outcomes before the first CAPS opening in response to future treatment. The common trends assumption requires that, in the absence of CAPS, treated municipalities and their not-yet-treated comparison municipalities would have followed similar trends in the relevant labor market outcomes within each cohort specific event window.

A remaining concern is selective CAPS adoption. Municipalities that received CAPS earlier may differ from later adopters not only in levels of administrative capacity or demand for mental health services, but also in broader policy and socioeconomic trajectories. These trajectories may affect female formal employment independently of CAPS openings. The stacked design absorbs common national shocks, such as minimum wage changes or the 2008 crisis, and compares treated municipalities with not-yet-treated municipalities within cohort-specific event windows. Nevertheless, identification would still be threatened if municipalities with different CAPS timing were already evolving differently in ways that affected the outcomes of interest. Similar pretreatment coefficients help reduce this concern, but they do not fully eliminate it.

## 4.2 Stacked Difference-in-Differences Construction

Using Stacked DiD involves several design choices. First, we define the comparison group using not-yet-treated municipalities rather than never treated municipalities. This choice is motivated by the fact that CAPS adoption was not completely random across municipalities. Dias and Fontes (2024) show that municipalities that adopted CAPS differed from those that did not: in particular, more disadvantaged areas were less likely to construct a CAPS. For this reason, their empirical strategy includes a wide set of municipal controls and differential trends based on pretreatment characteristics and time-varying local conditions.

The next decision concerns the period included in each sub-experiment. Following the stacked DiD approach, the event window has to be the same across sub-experiments. we choose an event window of [-3,3], which includes the three years before the first CAPS opening, the year of opening, and the following three years. This choice follows the literature, which tries to maximize the number of treated cohorts while including enough pretreatment periods to assess pretrends.

To obtain a balanced panel, we exclude municipalities that do not have observations for all years in the relevant event window, which accounts for approximately 1% of the sample. Municipalities are included as controls for a given cohort only if they do not receive their first CAPS during that event window. This window provides pretreatment periods to assess the plausibility of common trends while preserving a balanced structure around treatment.

Given the sample period 2002-2018 and the event window [-3,3], the feasible treatment cohorts are 2005 to 2014. Since the occupation panel starts in 2003, its feasible treatment cohorts are 2006 to 2014. As a result, the estimated effect corresponds to a trimmed average treatment effect on the treated, since only cohorts for which the full event window can be observed enter the stacked sample. Earlier cohorts do not have three pretreatment years, while later cohorts do not provide the required post treatment window and/or a clean not-yet-treated comparison group within the chosen design.

For example, in the sub-experiment for municipalities first treated in 2005, the event window runs from 2002 to 2008. The treated group is composed of municipalities that opened their first CAPS in 2005, while the comparison group includes municipalities that did not open their first CAPS during the 2002-2008 window and were treated only later. In this context, among the 1,726 municipalities that receive CAPS during the sample period, 1072 enter the stacked design as treated municipalities. The remaining municipalities are excluded as treated units because they adopt either too early or too late: 414 adopted between 2002 and 2004 and 240 adopted after 2014.

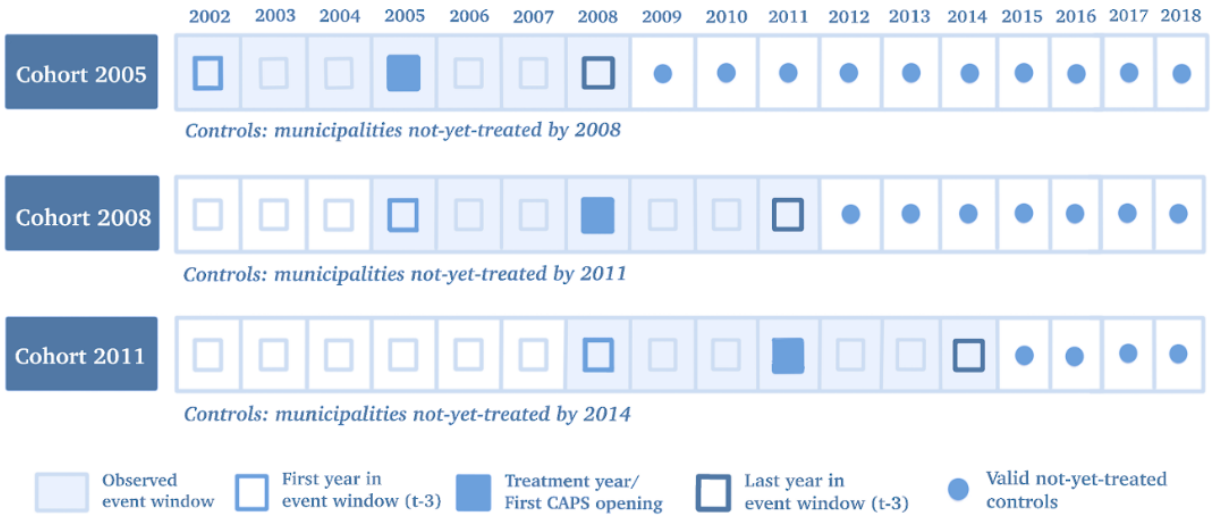



Figure 2: Stacked Difference-In-Differences Structure

## 4.3 Balancing tests

To be able to address the differences between municipalities' characteristics before the rollout of CAPS, we carry out an analysis of the pretreatment characteristics that is associated with earlier CAPS adoption. For each characteristic, we run a regression in which the municipality characteristic is the dependent variable and the main independent variable is an indicator equal to one if the municipality adopted CAPS early, between 2005 and 2007. We compare these early adopters separately with two groups of late adopters: municipalities treated between 2011 and 2012 and those treated between 2013 and 2014, the last cohorts included in our stacked DiD design. All regressions include state fixed effects. Therefore, the coefficients represent differences in pretreatment characteristics between early and late adopters within states.

We evaluate 21 variables including socioeconomic, labor and health areas. In Figure 3, we observe a selection of this study while the full set of results can be found in the Appendix in Table 6.

We observe that, indeed, some baseline characteristics differ depending on the timing of CAPS adoption. Specifically, population, poverty, illiteracy, and an indicator equal to one if the municipality had at least one psychiatric outpatient establishment in 1999 are associated with earlier or later CAPS adoption. Moreover, for some characteristics, the differences are larger when early adopters are compared with the latest treatment cohorts. This is particularly clear for population. Since eligibility for a CAPS depends on population thresholds, municipalities adopting later may not have satisfied the population requirement at the beginning of the sample period but may have reached it in subsequent years. Similarly, having a psychiatric infrastructure may indicate a higher demand or capacity of mental health services respect to those which did not have any, facilitating

the adoption of CAPS. In turn, municipalities with higher poverty and illiteracy rates may have had fewer resources or may have needed to prioritize other public services.

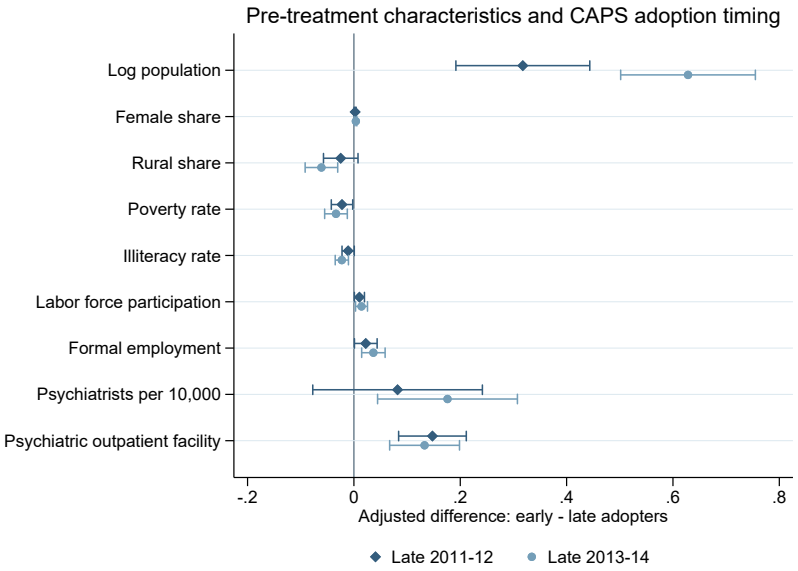



Figure 3: Pretreatment characteristics by CAPS adoption timing

*Notes:* The figure reports differences in selected pretreatment characteristics between municipalities treated in 2005–2007 and two groups of later adopters: municipalities treated in 2011–2012 and those treated in 2013–2014. Each coefficient comes from a separate regression of the pretreatment characteristic on an indicator for early adoption, including state fixed effects. Characteristics are expressed in their original units. The horizontal lines represent 95% confidence intervals based on robust standard errors.

The second diagnostic is a balancing test examining whether the treated municipalities and the eligible not-yet-treated control municipalities differ in their pretreatment characteristics within the stacked sub-experiments. For each characteristic, we estimate the difference between treated and control municipalities, including state and sub-experiment fixed effects, applying the corrective stack weights, and clustering standard errors at the municipality level. As in the analysis of CAPS adoption timing, we find statistically significant baseline differences in several characteristics, including population, rural population share, poverty, illiteracy, and the presence of any psychiatric outpatient facility.

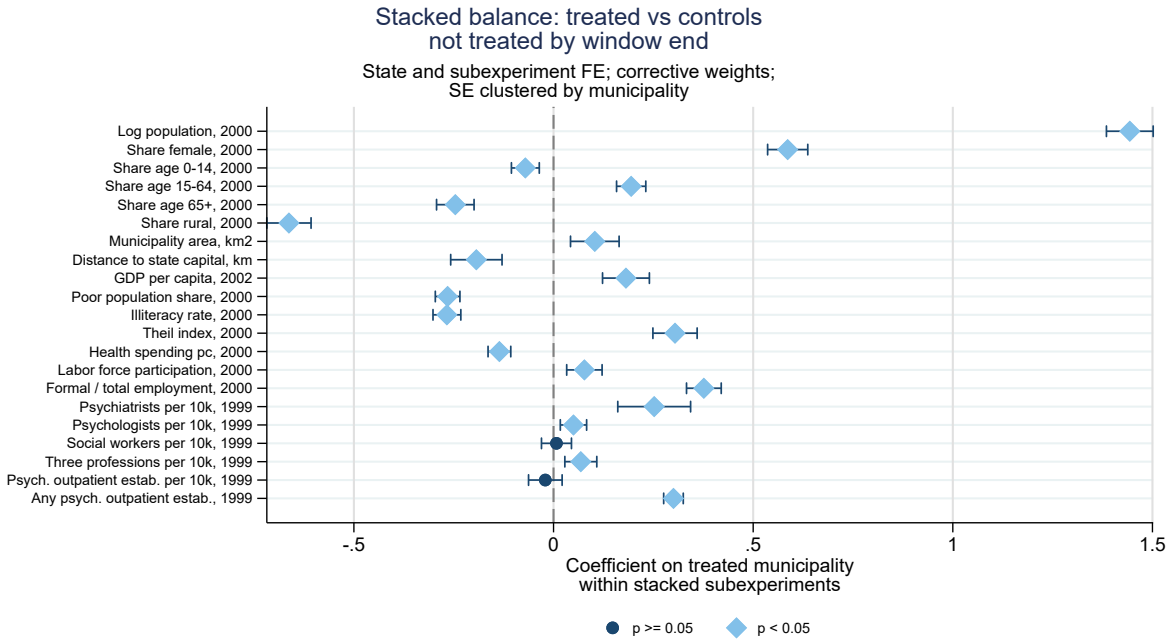



Figure 4: Balancing test

*Notes:* The figure reports differences in pretreatment characteristics between treated municipalities and eligible not-yet-treated control municipalities within the stacked sub-experiments. Continuous characteristics are standardized using their weighted means and standard deviations, while the indicator for the presence of any psychiatric outpatient establishment remains expressed as a binary variable. Each coefficient comes from a separate regression including state and sub-experiment fixed effects and applying the corrective stack weights. Standard errors are clustered at the municipality level. The horizontal lines represent 95% confidence intervals.

On account of the systematic differences in the baseline characteristics of municipalities that adopt CAPS at different points in time, we include three pretreatment characteristics in our preferred specification: the logarithm of population, the poverty rate, and an indicator for the presence of any psychiatric outpatient facility in 1999. These variables capture the main differences in municipal size, socioeconomic conditions, and initial mental health infrastructure. Then, we interact them with year indicators to address the possible differential trends without introducing the potential endogeneity associated with time-varying controls, such as population.

## 4.4 Empirical Strategy

Our preferred specification uses the Stacked DiD estimator developed by Wing et al. (2024). The main advantage compared to other estimators is the transparency of the comparison groups. In each sub-experiment, we use the same municipalities as controls throughout the entire event window. These are municipalities that are not treated during this period. This also makes the identifying assumption clearer: in the absence of treatment, the treated municipalities and this fixed group of controls would have followed

parallel trends over the event window. In this way, it is clearer which municipalities we are comparing in each sub-experiment and allows us to examine these comparisons directly.

For our main results, we estimate this Stacked Difference-in-Differences specification:

$$Y_{mst} = \sum_{k=-3,\, k\neq -1}^{3} \beta_k \left( Treat_{ms} \times \mathbf{1}[EventTime_{mst} = k] \right) + X'_m \delta_t + \alpha_{ms} + \lambda_{st} + \varepsilon_{mst}. \quad (1)$$

where $Y_{mst}$ is the outcome variable for municipality $m$, in year $t$, and sub-experiment $s$. It refers to labor market outcomes estimated separately for women and men. In the gender gap analysis, $Y_{mst}$ is the municipality-year difference between female and male outcomes. In the occupation analysis, $Y_{mst}$ alternatively measures workers, active workers, jobs, or active jobs for each occupational group, expressed per 10,000 inhabitants.

$Treat_{ms}$ is an indicator equal to one if municipality $m$ belongs to the treated group in sub-experiment $s$, that is, if it receives its first CAPS in the treatment year defining that sub-experiment. It is equal to zero for municipalities used as controls in that sub-experiment, which are not yet treated during the event window.

$\mathbf{1}[EventTime_{mst} = k]$ is an indicator equal to one when municipality $m$ in sub-experiment $s$ is observed at event time $k$. Event time is defined as the difference between year $t$ and the treatment year defining sub-experiment $s$. The event window is restricted to $k \in [-3, 3]$. The omitted category is $k = -1$, the year immediately before CAPS opening.

The coefficients $\beta_k$ are the event-study coefficients of interest. They measure the evolution of outcomes in treated municipalities relative to not-yet-treated municipalities in the same sub-experiment, compared with the year immediately before treatment. Coefficients for $k < 0$ are used to assess pretreatment trends, while coefficients for $k \geq 0$ capture the effect of CAPS after the first opening.

$X'_m \delta_t$ are the selected pretreatment municipality characteristics: the logarithm of population in 2000, the poverty rate in 2000, and an indicator equal to one if the municipality had at least one psychiatric outpatient establishment in 1999. These characteristics are interacted with year indicators, allowing their associations with the outcome to evolve differently over time.

$\alpha_{ms}$ are municipality $m$ fixed effects in sub-experiment $s$. $\lambda_{st}$ are year $t$ fixed effects in sub-experiment $s$. $\varepsilon_{mst}$ is the error term.

The equation is estimated using stacked sample weights. Since the same municipality can appear in more than one sub-experiment, standard errors are clustered at the municipality level.

Regarding the assumption of common trends, for the gender specific specifications, this assumption applies separately to outcomes for women and men. For the gender gap specifications, the relevant assumption is that the gender gap in labor outcomes would

have evolved similarly across municipalities with different CAPS timing. The relevant concern is whether municipalities treated earlier were already on different pretreatment trajectories in the gender gap relative to municipalities treated later.

For the occupation level specifications, the common trends assumption requires that, in the absence of CAPS expansion, treated and not-yet-treated municipalities would have followed similar trends in the number of formal jobs in each occupation within the cohort-specific event window. These occupation-level outcomes are intended to capture changes in the local employment structure associated with CAPS expansion.

# 5 Descriptive Analysis

For the period 2002-2018, we analyze men and women in working age from 18 to 65 years. RAIS data present important gender differences in formal labor market outcomes. Women work fewer hours than men on average and are substantially more likely to hold part-time jobs. Still, the difference in total weekly hours is relatively small, less than two hours per week. In contrast, the gap in earnings is more pronounced: women earn less than men both in terms of total monthly earnings and hourly wages. Women are also more likely to hold multiple active jobs, which is consistent with the idea that part-time work and multiple job holding partly offset each other in terms of total hours worked.

Table 1: Descriptive statistics by gender, pooled 2002–2018

| | Women Mean | Men Mean | Difference Women − Men |
|---|---|---|---|
| Age | 35.24 | 35.33 | -0.09 |
| Active at year-end (%) | 79.71 | 77.87 | 1.83 |
| Number of formal jobs during year | 1.20 | 1.23 | -0.02 |
| Number of active formal jobs at year-end | 0.84 | 0.80 | 0.04 |
| Total weekly hours | 41.63 | 43.26 | -1.63 |
| Total monthly earnings | 2,465.37 | 2,911.66 | -446.29 |
| Hourly wage | 14.94 | 16.49 | -1.56 |
| Part-time, $\leq$ 25 hours (%) | 6.04 | 1.68 | 4.36 |
| Part-time, $\leq$ 30 hours (%) | 11.67 | 4.30 | 7.37 |
| Multiple active jobs (%) | 5.06 | 2.84 | 2.22 |
| Worker-year observations | 347,476,456 | 491,511,207 | |

*Notes:* The table reports worker-year descriptive statistics using RAIS microdata, pooled over 2002–2018. RAIS covers formal employment relationships. Active workers are those with at least one formal employment relationship active on December 31. Hours and earnings are summed across active formal links. Hourly wages are calculated as monthly earnings divided by weekly hours multiplied by 4.33. Hours, earnings, hourly wages, part-time status, and multiple-job holding are measured among active worker-years. Part-time status is defined using total weekly hours across active formal links. The number of observations shown corresponds to the full worker-year sample; the number of observations used for outcomes defined only among active workers is smaller. Differences are calculated using the unrounded means.

Table 1 reports descriptive statistics for the RAIS worker-year sample used in the empirical analysis. Since RAIS only covers formal employment, Table 2 complements this evidence using Census data for 2000 and 2010, which also include informal and self-employed workers. The Census data show that, conditional on working, women are more concentrated in employee positions, both formal and informal, while men are more likely to be self-employed. Between 2000 and 2010, formal employment increased substantially for both genders. Despite this, women remained more concentrated in informal employee and domestic work, whereas men remained more represented in self-employment.

Table 2: Labor Market Category by Gender, Brazil 2000 and 2010

| | **2000** | | | **2010** | | |
|---|---|---|---|---|---|---|
| **Labor market category** | **Men** | **Women** | **Diff. W-M** | **Men** | **Women** | **Diff. W-M** |
| Formal employee / public sector | 42.9 | 47.5 | 4.7 | 52.9 | 53.5 | 0.5 |
| Informal employee / domestic worker | 20.7 | 26.2 | 5.5 | 17.1 | 22.7 | 5.6 |
| Self-employed | 28.2 | 16.8 | -11.4 | 24.2 | 17.1 | -7.1 |
| Employer | 3.6 | 2.1 | -1.5 | 2.3 | 1.5 | -0.8 |
| Unpaid / own-consumption work | 4.7 | 7.5 | 2.8 | 3.5 | 5.3 | 1.8 |

Figure 5 is evidence of the clear differences in the distribution of weekly hours by gender. Men are more concentrated in full-time schedules, almost 80 percent of active male worker-years are in the 41-44 hours category, compared with around 60 percent among women. Women, instead, are more represented in part-time categories, especially in schedules of 25 hours or less and 26-30 hours. This is consistent with the descriptive statistics in Table 1, where women are more likely to work part-time, although the difference in total weekly hours is relatively small.

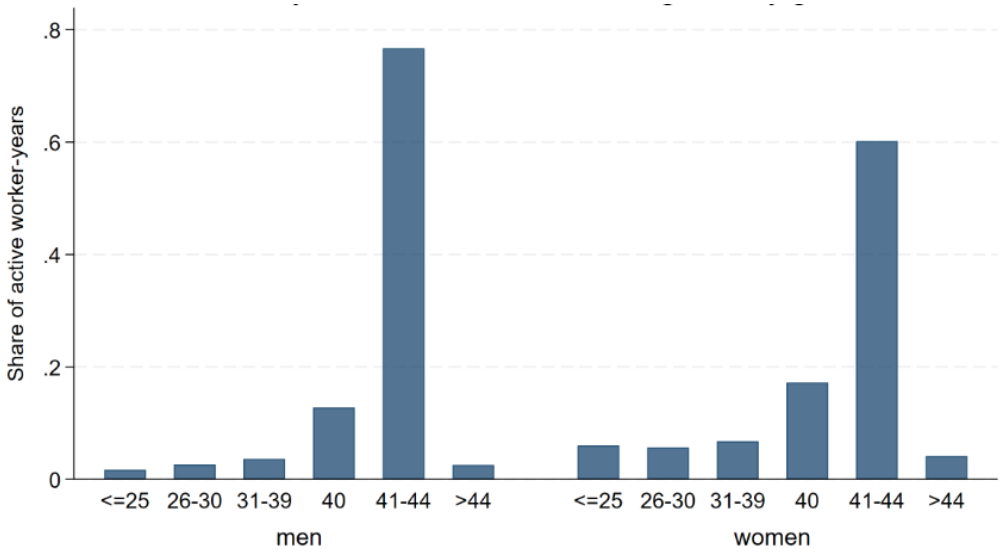



Figure 5: Distribution of weekly hours by gender

Figure 6 shows the distribution of log total active real earnings by gender. The distribution for women is shifted to the left relative to men, indicating lower earnings among women across much of the distribution as we saw in the earnings gap in Table 1.

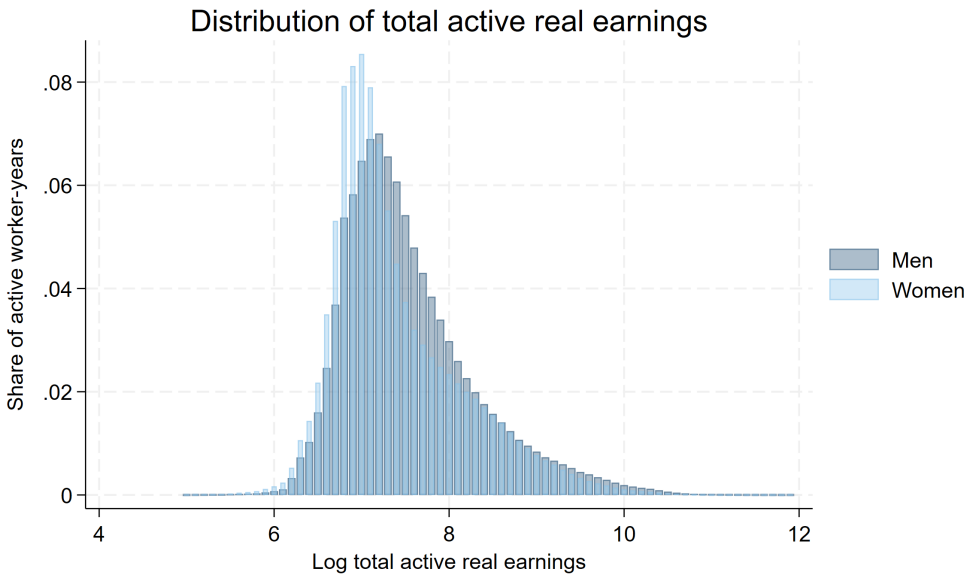



Figure 6: Distribution of log earnings by gender

*Notes:* The figure shows the distribution of log total monthly real earnings among active worker-years. Total earnings are summed across active formal links. The vertical axis reports the share of active worker-years within each gender.

# 6 Results

## 6.1 Direct Effects

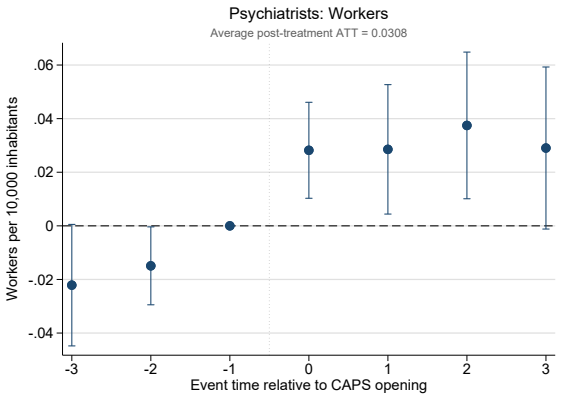
Direct effects refer to the effects that are expected to follow most directly from the opening of the first CAPS in a municipality. Here, we focus on occupations directly related to CAPS service provision: psychiatrists, clinical psychologists, social psychologists, social workers and occupational therapists. Dias and Fontes (2024) show that CAPS expansion increased the number of health professionals linked to health care facilities, including psychiatrists, psychologists, occupational therapists, and social workers. In this paper, the occupation level analysis is used mainly as a sanity check.

Figures 14, 15, 16 and 17 show positive post-opening coefficients for all four occupational groups, except psychiatrists, and for the aggregate measure of mental health professionals and psychologists. However, the pretreatment coefficients display an upward trend before CAPS opening. This indicates that the no anticipation or parallel trends assumption is not fully satisfied for these outcomes. At the same time, this pattern is consistent with Dias and Fontes (2024), who also find anticipation effects in the supply of mental health professionals, especially in the year before CAPS establishment. They argue that this may reflect the fact that municipalities need to formally hire professionals before the actual opening of the center, due to the length of the bureaucratic hiring process.

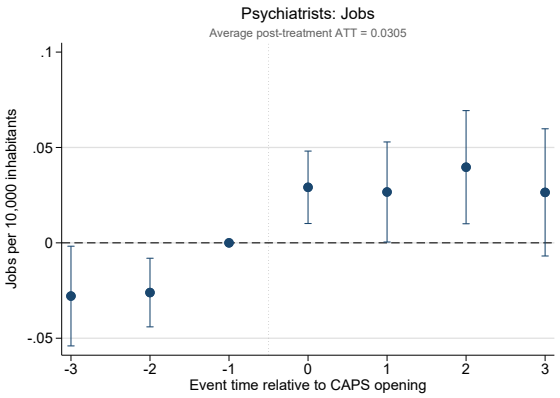
Therefore, among all the occupations analyzed, the clearest results are those for psychiatrists. Figure 7 presents the results for workers and jobs observed at any point during the year, as well as for workers and jobs that remain active at the end of the year. For workers and jobs observed during the year, there is already a positive trend before the opening of a CAPS. Therefore, we do not find clear evidence that the opening increased the total number of psychiatrists who worked during the year or the number of jobs they held. Nonetheless, we find a clearer positive significant effect on active workers and active jobs. This suggests that the main change may not be an increase in the number of psychiatrists entering the formal labor market, but an increase in the number who remain employed at the end of the year.

In Table 3, we notice that the number of active psychiatrists grows, on average, by 0.0236 workers per 10,000 inhabitants and 0.024 for active jobs per 10,000 inhabitants. These results indicate an increase in psychiatrist employment at the end of the year. Nonetheless, the similarity of the estimates does not allow us to determine whether this is driven by more psychiatrists in formal employment, changes in multiple job holding, or a combination of the two.

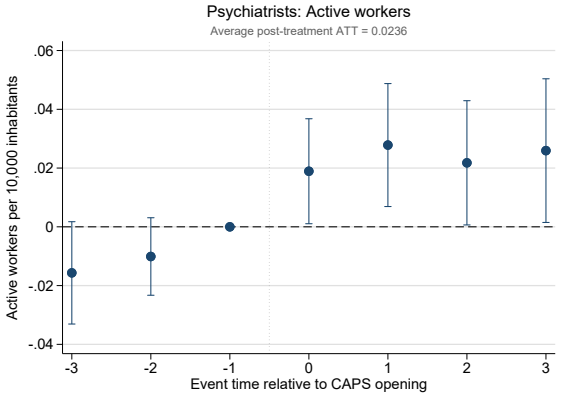
(a) Workers




(b) Jobs




(c) Active workers




(d) Active jobs

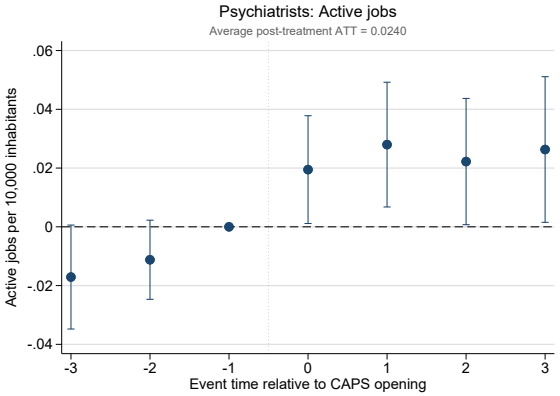



Figure 7: Effects of CAPS openings on psychiatrist employment

*Notes:* The figure reports stacked difference-in-differences event-study estimates for the number of psychiatrists per 10,000 inhabitants. Workers are unique professionals observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique professionals with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

Table 3: Stacked DiD Estimates for Psychiatrist Employment

| | Workers | Active workers | Jobs | Active jobs |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ***Panel A: Post-treatment average effect*** | | | | |
| Average ATT, event times 0 to 3 | 0.0308*** | 0.0236** | 0.0305** | 0.0240** |
| | (0.0112) | (0.0095) | (0.0121) | (0.0096) |
| ***Panel B: Event-study coefficients*** | | | | |
| Event time −3 | -0.0221* | -0.0157* | -0.0279** | -0.0171* |
| | (0.0116) | (0.0089) | (0.0133) | (0.0090) |
| Event time −2 | -0.0149** | -0.0101 | -0.0261*** | -0.0112 |
| | (0.0074) | (0.0067) | (0.0092) | (0.0069) |
| Event time 0 | 0.0282*** | 0.0189** | 0.0291*** | 0.0195** |
| | (0.0091) | (0.0091) | (0.0097) | (0.0094) |
| Event time 1 | 0.0285** | 0.0278*** | 0.0267** | 0.0280*** |
| | (0.0123) | (0.0107) | (0.0134) | (0.0108) |
| Event time 2 | 0.0375*** | 0.0218** | 0.0397*** | 0.0222** |
| | (0.0140) | (0.0108) | (0.0151) | (0.0110) |
| Event time 3 | 0.0290* | 0.0259** | 0.0265 | 0.0263** |
| | (0.0154) | (0.0125) | (0.0170) | (0.0126) |
| Municipality × sub-experiment FE | Yes | Yes | Yes | Yes |
| Year × sub-experiment FE | Yes | Yes | Yes | Yes |
| Baseline covariates × year indicators | Yes | Yes | Yes | Yes |
| Observations | 29,330 | 29,330 | 29,330 | 29,330 |
| Number of clusters | 1,323 | 1,323 | 1,323 | 1,323 |

*Notes:* The table reports stacked difference-in-differences estimates for psychiatrist employment per 10,000 inhabitants. Workers are unique psychiatrists observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique psychiatrists with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. The post-treatment average effect is computed over event times 0 to 3. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Standard errors are clustered at the municipality level and reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

## 6.2 Indirect Effects

The opening of CAPS may generate effects beyond the direct hiring of mental health professionals. As CAPS expand, psychiatric hospitalizations decrease, and in a context

of limited public resources, part of the responsibility for care may shift from hospitals to families. This burden is likely to fall disproportionately on women.

We distinguish between three possible labor market channels. The first is the extensive margin, women may leave formal employment or become less likely to remain formally employed. The second is the intensive margin, women may remain in the formal labor market but reduce their working hours, shifting to part-time jobs, for example. The third regards monetary compensation, even if hours worked do not change, women may experience lower earnings if they move to more flexible but lower-paying jobs or receive lower hourly remuneration.

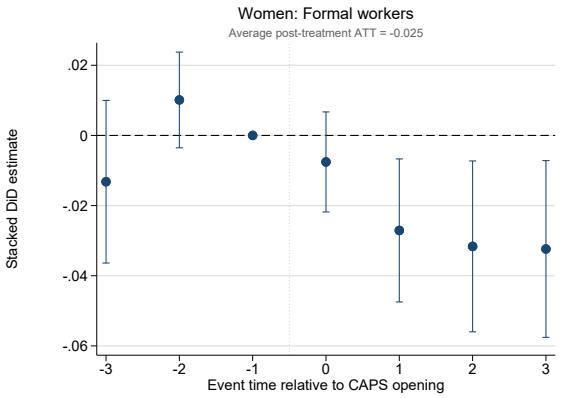
### 6.2.1 Extensive Margin

Figure 8 reports the effects of CAPS openings on the extensive margin labor market outcomes by gender. For women, the number of formal workers decreases significantly by 2.5%, indicating that fewer women have a formal job at some point during the year. The number of female workers active at the end of the year also decreases significantly, although slightly less, by 2.2%.

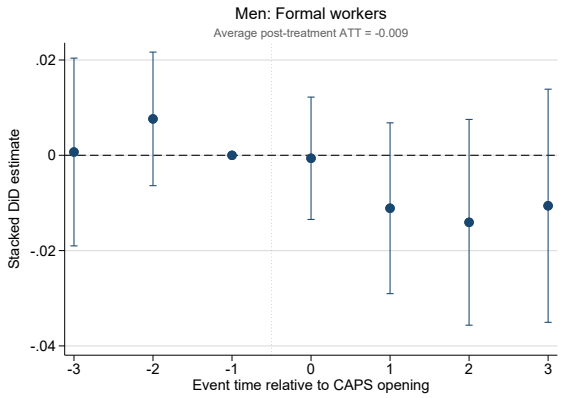
In particular, using the average baseline among the 1,072 treated municipalities included in the stacked design, this translates into approximately 58 fewer active female formal workers per treated municipality, from a baseline of around 2,632 active female workers. These municipalities had an average population of approximately 41,852 in 2000. As discussed in Section 4, these level magnitudes refer to the trimmed set of treated municipalities included in the stacked design, which excludes early adopters with the largest populations. Lastly, since the total number of formal workers and the number of active formal workers decrease by similar magnitudes, no statistically significant effect is found on the share of female formal workers who are active on December 31. This suggests a reduction in the overall number of women observed in formal employment, rather than a change in formal employment attachment among women at the end of the year observed in RAIS during the year.

For men, we see a negative trend for the number of formal workers, active or not, but no statistically significant effects are found on either the number of formal workers, the number active at the end of the year or the share of active male workers.

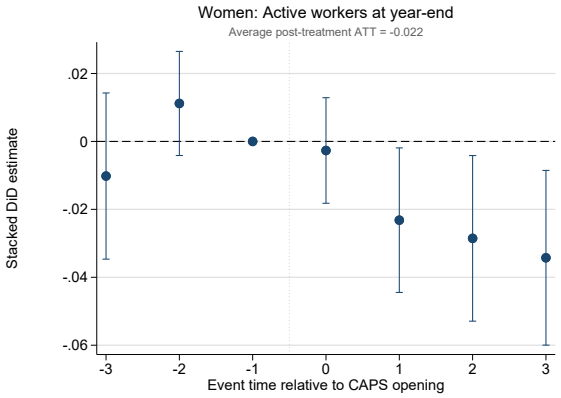
(a) Women: log formal workers



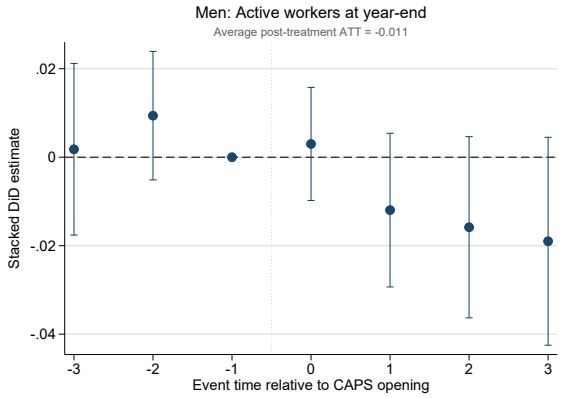

(b) Men: log formal workers



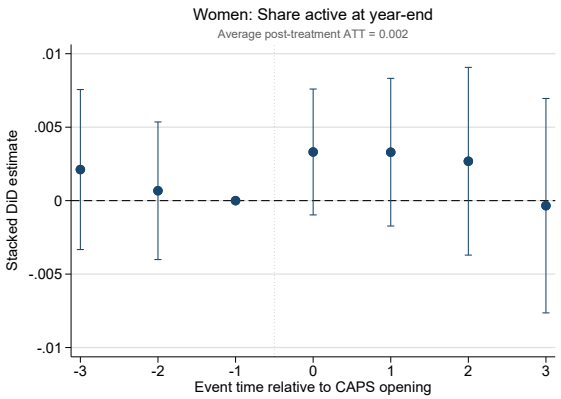

(c) Women: log active formal workers




(d) Men: log active formal workers




(e) Women: share active at year-end




(f) Men: share active at year-end

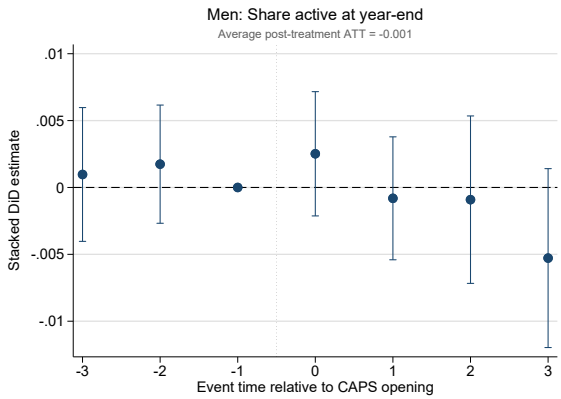
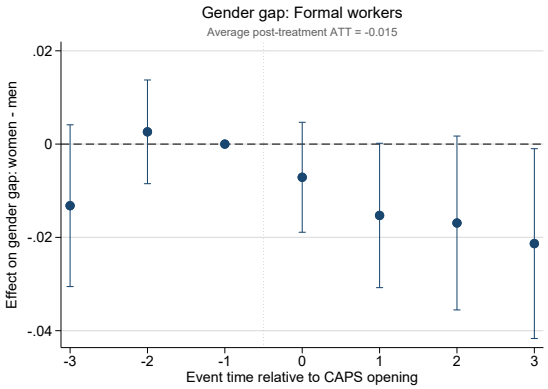



Figure 8: Effects of CAPS openings on extensive margin labor market outcomes, by gender

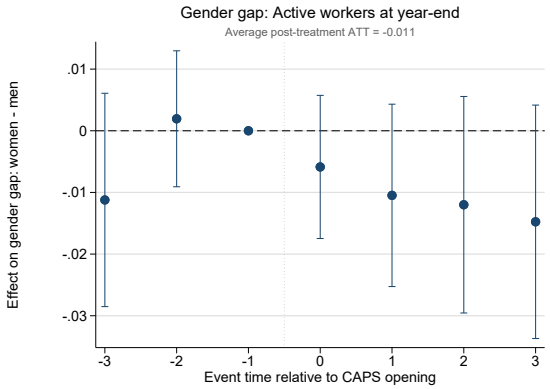
*Notes:* The figure reports stacked difference-in-differences event-study estimates by gender. Formal workers are individuals with at least one formal employment relationship observed during the year. Active formal workers are individuals with at least one formal employment relationship active on December 31. The share active at year-end is calculated as the number of active formal workers divided by the total number of formal workers within each municipality-year-gender cell. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

When looking at the gender gap, defined as female outcomes minus male outcomes, Figure 9 shows negative statistically significant point estimates for the gender gap in the number of formal workers. On average, the number of female formal workers decreases by 1.5% relative to the number of male formal workers. Thus, the gender gap in formal employment widens against women following the CAPS opening. Even though there is an effect of the same sign for active workers, this result is not significant, as it is not for the share of workers active at the end of the year.

(a) Log formal workers




(b) Log active formal workers




(c) Share active at year-end

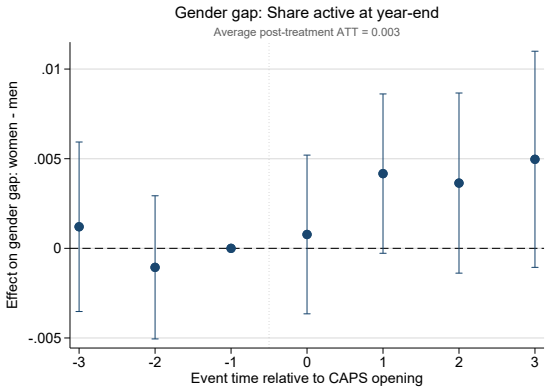



Figure 9: Effects of CAPS openings on gender gaps in extensive margin labor market outcomes

*Notes:* The figure reports stacked difference-in-differences event-study estimates for gender gap outcomes. Gender gaps are defined as female outcomes minus male outcomes within each municipality-year. Formal workers are individuals with at least one formal employment relationship observed during the year. Active formal workers are individuals with at least one formal employment relationship active on December 31. The share active at year-end is calculated as the number of active formal workers divided by the total number of formal workers. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

Table 4: Stacked DiD Estimates for Formal Employment

| | Log formal workers | | | Log active formal workers | | |
|---|---|---|---|---|---|---|
| | Women | Men | Gender gap | Women | Men | Gender gap |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Average ATT, event times 0 to 3 | -0.0247*** | -0.0091 | -0.0152** | -0.0222** | -0.0109 | -0.0108 |
| | (0.00946) | (0.00891) | (0.00736) | (0.00958) | (0.00852) | (0.00692) |
| Event time −3 | -0.0132 | 0.0007 | -0.0132 | -0.0102 | 0.0018 | -0.0112 |
| | (0.01183) | (0.01005) | (0.00885) | (0.01248) | (0.00989) | (0.00883) |
| Event time −2 | 0.0101 | 0.0076 | 0.0026 | 0.0112 | 0.0094 | 0.0019 |
| | (0.00696) | (0.00715) | (0.00567) | (0.00782) | (0.00741) | (0.00562) |
| Event time 0 | -0.0076 | -0.0006 | -0.0071 | -0.0027 | 0.0030 | -0.0059 |
| | (0.00728) | (0.00655) | (0.00601) | (0.00793) | (0.00652) | (0.00592) |
| Event time 1 | -0.0271*** | -0.0111 | -0.0153* | -0.0232** | -0.0120 | -0.0105 |
| | (0.01040) | (0.00914) | (0.00790) | (0.01085) | (0.00886) | (0.00754) |
| Event time 2 | -0.0316** | -0.0141 | -0.0169* | -0.0285** | -0.0158 | -0.0120 |
| | (0.01243) | (0.01102) | (0.00951) | (0.01244) | (0.01045) | (0.00896) |
| Event time 3 | -0.0324** | -0.0106 | -0.0213** | -0.0342*** | -0.0190 | -0.0148 |
| | (0.01285) | (0.01248) | (0.01038) | (0.01312) | (0.01199) | (0.00966) |
| Municipality × sub-experiment FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year × sub-experiment FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Baseline covariates × year indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 35,511 | 35,560 | 35,557 | 35,511 | 35,560 | 35,557 |
| Number of clusters | 1,322 | 1,323 | 1,323 | 1,322 | 1,323 | 1,323 |

*Notes:* The table reports stacked difference-in-differences estimates for formal employment outcomes. Formal workers are individuals with at least one formal employment relationship observed during the year. Active formal workers are individuals with at least one formal employment relationship active on December 31. The post-treatment average effect is computed over event times 0 to 3. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Standard errors are clustered at the municipality level and reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

### 6.2.2 Intensive Margin

The intensive margin refers to changes in the type or conditions of employment among people who remain in the formal labor market, specially in terms of hours worked.

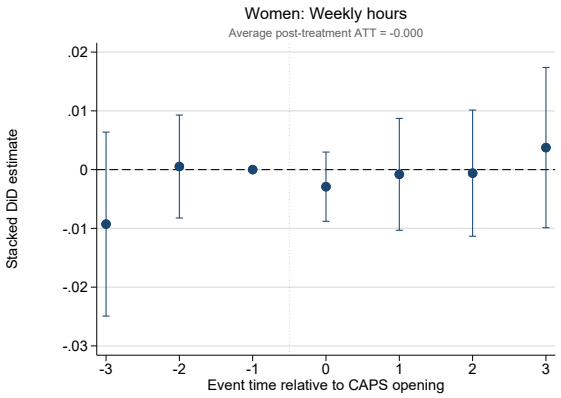
In Figure 10, we do not observe any statistically significant results by gender or gender gap for their hours worked.

Additional intensive margin outcomes, including part-time employment and multiple job holding, are reported in Appendix Figures 18 and 19. None of the estimated

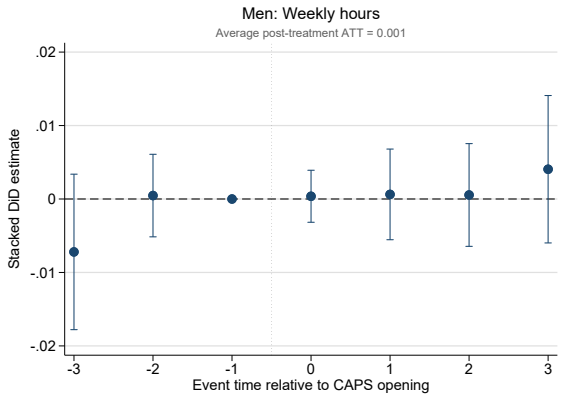
average effects are statistically significant. Nevertheless, the share of female workers in part-time employment shows an increasingly negative pattern after the CAPS opening, with an average reduction of approximately 0.5 percentage points. The corresponding decline is smaller for men, resulting in a negative estimate for the gender gap in part-time employment.

This pattern is consistent with women in part-time employment being more likely to leave formal employment following a CAPS opening, potentially because their weaker attachment to the formal labor market makes it easier or more necessary to leave employment to care for relatives. However, since the estimates are not statistically significant and the outcome is observed only among workers who remain active at the end of the year, this evidence is only suggestive.

(a) Women: log total hours




(b) Men: log total hours




(c) Gender gap: log total hours

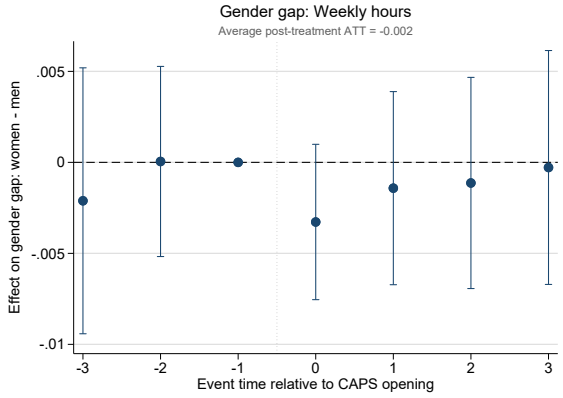



Figure 10: Effects of CAPS openings on intensive margin labor market outcomes, by gender and gender gap

*Notes:* The figure reports stacked difference-in-differences event-study estimates separately for women and men, together with the corresponding gender gap. Gender gaps are defined as the female outcome minus the male outcome within each municipality-year. Outcomes are constructed among active formal workers, defined as workers with at least one formal employment relationship active on December 31. For each worker, total weekly hours are calculated by summing hours across all active formal employment relationships and then transformed into logs. The outcome is the mean log total weekly hours within each municipality-year-gender cell. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

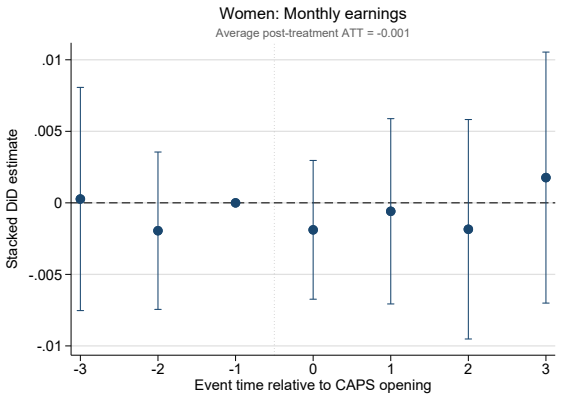
### 6.2.3 Monetary Compensation

The third margin that we may consider refers to responses regarding monetary compensation. Figure 11 shows little evidence of adjustments along this margin for women. In

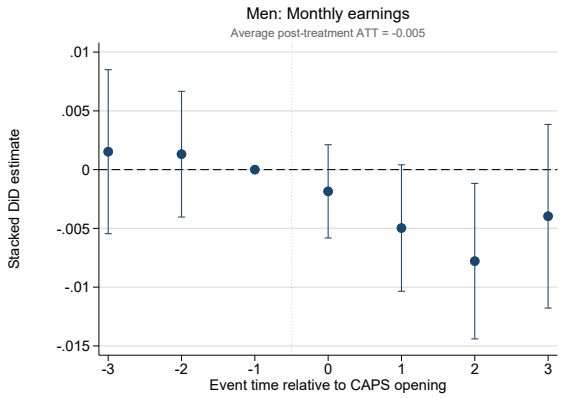
contrast, for men, as reported in Table 5, total earnings fall by approximately 0.5% after the first CAPS opening in a municipality, although the estimated average effect is statistically significant only at the 10% level. In economic terms, this reflects a loss of approximately 8 reais in average monthly earnings, from an average earnings of around 1,645 reais before a center opening.

This decline is not accompanied by a clear change in total hours. Instead, the estimates for hourly wages are also negative for men although they are not statistically significant. This suggests that the decline in earnings is more consistent with changes in hourly remuneration or in the composition of active formal employment than with an adjustment through hours worked. One possible explanation is that the mental health jobs increased by the opening of CAPS is disproportionately concentrated among women and is accompanied by a shift in the composition of male employment away from higher-paid or highly educated workers.

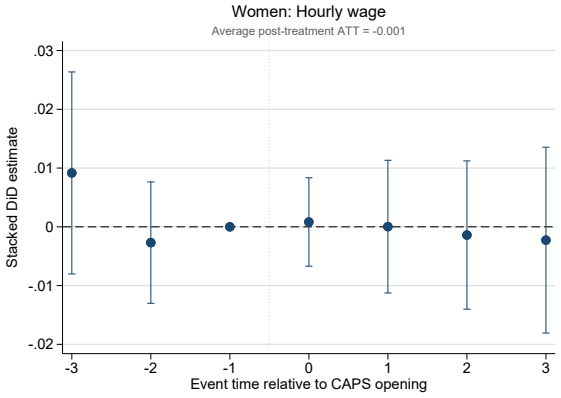
(a) Women: log total earnings




(b) Men: log total earnings




(c) Women: log hourly wage




(d) Men: log hourly wage


Figure 11: Effects of CAPS openings on monetary compensation

*Notes:* The figure reports stacked difference-in-differences event-study estimates separately for women and men. Outcomes are constructed among active formal workers, defined as workers with at least one formal employment relationship active on December 31. For each worker, total monthly real earnings are calculated by summing earnings across all active formal employment relationships and then transformed into logs. The hourly wage is calculated as total monthly real earnings divided by total weekly hours multiplied by 4.33, and then transformed into logs. The outcomes are the corresponding means within each municipality-year-gender cell. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

Table 5: Stacked DiD Estimates for Log Total Earnings

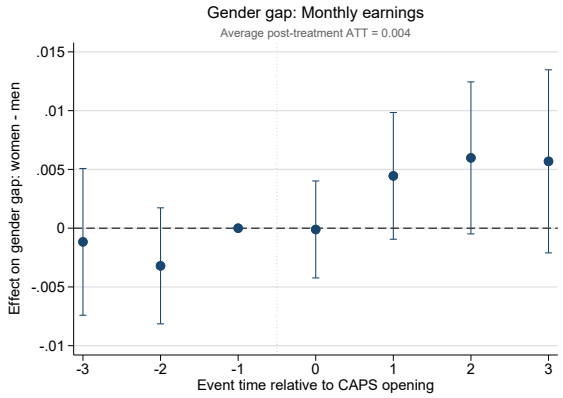
| | Women | Men | Gender gap |
|---|---|---|---|
| | (1) | (2) | (3) |
| ***Panel A: Post-treatment average effect*** | | | |
| Average ATT, event times 0 to 3 | -0.0006 | -0.0046* | 0.0040 |
| | (0.00314) | (0.00271) | (0.00266) |
| ***Panel B: Event-study coefficients*** | | | |
| Event time −3 | 0.0003 | 0.0015 | -0.0012 |
| | (0.00398) | (0.00356) | (0.00319) |
| Event time −2 | -0.0019 | 0.0013 | -0.0032 |
| | (0.00281) | (0.00273) | (0.00252) |
| Event time 0 | -0.0019 | -0.0019 | -0.0001 |
| | (0.00247) | (0.00202) | (0.00210) |
| Event time 1 | -0.0006 | -0.0050* | 0.0044 |
| | (0.00330) | (0.00275) | (0.00275) |
| Event time 2 | -0.0018 | -0.0078** | 0.0060* |
| | (0.00391) | (0.00337) | (0.00330) |
| Event time 3 | 0.0018 | -0.0040 | 0.0057 |
| | (0.00448) | (0.00399) | (0.00397) |
| Municipality × sub-experiment FE | Yes | Yes | Yes |
| Year × sub-experiment FE | Yes | Yes | Yes |
| Baseline covariates × year indicators | Yes | Yes | Yes |
| Observations | 35,511 | 35,560 | 35,557 |
| Number of clusters | 1,322 | 1,323 | 1,323 |

*Notes:* The table reports stacked difference-in-differences estimates for mean log total monthly real earnings among active formal workers. For each worker, total monthly real earnings are calculated by summing earnings across all formal employment relationships active on December 31 and then transformed into logs. The outcome is the mean within each municipality-year-gender cell. Gender gaps are defined as the female outcome minus the male outcome within each municipality-year. The post-treatment average effect is computed over event times 0 to 3. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Standard errors are clustered at the municipality level and reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

In line with this interpretation, the estimated effects on the gender gap in total earnings and hourly wages are positive but not statistically significant. As the gender gap is defined as female outcomes minus male outcomes, a positive coefficient would imply a narrowing

of the earnings gap. This pattern appears to be mainly related to changes in hourly wages rather than changes in total hours.

(a) Log total earnings




(b) Log hourly wage

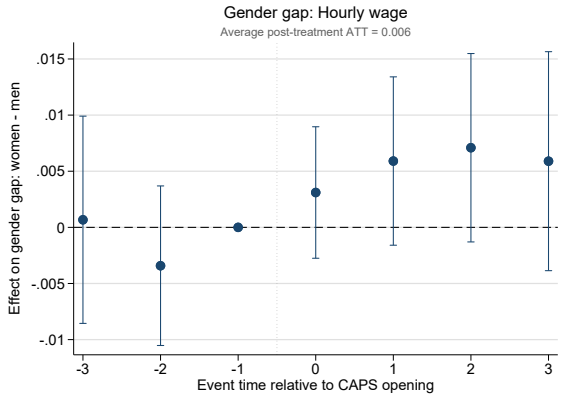



Figure 12: Effects of CAPS openings on gender gaps in monetary compensation

*Notes:* The figure reports stacked difference-in-differences event-study estimates for gender gap outcomes. Gender gaps are defined as the female outcome minus the male outcome within each municipality-year. For each gender, outcomes are constructed among active formal workers, defined as workers with at least one formal employment relationship active on December 31. Total monthly real earnings are summed across all active formal employment relationships for each worker and then transformed into logs. The hourly wage is calculated as earnings/(weekly hours $\times$ 4.33) and then transformed into logs. The outcomes are the corresponding means within each municipality-year-gender cell. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

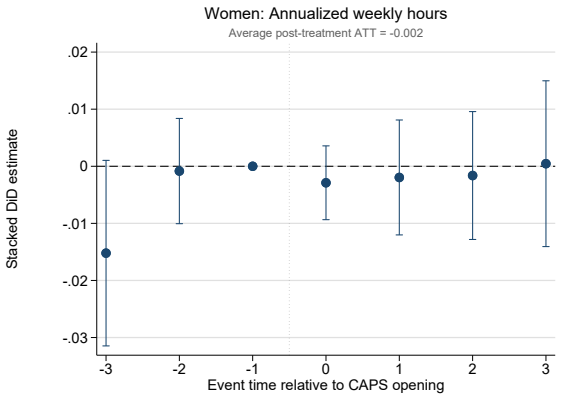
# 7 Robustness

First, for the analysis of occupations we use the estimator used by Dias and Fontes (2024), the De Chaisemartin and D'Haultfœuille (2020) estimator, with our three controls. The estimates in Figure 20 support our main results, the effect is positive, smaller in magnitude but still significant for active workers and active jobs. The results also remain similar when we estimate the stacked DiD without controls (Figure 21), use an event window of $[-4, 4]$ (Figure 31), and interact the baseline controls with a linear time trend instead of year indicators (Figure 33).

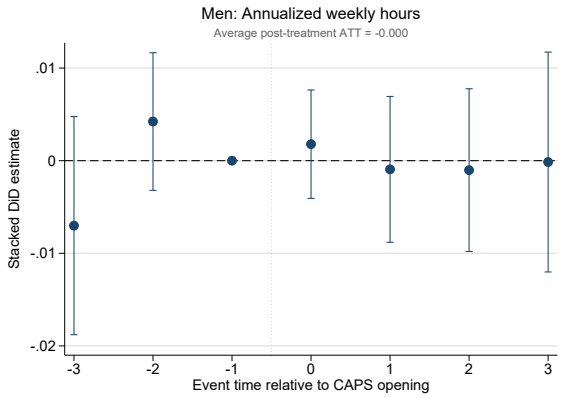
Second, for our main results on local labor market outcomes by gender, we re-estimate the intensive margin outcomes using an alternative RAIS panel that accounts for the duration of each job held by each individual during the year. In this way, we aim to capture changes in earnings or hours worked associated with job changes during the year.

In Figure 13 we note that the main intensive margin result is robust to this alternative definition. The estimated effects on monthly earnings and hourly wages remain negative and statistically significant for men, and their magnitudes increase. In contrast, the corresponding estimates for women remain close to zero.

(a) Women: log annualized hours

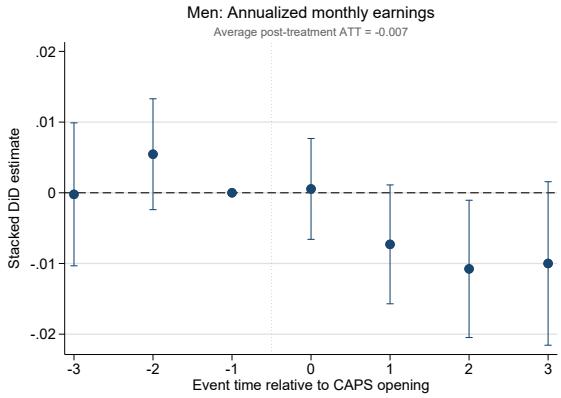



(b) Men: log annualized hours

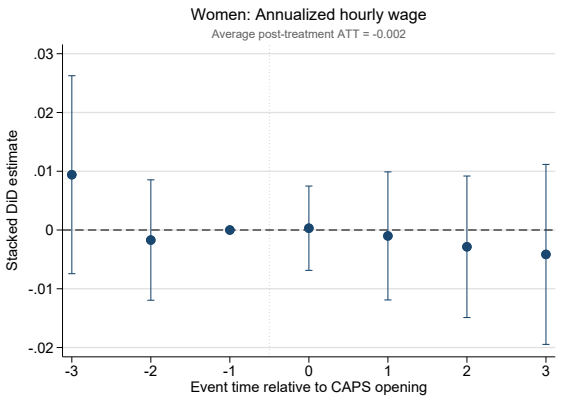



(c) Women: log annualized monthly earnings


(d) Men: log annualized monthly earnings




(e) Women: log annualized hourly wage




(f) Men: log annualized hourly wage

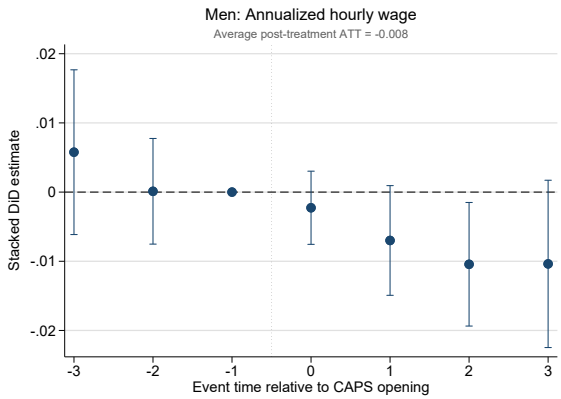



Figure 13: Effects of CAPS openings on duration-weighted labor market outcomes, by gender

*Notes:* The figure reports stacked difference-in-differences event-study estimates by gender using duration-weighted outcomes. These outcomes use all formal employment relationships held during the year. At the worker level, earnings and weekly hours from each relationship are weighted by the number of months it remained active, summed across relationships, and divided by 12. The annualized hourly wage is calculated as annualized monthly earnings/(annualized weekly hours $\times$ 4.33). Each worker-level measure is transformed into logs and then averaged within municipality-year-gender cells. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

Third, as a natural robustness check, we estimate the stacked DiD without controls, as shown in Figures 22, 23, 24, 25 and 26 . Certainly, the extensive margin results remain robust, though the estimated effects are smaller in magnitude, they remain negative and statistically significant for both the total number of female formal workers and the number active at the end of the year. Nonetheless, the estimated effect on the gender gap is not statistically significant. For earnings for men, there is a significant and negative coefficient for both earnings and hourly wage with a slightly higher effect of 0.7% decrease in earnings.

Fourth, the Callaway and Sant'Anna estimator is used as an additional robustness check for the indirect effects and the results are on the Appendix in Figures 27, 28, 29, 30. The specification excludes never treated municipalities and uses not-yet-treated municipalities as the comparison group, restricting the sample to the same feasible treatment cohorts as in the stacked DiD design. The extensive margin result remains but the monetary compensation becomes no longer statistically significant.

Fifth, expanding the event window from $[-3, 3]$ to $[-4, 4]$ strengthens the results for both margins as we can see in Figure 32. For women, there is a negative statistically significant effect of 3,1% on the number of active formal workers. In addition, men experience a decrease of 0,7% of their earnings. However, for the gender gap there is no a statistically significant result.

Sixth, we allow for linear time trends in Figure 34. Once again, we obtain the same main patterns for women and men, confirming the extensive margin result for women and the monetary compensation margin result for men.

# 8 Limitations and Future Research

The work of Dias and Fontes (2024) is the first step to support the plausibility of the CAPS rollout as exogenous across municipalities. However, the objective of this analysis, labor market outcomes, introduces us into a more complicated framework where multiple channels may be working at the same time.

The main limitation is that we do not observe CAPS patients, psychiatric patients, or their families. Ideally, if we had microdata on CAPS patients or psychiatric patients, we could match them to their household members and analyze whether female relatives experienced a reduction in hours worked or earnings compared to men. This would allow us to directly test the caregiving channel. With the available data, however, we only observe whether a municipality received a CAPS, not whether a specific individual or household was directly affected by the policy.

Another caveat is that RAIS only provides annual individual level information for formal workers. As we have seen in the descriptive analysis, the informal sector is a relevant part of the Brazilian labor market, even though its share decreased over time. Therefore, the analysis cannot capture effects on informal workers. This is important

because the effects of CAPS may be concentrated precisely among individuals who are not observed in RAIS. In addition, relatives working in the formal sector may shift to informal jobs if they need more flexibility or working conditions that facilitate caregiving responsibilities.

There is also a limitation regarding the geographic interpretation of treatment. RAIS identifies the municipality where the individual works, not necessarily the municipality where the individual resides. Since CAPS are local health services, exposure to the policy is conceptually closer to the municipality of residence. Thus, the results should be interpreted as effects on formal labor market outcomes observed in municipalities where CAPS opened, rather than as direct effects on residents of treated municipalities.

With the data available, we are limited in our ability to claim that the estimated results are caused only by the expansion of CAPS. During these years, female labor supply may also have been affected by other changes taking place in Brazil, such as the increase in women's participation in paid work, the expansion of formal employment, changes in minimum wages, the expansion of social policies, and the long run effects of trade liberalization. This matters if these changes did not affect all municipalities in the same way, especially if municipalities that adopted CAPS earlier were already following different socioeconomic or labor market trajectories.

Future research could exploit the individual level structure of RAIS more directly. Although the present analysis uses municipality-year aggregates, the microdata could allow a more detailed analysis. This could provide a more precise picture of how the policy affected formal labor market attachment and working conditions.

The earnings results open the door to a deeper analysis of composition. One possible explanation for the decrease among men is that CAPS openings increased the number of mental health professionals, who may be mainly women, and changed the composition of the local workforce. This may have reduced the employment opportunities of highly educated men. Future work could explore this possibility by analyzing changes in the gender and occupational composition of the health care workforce, as well as changes in employment, hours and earnings among CAPS related occupations.

Moreover, linking the analysis to Census data could help incorporate informal employment and labor force participation, which are not observed in RAIS. This would be particularly useful if the policy affected individuals by inducing shifts from formal to informal work or exits from the labor force. Finally, future steps could be build a treatment intensity measure based on the number of CAPS facilities in each municipality, or on CAPS type and capacity, rather than relying only on the first opening of a CAPS.

# 9 Conclusions

Mental health has become a worldwide concern, highlighting the importance of effective policies to support people suffering from mental health conditions. However, as we have seen, these policies may also generate unintended indirect effects, especially when care responsibilities are partly shifted from institutions to households.

The direct impact comes from the jobs created from the opening of CAPS. There is evidence of an increase in the number of psychiatrists and their number of contracts.

An underfunded policy may create new burdens at the same time as it tries to alleviate existing concerns. This may be the case with the opening of CAPS. Although CAPS increased the supply of mental health services and promoted a shift away from hospital centered care, this expansion may not have been sufficient to fully absorb the care needs of patients. As a result, part of the caregiving burden may have shifted to families, with a particularly relevant impact on women.

The results are consistent with this interpretation. After a CAPS opens for the first time in a municipality, the number of active female formal workers falls by approximately 2.2%. At the same time, there is no clear evidence of a significant reduction in hours worked or of a shift from full-time to part-time employment. This suggests that the adjustment may operate more through exits from the extensive margin than through the intensive margin. Additionally, the gender gap reflects a decrease by 1.5% in the number of formal workers.

For men, the results show a small decline in earnings, below 0,5%. Since hours remain largely unchanged, this pattern seems more related to changes in compensation among active formal workers than to changes in working time. There is still a long path ahead to disentangle the different channels that may explain this reduction, for example by limiting the sample to workers employed before and after a CAPS opening.

To conclude, the findings suggest that mental health reforms can affect not only patients and healthcare workers, but also the labor market outcomes of families and caregivers. A well implemented policy may improve access to care and strengthen the mental health system, but it may also raise new concerns if the expansion of services is not sufficient to reduce the burden placed on households. Thus, the importance of evaluating mental health reforms not only through health outcomes, but also through their broader social and labor market effects.

# A Balancing Tests

Table 6: Pretreatment characteristics by CAPS adoption timing

| | Early adopters | Late adopters: 2011–2012 | | Late adopters: 2013–2014 | |
|---|---|---|---|---|---|
| Characteristic | Mean | Mean | Early–Late | Mean | Early–Late |
| Log total population, Census 2000 | 10.494 | 10.246 | 0.318*** (0.064) | 9.914 | 0.628*** (0.065) |
| Female population share, Census 2000 | 0.501 | 0.499 | 0.002** (0.001) | 0.496 | 0.004*** (0.001) |
| Population share aged 0–14, Census 2000 | 0.322 | 0.325 | -0.003 (0.003) | 0.323 | -0.004 (0.003) |
| Population share aged 15–64, Census 2000 | 0.618 | 0.615 | 0.004* (0.003) | 0.615 | 0.006** (0.003) |
| Population share aged 65 and older, Census 2000 | 0.060 | 0.060 | -0.002 (0.001) | 0.061 | -0.002* (0.001) |
| Rural population share, Census 2000 | 0.314 | 0.320 | -0.025 (0.017) | 0.346 | -0.061*** (0.016) |
| Municipality area, 2000 series ($km^2$) | 1814.513 | 3412.279 | -190.238 (607.972) | 1703.347 | 674.190 (521.677) |
| Distance to state capital (km) | 216.463 | 241.162 | -5.095 (15.563) | 246.310 | -18.966 (16.415) |
| GDP per capita, IBGE 2002 (R$) | 5391.489 | 5333.989 | 331.931 (469.421) | 5318.761 | 156.667 (534.629) |
| Population below the poverty line, Atlas 2000 | 0.417 | 0.417 | -0.022** (0.010) | 0.417 | -0.034*** (0.011) |
| Illiteracy rate among population aged 15 and older, Census 2000 | 0.213 | 0.209 | -0.011* (0.006) | 0.216 | -0.023*** (0.006) |
| Theil-L inequality index, Atlas 2000 | 0.557 | 0.565 | -0.005 (0.011) | 0.537 | 0.023** (0.010) |
| Municipal health and sanitation spending per capita, 2000 (R$) | 71.166 | 71.341 | 2.296 (3.858) | 70.772 | 1.633 (4.154) |
| Labor-force participation among population aged 18–64, Census 2000 | 0.680 | 0.671 | 0.010** (0.005) | 0.667 | 0.014** (0.006) |
| Formal employment relative to broad employment, age 18–64, Census 2000 | 0.393 | 0.374 | 0.022** (0.011) | 0.367 | 0.037*** (0.011) |
| AMS psychiatrists per 10,000 inhabitants | 0.293 | 0.263 | 0.082 (0.081) | 0.150 | 0.176*** (0.067) |
| AMS psychologists per 10,000 inhabitants | 0.402 | 0.531 | -0.082 (0.104) | 0.400 | 0.039 (0.057) |
| AMS social workers per 10,000 inhabitants | 0.454 | 0.388 | 0.110 (0.095) | 0.369 | 0.095 (0.065) |
| AMS three-profession sum per 10,000 inhabitants | 1.149 | 1.181 | 0.110 (0.242) | 0.919 | 0.310** (0.144) |
| AMS psychiatric outpatient establishments per 10,000 inhabitants | 1.039 | 0.999 | 0.096 (0.080) | 1.150 | -0.057 (0.084) |
| Any AMS psychiatric outpatient establishment in 1999 | 0.923 | 0.795 | 0.148*** (0.032) | 0.798 | 0.133*** (0.034) |
| Observations | | 588 | | 581 | |

Continued on the next page

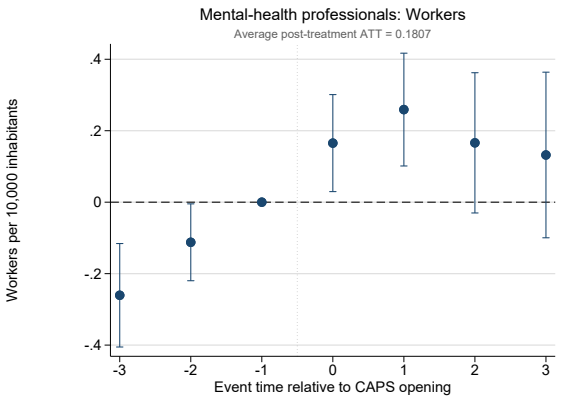
Table 6: Pretreatment characteristics by CAPS adoption timing (continued)

| | Early adopters | Late adopters: 2011–2012 | | Late adopters: 2013–2014 | |
|---|---|---|---|---|---|
| Characteristic | Mean | Mean | Early–Late | Mean | Early–Late |
| Joint test of baseline characteristics, $p$-value | | $< 0.001$ | | | $< 0.001$ |

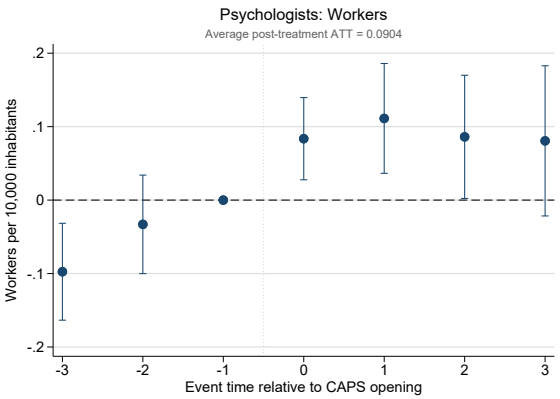
*Notes:* Early adopters are municipalities that opened their first CAPS between 2005 and 2007. They are compared separately with municipalities treated in 2011–2012 and 2013–2014. The means are unadjusted. The Early–Late columns report coefficients from separate regressions of each pretreatment characteristic on an early-adopter indicator, including state fixed effects. Robust standard errors are reported in parentheses. The joint test examines whether the non-redundant baseline characteristics jointly predict early adoption, conditional on state fixed effects. The population share aged 15–64 and the three-profession sum are excluded because they are mechanically determined by other included variables. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

# B Direct Effects

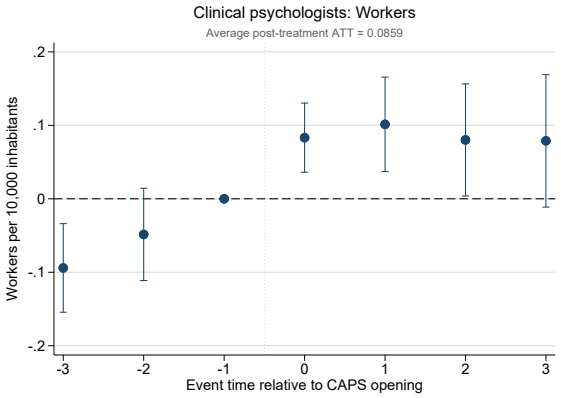
(a) All mental health professionals



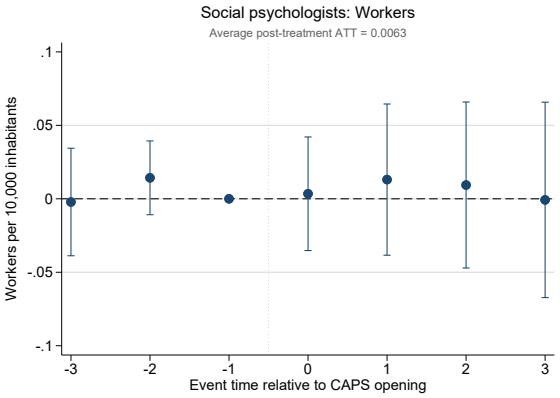

(b) Psychologists



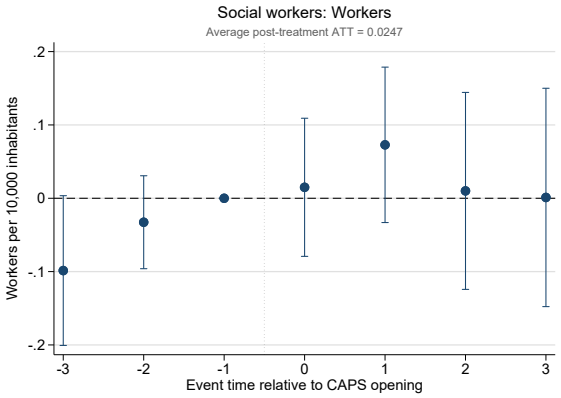

(c) Clinical psychologists




(d) Social psychologists




(e) Social workers




(f) Occupational therapists

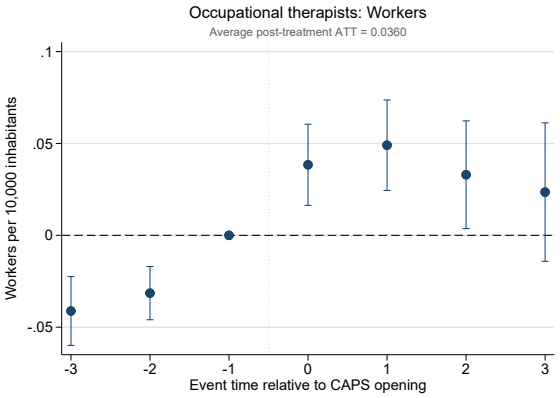
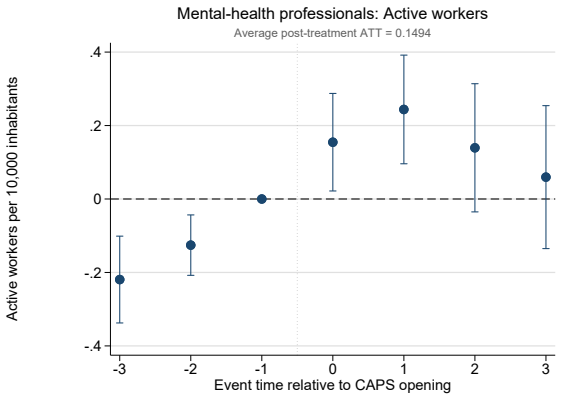


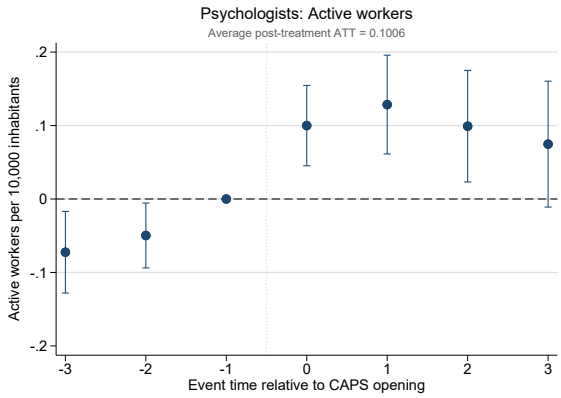

Figure 14: Effects of CAPS openings on mental health workers, by occupation

*Notes:* The figure reports stacked difference-in-differences event-study estimates for the number of unique professionals observed in formal employment at any point during the year, per 10,000 inhabitants. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

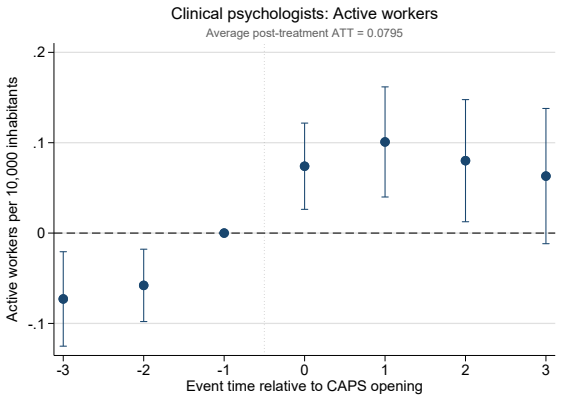
(a) All mental health professionals



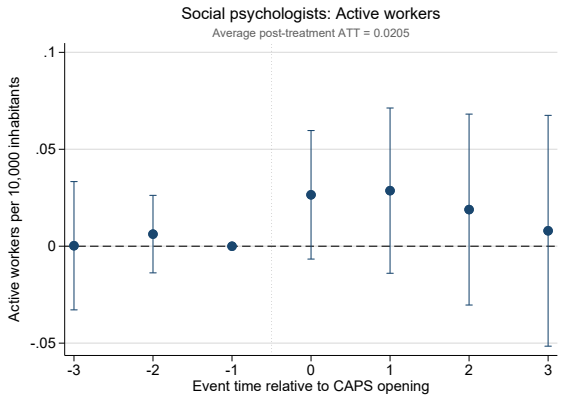

(b) Psychologists



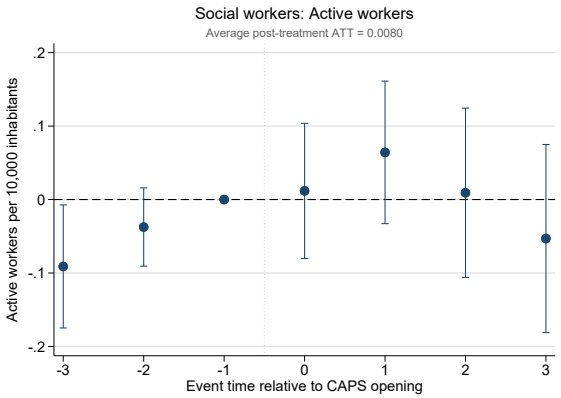

(c) Clinical psychologists




(d) Social psychologists




(e) Social workers




(f) Occupational therapists

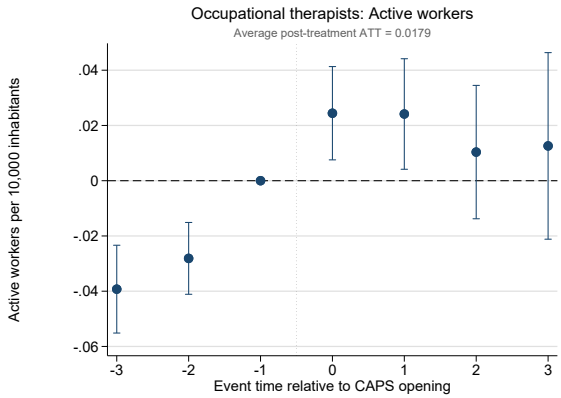
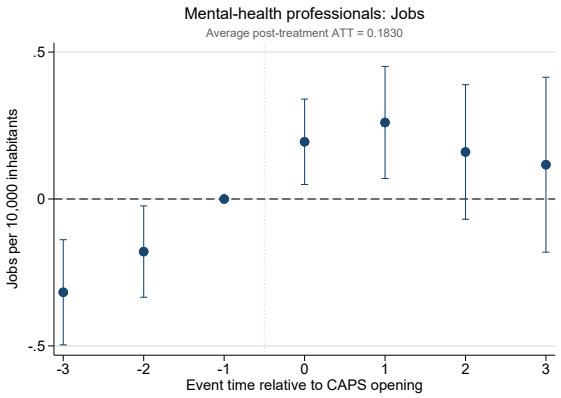


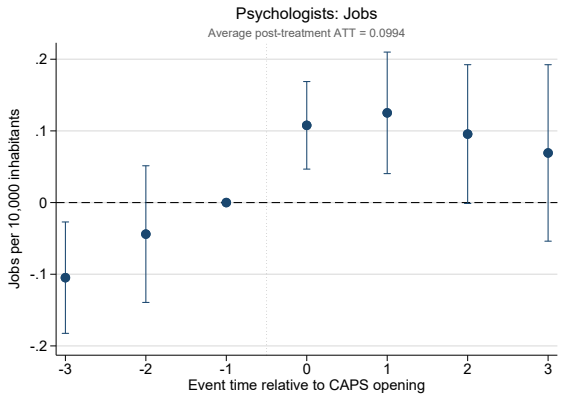

Figure 15: Effects of CAPS openings on active mental health workers, by occupation

*Notes:* The figure reports stacked difference-in-differences event-study estimates for the number of unique professionals with at least one formal employment relationship active on December 31, per 10,000 inhabitants. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

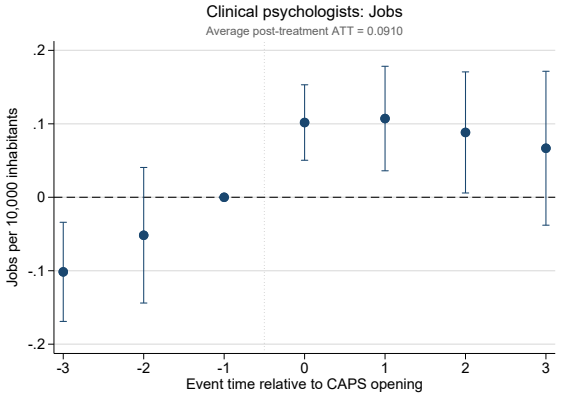
(a) All mental health professionals



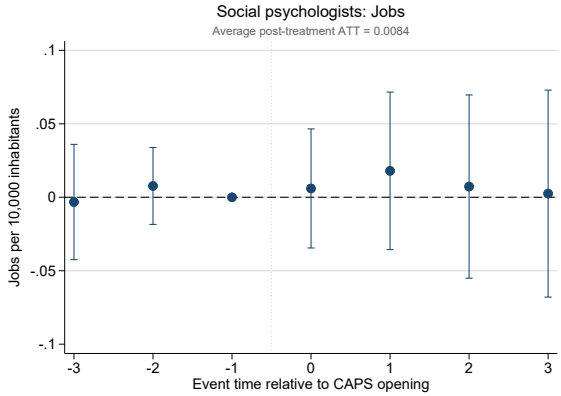

(b) Psychologists



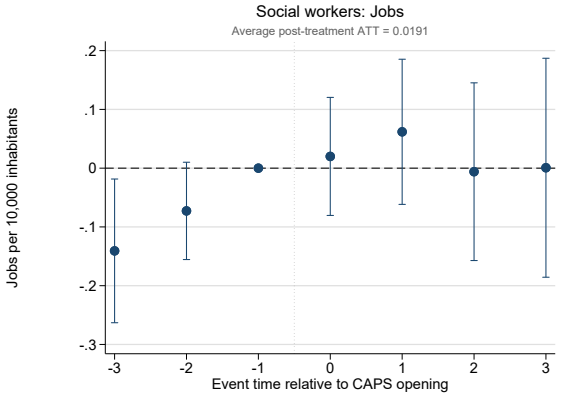

(c) Clinical psychologists




(d) Social psychologists




(e) Social workers




(f) Occupational therapists

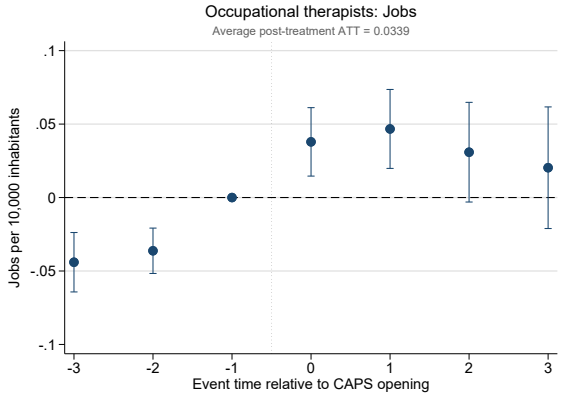
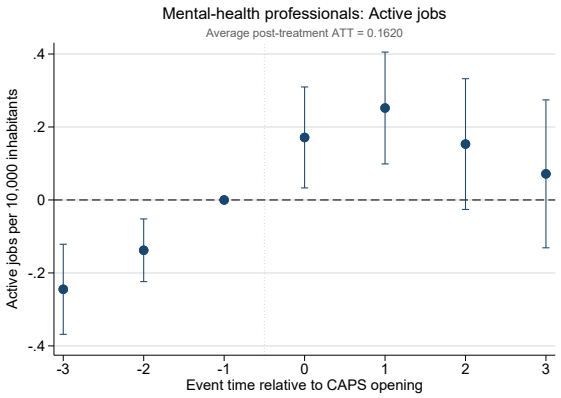


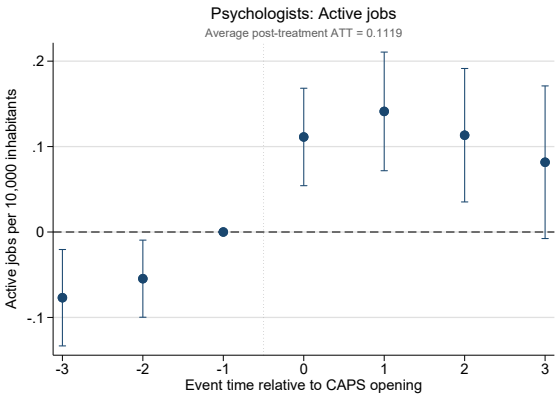

Figure 16: Effects of CAPS openings on mental health jobs, by occupation

*Notes:* The figure reports stacked difference-in-differences event-study estimates for the number of formal employment relationships held at any point during the year, per 10,000 inhabitants. A professional can contribute more than one job. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

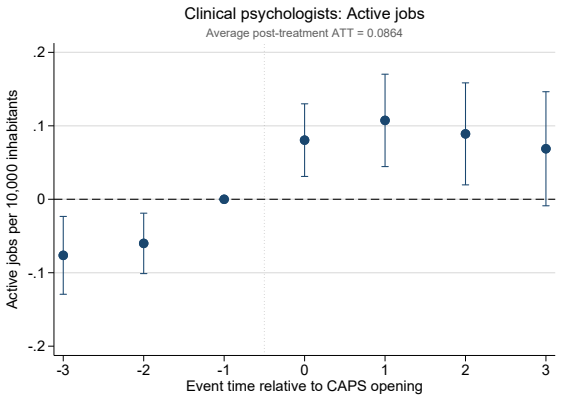
(a) All mental health professionals



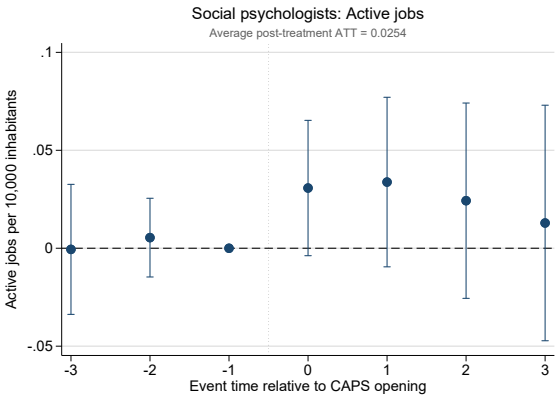

(b) Psychologists



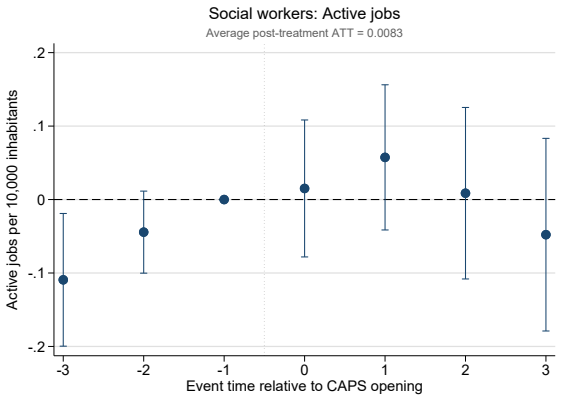

(c) Clinical psychologists




(d) Social psychologists




(e) Social workers




(f) Occupational therapists

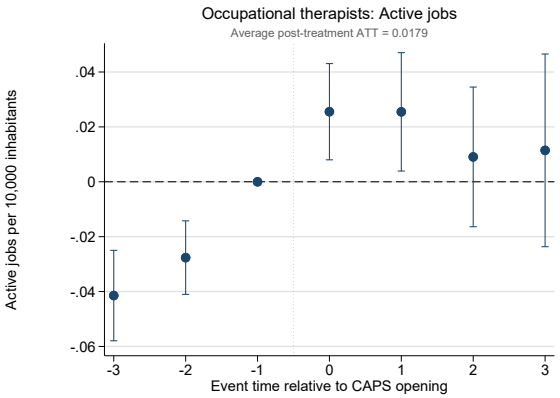


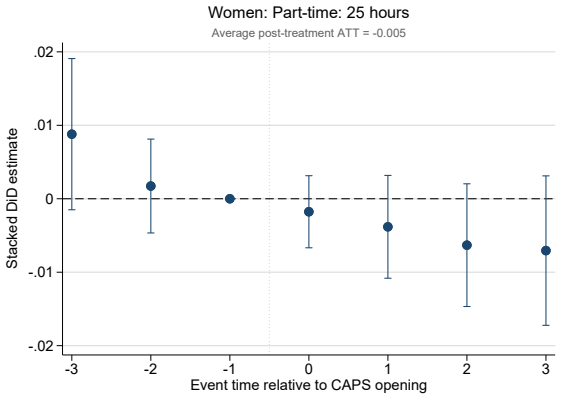

Figure 17: Effects of CAPS openings on active mental health jobs, by occupation

*Notes:* The figure reports stacked difference-in-differences event-study estimates for the number of formal employment relationships active on December 31, per 10,000 inhabitants. A professional can contribute more than one active job. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

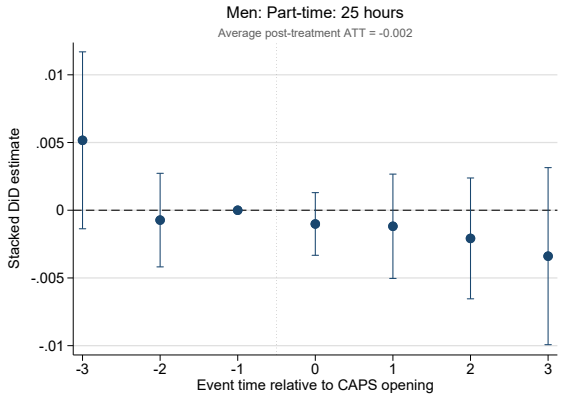
# C Indirect Effects

## C.1 Additional intensive margin outcomes

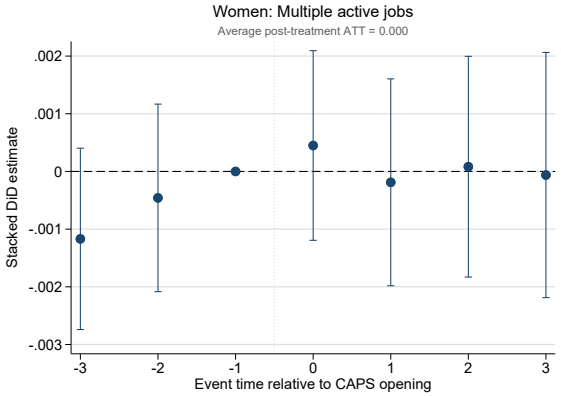
(a) Women: share part-time




(b) Men: share part-time




(c) Women: share multiple active jobs




(d) Men: share multiple active jobs

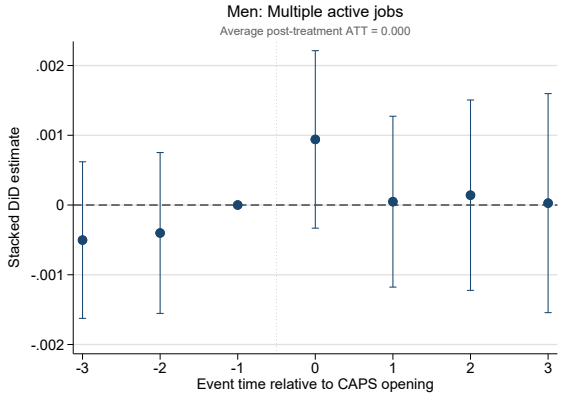
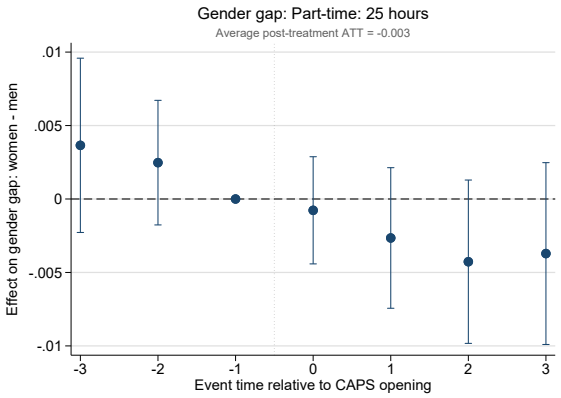



Figure 18: Effects of CAPS openings on part-time work and multiple jobs, by gender

*Notes:* The figure reports stacked difference-in-differences event-study estimates by gender. All outcomes are computed among active formal workers, defined as workers with at least one formal employment relationship active on December 31. The part-time share is the share of active formal workers with total weekly hours of 25 or less, summing hours across active links. The multiple-jobs share is the share of active formal workers with more than one active formal link on December 31. Event time −1 is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

(a) Gender gap: share part-time




(b) Gender gap: share multiple active jobs

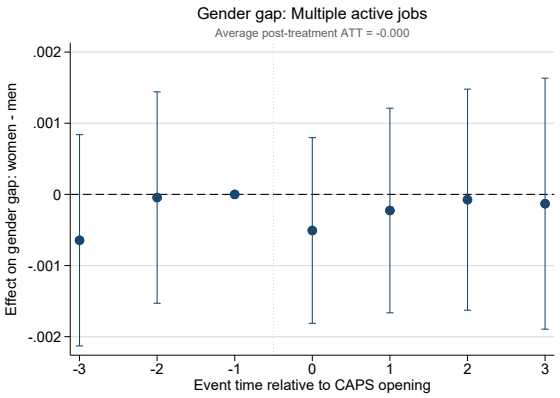



Figure 19: Effects of CAPS openings on gender gaps in part-time work and multiple jobs

*Notes:* The figure reports stacked difference-in-differences event-study estimates for gender gap outcomes. Gender gaps are defined as the female outcome minus the male outcome within each municipality-year. Both outcomes are computed among active formal workers, defined as workers with at least one formal employment relationship active on December 31. The part-time share is the share of active formal workers working 25 hours or less per week, summing hours across active links. The multiple-jobs share is the share of active formal workers with more than one active formal link on December 31. Event time $-1$ is the omitted category. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. Vertical lines represent 95% confidence intervals based on standard errors clustered at the municipality level.

# D Robustness

## D.1 De Chaisemartin and D'Haultfoeuille

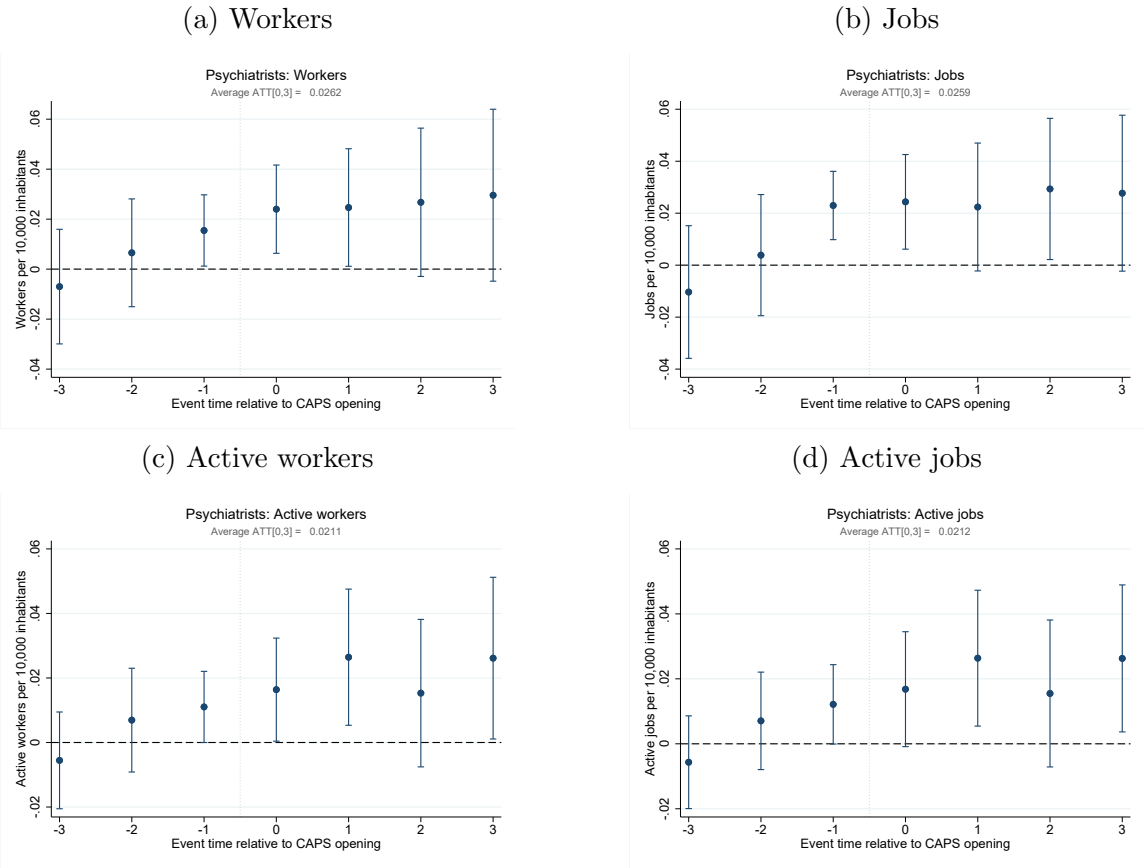
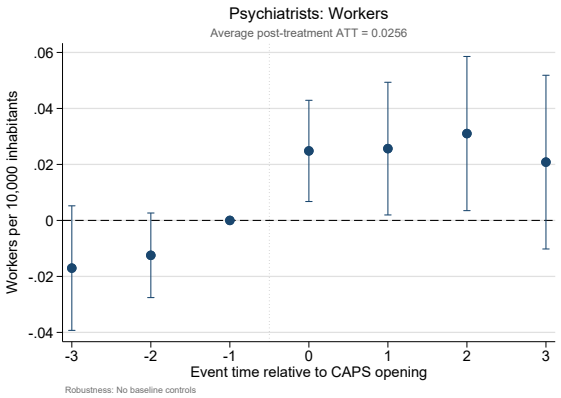



Figure 20: Effects of CAPS openings on psychiatrist employment using the de Chaisemartin and D'Haultfœuille estimator

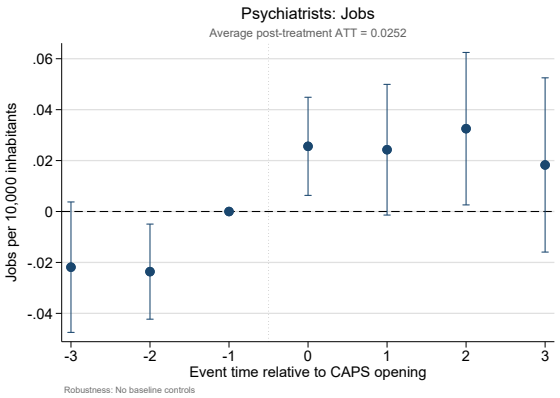
*Notes:* Each panel reports event-study estimates obtained using the de Chaisemartin and D'Haultfœuille estimator for psychiatrist employment per 10,000 inhabitants. Workers are unique psychiatrists observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique psychiatrists with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. The sample covers 2003–2018 and includes treatment cohorts from 2006 to 2014. Later-treated municipalities are used as controls while they remain untreated. Negative event times report placebo estimates, while dynamic treatment effects are estimated from event times 0 to 3. The reported ATT is the equally weighted average of the effects from event times 0 to 3. The specifications include differential linear trends based on baseline log population, the baseline poverty rate, and the presence of a psychiatric outpatient establishment in 1999. Standard errors are obtained from 100 bootstrap replications clustered at the municipality level, and vertical bars represent 95% confidence intervals.

## D.2 Stacked DiD No Controls

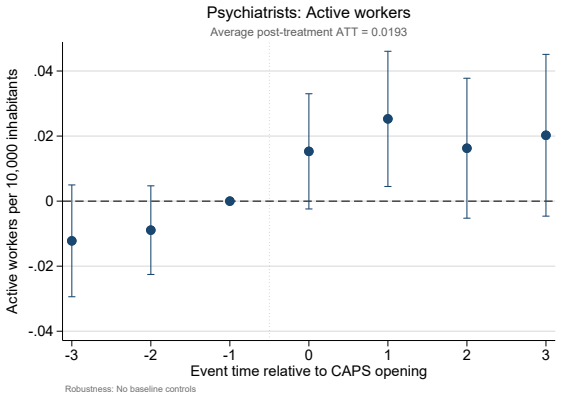
(a) Workers




(b) Jobs




(c) Active workers




(d) Active jobs

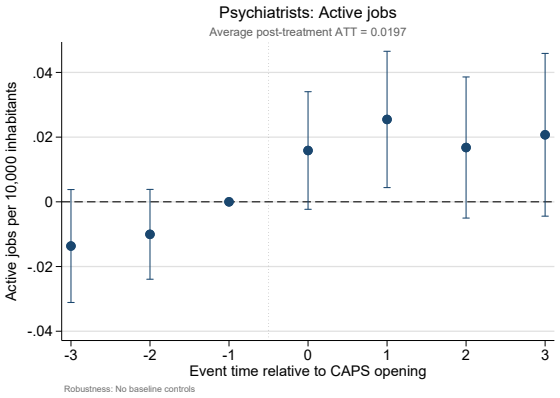
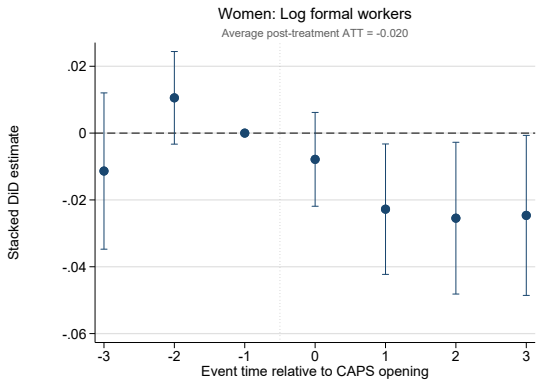
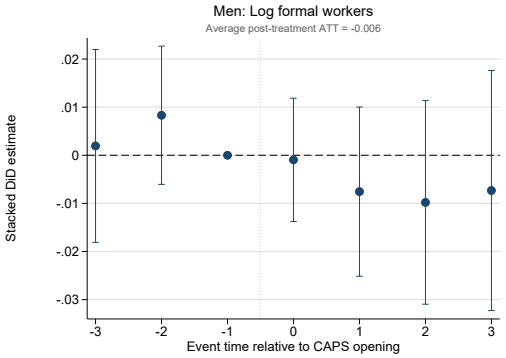



Figure 21: Effects of CAPS openings on psychiatrist employment: estimates without controls

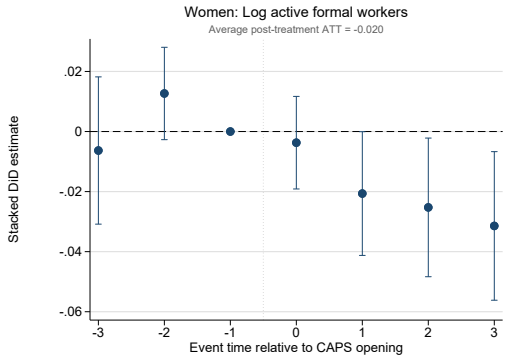
*Notes:* Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. Outcomes are measured per 10,000 inhabitants. Workers are unique psychiatrists observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique psychiatrists with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment, year-by-sub-experiment fixed effects, and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.



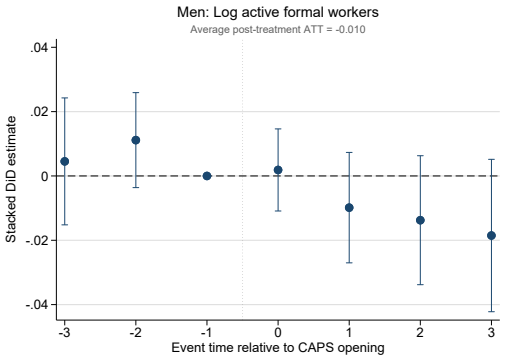

(a) Women



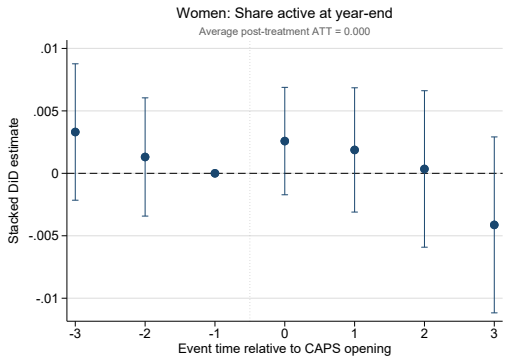

(b) Men



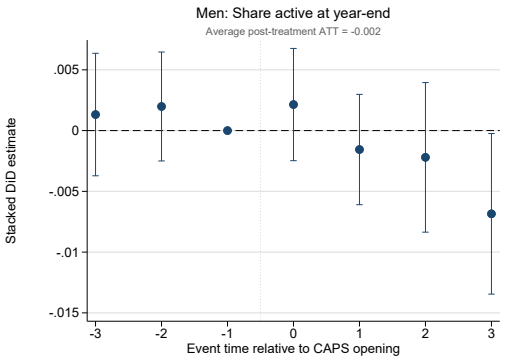

(c) Women




(d) Men



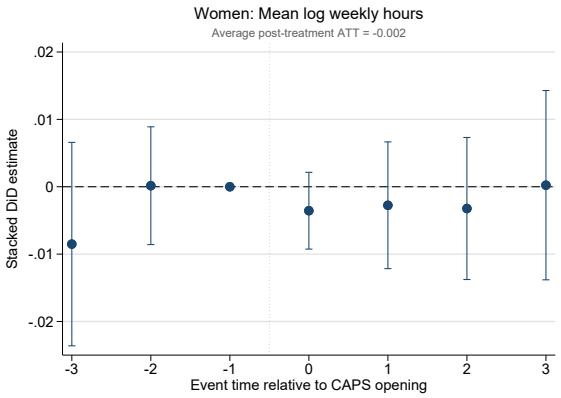

(e) Women



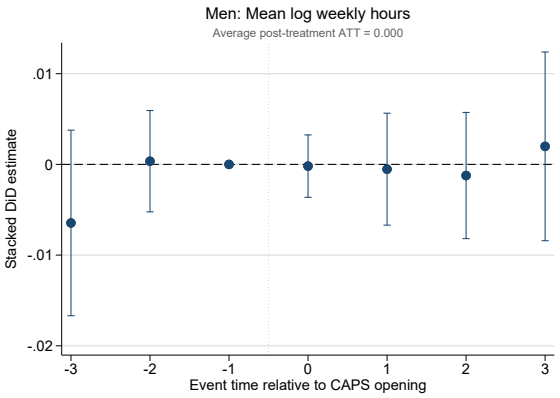

(f) Men

Figure 22: Effects of CAPS openings on extensive margin outcomes by gender: estimates without controls

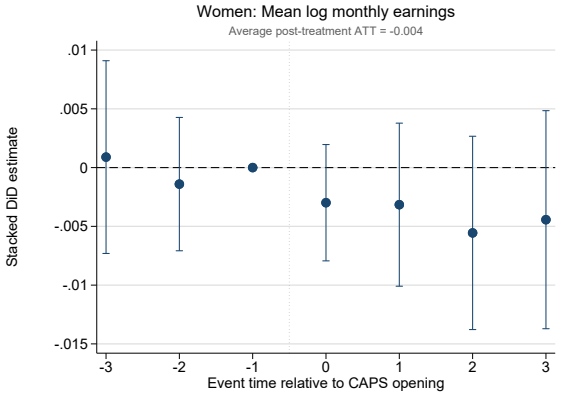
*Notes:* Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment, year-by-sub-experiment fixed effects and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.



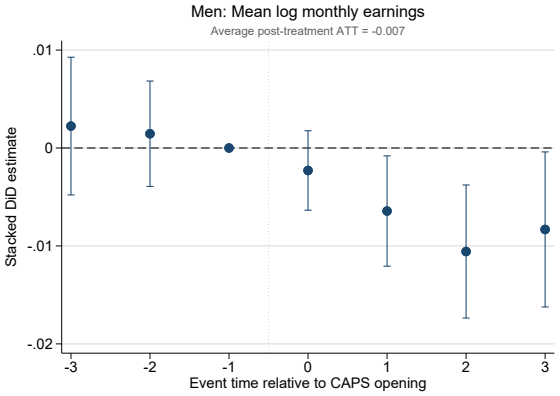

(a) Women



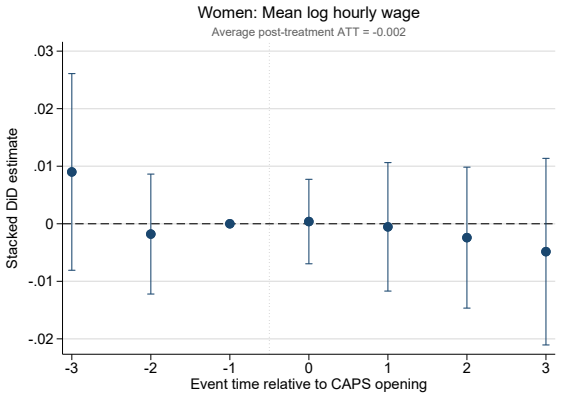

(b) Men



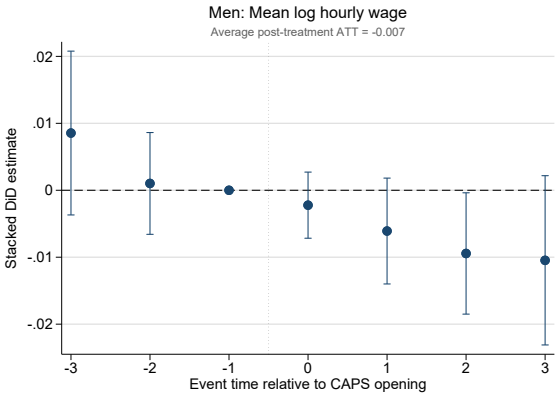

(c) Women




(d) Men



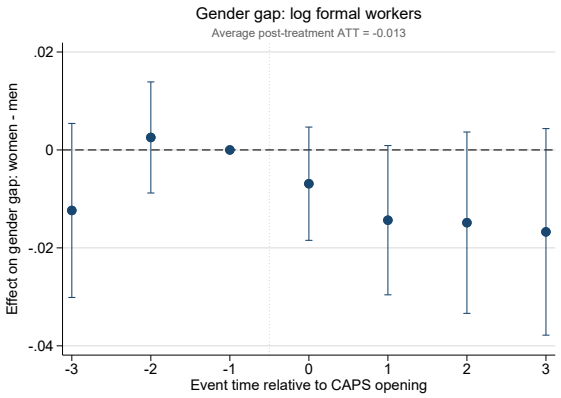

(e) Women



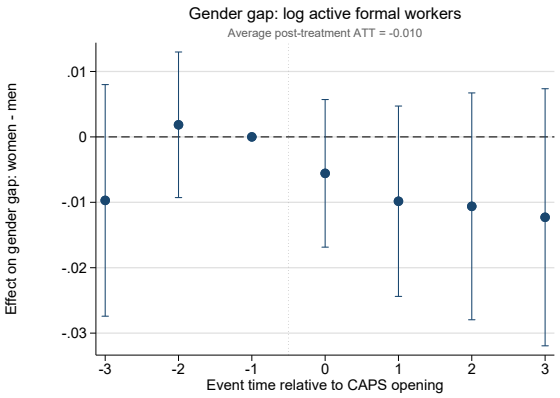

(f) Men

Figure 23: Effects of CAPS openings on intensive-margin outcomes by gender: estimates without controls

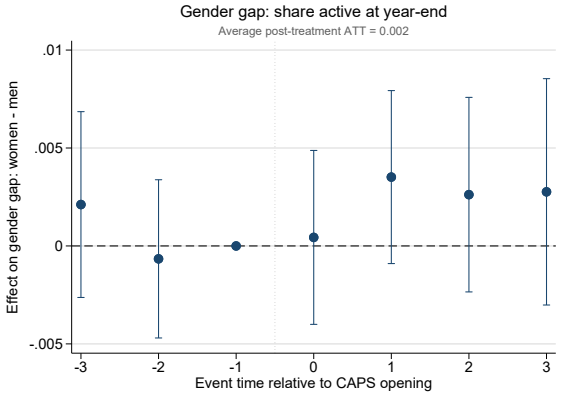
*Notes:* Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment, year-by-sub-experiment fixed effects and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.




(a) Formal workers



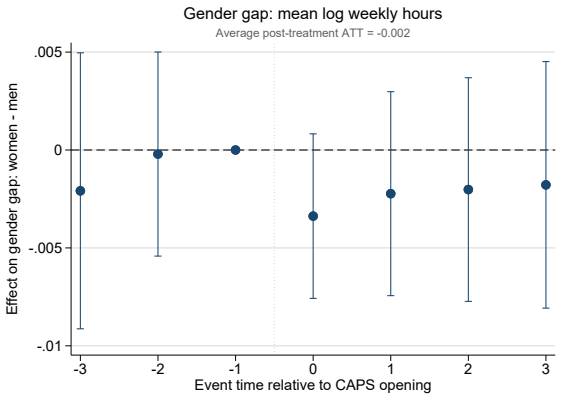

(b) Active formal workers



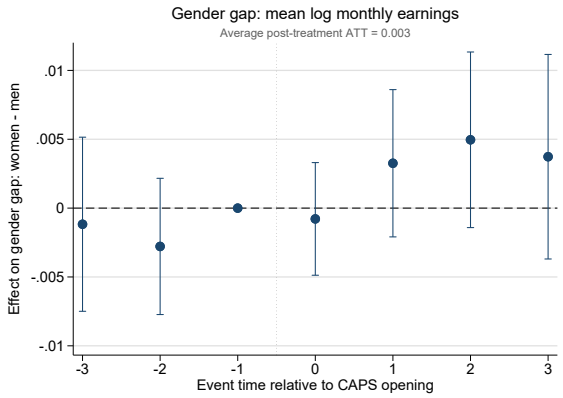

(c) Share active at year-end

Figure 24: Effects of CAPS openings on gender gaps in extensive-margin outcomes: estimates without controls

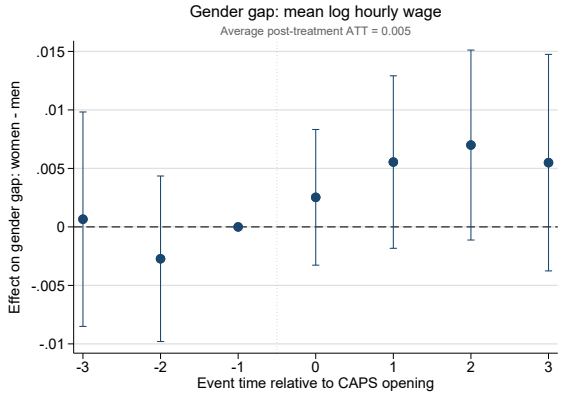
*Notes:* Gender gaps are defined as the outcome for women minus the corresponding outcome for men. Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment and calendar-year-by-sub-experiment fixed effects and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.




(a) Total hours



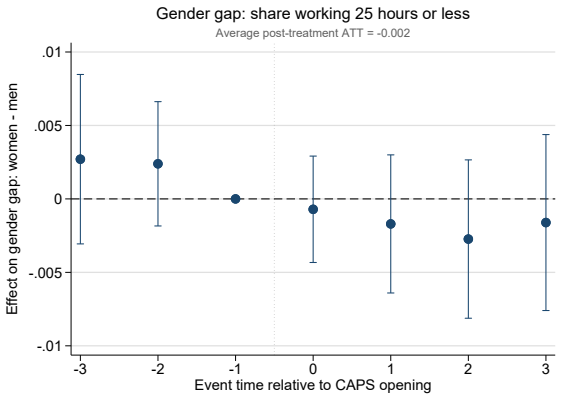

(b) Total earnings



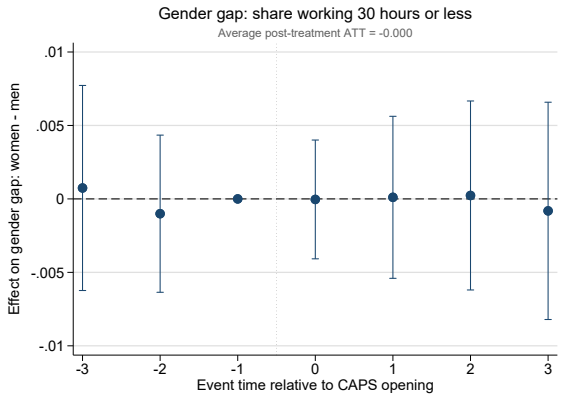

(c) Hourly wage

Figure 25: Effects of CAPS openings on gender gaps in intensive-margin outcomes: estimates without controls

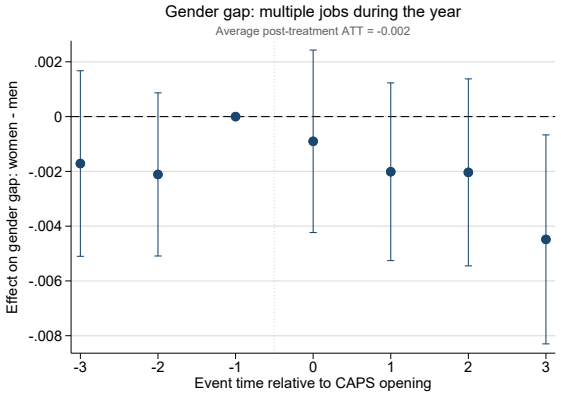
*Notes:* Gender gaps are defined as the outcome for women minus the corresponding outcome for men. Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment and calendar-year-by-sub-experiment fixed effects and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.



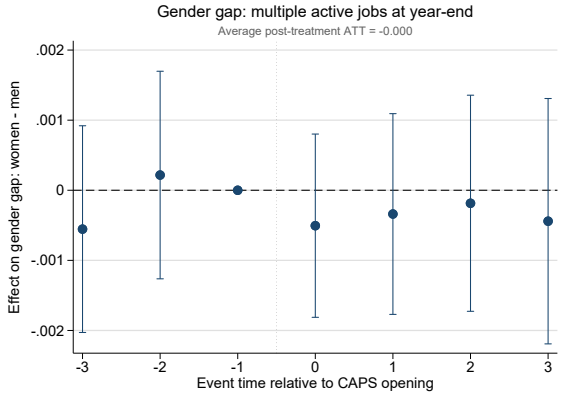

(a) Working 25 hours or less




(b) Working 30 hours or less




(c) Multiple jobs during the year



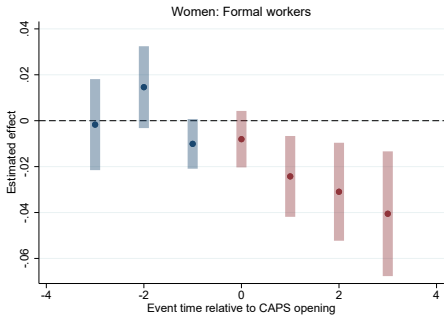

(d) Multiple active jobs at year-end

Figure 26: Effects of CAPS openings on gender gaps in alternative employment adjustments: estimates without controls

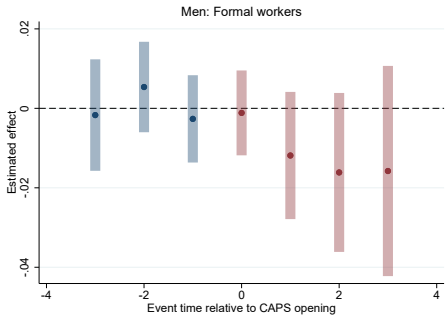
*Notes:* Gender gaps are defined as the outcome for women minus the corresponding outcome for men. Each panel reports event-study estimates from a weighted stacked difference-in-differences specification comparing municipalities opening their first CAPS in a given year with municipalities treated after the end of the corresponding event window. The event window is $[-3, 3]$, with event time $-1$ as the reference period. All regressions include municipality-by-sub-experiment and calendar-year-by-sub-experiment fixed effects and use corrective stack weights. Standard errors are clustered at the municipality level, and vertical bars represent 95% confidence intervals. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3.

## D.3 Callaway and Sant'Anna

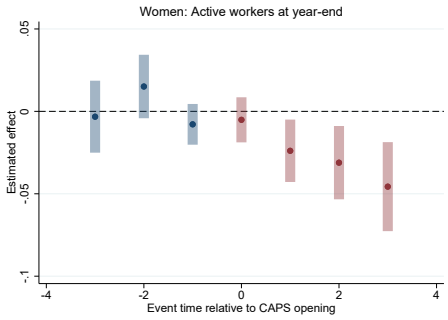
(a) Women: log formal workers



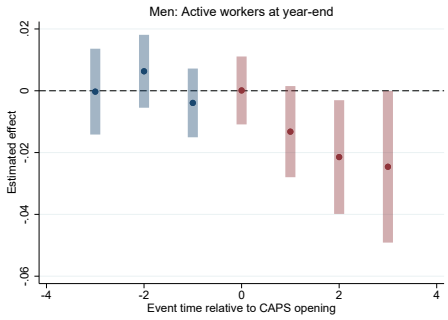

(b) Men: log formal workers



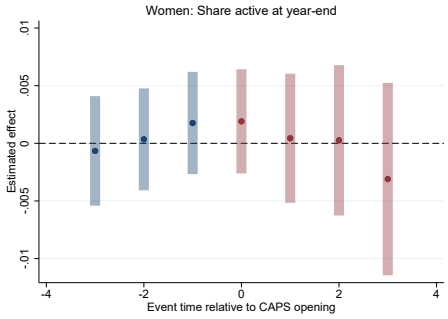

(c) Women: log active formal workers




(d) Men: log active formal workers




(e) Women: share active at year-end




(f) Men: share active at year-end

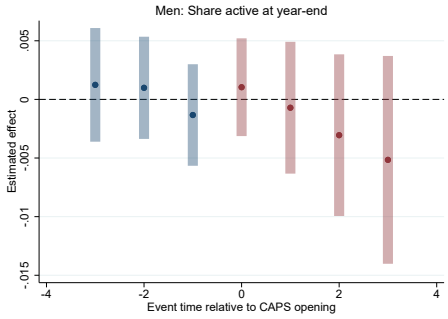
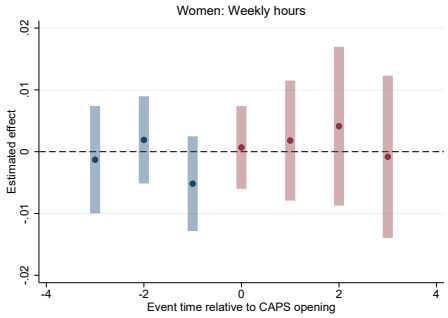


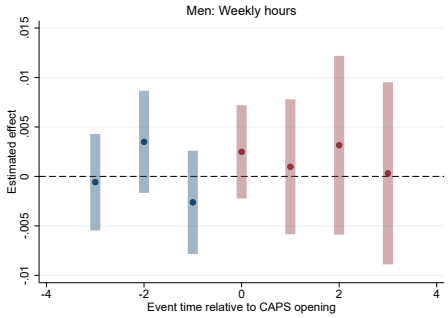

Figure 27: Callaway and Sant'Anna estimates for extensive margin outcomes, by gender

*Notes:* The figure reports event-study aggregations of group-time average treatment effects estimated using the Callaway and Sant'Anna difference-in-differences estimator. The specification uses doubly robust inverse probability weighting and municipalities that have not yet opened their first CAPS as the comparison group. The estimation conditions on baseline log population, the baseline poverty rate and an indicator for the presence of a psychiatric outpatient establishment in 1999. Formal workers are workers with at least one formal employment relationship observed during the year. Active formal workers are workers with at least one formal employment relationship active on December 31. The share active at year-end is the number of active formal workers divided by the total number of formal workers in the same municipality-year-gender cell. Event time $-1$ is the reference period. Vertical bars represent 95% confidence intervals based on wild bootstrap standard errors clustered at the municipality level.

(a) Women: log total hours



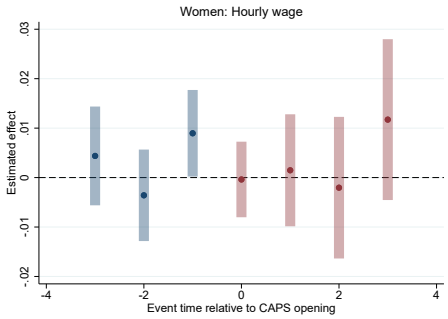

(b) Men: log total hours




(c) Women: log total earnings


(d) Men: log total earnings


(e) Women: log hourly wage




(f) Men: log hourly wage

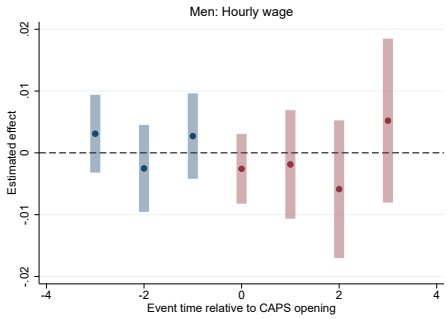
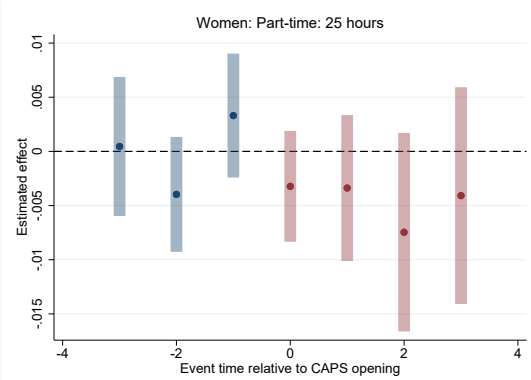


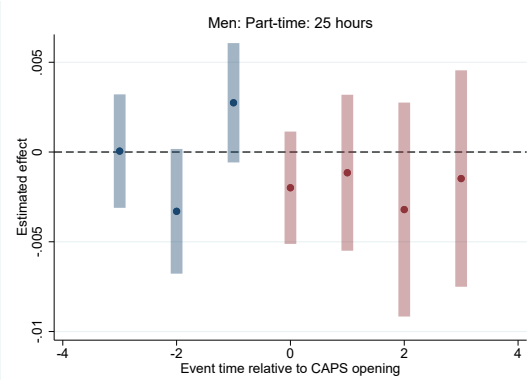

Figure 28: Callaway and Sant'Anna estimates for intensive margin outcomes, by gender

*Notes:* The figure reports event-study aggregations of group-time average treatment effects estimated using the Callaway and Sant'Anna difference-in-differences estimator. The specification uses doubly robust inverse probability weighting, not-yet-treated municipalities as the comparison group and the same baseline controls as the main specification. All outcomes are computed among active formal workers. Log total hours is the municipality-year-gender average of individual log total weekly hours, with hours summed across formal employment relationships active on December 31. Log total earnings is the corresponding average of individual log total monthly real earnings, summed across active formal relationships. Log hourly wage is calculated as total monthly real earnings divided by total weekly hours multiplied by 4.33. Event time $-1$ is the reference period. Vertical bars represent 95% confidence intervals based on wild bootstrap standard errors clustered at the municipality level.

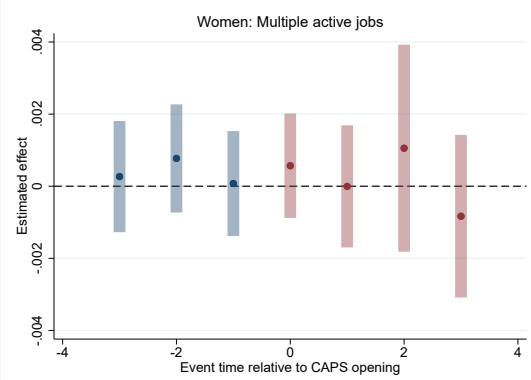
(a) Women: part-time share




(b) Men: part-time share




(c) Women: multiple-jobs share




(d) Men: multiple-jobs share

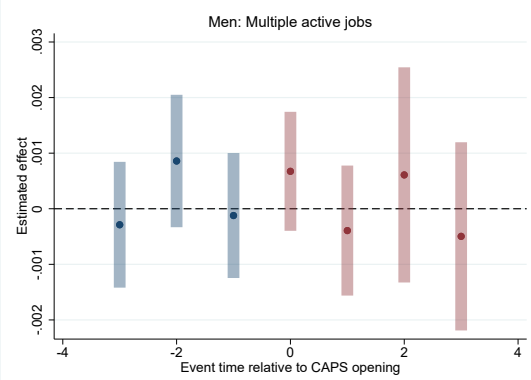



Figure 29: Callaway and Sant'Anna estimates for part-time employment and multiple-job holding, by gender

*Notes:* The figure reports event-study aggregations of group-time average treatment effects estimated using the Callaway and Sant'Anna difference-in-differences estimator. The specification uses doubly robust inverse probability weighting, not-yet-treated municipalities as the comparison group and the same baseline controls as the main specification. Both outcomes are computed among active formal workers. The part-time share is the share of active workers working 25 hours or less per week, after summing hours across all formal employment relationships active on December 31. The multiple-jobs share is the share of active workers with more than one formal employment relationship active on December 31. Event time $-1$ is the reference period. Vertical bars represent 95% confidence intervals based on wild bootstrap standard errors clustered at the municipality level.

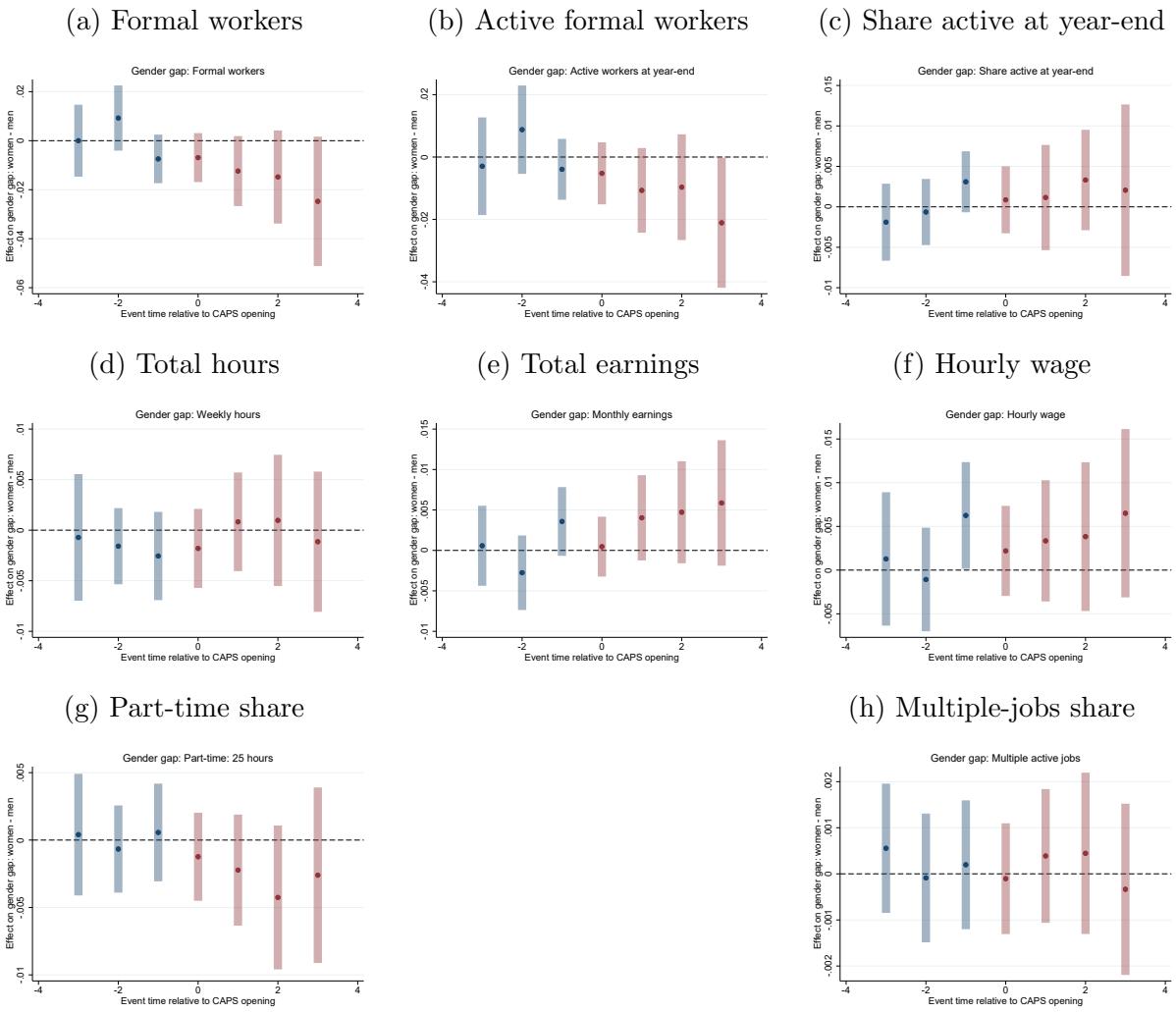



Figure 30: Callaway and Sant'Anna estimates for gender gap outcomes

*Notes:* The figure reports event-study aggregations of group-time average treatment effects estimated using the Callaway and Sant'Anna difference-in-differences estimator. Gender gaps are defined as the female outcome minus the corresponding male outcome within each municipality-year. The specification uses doubly robust inverse probability weighting, not-yet-treated municipalities as the comparison group and the same baseline controls as the main specification. Formal workers include workers with at least one formal employment relationship observed during the year, while active formal workers have at least one formal relationship active on December 31. Intensive-margin outcomes, part-time employment and multiple-job working 25 hours or less per week per week computed among active formal workers. Event time $-1$ is the reference period. Vertical bars represent 95% confidence intervals based on wild bootstrap standard errors clustered at the municipality level.

## D.4 Event Window $[-4, 4]$

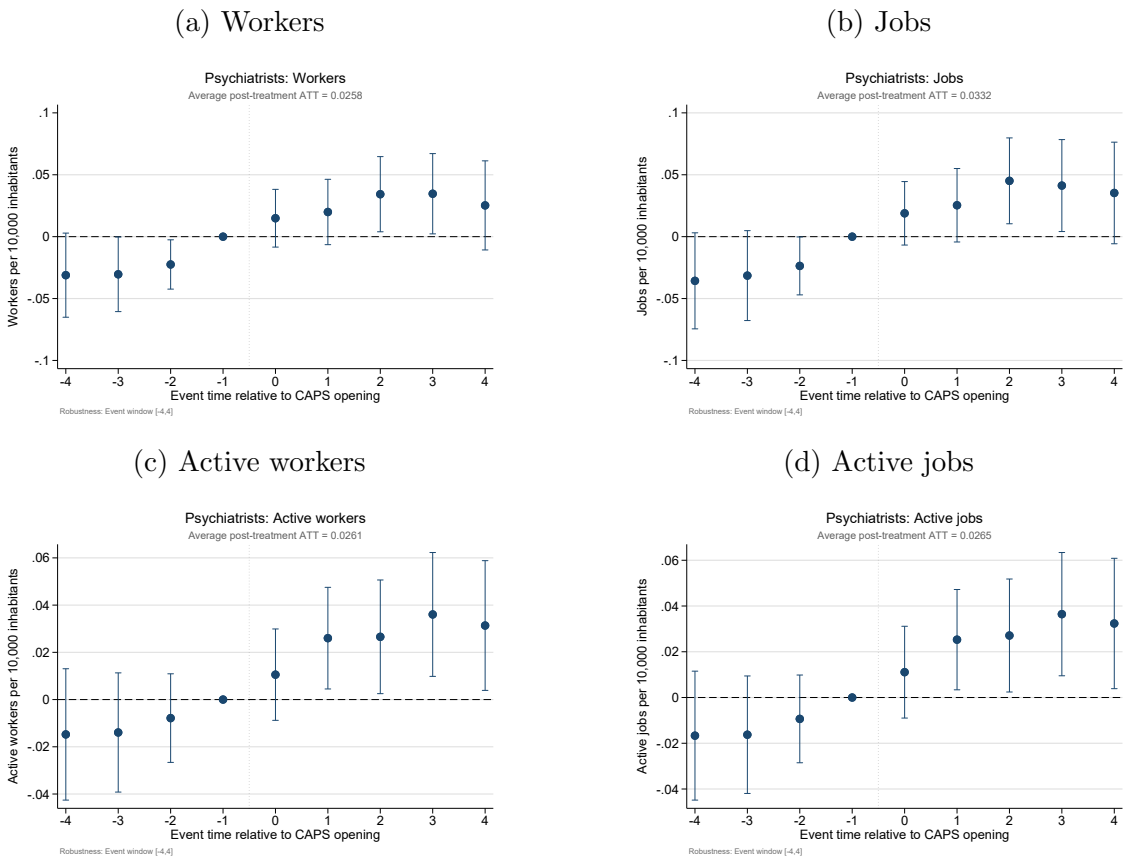
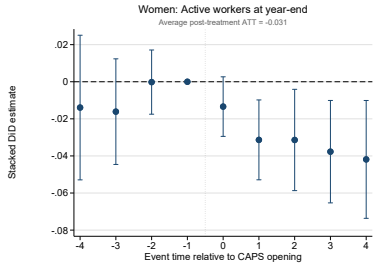


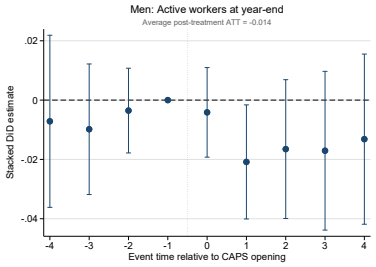

Figure 31: Effects of CAPS openings on psychiatrist employment using an alternative event window

*Notes:* Each panel reports weighted stacked difference-in-differences event-study estimates for psychiatrist employment per 10,000 inhabitants using a window from four years before to four years after the first CAPS opening. Workers are unique psychiatrists observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique psychiatrists with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. Event time $-1$ is the omitted category. The reported ATT is the equally weighted average of the coefficients from event times 0 to 4. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline log population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. The estimates use corrective stack weights. Vertical bars represent 95% confidence intervals based on standard errors clustered at the municipality level.

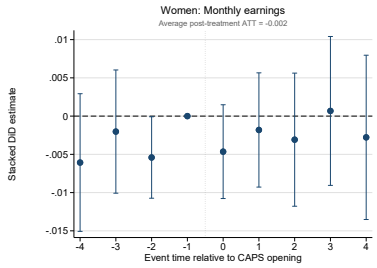
(a) Women: log active formal workers



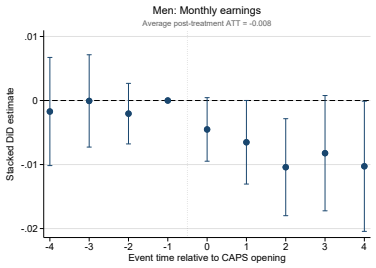

(b) Men: log active formal workers



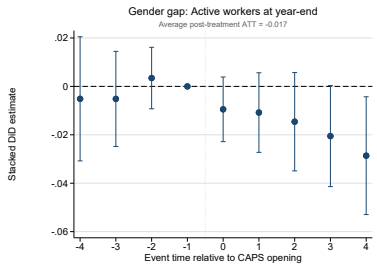

(c) Women: log total earnings




(d) Men: log total earnings




(e) Gender gap: log active formal workers




(f) Gender gap: log total earnings

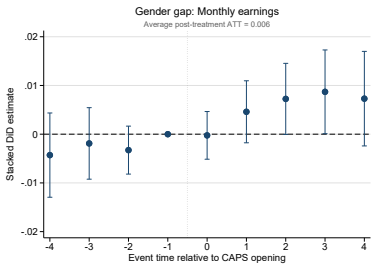



Figure 32: Main stacked DiD results using an alternative event window

*Notes:* The figure reports stacked difference-in-differences event-study estimates using a window from four years before to four years after the first CAPS opening. Active formal workers are workers with at least one formal employment relationship active on December 31. Monthly earnings refer to the mean log total monthly real earnings among active formal workers, summing earnings across all formal relationships active on December 31. Gender gaps are defined as the female outcome minus the corresponding male outcome within each municipality-year. Event time $-1$ is the omitted category. The reported ATT is the equally weighted average of the coefficients from event times 0 to 4. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects, together with interactions between year indicators and baseline population, the baseline poverty rate, and the presence of an outpatient psychiatric establishment in 1999. The estimates use corrective stack weights. Vertical bars represent 95% confidence intervals.

## D.5 Linear Time Trends

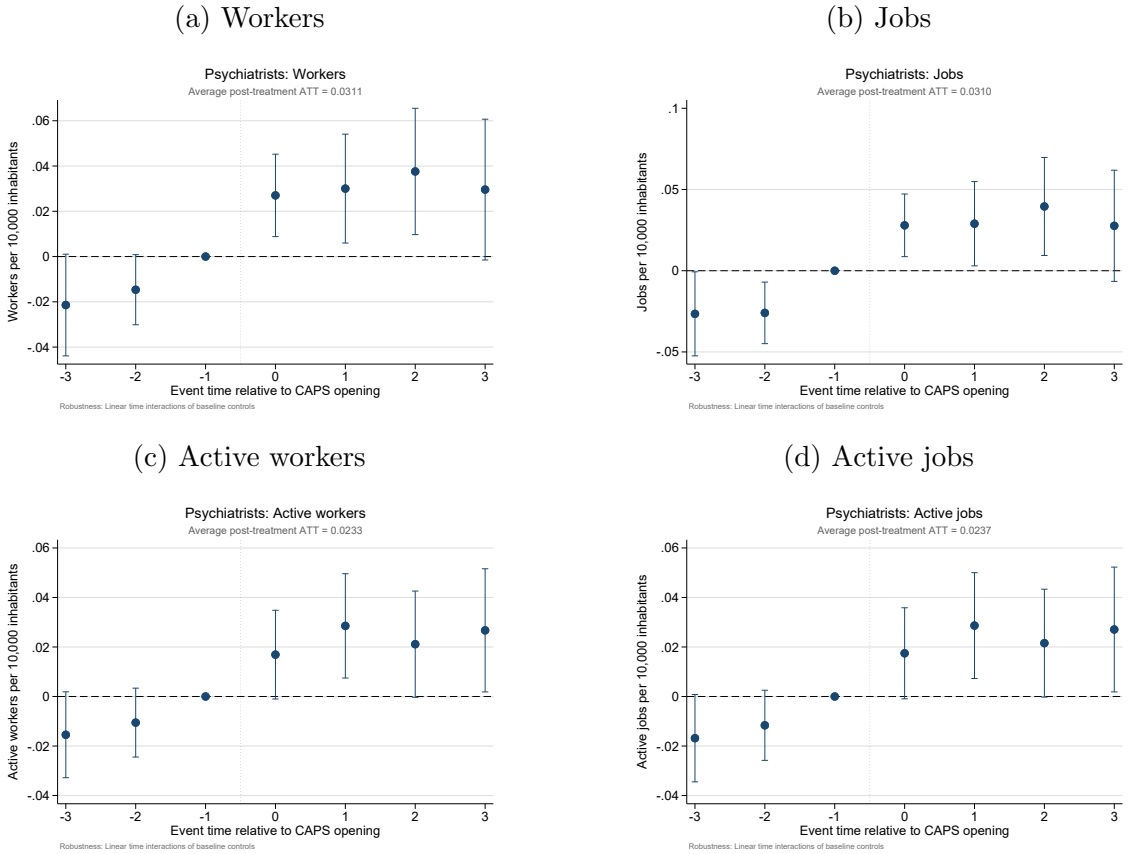
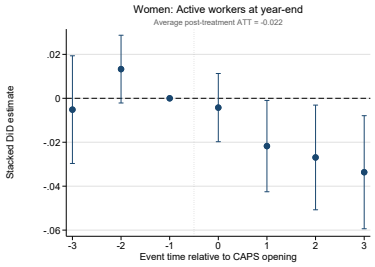


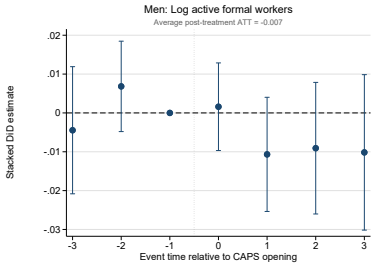

Figure 33: Effects of CAPS openings on psychiatrist employment using linear interactions of the baseline controls

*Notes:* Each panel reports weighted stacked difference-in-differences event-study estimates for psychiatrist employment outcomes per 10,000 inhabitants. The specifications replace the flexible year interactions of the baseline municipality characteristics with interactions between each characteristic and a linear year trend. The baseline characteristics are log population in 2000, the poverty rate in 2000, and an indicator for the presence of an outpatient psychiatric establishment in 1999. Workers are unique psychiatrists observed in formal employment at any point during the year, while jobs are formal employment relationships held at any point during the year. Active workers are unique psychiatrists with at least one formal employment relationship active on December 31, while active jobs are formal employment relationships active on that date. The event window is $[-3, 3]$, with event time $-1$ as the omitted category. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects. The estimates use corrective stack weights. Vertical bars represent 95% confidence intervals based on standard errors clustered at the municipality level.

(a) Women: log active formal workers

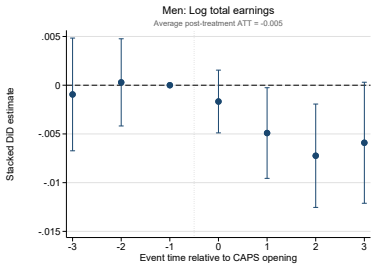



(b) Men: log active formal workers

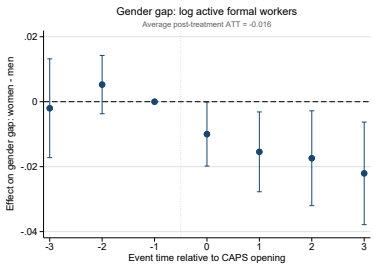



(c) Women: log total earnings


(d) Men: log total earnings




(e) Gender gap: log active formal workers




(f) Gender gap: log total earnings

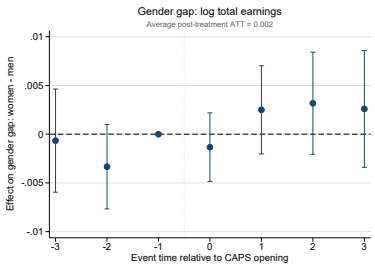



Figure 34: Main stacked DiD results using linear interactions of the baseline controls

*Notes:* The figure reports stacked difference-in-differences event-study estimates replacing the flexible year interactions of the baseline municipality characteristics with interactions between each characteristic and a linear year trend. The baseline characteristics are log population in 2000, the poverty rate in 2000, and an indicator for the presence of an outpatient psychiatric establishment in 1999. Active formal workers are workers with at least one formal employment relationship active on December 31. For each active formal worker, total monthly real earnings are calculated by summing earnings across all formal employment relationships active on December 31 and then transformed into logs. The outcome is the mean within each municipality-year-gender cell. Gender gaps are defined as the female outcome minus the corresponding male outcome within each municipality-year. The event window is $[-3, 3]$, with event time $-1$ as the omitted category. The reported ATT is the equally weighted average of the coefficients from event times 0 to 3. All specifications include municipality-by-sub-experiment and year-by-sub-experiment fixed effects. The estimates use corrective stack weights. Vertical bars represent 95% confidence intervals based on standard errors clustered at the municipality level.

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