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# The Effects of a Large-Scale Mental Health Reform: Evidence from Brazil[†]

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*This paper studies the Brazilian Psychiatric Reform, which reorganized the public mental health care provision by introducing mental health centers (CAPS) as a community-based substitute for inpatient care. Our research design exploits the rollout of CAPS in a difference-in-differences framework. We show that these centers increased outpatient mental health care production and reduced psychiatric hospitalizations. These reductions were more pronounced for long-stay admissions and among patients with schizophrenia. We find that the savings implied by fewer admissions do not offset the cost of the policy. Also, the reform did not reduce mental health mortality and it increased violent crimes.* (*JEL* H51, I12, I18, O15)

Mental disorders are a burdensome condition, having affected more than 1 billion people globally and being responsible for 19 percent of all years lived with disability in 2016 (Rehm and Shield 2019). Moreover, people with major depression and schizophrenia have a 40 percent to 60 percent greater chance of dying prematurely than the general population, and suicide is the second-most-common cause of death among young people globally (World Health Organization 2013). Mental health issues can also have severe negative economic consequences, disproportionally affecting the poor (Ridley et al. 2020). Despite such alarming patterns, health systems have not adequately responded to the burden of mental disorders; there is a significant gap between the need for treatment and its provision all over the world (World Health Organization 2022).

Despite broad consensus that access to mental health care is currently inadequate, debate over the best methods to provide such care continues. In the second half of the twentieth century, many countries transitioned from a model of mental health care centered on psychiatric hospitals to one based on community care. However, the

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effects of this change are far from clear. Research shows that nonhospital treatments can be effective for some mental disorders at a low cost (Patel et al. 2007, 2011). However, the international experience reveals that reduced inpatient treatment combined with inadequate community-based care, without proper integration with other medical services, may generate undesirable consequences (Lamb 2015; Sisti, Segal, and Emanuel 2015; Rosenbaum 2016).[1]

We investigate this question by examining Brazil's 2002 psychiatric reform. The Brazilian reform was centered on the introduction of Psychosocial Care Centers (*Centros de Atenção Psicossocial*—henceforth, CAPS) as a community-based substitute for inpatient care for people with mental disorders. More specifically, CAPS provide several psychosocial and outpatient procedures, such as talk therapies, occupational therapies, and medical consultations, and work as gateways to the public mental health care system.

We assess the effects of introducing a CAPS in a municipality on the density of mental health professionals and psychiatric beds, outpatient mental health care production, public spending on mental health, and hospital admissions and deaths by cause. As to the causes of deaths and admissions, we investigate mental and behavior disorders, suicide, illnesses caused by alcohol abuse, and overdose. Finally, we look at the effect on homicides, following the criminology literature that reports a correlation between deinstitutionalization and community-based care with violent crimes (Lamb 2015). Data limitations prevent us from evaluating the policy effects on well-being, which would require us to estimate the impact of the policy on less severe aspects of mental health.

Our empirical strategy exploits the rollout of CAPS across Brazilian municipalities in a difference-in-differences (DID) framework. Following De Chaisemartin and D'Haultfoeuille (2020), we use a DID estimator that is robust to heterogeneous treatment effects across cohorts and over time. Parallel pre-trends for the outcomes we evaluate support the design's internal validity. We first document that the introduction of CAPS increased access to and utilization of community-based mental health care. Specifically, implementing these centers was associated with immediate and significant increases in the density of mental health professionals and mental health outpatient visits. Consistent with these results, we also found an increase in drugs dispensed in outpatient care to treat psychiatric disorders.

Turning to hospitalization outcomes, we find that CAPS decreased hospital admissions due to mental illnesses. This reduction is driven by the reduction of long-stay hospitalizations, largely among individuals with schizophrenia, for whom CAPS may not be well equipped to provide care. As for public spending on mental health, we find a decrease in expenses related to mental health hospitalizations. However, based on the government expenditures on the program, our estimates indicate that the reform resulted in a net financial loss. Additionally, we find no effect of CAPS on deaths related to mental illnesses. Finally, we see a persistent increase in homicide rates following the reform. Although not definitive, these results align

[1] This idea is often connected with the concept of transinstitutionalization: when individuals are released from psychiatric institutions and no adequate alternative is offered, they may migrate to other institutions, usually part of the correctional system.

with the Penrose hypothesis, which states that inpatient care may impact violent crime through an incapacitation effect on patients more prone to violence, as either victims or perpetrators.

This paper contributes to the growing economic literature on mental health. Many authors study determinants of mental health, such as medication (Dalsgaard, Nielsen, and Simonsen 2014; Ludwig, Marcotte, and Norberg 2009), early life conditions (Persson and Rossin-Slater 2016; Almond and Mazumder 2011; Adhvaryu, Fenske, and Nyshadham 2019), economic shocks (Ruhm 2000; Schwandt 2018), and income shocks (Christian, Hensel, and Roth 2019; Baird, De Hoop, and Özler 2013). A recent set of experimental papers has evaluated the effects of psychological interventions on mental health and related outcomes (Baranov et al. 2020; Baranov, Haushofer, and Jang 2020). However, evidence of government policies' impact on mental health is scarce.

One of the few assessments of a government policy specifically aimed at mental health is that of Serena (2021), who examines the causal effects of increased insurance coverage for psychotherapy in Denmark. In contrast, we study the impact of increasing the supply of community-based alternatives to psychiatric institutionalization, a hotly debated policy choice that lacks solid empirical evidence. Although small-scale trials have studied the effectiveness of mental health care delivered in a community setting (Wiley-Exley 2007; Patel et al. 2007), the literature about the broader impacts of mental health policy changes at scale is still limited. This paper takes the first step in this regard. Importantly, we study this type of policy in a developing country, a setting in which the delivery of mental health care tends to be significantly hindered by a lack of resources.

This paper also contributes to the literature linking crime to mental health and services associated with it. Experts in criminology find that population-level crime rates correlate to mental health hospitalizations (Penrose 1939; Markovitz 2006; Raphael and Stoll 2013; Mundt et al. 2015). Similarly, medical research using individual-level data from discharged patients consistently reports a high prevalence of violent incidents involving former inpatients (e.g., Link, Andrews, and Cullen 1992; Walsh et al. 2003; Fleischman et al. 2014). However, most of these papers have focused on cross-sectional comparisons, which might be subject to omitted variable bias due to confounders affecting crime and inpatient care utilization.

The only attempt at a causal interpretation of the relation between mental hospitalization and crime is that of Landerso and Fallensen (2020). However, instead of studying hospital discharges, they analyze admissions at psychiatric hospitals and find that inpatient admittance reduces criminal behavior through incapacitation. Our paper exploits potentially exogenous changes in hospital admissions due to severe mental health conditions induced by the introduction of CAPS to study the causal effects of deinstitutionalization on violent crimes when alternative, community-based treatment becomes available.

The remainder of the paper is organized as follows. Section I summarizes the institutional and conceptual background. Section II introduces the data sources. Section III describes the empirical approach. In Section IV, we present and discuss our main results. Section V covers robustness checks. Section VI concludes.

## I. Background

### A. *Institutional Background*

Since the mid-twentieth century, many countries have shifted mental health care delivery away from psychiatric hospitals and toward community-based care, in what is known as deinstitutionalization. Proponents argue that community-based care is more humane and cost-effective (Rochefort 1984; Lamb and Bachrach 2001). This new paradigm influenced Brazilian psychiatrists, leading to the introduction of a bill to Congress in 1989 proposing the progressive substitution of psychiatric hospitals with community-based resources. The bill eventually became law in April 2001, but it was only made fully effective when the proposed CAPS were opened in 2002. According to the Brazilian Ministry of Health (Ministério da Saúde do Brasil 2015), this policy cost about 1.3 billion 2019 Brazilian Reais (BRL) between 2002 and 2014.[2] The total cost of the policy is comparable to the amount approved for mental health in 2019: 1.6 billion BRL. This amount is less than 1.5 percent of all approved health spending, and significantly less than the recent world average of 2.8 percent of health care spending (Biderman 2019).

The centerpieces of the psychiatric reform law are the CAPS, which are meant to be the primary source of care for the mentally ill. CAPS provide community-based substitute services for psychiatric inpatient care (Ministério da Saúde do Brasil 2005).[3] CAPS are also the main gateway to the public mental health system, triaging and referring patients to other services as needed. At a CAPS, a patient is treated by a multidisciplinary team composed of psychiatrists, psychologists, occupational therapists, and social workers. This team prescribes a set of specific treatments, which can include consultations with a psychologist, medications, occupational therapy, and talk therapy.

To receive a CAPS, a municipality must apply to the federal government. The federal government has no formally established criteria to prioritize which municipalities receive CAPS. A municipality typically applies for a certain type of CAPS according to population size thresholds. There are three standard types: CAPS I (15,000–75,000 inhabitants), II (75,000–150,000 inhabitants), and III (over 150,000 inhabitants). In our data, these population criteria are important, but not strictly binding.

If a municipality already has a CAPS, it can also apply for one of two "special" types of CAPS: CAPS i, which focus on treating children and teenagers, and CAPS AD (I or III, depending on size), specialized in drug and alcohol abuse.[4] When the application is approved,[5] municipalities receive some financial support from the federal government: between 800,000 and 1,000,000 BRL for construction, and between

[2] Approximately US$280 million.

[3] CAPS existed even before the bill was presented. The first CAPS was created in the city of São Paulo in 1987. However, the number of CAPS before 2002 is negligible when compared to the number of centers created after the psychiatric reform law.

[4] Having one of these specialized types nearby does not prevent standard CAPS from treating children and people suffering from drug or alcohol addiction.

[5] In principle, applications can be rejected. However, we had access to the decisions made by the federal government in 2019, and very few proposals were rejected. The main reason for rejection was that the municipality did not meet the minimum population criterion established for the type of CAPS requested.

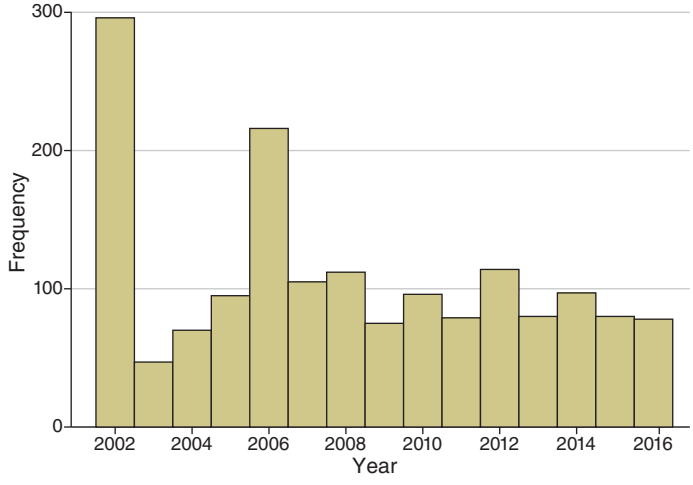


FIGURE 1. NUMBER OF MUNICIPALITIES RECEIVING A CAPS (OF ALL TYPES) BY YEAR

*Notes:* This graph plots the number of municipalities receiving a CAPS for the first time from 2002 to 2016. A number of CAPS were created before the period and were accredited in 2002, explaining the spike in 2002. For the remaining CAPS, the accreditation coincides with opening. These data show the first date of accreditation, or date of CAPS' opening for the vast majority. We ignore closures of CAPS since only 13 were closed during this period.

30,000 and 100,000 BRL per month for maintenance. Figure 1 shows the number of municipalities that received a CAPS each year from 2002 to 2016. Since most municipalities in Brazil are relatively small, it is unsurprising that the vast majority of CAPS are CAPS I. In Figure A1, we show how CAPS spread across the country over time. The rollout does not seem to have followed any clear geographical pattern.

Despite the formal guidelines and requirements, local administrations face many challenges in effectively implementing CAPS. Even with financial support from the federal government, many municipalities discover that more money is needed to maintain their CAPS in an ideal state. A lack of resources is particularly notable in terms of professionals, especially physicians. Doctors are typically hired part-time, and they work in the CAPS just a few times per week. On the one hand, this facilitates the hiring process, since even doctors already working in the private sector can fit the CAPS work into their schedules. On the other hand, limited availability of physicians may prevent patients from receiving proper treatment.

It is also important to distinguish the most common types of CAPS in Brazil from the community mental health centers (CMHC) in the United States. Since CAPS are usually much less well equipped, are not open 24 hours, have no beds, and can lack psychiatrists most of the time, they tend to be ill-

suited to treat patients who need a higher level of care. CAPS III is a type of center that is more similar to the CMHCs, but only a handful exist in Brazil.

Another key piece of information is related to the type of patients treated at CAPS. Online Appendix Table A1 compares patients treated in CAPS to those who report having been diagnosed with a mental illness in a national survey conducted in 2019 (Instituto Brasileiro de Geografia e Estatística 2019). Although the survey only asks about a limited number of conditions, comparing the responses to the data

from CAPS uncovers a clear pattern. Most people diagnosed with mental illnesses in Brazil suffer from depression (76 percent), while only a tiny fraction have schizophrenia (6 percent), a much more severe condition. In contrast, only 12 percent of people treated in CAPS suffer from depression, while schizophrenia is the most common illness treated in the centers (28 percent of the patients).

On the one hand, this profile aligns with policy guidelines that specify that CAPS should provide treatment to individuals with medium- to high-severity mental illnesses. On the other hand, the profile of the typical patient may not be consistent with the resources available in the centers. Because of this mismatch, some specialists and families of severely ill patients in Brazil favor a model that includes psychiatric hospitals (e.g., Filho 2021; G1-BA 2017; Pinheiro 2019; Teixeira and Coelho 2019).

Along similar lines, we must understand how CAPS are related to the other aspect of the psychiatric reform: a reduced number of psychiatric beds. CAPS are explicitly intended to be substitutes for psychiatric hospitals. Thus, it is not unreasonable to assume that acquiring CAPS and closing psychiatric beds are intrinsically and strongly related processes. However, this is not what happens in Brazil. Even before the psychiatric reform law, the idea that mental health care should not be primarily inpatient was already gaining traction. Since the 1980s, Brazil has experienced a significant reduction in the total number of psychiatric beds, under the assumption that primary care is more appropriate than inpatient care. However, policymakers have attempted to ensure that this transition be gradual, and that a sudden loss of psychiatric beds would not disrupt local mental health care systems. They advocate that municipalities should be given guidelines to reduce inpatient psychiatric capacity efficiently, regardless of the presence or absence of CAPS. We investigate the relationship between CAPS and psychiatric beds and report our findings in Section IV.

### B. *Theory of Change*

Here, we begin to investigate how CAPS impact outcomes of interest. We discuss the theory of change and defer to the next section our assessment of measurement and data limitations. Figure 2 summarizes our understanding of the impact of CAPS on our outcomes of interest.

When a CAPS is established, it becomes the primary source of care for individuals with mental illness. The goal is to replace hospitals. The center also increases availability of outpatient services. As a result, we may expect changes in the allocation of mental health care resources in this new model of care.

These process changes should shift the market toward a new equilibrium, with different levels of community- and hospital-based mental health care capacity. In particular, the policy should influence (i) the decision to seek treatment or not, and (ii) conditional on seeking treatment, which type of treatment patients receive. After the reform, we expect that some people would switch from their original choice (seek outpatient care at public clinics, seek inpatient care, or not seek treatment at all) toward outpatient care at CAPS, and that other health units would increase referrals to CAPS. Given the goals of the reform, we focus on the substitution of inpatient with outpatient care.

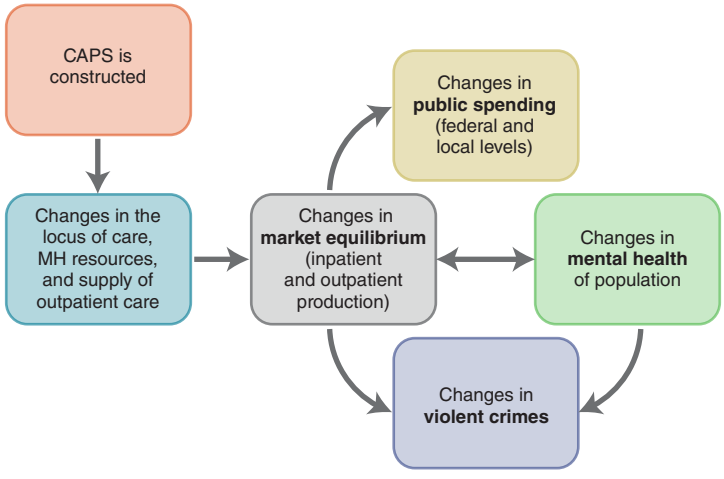


FIGURE 2. THEORY OF CHANGE

*Notes:* This diagram summarizes the changes we expect to happen following the construction of a CAPS in a given municipality. "MH" stands for mental health.

A new equilibrium may affect public spending related to mental health. This is particularly important, since one of the rationales behind the reform is its cost-saving potential, compared to inpatient mental health care delivered through hospitals. It may also change mental health outcomes. The consequences could be positive or negative for patients, depending on how well outpatient care suits each case, compared to inpatient care. If the effect on mental health is large enough, it could affect mortality rates, especially those related to serious illnesses (Cavanagh et al. 2003; Olfson et al. 2015).[6]

Individuals with more serious mental illnesses are prone to violent behavior and victimization (Brekke et al. 2001a; Teplin et al. 2005a; Fazel and Grann 2006; Rueve and Welton 2008). Inpatient treatment may help lower violent crime through incapacitation. The overall effects of the reform depend on its impact on people's mental health, and whether CAPS meet the treatment needs of individuals with severe mental illnesses. Analyzing the rate of violent crimes is particularly pertinent given the accounts of violence related to CAPS patients reported by the media (e.g., Sousa 2012; G1-Sorocaba/Jundiaí 2016; Vilela 2018; Vaccari 2019).

## II. Data

Most of the data we use come from the Brazilian Ministry of Health. We combine records of deaths, hospitalizations, outpatient procedures, and health facilities from all over the country. Since the policy is implemented at the geographical level of municipalities, we use them as our unit of analysis.[7]

[6] Due to data limitations, we are unable to assess how finer, less extreme mental health measures are affected.

[7] Since some cities were founded during the period of study, we aggregate them into minimum comparable areas using data from the Institute of Applied Economic Research (IPEA) (Instituto de Pesquisas Ecônmicas e Aplicadas

The Brazilian Ministry of Health provides the data regarding the opening of each CAPS (Ministério da Saúde do Brasil 2019).[8] The data cover the period between 2002 and 2019. Included are the opening date and type of every CAPS created after the psychiatric reform law was sanctioned. For the few CAPS that predate this period, the data indicate 2002 as the year of opening. Hence, we only exploit variation from 2003 onwards. Most municipalities with a center have a CAPS I (80 percent) and adopted only one center during this period.[9]

To assess the effects of CAPS on the supply of outpatient services, we use administrative data from the National Registry of Health Establishments (*Cadastro Nacional de Estabelecimentos de Saúde*—CNES) (DATASUS 1979–2024a). This registry includes detailed data on all public and private health facilities in Brazil. It has information about health professionals linked to health care facilities, their area of practice, and levels of specialization. For each municipality, we select the number of mental health providers that usually constitute community mental health teams: psychiatrists, psychologists, occupational therapists, and social workers. These providers represent, on average, 87 percent of all the professionals working at the psychosocial care centers. The data also provide information on the number of hospital beds in each municipality, and we select those used for psychiatric patients according to the registries.

To study the effects of CAPS on the equilibrium of the mental health care market, we use two datasets: one for outpatient care and the other for inpatient care. The first is the National System of Information on Ambulatory Care (*Sistema de Informações Ambulatoriais de Saúde*—SIA) (DATASUS 1979–2024d). SIA provides administrative information on all visits funded by the public sector in which medical care is provided on an outpatient basis. Visits may take place in any facility that provides primary health services. Severe compatibility issues limit the usefulness of this dataset. Its data are at the procedure level, and many procedure codes change over time, making comparisons impossible. With few exceptions, therefore, we avoid evaluating specific outpatient procedures. From 2008 onward, we can identify the type of health professional providing outpatient care. We then select the overall number of outpatient services provided by each type of mental health provider we are evaluating. SIA also contains information on "outpatient pharmaceutical assistance" regarding medications provided to patients. We focus on antipsychotic medications distributed in each municipality.

Regarding inpatient care, the Hospital Information System of the Unified Health System (*Sistema de Informações Hospitalares*—SIH) provides information about hospital admissions that use beds in the public health care sector (DATASUS 1979–2024e). This dataset includes information on admissions by patient's municipality of residence and by cause of admission, coded using the ICD-10. We rely on the ICD classification to identify causes of hospitalization related to mental illnesses

1995–2004). For the sake of clarity, we refer to minimum comparable areas as municipalities.

[8] These data were requested directly to the Ministry of Health through the Information Access Law. These data are not formally published anywhere as far as we know, but are public nonetheless and are provided with the rest of our data.

[9] For more details on the last two points, see online Appendix Figures A2 and A3, respectively.

and group them. Online Appendix Table A3 summarizes the relationship between groups and ICD-10 codes.

We also use several data sources to assess the effect of CAPS on public spending. One limitation here is that spending is divided between federal and local governments. The spending data at these levels are aggregated in different ways, which can prevent accurate comparisons across them. With this caveat in mind, we use data on federal spending on hospital admissions from the Ministry of Health's SIH (DATASUS 1979–2024e) and on municipal health spending from Brazil's National Treasury (Instituto de Pesquisas Econômica Aplicada 2023b).

For mortality outcomes, we use data from the Mortality Information System (*Sistema de Informação sobre Mortalidade*—SIM) (DATASUS 1979–2024c). SIM records all deaths in the country, with their causes (coded using the ICD-10). We focus on deaths related to alcohol, overdose, suicide, and the larger category of deaths of despair, which is the aggregation of the other three (Case and Deaton 2015, 2017). In addition, we gather data on homicides, which can be a proxy for violent crimes more generally (Dix-Carneiro, Soares, and Ulyssea 2018). We also investigate overall mortality and infant mortality as placebo outcomes. To measure infant mortality, we collect data on the number of children born alive at each municipality from the Ministry of Health's registers (DATASUS 1979–2024b).

To control for differential trends in important determinants of mental health in our empirical strategy, we use several sources of data on municipality characteristics. The Brazilian Statistical Office (*Instituto Brasileiro de Geografia e Estatística*—IBGE) estimates each municipality's GDP, from 2002 to 2016 (Instituto Brasileiro de Geografia e Estatística 2002–2021). The Ministry of Social Development (MDS/SAGI) provides data on the main conditional cash transfer policy in Brazil, the *Bolsa Família* program (PBF) (Instituto de Pesquisas Econômica Aplicada 2023a). The Brazilian Ministry of Health records data on the age and gender composition of people in each municipality (DATASUS 1980–2021). The IPEA provides a set of geographical variables from the municipalities in the baseline (Instituto de Pesquisas Econômicas e Aplicadas 1995–2024). We also gather information on the number of mental health providers and offices in the baseline from the IBGE's 2002 Health and Medical Care Survey (*Pesquisa de Assistência Médico-Sanitária*—AMS) (Instituto Brasileiro de Geografia e Estatística 2002). Finally, we use the results of the 2000 Brazilian Census for data on socioeconomic variables (Instituto Brasileiro de Geografia e Estatística 2000).

Merging all the data described above, we end up with a sample of balanced yearly data for 5,180 municipalities covering all years between 2002 and 2016. Online Appendix Table A2 provides summary statistics.

## III. Study Design and Estimation Strategy

We exploit the staggered implementation of CAPS starting after 2002 within a DID framework to analyze the effects of this intervention on outcomes related to public mental health. In such a setting, traditional two-way fixed effects estimators are biased if treatment effects are heterogeneous over time or across cohorts (Goodman-Bacon 2021; Sun and Abraham 2021). To circumvent this problem, we

follow De Chaisemartin and D'Haultfoeuille (2020) and estimate well-defined and relevant causal parameters, robust to treatment effect heterogeneity.[10]

In summary, for any period $t$ when a group of municipalities receives CAPS, we can use standard $2 \times 2$ DID to get an estimate for that group. For a fixed event-time $k \geq 0$, this is the result of comparing across periods $t-1$ and $t+k$ units that switched their status from untreated to treated at $t$ with those remaining untreated until $t+k$. We then aggregate these separate DIDs based on the size of each treated cohort to estimate the average effects for all groups of treated municipalities, $k$ periods away from the year they received CAPS. Online Appendix Section B formalizes this strategy. Regarding inference, we compute standard errors using a municipality-level clustered bootstrap.

Our research design uses groups whose treatment is stable, to infer the trends that would have affected switchers if they had not gained a CAPS. The only way that this design would be invalid would be if trends in time-varying determinants of outcomes across switchers and nonswitchers diverged for some reason other than the CAPS. To deal with such concerns, our main specification includes state-specific nonparametric trends. These seem particularly relevant in Brazil since many public policies—such as those related to education and public security—are largely determined at the state level.

As we show next, disadvantaged areas were less likely to construct a CAPS than areas with more resources. Although these differences tend to be small in magnitude, a risk exists that differential trends (driven by these baseline differences) spuriously correlate with the treatment effect. This could occur if disadvantaged and advantaged areas had already started on different paths, in terms of the outcomes of interest, before the psychiatric reform. To account for the possibility of this threat to internal validity, we control for interactions of a wide range of pretreatment municipality characteristics with a linear time trend: Theil index, poverty rate, illiteracy rate, the share of population living in rural areas, population, social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to the capital, temperature, and rainfall (Carrillo and Feres 2019).

We also control for differential trends according to temporal variation in potential confounders. During the 2000s, Brazil experienced high economic growth and demographic changes. Therefore, we control for local economic conditions measured by GDP per capita and the age-by-gender composition of municipal population (the share of inhabitants within each nine-year-by-gender bracket, from 10–19 up to 79 years). We also consider more flexible trends according to annual per capita PBF expenditure, as the Brazilian cash transfer program started expanding across municipalities in 2003. Reassuringly, point estimates are stable to different specifications, suggesting that our results are unlikely to be driven by differential trends across switchers and nonswitchers. Section V discusses whether other policy changes could confound our results.

To further assess the plausibility of our identification assumption, we follow Galiani, Gertler, and Schargrodsky (2005) and Rocha and Soares (2010) and estimate a hazard

[10] In such a staggered design, the estimators we use are very similar to those proposed by Sun and Abraham (2021) and Callaway and Sant'Anna (2021) (for this particular case, in a specification without covariates).

model of the probability that a municipality gains a CAPS.[11] Results are reported in online Appendix Table A4, with marginal effects calculated on independent variables averages. Overall, we find that some baseline characteristics are correlated with the probability of receiving a CAPS. Still, the effects are quantitatively small: the effects of one standard deviation are smaller than 2 percentage points in all cases. Furthermore, and fundamentally for our identification assumption, we see no correlation between receiving a CAPS and the pretreatment dynamic of mental health, crime, and economic indicators. These results support the validity of our empirical strategy.

We can also estimate treatment effects in pre-CAPS periods to test the plausibility of the underlying parallel trends assumption defining our DID design. Following De Chaisemartin and D'Haultfoeuille (2020) and Callaway and Sant'Anna (2021), we use a placebo estimator that replaces the "long comparisons" across $t-1$ and $t+k$ (between groups that switched from untreated to treated at $t$ and those remaining untreated until $t+k$) with "short comparisons" across $t'-1$ and $t'$ for all $t' < t$. Different from TWFE regressions, we do not have a universal baseline period. For each placebo treatment period $t'$, we compare it to the immediately preceding period. In this case, our pretreatment parameters are pseudo-ATTs: they are the treatment effects we would have estimated had the treatment taken place at the placebo date.[12] Finding coefficients statistically different from zero would indicate that the parallel trends assumption is violated. The placebo effects are generally statistically indistinguishable from zero for our primary outcomes, again bolstering our design validity. In the few cases with significant pretreatment effects, these are close to zero and have much lower magnitude than the post-treatment effects, making it unlikely that the existing slight variation across groups explains the treatment effects.

## IV. Main Results

We present our main results in graphical form, plotting together in one figure dynamic effects, placebo effects, and their 95 percent confidence intervals. In the text, we report the standard errors in parentheses. We group our results into five sets, each connected to one element of the diagram in Figure 2.

### A. *Mental Health Care Supply*

The effects of Brazil's psychiatric reform on the supply of mental health practitioners are presented in Figure 3 and Table 1. Our estimates indicate a remarkable

[11] We perform this estimation by modifying our dataset, so each municipality leaves the sample after getting a CAPS. Then, we estimate logit models controlling for a flexible polynomial of time, where the dependent variable is a dummy indicating that a municipality gained a CAPS and the independent variables are socioeconomic variables. We estimate three models considering the first, second, and third lags of the changes in our variables of interest. We additionally include some baseline characteristics as independent variables.

[12] In this case, all our event-study estimates have a similar interpretation. In TWFE regressions, one has to normalize relative to a universal baseline period (generally the period immediately before the treatment starts); otherwise, parameters are not identified due to perfect multicollinearity. Under this strategy, event study estimates in pretreatment periods are not treatment effect parameters, so they differ to our pretreatment estimates in terms of interpretation. Anyway, either case is just a linear combination of the other, so both are valid strategies to pretest the parallel trends assumption.

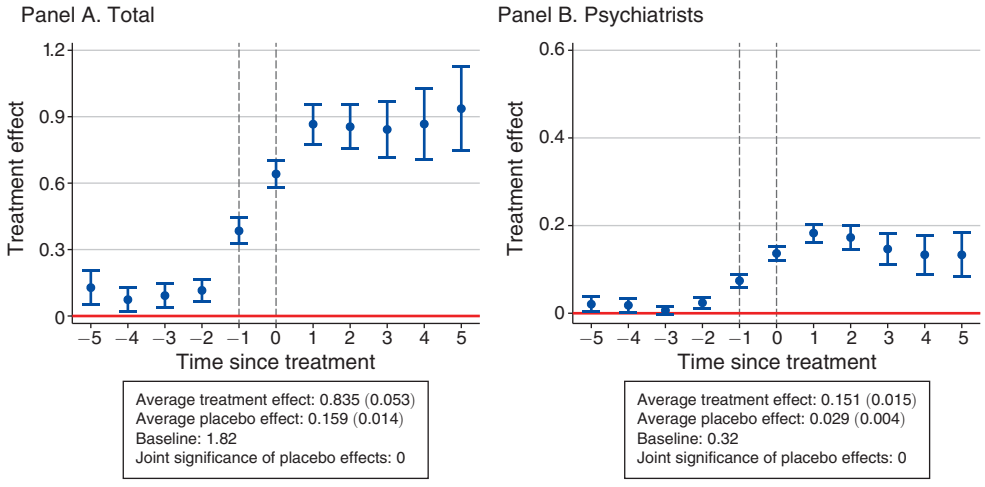


FIGURE 3. EFFECTS OF CAPS ON MENTAL HEALTH PRACTITIONERS, PRACTITIONERS PER 10,000 PEOPLE

(*continued*)

increase in the supply of professionals: the average treatment effect indicates an increase of 0.84 professionals per 10,000 people (46 percent over the average of the year before the event,[13] s.e., 0.05). The increase precedes the CAPS' introduction, and reaches its peak after the policy took effect. For psychiatrists, the treatment effects indicate a statically significant increase of 0.18 professionals per 10,000 people (60 percent, s.e., 0.01) one year after municipalities receive a CAPS. Treatment effects decay monotonically after that, reaching 0.13 (s.e., 0.02) points five years after treatment.

According to the law regulating CAPS implementation, if smaller municipalities fail to hire psychiatrists, they can hire general practitioners and family doctors. We estimate the policy effects on the supply of these professionals in Table 1 and online Appendix Figure A4. The point estimates are generally positive, albeit imprecise, and have a much lower magnitude than those for psychiatrists. We also look at the effects on the overall supply of physicians, which is positive and increases over time. It can be accounted for mainly by the increased supply of psychiatrists, suggesting no substitution across specialties.

In the same figure and table, we also show estimates for the effect of CAPS on other mental health providers that usually constitute community mental health teams: psychologists, occupational therapists, and social workers. Overall, the pattern is similar to the one for psychiatrists, and CAPS' effects on the supply of these professionals are significant and high in magnitude. As before, there are anticipation effects, most marked in the year before the establishment of CAPS. Treatment effects then rise after the beginning of the intervention. The number of psychologists per 10,000 inhabitants increased by 0.22 (27 percent, s.e., 0.02) in

[13] Henceforth, percentages indicate the size of the effect in percentage terms compared with the average of each outcome in the year before the event, unless otherwise noted.

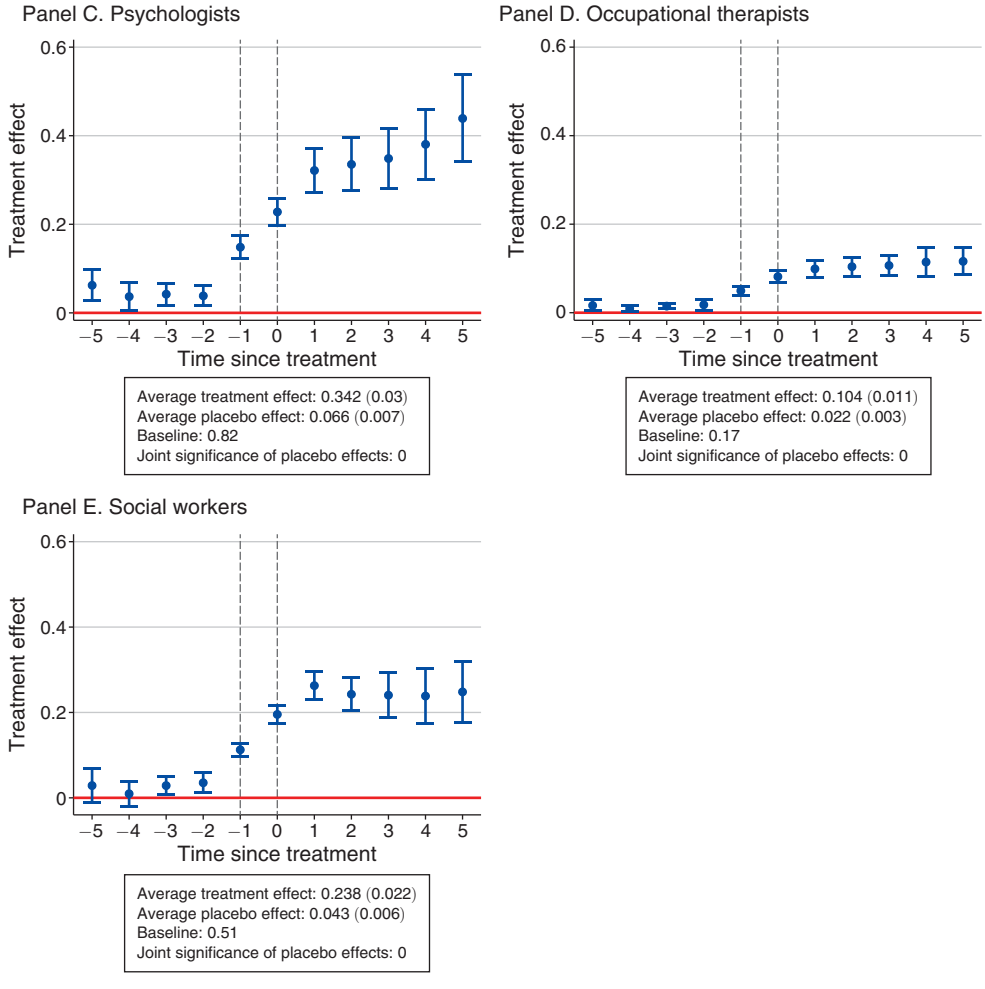


FIGURE 3. EFFECTS OF CAPS ON MENTAL HEALTH PRACTITIONERS, PRACTITIONERS PER 10,000 PEOPLE (*continued*)

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS' effects on the number of mental health practitioners per 10,000 people. We present the effects on the overall number of mental health practitioners—the sum of the next categories (panel A), psychiatrists (panel B), psychologists (panel C), occupational therapists (panel D), and social workers (panel E). Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the first pre-CAPS period.

the first year after CAPS' adoption. Treatment effects then rise until the fifth year, reaching 0.43 professionals per 10,000 people (53 percent, s.e., 0.05). One year after the program, the effects of CAPS on the rates of occupational therapists and

Table 1—Effects of CAPS on the Supply of Mental Health Practitioners: Number of Professionals per 10,000 People

| | 2-year effect (1) | 5-year effect (2) | Placebo effect (3) | Mean at baseline (4) |
|---|---|---|---|---|
| *Mental health practitioners* | | | | |
| Total | 0.787 | 0.834 | 0.159 | 1.82 |
| | (0.051) | (0.053) | (0.014) | |
| Psychiatrists | 0.164 | 0.151 | 0.029 | 0.316 |
| | (0.014) | (0.015) | (0.004) | |
| Psychologists | 0.295 | 0.342 | 0.066 | 0.819 |
| | (0.030) | (0.030) | (0.007) | |
| Therapists | 0.095 | 0.104 | 0.022 | 0.173 |
| | (0.011) | (0.011) | (0.003) | |
| Social workers | 0.234 | 0.238 | 0.043 | 0.512 |
| | (0.021) | (0.022) | (0.006) | |
| *Other types of health care practitioners* | | | | |
| Family doctors | 0.030 | 0.044 | 0.016 | 1.332 |
| | (0.037) | (0.036) | (0.010) | |
| General practitioners | 0.003 | 0.057 | 0.035 | 2.660 |
| | (0.068) | (0.071) | (0.021) | |
| Overall supply of physicians | 0.152 | 0.286 | 0.097 | 5.620 |
| | (0.087) | (0.096) | (0.032) | |

*Notes:* This table reports the average effects of CAPS on the number of mental health practitioners working in the public system per 10,000 people. Standard errors in parentheses are computed using a municipality-level clustered bootstrap. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. Columns 1 and 2 report the average effect after two years and after five years, respectively. Column 3 reports the placebo effect. Column 4 shows the mean of each variable at baseline—i.e., one period before the event.

social workers are 0.1 (60 percent, s.e., 0.01) and 0.26 (50 percent, s.e., 0.02), respectively. Unlike what we see for psychologists, these effects remain relatively stable in subsequent years.

Two important observations relate to the effects we see on the number of mental health professionals. First, we estimate an increase in the year before CAPS open. This increase happens because municipalities need to formally hire professionals well before the actual opening, since the bureaucratic process for hiring professionals is somewhat lengthy.[14] Later, we show how the results on demand for mental care help to strengthen this point. The second observation relates to the size of the increase, which might seem suspiciously large at first. However, as mentioned in the background section, psychiatrists only work part-time at CAPS. Hence, it is easy for professionals working in the private sector to also work at CAPS and accommodate the center working hours in their schedules.

[14] We have some anecdotal evidence from private conversations with municipality health officials indicating that this timing mismatch between the hiring process and the CAPS actual opening frequently happens.

The same observations made for psychiatrists are valid for other professionals. The only difference is that professionals who have to work more time at CAPS are easier to train, and the yearly supply of new graduates is significantly larger than that of psychiatrists.[15]

In online Appendix Table A5, we show the policy effects on overall hours worked by each type of professional. The results indicate that the policy had a positive effect on hours worked and are in line with our results on the number of professionals. Our results indicate a *de facto* expansion in the supply of mental health care, instead of just a substitution from other parts of the health care system. This increase is significantly smaller for psychiatrists, which is consistent with our prior discussion that psychiatrists work only part-time at CAPS.

Finally, since CAPS are viewed as a substitute for psychiatric hospitals, we look at the effects of CAPS on the number of psychiatric beds in online Appendix Figure A7. We find no pre-trends nor any statistically significant impact, although the imprecisely estimated effects starting three years after the program suggest a slight decrease. In any case, these effects do not appear immediately after the program like most of our results, especially those regarding mental health inpatient care (to be shown next). Therefore, if anything, the decrease in the supply of psychiatric beds is a consequence of introducing CAPS, instead of being part of the treatment itself.

Overall, our results indicate that the reform represents a large increase in the local supply of mental health providers, without a significant decrease in the supply of inpatient services. These results may be particularly important for small municipalities with a limited supply of such professionals.

### B. *Effects on Equilibrium Outcomes*

*Outpatient Care*.—Figure 4 and Table 2 present the effects of CAPS on the availability of outpatient, community-based care. We find that after a CAPS opens, the number of outpatient procedures performed by mental health professionals increases remarkably: on average, they increase by 264.2 procedures per 10,000 people per year (or 113 percent of the baseline mean, s.e., 23.6). Treatment effects for the first year are 132 (s.e., 13.7) for psychiatrists, 83 (s.e., 9.6) for psychologists, 18 (s.e., 4.4) for occupational therapists, and 34 (s.e., 4.6) for social workers. Relative to the average within the treated in the pre-CAPS period, these effects are 197 percent for psychiatrists, 75 percent for psychologists, 74 percent for occupational therapists, and 94 percent for social workers. The effect on the number of outpatient procedures performed by psychiatrists decreases in the subsequent years, while it increases over time for the procedures performed by other professionals.

As previously mentioned, outpatient procedure codes change over time. Therefore, we avoid evaluating specific procedures related to mental health. There is one exception, though: occupational therapies, which involve activities like craft, music, and

[15] If we look at estimates for the number of these professionals between 2002 and 2015 using the National Household Sample Survey—with the caveat that some categories of occupations are somewhat coarser than what would be ideal—all mental health providers had their numbers increased by at least 88,000 during this period, while the number of professionals needed between 2002 and 2016 based on our estimates is around 5,000.

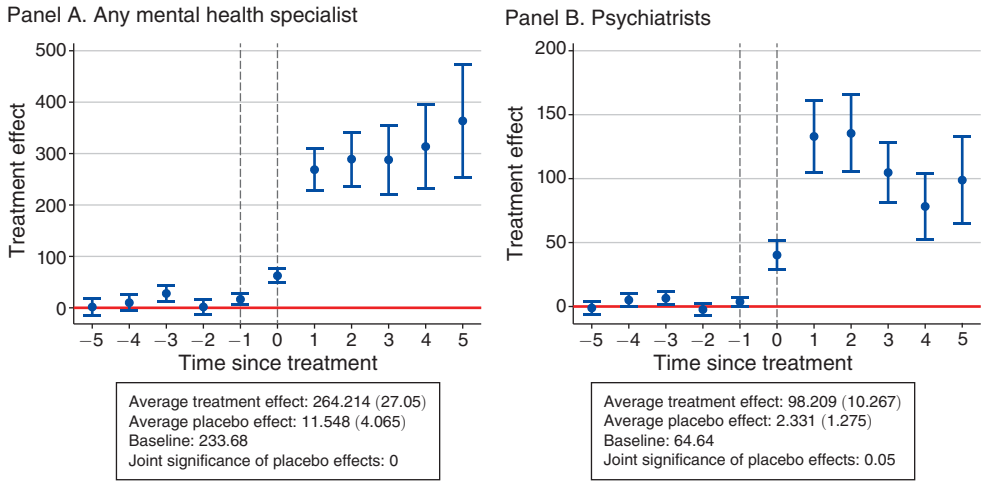


Figure 4. Effects of CAPS on Outpatient Care by Mental Health Specialists: Number of Outpatient Procedures Performed by Each Type of Specialist per 10,000 People

(*continued*)

dance for therapeutic purposes. In principle, this kind of procedure can be delivered at any primary-care health facility. Table 2 shows that therapeutic workshops increase by approximately 0.7 per 10,000 inhabitants after a CAPS is introduced, or a 60 percent increase, compared to the average in the pre-CAPS period.[16]

We also look at the effect of CAPS on the distribution of antipsychotic drugs. These are used mainly to treat schizophrenia. We find that the number of antipsychotic medications distributed (per 10,000 people) increases steadily in treated areas after the policy reform, compared to control municipalities (see Table 2 and online Appendix Figure A6). Our estimates are noisy and statistically insignificant, but their sizes are comparable to the pretreatment mean. In particular, these procedures increase from 3.7 (s.e., 6.8) two years after CAPS open, to 16.7 (s.e., 12.4) five years after (or 48 percent and 217 percent relative to the baseline mean, respectively). As medical therapy is one of the most common treatments within mental health outpatient care, this is another piece of evidence consistent with CAPS increasing utilization of outpatient mental health care.[17]

*Inpatient Care*.—Next, we investigate the effects of CAPS on mental health hospital admissions (per 10,000 people). Figure 5 and Table 3 show that the opening of a CAPS leads to a sharp reduction in mental health hospitalization rates: the average

[16] The event-study is shown in online Appendix Figure A6.

[17] We also estimate the relationship between the policy reform and psychosocial care procedures (per 10,000 people), a measure comprising all the outpatient services delivered at CAPS. Although this association should be mechanical, it provides evidence about the timing and compliance with the policy. Online Appendix Figure A5 shows that psychosocial care procedures jump right after the introduction of CAPS and then rise steadily, suggesting wide use of these centers by the local population as soon as they are implemented. As already discussed in the background section, most of the services delivered at CAPS target patients with schizophrenia (panel B).

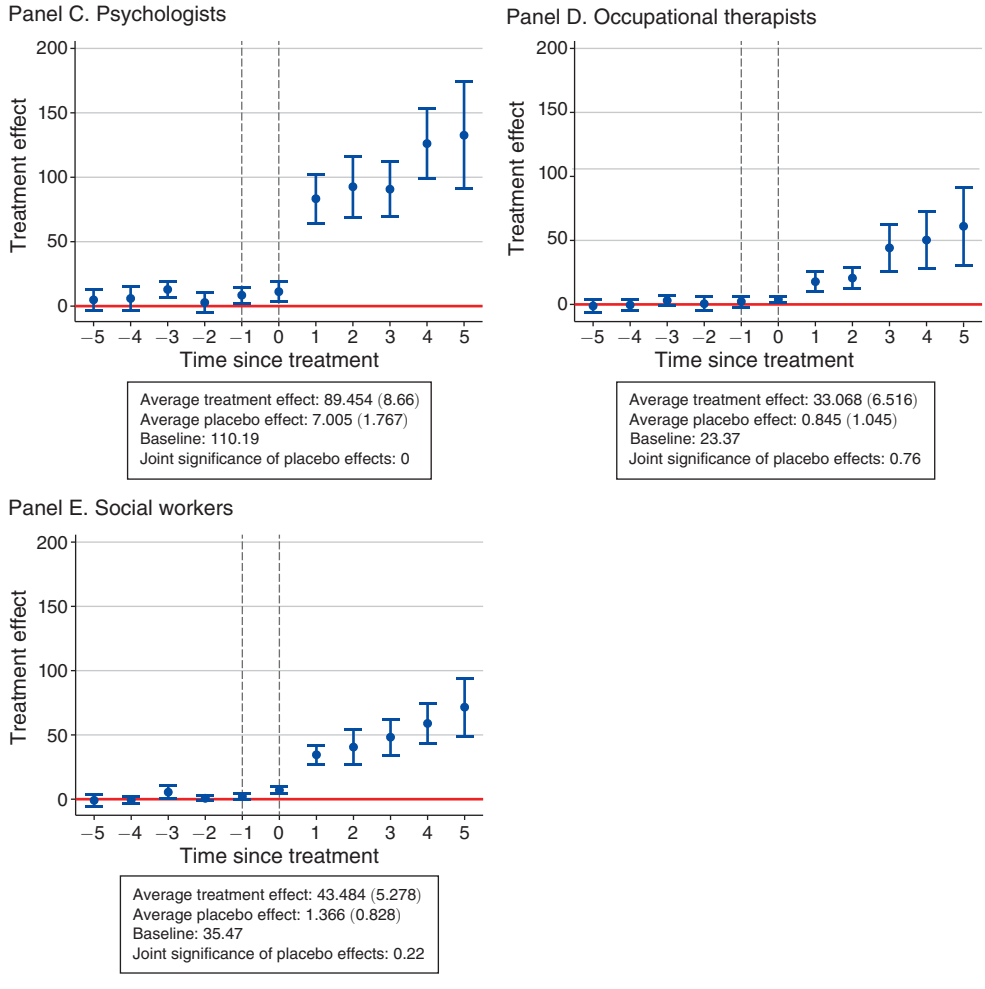


FIGURE 4. EFFECTS OF CAPS ON OUTPATIENT CARE BY MENTAL HEALTH SPECIALISTS: NUMBER OF OUTPATIENT PROCEDURES PERFORMED BY EACH TYPE OF SPECIALIST PER 10,000 PEOPLE (*continued*)

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS' effects on the number of outpatient procedures per 10,000 people performed by mental health practitioners. We present the effects on procedures performed by any mental health professionals—the sum of the next categories (panel A), psychiatrists (panel B), psychologists (panel C), occupational therapists (panel D), and social workers (panel E). Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the first pre-CAPS period.

effect is −0.948 (s.e., 0.28) or 7.5 percent relative to baseline. Our results further indicate that the policy reform is mainly associated with reductions in long-stay

Table 2—Effects of CAPS on Outpatient Care by Mental Health Specialists and for Selected Procedures: Number of Outpatient Procedures per 10,000 People

| | 2-year effect (1) | 5-year effect (2) | Placebo effect (3) | Mean at baseline (4) |
|---|---|---|---|---|
| *Outpatient services by MH practitioners* | | | | |
| Total | 206.754 | 264.213 | 11.548 | 233.683 |
| | (24.357) | (27.050) | (4.05) | |
| Psychiatrists | 102.864 | 98.209 | 2.331 | 64.644 |
| | (12.801) | (10.267) | (1.275) | |
| Psychologists | 62.409 | 89.454 | 7.005 | 110.192 |
| | (8.647) | (8.660) | (1.767) | |
| Therapists | 14.089 | 33.068 | 0.845 | 23.375 |
| | (4.889) | (6.516) | (1.045) | |
| Social workers | 27.392 | 43.484 | 1.366 | 35.473 |
| | (5.283) | (5.278) | (0.828) | |
| *Specific outpatient services* | | | | |
| Antipsychotic drugs | 3.672 | 16.693 | −0.112 | 7.691 |
| | (6.795) | (12.396) | (0.613) | |
| Occupational therapy | 0.579 | 0.794 | −0.001 | 0.717 |
| | (0.191) | (0.204) | (0.028) | |

*Notes:* This table reports the average effects of CAPS on the number of outpatient procedures performed in the public system per 10,000 people. The first panel reports results for each type of mental health practitioner, and the second shows results for two selected procedures that remain comparable over time in the data. Standard errors in parentheses are computed using a municipality-level clustered bootstrap. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. Columns 1 and 2 report the average effect after two years and after five years, respectively. Column 3 reports the placebo effect. Column 4 shows the mean of each variable at baseline—i.e., one period before the event.

hospitalizations ($>$ 30 days). Based on average effects, long-stay hospitalizations account for 65.5 percent of the effect. However, the impact takes more time to reach stability. It goes up to 80 percent if we consider only the effect after five years. Turning to specific causes, we see that our results are driven by the reduction of admissions among people with schizophrenia (86.6 percent), which is the only diagnosis for which we find significant effects.

Our findings indicate that CAPS indeed drive a change in the equilibrium of mental health care delivery from inpatient to outpatient care. However, combined with our previous evidence on the profile of people treated at CAPS, our results indicate that most patients diverted from inpatient care are those with schizophrenia no longer staying long-term in hospitals. As already discussed, CAPS are usually ill-equipped to deal with this type of patient.

### C. *Public Spending*

Next, we turn to the impact of CAPS on public spending (Table 4 and Figure 6). In line with our previous results, we find strong and persistent effects on federal

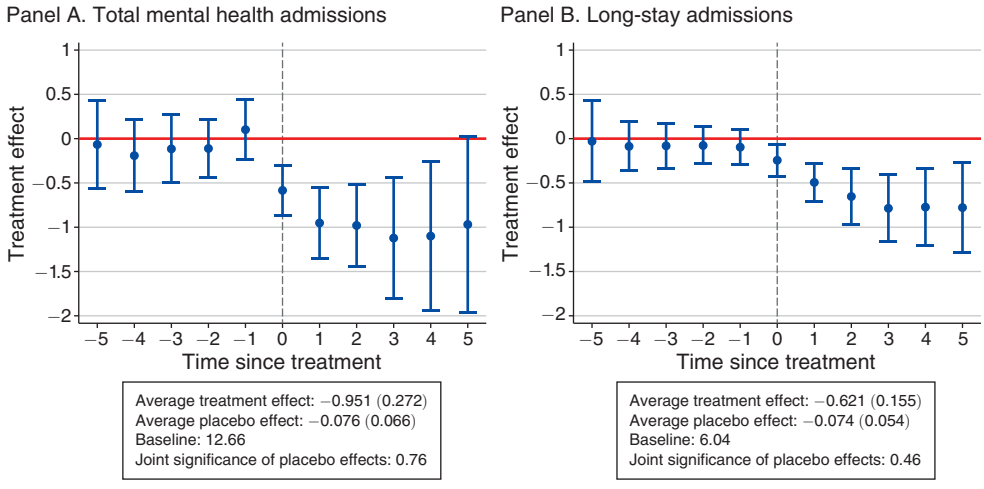


FIGURE 5. EFFECTS OF CAPS ON MENTAL HEALTH HOSPITALIZATION: ADMISSIONS PER 10,000 PEOPLE

(*continued*)

expenditures with mental health hospital admissions: an average reduction of 15.7 (s.e., 0.04) percentage points. We see no reduction in federal spending on other hospitalizations, suggesting that the reform does not save money elsewhere in the inpatient health care system.[18] Moreover, we find no detectable effects on local health expenditures. These results align with our expectations: most money spent on psychiatric admissions comes from the federal government; municipal governments receive subsidies along with CAPS. Therefore, CAPS does not push local governments to spend more.

With these estimates and the data on total expenses with CAPS, we can assess whether the net financial result is positive or negative. Based on aggregate information provided by the government (Ministério da Saúde do Brasil 2015), the policy's total cost from 2002 to 2014 is 950 million BRL. During the same period, we have 11,545 municipality-years with CAPS. Using the average effect estimated for federal spending in mental health hospitalizations, we have a total cost reduction of 720 million BRL. Hence, the net result is a cost of 230 million BRL. Therefore, from a purely financial point of view and in our particular context, more money is spent with CAPS, contrary to what proponents of the psychiatric reform still argue.

Our results align with descriptive evidence that federal mental health spending increased significantly after the Brazilian psychiatric reform, despite a notable shift in composition favoring community-based instead of inpatient care (Desinstitute and Weber 2021). However, it is important to stress that the financial aspect is only part of the motivation for reform. Data limitations preclude an evaluation of the total

[18] In unreported results, we also see no effects on diabetes and heart attacks, diseases with higher prevalence on the mentally ill population (Ridley et al. 2020), nor on hospitalizations for other causes. These results are in line with our null result on total spending on overall hospitalizations.

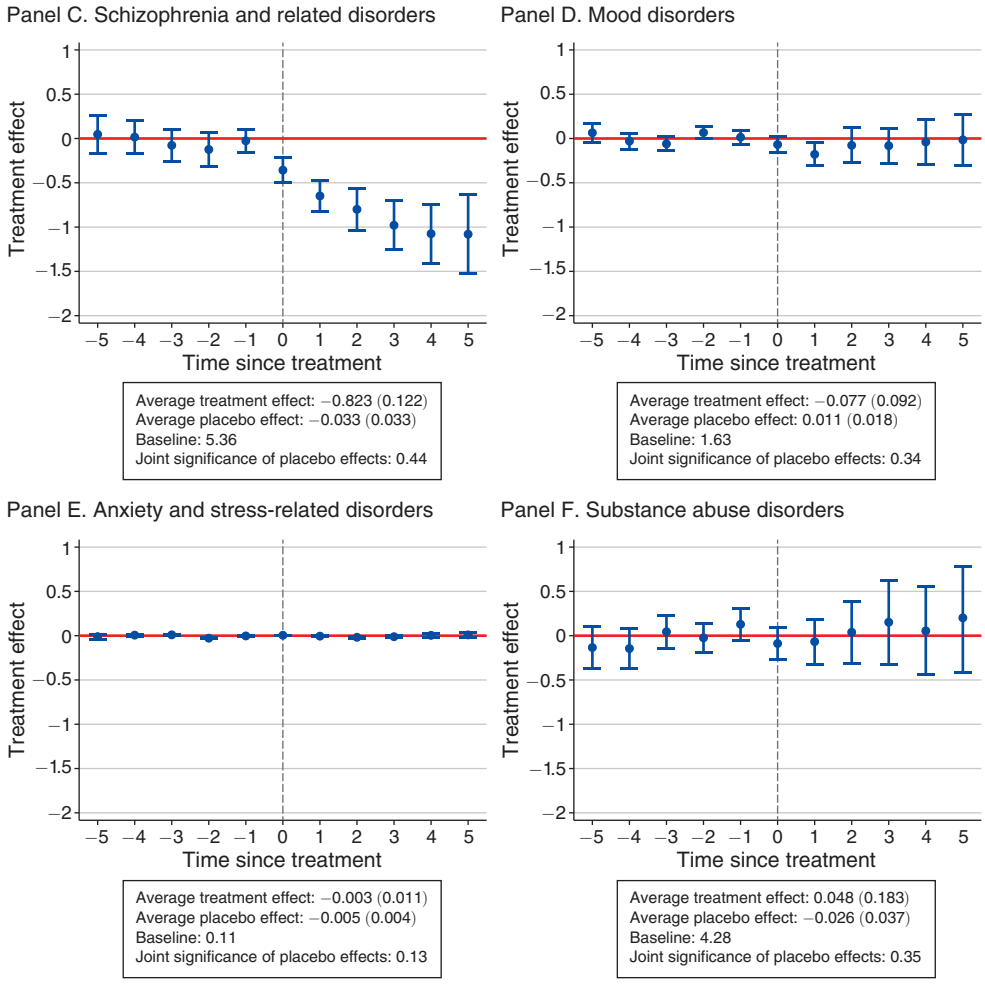


FIGURE 5. EFFECTS OF CAPS ON MENTAL HEALTH HOSPITALIZATION: ADMISSIONS PER 10,000 PEOPLE (*continued*)

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS' effects on the number of hospital admissions per 10,000 people due to mental and behavioral disorders (ICD-10 F00-F99)—panel A, long-stay hospital admissions due to mental and behavioral disorders—panel B, and hospital admissions by groups of cause: schizophrenia and related disorders (F20-F29)—panel C, mood disorders (F30-F39)—panel D, anxiety and stress-related disorders (F40-F48)—panel E, and substance abuse disorders (F10-F19)—panel F. Long-stay hospitalizations are defined as those in which the patient is hospitalized for more than 30 days. Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for those treated in the first pre-CAPS period.

TABLE 3—EFFECTS OF CAPS ON MENTAL HEALTH HOSPITALIZATION: ADMISSIONS PER 10,000 PEOPLE

| | 2-year effect (1) | 5-year effect (2) | Placebo effect (3) | Mean at baseline (4) |
|---|---|---|---|---|
| *Mental health admissions* | | | | |
| Overall | −0.839 | −0.951 | −0.076 | 12.662 |
| | (0.249) | (0.272) | (0.066) | |
| Long-stay | −0.464 | −0.621 | −0.074 | 6.035 |
| | (0.151) | (0.155) | (0.054) | |
| *Admissions by cause* | | | | |
| Schizophrenia | −0.602 | −0.823 | −0.033 | 5.359 |
| | (0.113) | (0.122) | (0.033) | |
| Psychoactive substance abuse disorders | −0.039 | 0.048 | −0.026 | 4.280 |
| | (0.179) | (0.183) | (0.037) | |
| Mood disorders | −0.108 | −0.077 | 0.011 | 1.632 |
| | (0.091) | (0.092) | (0.018) | |
| Anxiety and stress-related disorders | −0.006 | −0.003 | −0.005 | 0.111 |
| | (0.011) | (0.011) | (0.004) | |

*Notes:* This table reports the average effects of CAPS on the number of mental health hospital admissions related to mental health per 10,000 people. The first panel reports results on overall mental health admissions (ICD-10 F00-F99) and long-stay hospital admissions due to mental and behavioral disorders ($>$ 30 days). The second panel shows results by cause: schizophrenia and related disorders (F20-F29), mood disorders (F30-F39), anxiety and stress-related disorders (F40-F48), and substance abuse disorders (F10-F19). Standard errors in parentheses are computed using a municipality-level clustered bootstrap. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state $\times$ year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. Columns 1 and 2 report the average effect after two years and after five years, respectively. Column 3 reports the placebo effect. Column 4 shows the mean of each variable at baseline—i.e., one period before the event.

effects on well-being. To evaluate this impact, we would need a way to estimate the impact of the policy on less severe aspects of mental health, the benefits of more humane treatment compared to treatment delivered at hospital inpatient settings, and the potential externalities of deinstitutionalization to the community and patients' families.

### D. *Deaths of Despair*

We now turn to our main measures of mental health, mortality rates (per 10,000 people). In particular, we look at deaths related to alcohol abuse, overdose, suicide, and deaths of despair. Figure 7 and Table 5 present our results. Overall, we find no statistically significant effect on mortality rates. Based on average effects, we have negative and insignificant estimates for alcohol-related deaths and deaths of despair in general. Based on the estimated standard errors, we would be able to detect effects at least as large in absolute value as 0.051 and 0.066 deaths per 10,000 people for alcohol-related deaths and deaths of despair, respectively. These correspond to 3.8 percent and 3.2 percent of the baseline values of these outcomes, respectively. As for suicides and overdoses, the estimates are small and positive.

Table 4—Effects of CAPS on Public Health Spending

| | log(*Mental health hospital spending*) (1) | log(*Overall hospital spending*) (2) | log(*Municipality health spending*) (3) |
|---|---|---|---|
| 2-year effect | −0.138 | −0.004 | 0.117 |
| | (0.038) | (0.006) | (0.092) |
| 5-year effect | −0.157 | 0.003 | 0.048 |
| | (0.032) | (0.007) | (0.082) |
| Placebo effect | −0.003 | 0.001 | 0.046 |
| | (0.010) | (0.002) | (0.030) |
| Baseline | 8.95 | 13.969 | 14.609 |

*Notes:* This table reports the average effects of CAPS on the log of different health spending measures. *Mental health hospital spending* is the federal expenses with hospitalizations associated with mental health. *Overall hospital spending* is the federal expenses with hospitalizations in total. *Municipality health spending* is the health expenses incurred by local governments. Standard errors in parentheses are computed using a municipality-level clustered bootstrap. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. In lines 1 and 2 we report the average effect after two and five years, respectively. Line 3 shows the placebo effect. Line 4 shows the mean of the outcome at baseline—i.e., one period before the event.

Furthermore, the event studies show that they fluctuate around zero similarly in pre- and post-event periods, suggesting no effect.

Three other points are worth mentioning regarding our results on mortality. First, these estimates are somewhat unstable to the inclusion of other controls. If we control for time trends based on some baseline characteristics, the estimates of the average effects for deaths of despair and alcohol-related deaths, for instance, change their signs. Second, if we look at a longer horizon (up to eight years after the event), the estimates converge to zero, which would not be expected if we had nonzero effects but lacked statistical power to detect them. Third, we also estimate the cumulative effects of the reform eight years after CAPS' introduction and find no significant effects.

In sum, we find no evidence of an effect of CAPS on mental health mortality. We doubt that lack of statistical power plays a major role here, since we can detect reasonably small effects for most outcomes, especially our grouped measure of deaths of despair. Moreover, since most patients switching from inpatient care have a severe condition, we expect these effects to be larger than they would be if CAPS treated only people with milder conditions. Of course, mortality does not reflect mental health perfectly; hence, it is still possible that the centers affect mental health in other, less extreme ways.

### E. *Homicides*

Figure 8 and Table 5 show the effects of CAPS on homicides. Estimates indicate that before CAPS' introduction, treated and control municipalities had very similar

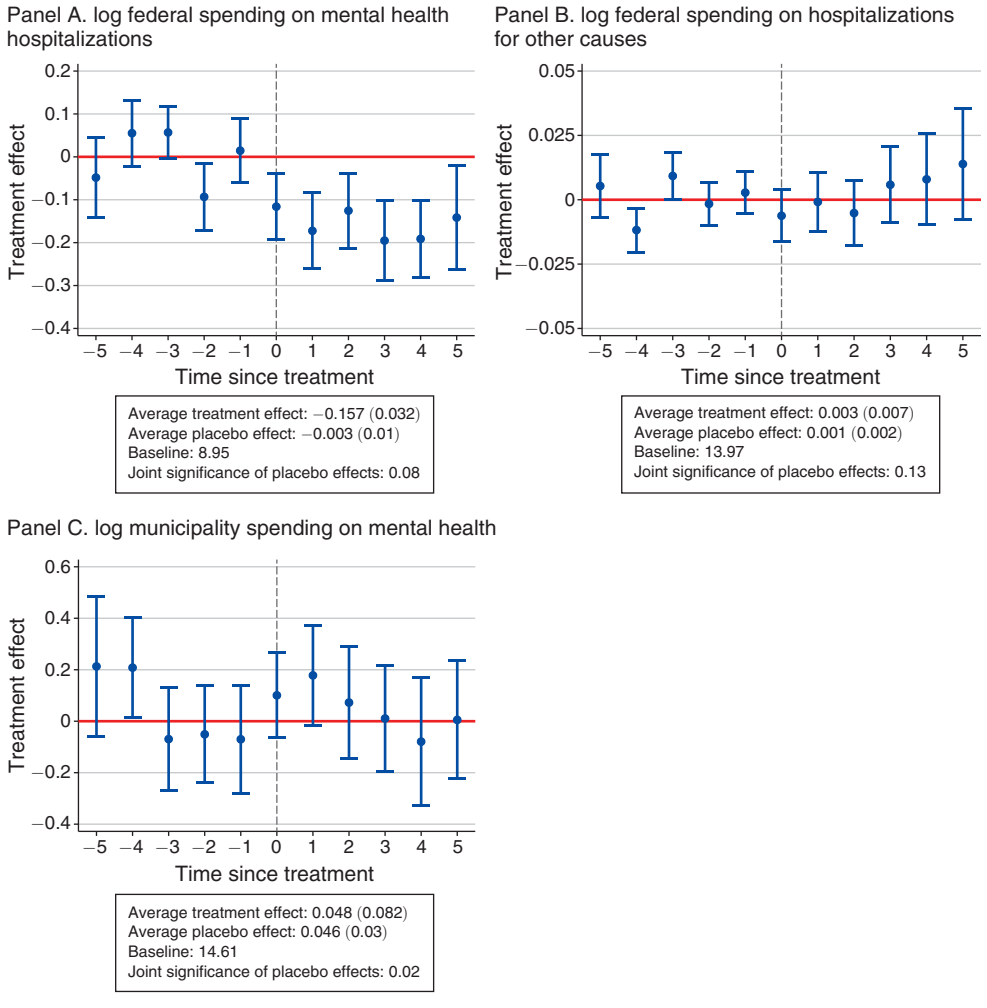


FIGURE 6. EFFECTS OF CAPS ON log(*PUBLIC HEALTH SPENDING*)

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for CAPS' effects on federal spending on mental health hospitalizations (panel A), federal spending on hospitalizations for other causes (panel B), and municipal mental health spending (panel C). All spending variables are in logs. Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the first pre-CAPS period.

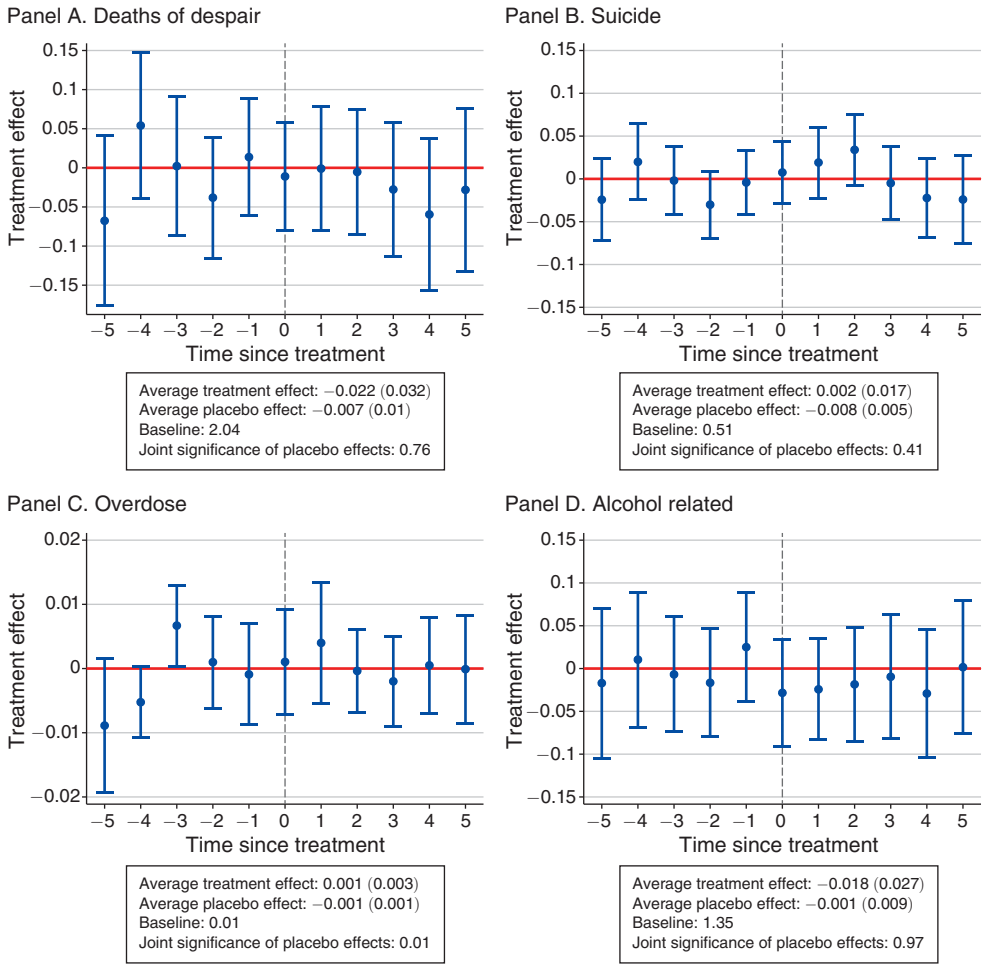


FIGURE 7. EFFECTS OF CAPS ON DEATHS OF DESPAIR: DEATHS PER 10,000 PEOPLE

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS' effects on the number of deaths per 10,000 people associated with mental health. Panel A shows the results on deaths of despair, which include deaths by suicide (ICD-10 X60-X84), overdose (ICD-10 X40-X45, Y10-Y15, Y45, Y47, Y49), and alcohol-related deaths (which are a combination of alcoholic liver disease (ICD-10 K70, K73-K74) and deaths by mental disorders, which in the data are mainly associated with alcohol (ICD-10 F00-F99)). Panels B, C, and D show the results for each category separately. Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the first pre-CAPS period.

Table 5—Effects of CAPS on Mortality Outcomes: Deaths per 10,000 People

| | 2-year effect (1) | 5-year effect (2) | Placebo effect (3) | Mean at baseline (4) |
|---|---|---|---|---|
| Deaths of despair | −0.006 | −0.022 | −0.007 | 2.038 |
| | (0.034) | (0.032) | (0.010) | |
| Suicide | 0.020 | 0.002 | −0.008 | 0.512 |
| | (0.018) | (0.017) | (0.005) | |
| Overdose | 0.002 | 0.001 | −0.001 | 0.015 |
| | (0.003) | (0.003) | (0.001) | |
| Alcohol related | −0.024 | −0.018 | −0.001 | 1.349 |
| | (0.029) | (0.027) | (0.012) | |
| Homicides | 0.082 | 0.149 | 0.016 | 1.940 |
| | (0.041) | (0.044) | (0.012) | |
| Infant mortality | 0.183 | −0.001 | −0.013 | 173.782 |
| | (6.341) | (2.445) | (0.79) | |
| All-cause mortality | −0.040 | 0.042 | 0.035 | 47.104 |
| | (0.174) | (0.170) | (0.063) | |

*Notes:* This table reports the average effects of CAPS on the number of deaths related to mental health per 10,000 people. Deaths of despair is an aggregate category that includes deaths by suicide (ICD-10 X60-X84), overdose (ICD-10 X40-X45, Y10-Y15, Y45, Y47, Y49), and alcohol-related deaths (which are a combination of alcoholic liver disease (ICD-10 K70, K73-K74) and deaths by mental disorders, which in the data are mainly associated with alcohol (ICD-10 F00-F99)). Standard errors in parentheses are computed using a municipality-level clustered bootstrap. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. Columns 1 and 2 report the average effect after two years and after five years, respectively. Column 3 reports the placebo effect. Column 4 shows the mean of each variable at baseline—i.e., one period before the event.

trends in homicide rates. The creation of CAPS is associated with a significant increase of 0.149 (s.e., 0.04) deaths per 10,000 people on average, or 7.7 percent of the value at baseline.[19]

The Penrose hypothesis is a particularly prominent and extensively discussed explanation for our results. In particular, psychiatric hospitals have an incapacitation effect on mentally ill individuals who may be prone to violent incidents (as perpetrators or victims). Thus, substituting psychiatric hospitals with CAPS could increase rates of violent crimes, especially considering that most of the substitution has affected patients with schizophrenia. In principle, other channels can be consistent with our results. For instance, the increased presence of psychiatric patients in the community can make the neighborhood feel unsafe and increase crime, or CAPS may make it easier for criminals to find victims. Although we cannot directly rule out these other potential explanations, we focus on the Penrose hypothesis. Next, we conduct a quantitative exercise to investigate the plausibility of our results under this mechanism.

[19] Even though the numbers seem large, when put in perspective to externalities from other Brazilian policies on homicides—such as the trade liberalization (Dix-Carneiro, Soares, and Ulyssea 2018) and the transition of a market from legal to illegal (Chimeli and Soares 2017)—our results have much lower magnitude.

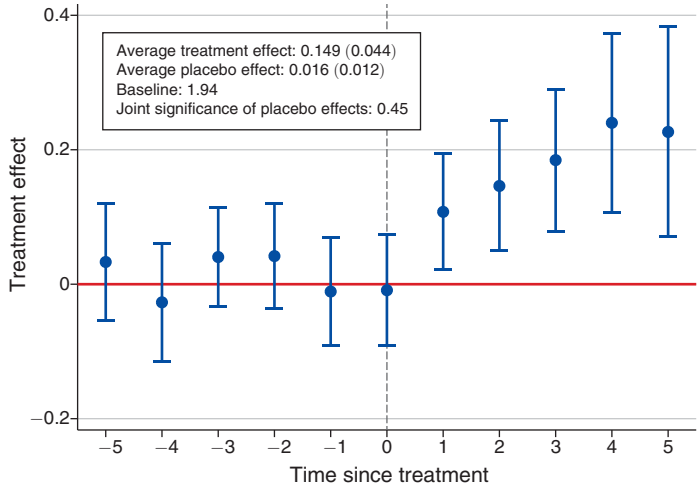


FIGURE 8. EFFECTS OF CAPS ON HOMICIDES: DEATHS PER 10,000 PEOPLE

*Notes:* This figure plots 95 percent confidence intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS' effects on the number of deaths per 10,000 people by homicide (ICD-10 X85-Y09). Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Placebo DID estimators estimate the CAPS' effects had the treatment occurred in a placebo, pretreatment period. They use a varying baseline period—the one immediately before the placebo treatment date—so we do not normalize relative to a unique period. Controls include municipality GDP per capita, PBF spending per capita, a series of indicators for age-by-gender population bins, state × year fixed effects, and linear trends of pretreatment characteristics. Pretreatment municipality characteristics included are: Theil index, poverty rate, unemployment rate, illiteracy rate, share of rural population, log of population, log of social spending, number of mental health providers, number of mental health offices, municipality area, altitude, distance to capital, temperature, and rainfall. The Average Treatment Effect computes a weighted average of the dynamic estimators, giving to each estimator a weight proportional to the number of switchers the estimator applies to. In parentheses, standard errors are computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the first pre-CAPS period.

If the only channel by which CAPS affect mortality by assault is through dehospitalization, the ratio between the CAPS' effects on homicides and the CAPS' effects on mental health hospitalizations estimates the impact of deinstitutionalization on homicides induced by the policy reform. As the average effect of CAPS on psychiatric admissions is -0.95, our reduced-form estimates predict that 15.7 percent of deinstitutionalized patients are involved in a violent death. Based on medical research that follows mentally ill patients years after a psychiatric discharge, it is plausible to use as a benchmark that at least 40 percent of discharged inpatients eventually get involved in a violent crime, either as victims (Hiday et al. 1999; Walsh et al. 2003) or perpetrators (Link, Andrews, and Cullen 1992; Fazel et al. 2009; Fleischman et al. 2014).

Even if our reduced-form prediction is within this range, one could argue that violent crime is a broader category, and homicides represent just a small fraction. Still, they are overrepresented among the severely mentally ill. Indeed, Fazel and Grann (2006) find that the population-attributable risk of patients with severe mental illness concerning homicides is 18 percent, the highest among all types of violent crime (it

is 5 percent for all violent crimes). Moreover, data from Murray, de Castro Cerqueira, and Kahn (2013) imply that roughly 1 in 15 violent crimes in Brazil is a homicide. We can then perform a back-of-the-envelope calculation to estimate the ratio of the number of homicides in which severely mentally ill people are involved to the number of violent crimes involving a mentally ill agent by calculating

$$r = \frac{0.18 \times 1}{0.05 \times 15} = 0.24$$

Multiplying this number by the benchmark that 40 percent of discharged inpatients are eventually involved in a violent crime, we calculate that 9.6 percent of discharged patients might become involved in homicides.[20] Therefore, if we take our point estimates literally, this exercise illustrates that the effect size we find is within a plausible range. However, given the extent of the noise in the estimation, we remain cautious in our interpretation. Depending on whether one increases or decreases the CAPS' effects on homicides and hospitalization rates by one standard error, the reduced-form prediction varies from 8 percent to 28 percent, making the channel we propose more or less likely.

Our results are consistent with the hypothesis from some criminologists that inpatient mental care can affect crime via an incapacitation effect (Lamb 2015)—i.e., that being treated in an inpatient setting prevents these patients from committing or becoming victims of violent crimes. This is particularly relevant in our context, given that most people switching away from inpatient care are patients with schizophrenia, who tend to be more exposed to violent incidents than the general population (Brekke et al. 2001a; Fazel et al. 2014; Fleischman et al. 2014). However, given the data limitations, we cannot explore this channel further.

## V. Robustness Checks

### A. *Unobserved Policy Changes*

One concern regarding the effects we find is that the presence of CAPS could be related to unobserved policy changes that might also affect our main outcomes. Indeed, during the 2000s, Brazil saw other important public policies, like the PBF and Family Health Program (*Programa Saúde da Família*—henceforth, PSF), created or expanded substantially. To partially address these concerns, we control for differential trends according to PBF spending changes and state-year fixed effects, which account for contemporaneous programs partially determined at the state level. Still, one might worry whether the timing of CAPS adoption correlates with the rollout of PSF across municipalities, which is known to improve health outcomes.

We believe this should be less of a concern, given that PSF municipality coverage had already reached nearly 80 percent when psychiatric reform began. What is more, the correlation between the timing of these programs is low (see online

[20] We thank an anonymous referee for suggesting this discussion.

Appendix Figure A8 for a scatter plot).[21] Still, we test the stability of our baseline estimates to the inclusion of controls for PSF adoption and adoption year $\times$ time trends (see online Appendix Table A6).[22] Our results are unsurprisingly robust to these alternative specifications. Finally, given previous research documenting remarkable negative effects of PSF on infant mortality (Bhalotra, Rocha, and Soares 2019; Macinko et al. 2007), and the fact that other Brazilian health care policies have significantly affected inputs in the production function of infant health (Carrillo and Feres 2019), we assess the impact of CAPS on infant mortality and interpret it as a placebo test. Consistent with previous results, online Appendix Figure A9, panel A shows that the effects are flat around zero. We also estimate effects flat around zero for all-cause mortality (panel B). The evidence above is consistent with the interpretation that our results capture the effects of CAPS and not of other unobserved policy changes.

### B. *Alternative Specifications*

In principle, our strategy should be independent of including controls. Finding that results change significantly depending on the controls used could threaten our identifying assumption. In columns 2 and 3 of online Appendix Table A7, we report the average effects on our main outcomes varying the set of controls used in the estimation. In column 2, we estimate the effects without any control. In column 3, we present the estimates from a specification without controlling for our time-varying controls: population composition by age and gender, GDP per capita, and PBF expenditures per capita. Results remain similar, with slightly larger but not substantial differences in the specification without controls. The same table reports our main results when we weigh municipalities by baseline population (column 4). The standard errors increase remarkably in this specification, so all our results remain within reasonable bounds.[23]

Column 5 reports the average effect for our main outcomes using a restricted, treated sample that includes only municipalities with at least five years of exposure to treatment. All our main results remain stable when we explore this alternative composition of treated cohorts. These results indicate that compositional changes (caused by the fact that some late-treated cities have missing post-CAPS data) do not affect our results. There also exists a possibility that our choice of restricting the event time to the fifth year after CAPS adoption trims an important part of the effects if they take more time to fully materialize. To assess whether our results significantly underestimate the effects, column 6 reports the average effects for our main outcomes, extending the event time horizon to eight years after CAPS. Indeed, we see slightly stronger effects. However, we estimate that the effects are already stable

[21] The raw correlation is 0.06. If we weight observations based on baseline population, it decreases to -0.03, indicating that larger municipalities are not more likely to adopt both programs in parallel.

[22] We thanks Bhalotra, Rocha, and Soares (2019) for providing data on the implementation of PSF across municipalities.

[23] As shown by Solon, Haider, and Wooldridge (2015), if the assumed covariance structure is a good approximation to the true one but not exactly the right one, including weights leads to less precise estimates even in the absence of heterogeneity in treatment effects.

at five years, and these slightly stronger average effects arise simply from having more periods with a stable and fully realized effect.

### C. *Spillover Effects*

A potential concern with our empirical strategy is whether displacement or spillover effects could be relevant to our main results. Online Appendix Figure A10 addresses this concern by comparing means for treated and nontreated units over event-times.[24] If spillover effects were a problem, we should see at least part of the effect on outcomes of interest coming from nontreated units breaking their trend when treated units receive CAPS. However, the changes in our outcomes during the postintervention period are concentrated in the treatment group's outcomes and we never see changes in the opposite direction in control areas. These results are consistent with the absence of displacement effects.

## VI. Conclusion

The 2002 Brazilian psychiatric reform reorganized public mental health care delivery. A network of community-based services centered on CAPS was built across the country. To identify the causal effects of this reform, we exploit municipality-level variation in the establishment of CAPS. We find that the reform achieved its primary goal of increasing the supply of outpatient mental care and shifting care away from inpatient to outpatient settings. However, the effects are concentrated on long-stay admissions and patients with schizophrenia, an illness more severe than most CAPS are equipped to treat adequately. Despite community-care treatment being generally less expensive than inpatient treatment, we find evidence of increased public spending with the reform. Finally, we find a persistent increase in homicide rates following the reform, possibly caused by the loss of incapacitation effects that long-stay hospitalizations have on the crime-prone mentally ill.

To the best of our knowledge, this paper provides the first causal evaluation of a large-scale mental health reform. Our estimates are thus particularly informative for countries with deficits in providing mental health treatments (World Health Organization 2022), countries facing barriers to the implementation of a community-based strategy (e.g., due to financial constraints), and countries developing or revising deinstitutionalization interventions. According to the UN (2020), deinstitutionalization should be a priority following the COVID-19 pandemic.

Large-scale reforms that aim to change the nature of mental health care delivery, moving it toward community-based care, involve many aspects. Policymakers must be mindful of many factors that may affect mentally ill people. Our results from Brazil show that providing an inadequate substitute for more severe illnesses—in particular, schizophrenia—can generate undesirable effects for this population and the community in general. At the extreme, the negative consequences of failing to

[24] To construct such time series, we first stack residualized (state $\times$ year fixed effects) long differences of the outcome (relative to event-time $-1$) for each treated cohort and its respective control group. We then collapse data at the event-time using weighted averages based on the treated cohort size.

offer necessary treatments can call into question the entire mental health care delivery strategy, regardless of its effectiveness when properly implemented.

A few details particular to our setting might offer important insights for future reforms in other contexts. The deinstitutionalization process in Brazil occurred late and still needs investment. Less than 3 percent of Brazilian municipalities have access to mental health centers open 24 hours a day, with outpatient beds and crisis intervention services. Such infrastructure is fundamental to reducing violence by people with serious mental illness in the community (Thornicroft and Tansella 2013; Lamb and Weinberger 2005; Dvoskin and Steadman 1994). Therefore, incorporating intensive care models into community settings can improve policy outcomes of mental health reforms. Evaluating these interventions is left to future work.

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