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The Effects of a Large-Scale Mental Health Reform: Evidence from Brazil*

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Abstract

This paper assesses the effects of the introduction of Psychosocial Care Centers (CAPSs) in Brazil on mental health. These units are the centerpiece of the Brazilian psychiatric reform, meant to deliver community-based mental health services for people with moderate or severe disorders, including substance abuse. Using a differences-in-differences design that exploits the roll-out of the CAPSs across the country, we show that these centers improved access and utilization of outpatient mental health care and reduced hospital admissions due to mental and behavioral disorders. Those reductions were more pronounced for long-stay admissions and among patients with schizophrenia. We also find that the introduction of centers delivering substance abuse treatment reduced deaths caused by alcoholic liver disease. Despite these positive effects, our evidence indicates that this shift away from inpatient care increased homicide rates.

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1 Introduction

Mental and addictive disorders affected more than 1 billion people globally in 2016, being considered one of the most burdensome non-communicable diseases in the world (Rehm and Shield, 2019). People with major depression and schizophrenia have a 40% to 60% greater chance of dying prematurely than general population. Besides that, suicide is the second most common cause of death among young people worldwide (WHO, 2013). The absence of mental health may also have devastating effects on an individual's ability to lead a balanced professional, social and family life (Roy and Schurer, 2013; Frijters et al., 2014; Kessler et al., 1998). The economic consequences of these are equally large. Estimates suggest that the global impact of mental disorders in terms of lost economic output for the period 2011-2030 will amount US\$ 16.3 trillion (Bloom et al., 2012). Yet, health systems have not adequately responded to the burden of mental health disorders. The gap between the need for treatment and its provision is large all over the world. In low and middle income countries, between 76% and 85% of people with severe mental illness do not receive treatment for their disorder. The corresponding range for high income countries is also high: between 35% and 50% (WHO, 2013).

The numbers indicate that a better provision of mental health care might significantly improve a given population's well-being. It is not obvious, though, how to optimally provide this type of care. In the second half of the twentieth century, many countries started to switch from a model of mental health care centered on psychiatric hospitals toward a model based on community care. However, international experience shows that this process is not something simple to be done. Reducing hospital admissions without offering adequate community-based care with proper integration to other medical services may not only fail to improve the delivery of mental health care services, but also generate undesirable consequences (Sisti et al., 2015; Rosenbaum, 2016; Lamb, 2015).¹ Hence, it is important to understand how different strategies to implement community-based mental health care work and how deinstitutionalization can be done while avoiding the potential pitfalls associated with it.

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

Despite this significant change in philosophy and its importance, there is little rigorous research measuring the effects of policies directed toward the effective provision of community-based mental healthcare. This paper investigates this question by examining the psychiatric reform in Brazil. The Brazilian reform was centered on the introduction of Psychosocial Care Centers (*Centros de Atenção Psicossocial* – henceforth, CAPSs) as a community-based substitute for inpatient care for people with moderate or severe mental disorders. More specifically, CAPSs provide a number of outpatient procedures such as medical consultations, individual and group therapy, and therapeutic workshops. Also, CAPSs, as part of the national healthcare system (*Sistema Único de Saúde* – SUS), work as gateways to the mental health care system and, if adequate care cannot be provided there, the person can be referred to the adequate facility. Hence, to study the psychiatric reform, we focus on the effects of introducing a CAPS in a municipality. In particular, we assess the CAPSs' effects on density of mental health professionals, on utilization of outpatient mental healthcare, and on deaths and hospital admissions by cause. Among causes, we investigate mental and behavior disorders, suicide, alcoholic liver disease, and overdose.

We additionally study the reform's effect on homicides. A recurrent concern about deinstitutionalization – the process of reducing mental hospitalization and providing community-based alternative services (Lamb and Bachrach, 2001) – is with increased violence. There is extensive evidence that severe mental illness is closely associated with an increased risk of aggressive behavior, crime and victimization (Hodgins et al., 1998; Rueve and Welton, 2008; Fazel et al., 2009; Teplin et al., 2005). Historically, there has been a debate about criminality and inpatient versus community-based mental healthcare. For example, in the 70's, when community services started to expand in the U.S., people fearing an increase in crime in their community made so much opposition that several new psychiatric centers had to be closed (Rabkin, 1979). A few specialists share a similar concern by advocating that community care is not suited for all mentally ill persons, especially those at risk of becoming criminalized (Lamb and Weinberger, 2005). Contrary, others defend that this type of care can be successful in such cases, provided that adequate community treatment resources are available (Slate et al., 2013). Empirically, this is still an open question.

Our empirical strategy exploits the roll-out of CAPSs across the the Brazilian mu-

nicipalities in a differences-in-differences framework. Following [de Chaisemartin and d’Haultfoeuille \(2019\)](#), we use a DID estimator that is robust to heterogeneous treatment effects across cohorts and over time. Parallel pre-trends for the set of outcomes we evaluate provide evidence on the design validity. To evaluate the policy effects, we use several administrative data. These include information on mental health providers, psychiatric beds, outpatient mental health production, and mortality and hospital admissions by cause. We first document that the introduction of CAPSs increased access and utilization of community-based mental health care. More specifically, the implementation of these centers was associated with immediate and large increases in the density of mental health professionals, as well as in outpatient visits made by them. Consistent with these results, we also found an increase in the number of drugs dispensed in outpatient care for the treatment of psychiatric disorders. Turning to morbidity and mortality outcomes, we find that CAPSs decreased hospital admissions due to mental illness. The effects are driven by the reduction of long-stay hospitalizations of individuals with schizophrenia. Additionally, centers specialized in substance abuse treatment reduced deaths due to alcoholic cirrhosis. Despite these positive effects, we also find a modest increase in homicide rates, potentially caused by the CAPSs’ effects on mental hospital admissions. In particular, we found a relation of 1.7 homicides for each 10 less mental hospitalizations, which is line with the prevalence of violent crimes committed by former psychiatric in-patients reported by the literature. Heterogeneous effects suggest that increased victimization is not the main driver producing these results.

The novel results we present in this paper suggest a few take-aways for policy design. First, our immediate and positive effects on outpatient mental healthcare production suggest that a public policy that introduces community mental health units may increase the population share covered by mental healthcare, being a key tool to reduce commonly observed gaps between the need for mental treatment and its provision. Second, our evidence highlight the introduction of community centers specialized in substance abuse treatment as a tool to reduce deaths caused by alcohol-related liver diseases, which contribute markedly to the global burden of mortality ([Rehm et al., 2013](#); [Ventura-Cots et al., 2019](#)). Third, our results on hospital admissions may be informative for policy makers who seek to accentuate the deinstitutionalization process

and reduce the need for inpatient care. However, our findings on homicides highlight that there are important trade-offs to be considered when choosing the optimal delivery of mental health care, being necessary to understand what are the pitfalls of community-based care (as compared to inpatient care) to improve the way mental health care is delivered.

This paper contributes to the rich economic literature on mental health. Many papers study different determinants of mental health, as medication ([Dalsgaard et al., 2014](#); [Ludwig et al., 2009](#)), early life conditions ([Persson and Rossin-Slater, 2016](#); [Almond and Mazumder, 2011](#); [Adhvaryu et al., 2019](#)), economic shocks ([Ruhm, 2000](#); [Schwandt, 2018](#)), and income shocks ([Christian et al., 2019](#); [Baird et al., 2013](#)). A recent set of experimental papers evaluate the effects of psychological interventions on mental health or related outcomes ([Baranov et al., 2020a,b](#)). Finding evidence of government policies that impact mental health, however, is a much harder task. A few papers study policies that look at some measure of mental health as a secondary outcome, like [Katz et al. \(2001\)](#) on the Moving to Opportunity program or [Milligan and Stabile \(2011\)](#) on child tax benefit expansions. We are not aware, though, of any paper that studies a large scale public policy targeted specifically at mental health. This paper helps to fill this gap.

Despite being one of the first studies to study the effects of community-based mental healthcare, we are well aware that the question of whether this type of care is effective is not new to the public health and medical literature ([Wiley-Exley, 2007](#)). Community-based mental health services has been linked negatively to mental hospitalizations ([Wanchek et al., 2011](#); [Madianos and Economou, 1999](#)), suicide rates ([Pirkola et al., 2009](#); [While et al., 2012](#)), and measures capturing symptoms of schizophrenia and bipolar disorders ([Chatterjee et al., 2003](#); [Chisholm et al., 2005](#); [Hickling et al., 2001](#)). However, the existing studies do not aim a causal interpretation and many of them use very small samples. To the best of our knowledge, the present study is the first to exploit a quasi-experimental design to investigate the causal effects of the introduction of community-based mental healthcare in large scale.

As we also evaluate centers providing substance abuse treatment, our findings complement evidence by [Swensen \(2015\)](#). Using U.S. data, the author conducted the first nationwide analysis documenting the causal benefits of substance-abuse treat-

ment on mortality. In particular, drug-overdose deaths. Differently, we present evidence for a developing country and study health facilities whose access is fully subsidized. Additionally, we study a context where the prevalence of substance-abuse-related mortality is very different from that observed in the U.S.. Brazil is one of the countries with the lowest overdose death rates in the world (UNODC, 2013). However, it has a high prevalence rate of heavy episodic drinking, one of the most important indicators for acute consequences of alcohol abuse (WHO, 2019).

Our paper also contributes to the literature linking crime and mental health or mental health services. Eighty years ago, before the advent of full-scale deinstitutionalization, Penrose (1939) found a negative correlation between the proportion of people placed in mental hospitals and the proportion held in prison using cross-country data from European countries. Since then, the Penrose' Hypothesis has been a subject of interest and controversy (Lamb, 2015). Using similar data, some papers found similar results (Mundt et al., 2015; Markovitz, 2006; Raphael and Stoll, 2013), while others did not (Large and Nielssen, 2009). A more clear pattern has been found by medical researches that use individual-level data from discharged patients. These papers have systematically reported a high prevalence rate of violence among former inpatients in a post-discharge period (e.g., Link et al. (1992), Fleischman et al. (2014)). Most of these papers have focused on cross-sectional comparisons, which might be subject to omitted variable bias that can affect both crimes and in-patient care utilization. The only paper aiming a causal interpretation for the relation between mental hospitalization and crime is Landerso and Fallensen (2020). But, instead of studying discharge, they analyze the event of admission at a psychiatric hospital and find that inpatient admittance reduces criminal behavior through incapacitation. Our paper exploits potentially exogenous changes in severe mental hospital admissions induced by CAPSs' introduction to study the relation between dehospitalization and homicides in the presence of alternative, community-based treatment.

The remainder of the paper is organized as follows. Section 2 describes the institutional background. Section 3 presents the data sources and discuss expected effects. Section 4 describes the empirical approach. In Section 5 we present and discuss our main results. Section 6 presents heterogeneous effects by different CAPSs' types. Section 7 presents robustness checks. Section 8 concludes.

2 Background

2.1 Institutional Background

Since the mid-20th century, many countries started to shift mental health care away from psychiatric hospitals toward community-based care – in what became known as deinstitutionalization. The rationale behind this change was based on several assumptions: community-based care is more human than inpatient care, given the miserable condition of people in psychiatric hospitals; community-based care is more adequate than hospital-based care in general; and community-based care is cheaper than care provided by hospitals (Lamb and Bachrach, 2001). This new paradigm also influenced Brazilian psychiatrists, which ultimately led, in 1989, representative Paulo Delgado to present a bill to the Congress proposing the progressive substitution of psychiatric hospitals by other, community-based resources.

The psychiatric reform bill, with some modifications, eventually became a law – Law 10.216, or the Psychiatric Reform Law – in April 2001, having the CAPSs as its centerpiece. During this period, some states passed laws in the same spirit and some CAPSs and other related services were created (Britto, 2004). However, only after the Psychiatric Reform Law was passed in 2001 and a Regulatory Ordinance was issued by the Ministry of Health in 2002 the construction of CAPSs gained traction and started to happen all over the country (BRAZIL. Ministry of Health, 2005).² Figure 1 shows the number of municipalities receiving a CAPS by year, from 2002 to 2016. According to the Brazilian Ministry of Health (BRAZIL. Ministry of Health, 2015), about 900 million Brazilian Reals (BRL) were spent with this policy from 2002 to 2014.³

The main goal of the Brazilian psychiatric reform was to implement community-based care services for mental health and substance misuse through CAPSs while facilitating deinstitutionalization from hospitals. CAPSs also became the main gateway to the public mental health system, referring less severe cases to the Basic Healthcare Units and more severe cases to public hospitals.⁴ The Regulatory Ordinance of 2002

²Actually, CAPS existed even before the bill was presented: the first CAPS was created in the city of São Paulo in 1987. However, as already mentioned, the number of CAPSs before 2002 is negligible compared to the number of centers that were created after the Psychiatric Reform Law.

³Approximately US\$ 220 million.

⁴The primary care offered by the public system in Brazil is organized around units called Basic Healthcare Units (or UBS).

defined six different CAPSs' types, based on municipality size and target population. The basic and most common type of CAPS is CAPS I, for all ages and cities with 15,000 people or more. The other general type of CAPS are CAPS II and CAPS III, aimed at people of all ages and cities with 70,000 people or more and 150,000 people or more, respectively. There are also CAPSs' types for specific groups of people. CAPS i are specialized in children and teenagers, while CAPS AD and CAPS AD III are specialized in substance abuse treatment.

The Regulatory Ordinance of 2002 also defined that the federal government should provide financial support for the construction of the centers, and then monthly financial support for their maintenance. For construction, financial support varies from 800,000 to 1,000,000 BRL. For maintenance, monthly support varies from 30,000 to 100,000 BRL. In order to get a CAPS and the financial support, a municipality must send an application to the federal government, which then approves it or not. We had access to the decisions made by the federal government in 2019. Very few proposals were rejected. Among those rejected, the main reason for rejection was the population criterion.

In general, all types of CAPSs deliver care following standard procedures. When a patient visits a CAPS for the first time, he/she is interviewed by a professional responsible for giving an initial diagnosis. If it is decided that the patient will be treated in CAPS, a multidisciplinary team – composed mainly of psychiatrists, psychologists, occupational therapists, and social workers – takes care of the case. Then, it develops actions related to the patient's needs, such as consultations with a psychologist, medication use, participation in therapeutic workshops, clinical exams, and group therapies. Overall, mental health treatment delivered at CAPSs has as an explicit goal the social reintegration of individuals into the society and the strengthening of community and family ties.

Following a similar logic, the centers also deliver substance abuse treatment, with Psychosocial Care Centers Alcohol and Drugs (CAPSs AD) being designed specifically for such purpose. In particular, they offer individual and group care, as well as home visits and outpatient detoxification. Still, these centers can work in partnership with hospitals, referring more severe cases for inpatient detoxification. The multidisciplinary teams from CAPSs adopt several prevention practices aiming to reduce the

abuse of substances by its patients. They carry out educational activities to warn about the consequences of alcohol and other drug abuse, offer alternative leisure activities such as physical activities and crafts, and work with the community and the patient's family to reduce risk factors associated with substance abuse.

Finally, there are a few differences between the different CAPSs' types regarding infrastructure. CAPSs III and CAPSs AD III are the only centers that open on weekends and deliver night care; the other centers operate from Monday to Friday in two 4-hour shifts. They are also the only centers with ambulatory beds that can shelter patients needing monitoring.

2.2 Conceptual Background

Since the main component of the psychiatric reform are the CAPSs and we exploit their implementation to assess the effects of the reform, it is important to understand conceptually how these centers may affect the outcomes of interest and how we can empirically investigate these channels.

In the "first-stage", the implementation of a CAPS may affect the supply of mental health practitioners – in particular, psychiatrists, psychologists, social workers, and occupational therapists. As CAPSs should offer treatment through a multi-disciplinary team constituted mainly from these professionals, we expect a positive effect. We have in mind, though, that this could not be the case if local governments were just reallocating mental health practitioners from other sectors to work at CAPSs. Then, we will investigate the *per capita* number of ambulatory services provided by mental health professionals. If CAPSs are indeed effective at increasing the availability of outpatient mental healthcare, we should expect to see an increase in these outcomes. This would be consistent with previous research which found that the strengthening of primary care in Brazil led to greater utilization of ambulatory services (Bhalotra et al., 2019; Carrillo and Feres, 2019; Mattos and Mazetto, 2019). We will also look at the rate of dispensed antipsychotic drugs in the outpatient-level of care. The rationale stems from the fact that antipsychotic drugs are the mainstay of the treatment for psychotic illnesses such as schizophrenia. Thus, if the introduction of community-based mental healthcare is associated with increased utilization of outpatient care among severely mentally ill persons, we should expect positive effects on the number of publicly dis-

pensed antipsychotic drugs.

If the reform is affecting the delivery of mental healthcare in the intended way, we should see an effect of CAPSs on mental hospital admissions. This effect may be associated to supply- and demand-driven declines in hospitalizations due to mental illness. Since one of the psychiatric reform's goals is to replace inpatient care, it is possible that the CAPSs' establishment in a municipality is followed by the closure of psychiatric beds, which in turn could lead to a reduction in hospitalizations. We can empirically test such a hypothesis. We can also estimate the CAPSs' effects on hospitalizations rates due to mental illness and look for heterogeneous effects according to some groups of causes within mental illnesses.

As previously mentioned, CAPSs also became the first point of entry into publicly funded mental health services after the Psychiatric Reform Law. So, if they have increased access to mental health care, they may also have increased hospital admissions by referring more previously under-served individuals to inpatient care. However, this should happen only for exceptional cases, so we do not expect this to be a major driver behind our results. Several studies suggest that community-based care through outpatient services may substitute hospital admission related to mental illness. In particular, researchers often advocate that community mental health services provide a filter-effect along the pathway to inpatient care ([Shaeffer et al., 1978](#); [Wanchek et al., 2011](#); [Madianos and Economou, 1999](#)). Thus, if the CAPSs' opening is associated with increased utilization of outpatient mental healthcare, we expect an increase in the number of treatments delivered in the community, reducing demand-driven inpatient care.

All the aforementioned effects constitute the channels through which mental health can be affected, which is the ultimate goal of the reform. We can evaluate this effect on mental health by looking at mortality outcomes. Premature death among individuals with mental disorders can be related to several chronic conditions such as cardiovascular, respiratory, and infectious diseases, diabetes and hypertension. However, these conditions are not just related to mental illness. Therefore, we evaluate causes of death more directly associated with mental health, all of them recently entitled under the label "deaths of despair" ([Case and Deaton, 2015, 2017](#); [Ruhm, 2018](#)). More specifically, we evaluate suicide, overdose, and alcoholic liver disease.

The literature has reported striking associations between mental illness and suicide, as well as high prevalence rates of comorbidity between substance use disorders and other mental and behavioral disorders. To put it in perspective, it has been found that about 90% of suicides are associated with a psychiatric illness (Cavanagh et al., 2003), and that the proportion of schizophrenic individuals with substance abuse disorders can reach 70% (Winklbaur et al., 2006). Following Case and Deaton (2015, 2017) we also consider a broader definition of alcohol-related mortality, by incorporating deaths coded as unspecified sources of chronic liver diseases into the alcoholic liver disease category. In particular, chronic hepatitis and cirrhosis.⁵ Finally, we also investigate deaths coded with an underlying primary cause of mental and behavior disorders.

The expected effects of CAPSs on mortality rates depends on how centers affected the demand for mental health care and the effectiveness of CAPSs' care relative to alternatives to prevent deaths by these causes. The effects are far from obvious. If CAPSs increased the demand for mental health care, and these centers are suitable to prevent deaths related to mental illness among individuals that, in the absence of CAPSs, would not be demanding mental healthcare, we should expect a decrease in mortality rates. If the CAPSs' introduction in a municipality causes a shift from inpatient to community-based care, and both types of care are not perfect substitutes, the effects may be ambiguous.

The effects may also depend on the type of mental health services provided at CAPSs and the specific cause of death being evaluated. While et al. (2012) and Pirkola et al. (2009) report a positive association between community care and suicide prevention in the presence of well-developed community mental-health services. In particular, the results presented by Pirkola et al. (2009) are driven by the provision of community services available 24 hours a day. In our setting, only CAPSs III and CAPSs AD III deliver night care and stay open on weekends. These centers are available for only 3 percent of the Brazilian municipalities.⁶ Thus, it is possible that the association between community-based care and suicide is not present in our context. Previous

⁵As suggested by Ruhm (2019), this definition may be too broad since the added deaths will not necessarily involve alcohol.

⁶Based on the last Brazilian Census (2010), only 187 cities – out of 5570 – had at least 150,000 inhabitants.

research has also shown that service-related risk-factors for suicide include poor continuity of care in the community after hospital admissions, nonadherence to treatment, reduced care in the community, and short length of inpatient care ([Bassett and Tsourtos, 1993](#); [Hunt et al., 2009](#); [King et al., 2001](#)) Hence, the CAPSs' effects on suicide may be positive or negative depending on the quality of care provided in both mental health centers and hospitals, and which of these types of care are more adequate to prevent deaths caused by suicide.

Besides suicide, the others causes of death we investigate are related to substance abuse. As improved mental health may reduce substance abuse, any CAPS providing mental health treatment has the potential to reduce substance-abuse-related deaths. However, the CAPSs Alcohol and Drugs (AD) were created specifically to deliver substance abuse treatment. [Swensen \(2015\)](#) shows that increased supply of mental units providing substance abuse treatment reduced drug-related deaths in the U.S. Similarly to our context, the treatment facilities studied by the author deliver mostly outpatient treatment services, but may also refer more severe cases to residential and hospital inpatient settings. Hence, it is possible that similar effects are also present in our context. Yet, one may also consider that the CAPSs' effects on substance-abuse-related mortality depend on the incidence of causes CAPSs might prevent. Differently from the U.S. context, overdose is an extremely rare event in Brazil. In the period 2002-2016, such cause of death corresponded to only 0.05% of total deaths. The fraction attributed to alcoholic liver disease is 0.83% (1.7% if we consider the broader definition).

Given the depicted pattern, we should expect that the introduction of CAPSs AD is more likely to be associated with a decrease in alcohol-related deaths than other drug-related deaths. Indeed, the literature reports that alcoholic cirrhosis deaths can be entirely preventable by treatment for alcohol use disorders (e.g., [Rehm et al. \(2013\)](#)). Still, the development of the disease into more severe stages takes some time. Therefore, we do not expect that CAPSs AD would prevent future comorbidities among healthy individuals in the short run. The climbing of liver disease among individuals already compromised by alcohol abuse, however, can be deterred. According to experts, liver cirrhosis has no cure and is associated with high mortality rates. However, a prolonged life expectancy exceptionally requires patients to stop drinking. Any medical and surgical treatments for alcoholic liver disease are limited when drinking continues

(Addolorato et al., 2016; Tilg and Day, 2007). Empirical evidence on the topic indicates that abstinence increases the survival rates of patients with alcoholic cirrhosis, even in the short run (Xie et al., 2014). Hence, if treatment delivered at CAPSs effectively reduces alcohol abuse, the centers' adoption may cause a reduction in mortality due to alcoholic liver disease among patients already with some liver comorbidity. Based on reports from professionals working at CAPSs, de Souza et al. (2014) reveal that liver cirrhosis's prevalence is high among patients.

Finally, we analyze the CAPSs' effects on crime using homicide rates.⁷ Theoretically, the effects may again be ambiguous. The psychiatric reform may have increased homicides mechanically by reducing mental hospitalizations as inpatient admittance has an incapacitation effect (Landerso and Fallensen, 2020). Such an impact may be economically significant as there is extensive evidence in the literature that individuals with severe mental illness are at high risk of involvement with violent crimes (Rueve and Welton, 2008). However, we study hospital depopulation parallel to the expansion of community-based mental treatment. The introduction of CAPSs may reduce crime if these centers are able to improve the mental health of under-served people to the point of controlling violent behavior. Still, the impacts depend on whether treatment meets the medical needs of crime-prone patients. This goes back to a long-standing debate.

Severe mentally ill persons at risk of becoming criminalized need a safe and secure setting, where staff can monitor and contain aggressive behavior, formulate an appropriate treatment, and monitor psychiatric medications (Lamb, 2015). Hospitals often share this structure. Some specialists advocate that these needs can also be met in community treatment facilities provided there are enough investment (Slate et al., 2013). In particular, Dvoskin and Steadman (1994) highlight that intensive case management available 24-hours per day and a comprehensive array of community support services are the keys to reduce the risk of violence by people with serious mental illness in the community. However, Lamb and Bachrach (2001) argue that the inadequate and underfunded community treatment of persons who are the most difficult to treat is a common reality of the deinstitutionalization process in several countries that

⁷Brazil does not have a reliable and easily accessible crime data. Still, using data compiled by the police in two Brazilian states from 2001 to 2011, Dix-Carneiro et al. (2018) show that homicides recorded by the health system is highly correlated with police-recorded homicides and violent crimes against the person (excluding homicides).

may have set the stage for criminalization. In Brazil, the vast majority of CAPSs does not have the infrastructure to deal with patients in outbreak situations. Thus, the reform may have increased violent crime by shifting away mentally ill individuals from hospital admissions without providing structure for intensive care in the community. Finally, persons with mental disorders may be more vulnerable to violent situations that take place in the community. Therefore, increased risk of crime victimization may also explain a potential association between the mental health reform and homicides (Walsh et al., 2003).

3 Data

We employ administrative data from the Brazilian Ministry of Health. With the support of local and regional public health agencies, the Ministry of Health is responsible for managing different information systems that result in datasets containing records of deaths, hospitalizations, ambulatory procedures, and health facilities. These data are described in more detail next. We merge information across datasets using identifiers for municipalities, which constitute our unit of analysis. Municipalities are the smallest administrative units in the Brazilian political system and are the geographical level in which the policy takes place. Since some cities were created during the period, we aggregate them into minimum comparable areas.⁸ For ease of exposition, we will hereinafter refer to minimum comparable areas as municipalities.

Through the Information Access Law, the Ministry of Health also provided us with data on the implementation of each CAPS. These data cover the period between 2002 and 2019 and contain the date of opening and the type of every CAPSs active as of June 2019 created after the CAPS law was sanctioned in 2001. A few CAPSs created before this period were accredited to 2002. So, we were unable to distinguish municipalities that adopted a CAPS in 2002 from those that previously adopted. Hence, we only exploit variation from 2003 onwards. Appendix Figure A.1 depicts the number of municipalities adopting a mental health center by CAPSs' types over time, starting in 2003. The vast majority of municipalities with a community mental health center implemented a CAPS I (80%). Additionally, most of the cities adopted only one center

⁸To do so, we rely on data provided by the Institute of Applied Economic Research (IPEA).

during this period (see Appendix Figure A.2).

The Hospital Information System of the Unified Health System (SIH) provides information about hospital admissions using beds of the public health care sector. The data provide information on admissions by municipality of residence of the patient by cause, coded using the ICD-10. The whole dataset covers the period between 1995 and 2019. Also from the Ministry of Health, the Mortality Information System (SIM) provides data on deaths in the country from 1996 to 2017, including causes coded using the ICD-10. We rely on the ICD classification to identify causes of death and hospitalization related to mental illnesses and group them. Table A2 summarizes the relationship between groups and ICD-10 codes.

The Ministry of Health provides detailed data on all private and public health facilities in Brazil through the National Registry of Health Establishments (CNES). This dataset includes information about health professionals linked to some healthcare facility, including practice and levels of specialization. For this study, we select, for each municipality, the number of different mental health providers that usually constitute community mental health teams: psychiatrists, psychologists, occupational therapists, and social workers. These represent, on average, 87% of all the professionals working at the psychosocial care centers. These data also provide information regarding the number of hospital beds in each municipality. We select those that, according to the registries, are specifically used for psychiatric patients.

We use the National System of Information on Ambulatory Care (SIA) to investigate the CAPSs' effects on outpatient mental health care. Ambulatory visits may take place in any health facility that provides primary health services. For the period 1994-2019, SIA provides administrative information on all ambulatory visits funded by SUS in which medical care is provided on an outpatient basis. Severe compatibility issues limit the use of this dataset. Microdata is at the procedure level, and many procedure codes change over time. There is no direct way to make codes compatible. Hence, with few exceptions, we avoid evaluating specific ambulatory procedures. From 2008 onward, we are able to identify the type of health professional that provided the outpatient care. We then select the overall number of ambulatory services made by each of the mental health providers we are evaluating (psychiatrists, psychologists, occupational therapists, and social workers) to analyze the supply of outpatient health care.

To assess indicators of compliance with the policy, we also select "psychosocial care procedures", which are available since 2002. Those include a roll of outpatient services (medical consultations, psychotherapy, group therapy, etc.), specifically defined to be performed at CAPS. SIA also contains information on "outpatient pharmaceutical assistance", in which drugs are dispensed for patients to use at home. We select the number of antipsychotic drugs dispensed in each municipality.

We also use additional data on municipality characteristics to control for differential trends in important determinants of mental health in our estimates of the CAPSs' effects on the outcomes of interest. The Brazilian Statistical Office (IBGE) provides estimates of population for each municipality by year, and GDP for each municipality from 2002 to 2016. From the Ministry of Social Development (MDS/SAGI), we collect data on *Bolsa Família* Program (PBF) spending for each municipality.⁹ Finally, we obtained data from the Brazilian Ministry of Health on the age and gender composition of the municipalities' population.

Our main sample consists of balanced yearly data for 5,180 municipalities and covers the interval between 2002 and 2016. Table 1 provides summary statistics.

4 Study Design and Estimation Strategy

We exploit the sequential process of implementation of CAPSs starting after 2002 and adopt a difference-in-differences (DID) strategy to analyze the effects of this intervention on public mental health and mortality by assaults. In such a setting, researchers often employ two-way fixed effects regression models. There are two commonly adopted specifications. In one of them, a single treatment dummy is added to the regression. This approach has been shown to be invalid if treatment effects are heterogeneous over time or across cohorts. In particular, the linear regression coefficient may be negative even if the treatment effect is positive (Goodman-Bacon, 2018). The other widely used specification results from adding lags and leads of treatment to the regression. However, estimates from these models may not be causally interpretable (Abraham and Sun, 2018). In this paper, we follow de Chaisemartin and d'Haultfoeuille (2019) and estimate well-defined and relevant causal parameters, ro-

⁹PBF is the main conditional cash transfer policy in Brazil.

bust even if treatment effects are heterogeneous across groups or over time.¹⁰

We start defining our causal estimands of interest. Let D_{mt} denotes our treatment dummy. For our main empirical strategy, it indicates whether a municipality m gained a CAPS (of any type) for the first time in year t . We are interested in the average treatment effects across the municipalities that sequentially implemented a mental health center after 2002. That is, (m, t) cells such that $D_{mt-1} = 0$ and $D_{mt} = 1$ for any pair of consecutive time periods $t - 1$ and t . Let \mathcal{S} denotes the set of switching cells and $N_{\mathcal{S}}$ its cardinality.¹¹ One of our primary causal estimands is

$$\beta_{\mathcal{S}} := \frac{1}{N_{\mathcal{S}}} \sum_{(m,t) \in \mathcal{S}} Y_{mt}(1) - Y_{mt}(0), \quad (1)$$

where $(Y_{mt}(0), Y_{mt}(1))$ are the potential outcomes without and with treatment of municipality m at period t . $\beta_{\mathcal{S}}$ is the average treatment effect across all groups of switchers, at the time when a groups starts receiving the treatment. We are also interested in dynamic treatment effects. These parameters can be defined similarly to (1), by evaluating $Y_{mt}(1) - Y_{mt}(0)$ one time period or more after t across the treated (m, t) cells.¹² The CAPSs' implementation process across the municipalities has been taking place slowly and steadily over the years. So, we may not be able to identify longer-run effects due to compositional changes arising from the fact that late switchers will have a lot of missing post-CAPS years. For example, if municipalities selection timing is based on expected future gains, the dynamic effects for early-treated cities may not be representative for those who received a CAPS later and have missing post-CAPS data. In our primary analysis, we will look up to five post-intervention effects. About 65 per cent of our treated units had a CAPS in operation for at least 5 years. We also consider an estimand that restrict the dynamic effects only for cities that have at least five periods of post-CAPS observations.¹³

¹⁰In such a staggered design, the estimators we use are very similar to those proposed by [Abraham and Sun \(2018\)](#) and [Callaway and Sant'Anna \(2019\)](#) (for this particular case, in a specification without covariates).

¹¹In particular, $\mathcal{S} := \{(m, t) \in \{1, \dots, M\} \times \{1, \dots, T\} : t > 1, D_{mt-1} = 0, D_{mt} = 1\}$, where M is the size of our population and T denotes the last year of our panel.

¹²We shall also impose additional restrictions on \mathcal{S} . For example, $t < T$ for the treatment effects one year after CAPSs' implementation.

¹³In this case, since the composition of municipalities is the same across all event times, longer-run dynamic effects cannot be biased due to compositional changes. However, the loss of groups used to compute the dynamic effects can lead to less informative inference. See [Callaway and Sant'Anna \(2019\)](#) for an interesting discussion on compositional changes and dynamic effects.

Under a parallel trends assumption, the outcome evolution among the non-switchers can be used as the counterfactual evolution of the switchers, and a DID estimator that compares the outcome of both groups before and after the intervention can estimate average treatment effects among the switchers. We now present such an estimator. For any $t > 1$, let \mathcal{S}_t be the set of municipalities that became treated at period t . Define \mathcal{C}_t as the set of control municipalities at period $t - 1$ that did not gain a CAPS at period t . Let $N_{\mathcal{S}_t}$ and $N_{\mathcal{C}_t}$ be the number of municipalities in each set. We first define the DID estimator for the cohort of municipalities that implemented a CAPS at period t :

$$DID(t) := \frac{1}{N_{\mathcal{S}_t}} \sum_{m \in \mathcal{S}_t} (Y_{mt} - Y_{mt-1}) - \frac{1}{N_{\mathcal{C}_t}} \sum_{m \in \mathcal{C}_t} (Y_{mt} - Y_{mt-1}). \quad (2)$$

$DID(t)$ compares the evolution of the mean outcome between $t - 1$ and t in two sets of groups: the municipalities that gained a CAPS at the period t (\mathcal{S}_t), and those remaining untreated (\mathcal{C}_t). Under the assumption that the mean outcome of municipalities in \mathcal{S}_t and \mathcal{C}_t would evolve in parallel in the absence of CAPSs' implementation, $DID(t)$ estimates the average treatment effect for the switchers of period t , at the period they became treated. We can then define the estimator for $\beta_{\mathcal{S}}$, which is a weighted average of the $DID(t)$ estimators:

$$DID_M := \sum_{t=2}^T \frac{N_{\mathcal{S}_t}}{N_{\mathcal{S}}} DID(t). \quad (3)$$

de Chaisemartin and d'Haultfoeuille (2019) show that under a parallel trends assumption, DID_M is an unbiased estimator for the average treatment effect among switchers, at the time period when they switch. The estimators for the dynamic treatment effects can be defined in a similar way, by using long-differences $Y_{mt+k} - Y_{mt-1}$, for $k > 0$, instead of first-differences, provided that there are stable control municipalities in the post-CAPS periods.¹⁴

Our main estimates are based on estimators like DID_M . Additionally, we consider alternative specifications based on a generalization of DID_M , which allows for the inclusion of covariates.¹⁵ We control for determinants of mental health as local economic

¹⁴It must exist a non-empty subset $\mathcal{C}_{t+k} \subset \mathcal{C}_t$ of not-yet treated municipalities at period $t + k$. This is always satisfied in our context as there is a group of never-treated cities.

¹⁵Notice that 2 can be estimated by an OLS regression of the first differences $Y_{mt} - Y_{mt-1}$ on $1\{\mathcal{S}_t\}$. In the DID estimator with covariates, we use residualized first differences.

condition measured by GDP per capita, and the age-by-gender composition of the municipality population (the share of inhabitants within each 9-year-by-gender bracket, from 10-19 up to 79 years). We also adjust for per capita spending with *Bolsa Família* Program. Further, we adjust for state-year fixed effects. Regarding inference, standard errors are computed with 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 implemented a CAPS. This design would not be valid only if, independent of CAPSs' implementation, there were differential trends in time-varying determinants of outcomes across switchers and municipalities whose treatment is stable. This could be the case if, for example, unobserved policy changes coincided with the arrival of the CAPSs across municipalities. To deal to some degree with those issues, some of our estimates are based on the DID_M with covariates. By adjusting for GDP per capita and municipality age-by-gender composition, we control for differential trends in the changes of these health determinants, which may have coincided with CAPSs' adoption. We also consider more flexible trends according to *Bolsa Família* spending as the program started expanding across the Brazilian municipalities in the beginning of our sample.¹⁶ Finally, by adjusting for state-by-year indicators, we allow for non-parametric state-specific trends. These may be particularly relevant in the Brazilian context as various public policies – such as those related to education and public security – are at least partly determined at the state level. Reassuringly, point estimates are largely unaffected by the inclusion of those covariate specific trends, suggesting that our results are unlikely to be driven by differential trends across switchers and non-switchers. Next, we present more formal analysis that support our design validity.

One way to assess the plausibility of our identification assumption is to follow [Galiani et al. \(2005\)](#) and [Rocha and Soares \(2010\)](#) and estimate a hazard model of the probability of a municipality receiving a CAPS. We perform this estimation by modifying the data, so each municipality leaves the sample after receiving a CAPS. Then, we estimate a logit model controlling for a flexible polynomial of time, where the dependent variable is a dummy indicating that a municipality received a CAPS and the independent variables are socioeconomic variables. More specifically, we estimate three

¹⁶The results are the same if instead of adjusting for PBF spending, we control for poverty population coverage of this program in the municipality.

models considering the first, second, and third lags of the changes in our variables of interest, as well as these variables at the baseline year. We additionally selected some socioeconomic variables from the 2000 Census to use as controls.¹⁷

Our goal is to evaluate whether municipalities actually applied and received a CAPS after being subject to an economic or mental health shock, which could violate our identification assumption. Results are reported in Table A1 as marginal effects calculated on averages of the independent variables. Overall, we find that some baseline characteristics are correlated with the probability of receiving a CAPS. Municipalities with greater homicide rates at the baseline, with more inequality (based on the Theil index), and more urban had a greater probability of receiving a CAPS. However, the effects are quantitatively small – the effects of one standard deviation are all smaller than two percentage points. Furthermore, and fundamental for our identification assumption, we don't see any correlation between receiving a CAPS and past shocks on mental health indicators or income. This provides further evidence of the validity of our empirical strategy.

More generally, we can estimate treatment effects for the treated (m, t) cells using pre-CAPS periods $t' < t$, to judge directly the plausibility of the underlying parallel-trends assumption defining our DID design. [de Chaisemartin and d'Haultfoeuille \(2019\)](#) propose a placebo estimator, DID_M^{pl} , that compares switchers and non-switchers before the switchers switch treatment. The placebo estimator for the year before the CAPSs' introduction, for example, compares the outcome's evolution from $t - 2$ to $t - 1$, in municipalities that switch and do not switch treatment between $t - 1$ and t . $E[DID_M^{pl}] = 0$ under basically the same assumptions that guarantee that $E[DID_M] = \beta_S$.¹⁸ Following the same logic of the dynamic estimators, we can also estimate more distant placebo effects, provided that there are stable municipalities. In our baseline results, we estimate five placebo effects. Nearly all outcomes we evaluate display no pre-trends.

¹⁷We consider lagged changes in rates (per 10,000 people) of: hospitalizations related to mental & behavioral disorders; deaths related to mental & behavioral disorders; deaths related to self-inflicted injuries; deaths of despair; homicides; and $\sinh^{-1}(\text{GDP per capita})$. For the independent variables at the baseline, we consider the values of the same variables in 2003 as well as some variables we get from the 2000 Census: the Theil index, the shares of illiterate people, poor people, and people living in rural areas in the municipality.

¹⁸We also need the existence of stable groups to estimate the placebo effects.

5 Main Results

We present our main results in graphical form, plotting together in the same figure the estimated dynamic treatment effects using the DID_M estimator, the estimated placebo treatment effects using the DID_M^{pl} estimator, and respective 95% confidence intervals computed with a municipality-level clustered bootstrap. We also present in the same figure, two alternative specifications. First, we include non-parametric state-specific trends. Further, we adjust for GDP *per capita*, PBF spending, and the age-by-gender composition of the municipality population.

The different sets of results are presented in three subsections. In the first, we present the relation between CAPSs' implementation and indicators of access and utilization of outpatient mental health care. In the second, we present the CAPSs' effects on mental health measured by hospitalization and mortality caused by mental illness and behavior disorders, suicide, and substance abuse. In the third and final subsection, we present the relation between CAPSs' opening and homicide rates.

5.1 Access and Utilization of Mental Health Care

5.1.1 Psychosocial Care Procedures

Before proceeding to our main results, we first use the proposed estimation strategy to present the relation between the CAPSs' introduction and psychosocial care procedures, which include a roll of outpatient services delivered at these centers. Figure 2 plots the event-study results for this relation. Although such effects should be mechanical, they provide evidence of timing and compliance with the policy. Point estimates indicate that psychosocial care procedures jump right after the introduction of CAPSs in a municipality and then rise steadily. This evidence suggests a wide use of these centers by the local population as soon as they are implemented. A summary parameter defined by the average of the event-specific estimates indicates that CAPSs delivered yearly 461 (s.e. 13) procedures by 10,000 people.

We can also characterize the type of care provided at CAPSs, using a restricted sample. Until 2012, most of the provided care were labeled as non-intensive – for patients needing monthly assistance, semi-intensive – for weekly visits, and intensive – for patients with almost daily care. Appendix Figure A.3 shows that during the period

2002-2012, most visits took place on a monthly basis (an average of 234, s.e. 8, procedures by 10,000 people), followed by weekly care (153, s.e. 6), and then daily care (84, s.e. 3). Until 2012, psychosocial care procedures were also registered alongside with an ICD-10 (Chapter V). Appendix Figure A.5 shows that most CAPSs' patients had schizophrenia and mood disorders, which consists basically of bipolarity and severe depression.

5.1.2 Mental Health Practitioners

We start by showing the policy effects on the supply of mental health practitioners. Figure 3 plots such results. Estimates presented in panel (a) indicate a remarkable increase in the supply of psychiatrists that precedes the CAPSs' introduction and reaches its peak one year after the policy's beginning, declining slowly after that. The treatment effects estimate for the year before the policy implementation indicates a statistically significant increase of 0.07 psychiatrists per 10,000 people, or a 25 percent increase compared to the average in period -5 for the treated. One year after the CAPSs' introduction, treatment effects are almost three times higher: 0.19 points (70 per cent). Treatment effects decay monotonically after that, reaching 0.11 points by year-5. We also estimate the relation between CAPS and other mental health providers that usually constitute community mental health teams: psychologists (panel (b)), occupational therapists (panel (c)), and social workers (panel (d)). Overall, the pattern is similar to what we have found for psychiatrists. CAPSs' effects on the supply of these professionals are significant and high in magnitude. As before, there are anticipation effects, most marked at the year before the establishment of CAPSs. Treatment effects then rise until one year after the beginning of the intervention. The number of psychologists per 10,000 inhabitants increased by 0.26 (35 per cent) in the first year after CAPSs' adoption. Point estimates decline until the fifth year, reaching treatment effects of 0.11–0.22 (15–30 percent), depending upon the specification. One year after the program, CAPSs' effects on the rates of occupational therapists and social workers are 0.09 (60 per cent) and 0.27 (61 per cent), respectively. They remained constant in subsequent years.

Overall, our results indicate that the CAPSs' implementation in a municipality represents a large increase in the local supply of mental health providers. This may be par-

ticularly important for small municipalities, which lack an appropriate supply of such professionals. Moreover, the results are in line with the best practices recommended by researchers regarding the supply of community-based mental health care. Studies argue that this type of care should rely heavily on human resources, and should be based on a multidisciplinary team (e.g., [Thornicroft and Tansella \(2004\)](#)).

The psychosocial care centers take some time to be built, and local governments are expected to hire new professionals in advance to work in the centers when they start operating. We have some anecdotal evidence from private conversations with municipality health officials that this indeed frequently happens. This practice is probably the reason behind the anticipation effects we have found. In line with this channel, we show next that overall outpatient procedures made by mental health providers increased only after the CAPSs' introduction. This is also indicative that the practice of hiring professionals in advance shall not cause differential pre-trends in our primary mental health outcomes.

5.1.3 Outpatient Care

After confirming that CAPSs led to a substantial increase in the supply of mental health practitioners, we turn to the analysis of outpatient care made by these professionals, which constitute our main indicator of the CAPSs' effects on the usage of community-based mental health care. These results are shown in [Figure 4](#). In the pre-CAPS period, estimated treatment effects provide no evidence of differential trends across treated and untreated areas. One year after the CAPSs' introduction, the number of outpatient procedures made by mental health providers increased remarkably, attaining treatment effects of 132 (197 per cent) for psychiatrists, 75 (66 per cent) for psychologists, 18 (78 per cent) for occupational therapists, and 34 (94 per cent) for social workers.¹⁹ The gap in the number of ambulatory procedures made by psychiatrists decreased in the subsequent years. Contrary, the gap increased for the procedures made by psychologists, occupational therapists, and social workers. Overall, our results indicate that, despite any potential substitution effects within local outpatient mental health care, the overall number of procedures increased remarkably after

¹⁹In parenthesis, we present the effects relative to the average within the treated in the pre-CAPS period.

the CAPSs' introduction in a municipality.

As most of the ambulatory procedure codes change over time, we avoid evaluating specific procedures related to mental health. There is one exception: therapeutic workshops, whose primary goal is to reinsert patients with mental and behavior disorders into social life. The workshops are taught by professionals with complete college and involve activities like craft, music, dance, among others. This kind of procedure can be delivered at any primary-care health facility. Figure Appendix Figure A.5 presents the relation between CAPSs' opening and therapeutic workshops. In the subsequent years after the CAPSs' introduction, therapeutic workshops increased by approximately 0.7 per 10,000 inhabitants, which is equivalent to an 60 percent increase compared to the average in the pre-CAPS period. This result is consistent with one of the CAPSs' goals, which is to provide more humane mental health treatment.

Finally, we analyze the relation between CAPSs' introduction and the dispense of antipsychotic drugs. These medications are mostly used to treat schizophrenia, but they may also be suited for other diseases that cause psychotic episodes. Figure 5 presents our event-study plots. The number of dispensed antipsychotic medications (per 10,000 people) increased steadily in treated areas one year after the CAPSs' introduction compared to control municipalities. Estimates are less precisely estimated for the last years. But, even if we consider the lower bound of the 95%-confidence-interval, results indicate that by year-5, CAPSs increased the rate of dispensed antipsychotic drugs by at least 7 points, or 175 per cent compared to a pre-CAPS mean of 4 drugs per 10,000 people. As medical therapy is one of the most common treatments within mental health ambulatory care, this is another evidence consistent with CAPSs increasing utilization of outpatient mental health care.

5.2 Hospital Admissions and Mortality

Consistent with a new emphasis on community-based services to provide mental health care after the psychiatric reform, municipalities constructing a CAPS may have closed psychiatric beds. Such a reaction could be behind any potential effects of the policy on hospitalization outcomes. Figure 6 presents the relation between CAPSs' opening and psychiatric beds. There is no evidence of differential trends in the number of psychiatric beds neither before nor after the CAPSs' introduction.

We then turn the analysis to the CAPSs' effects on hospital admissions due to mental and behavioral disorders. In Figure 7, we do observe a clear tendency for sharp reductions in mental hospitalization rate upon the arrival of CAPSs. In the first year after the CAPSs' establishment, the admissions rate decreased by 0.9 points in treated areas compared to control municipalities, or by 7.2 percent from the pre-CAPS mean. In the subsequent years, the point estimates are marginally lower. Still, the average effect over the post-CAPSs period points to a yearly reduction of 0.64 (s.e. 0.29) – 0.8 (s.e. 0.27) hospitalizations by 10,000 people, depending upon the specification. Figure 8 indicates that the CAPSs' introduction is mostly associated with reductions in long-stay hospitalizations (> 30 days). Differently from overall hospitalizations, the reduction of long-stay admissions is less pronounced in the short run, but the treatment effects are monotonically increasing over time. Our evidence indicates that community-based services introduced by the centers may have shifted patients away from hospitals, especially those who, otherwise, would be hospitalized for an extended period.

Figure 9 examines hospitalization results by different groups of causes. Panel (a) suggests that reductions in hospital admissions due to schizophrenia primarily drive the CAPSs' effect on hospitalization rates. These rates decreased by 0.6 points (11 percent) in the first year, and remained nearly constant after that. For the other groups of causes, there are very few statistically significant and negative effects. After CAPSs' introduction, there seems to be a tendency for hospitalizations due to mood disorders (panel (b)) to decline in treated areas. However, treatment effects are less precisely estimated depending upon the specification.

Figure 10 presents the CAPSs' effect on mental health measured by deaths caused by suicide (panel a), alcoholic liver disease (panel b), overdose (panel c), and mental and behavioral disorders (d). The estimates, in general, indicate no effect. Breaking down suicides and mental disorders by groups of cause and using a broader definition of alcohol-related mortality do not reveal any new evidence. One may consider that mortality is an extreme outcome in our setting. So, considering the variability of the measures, it can be that the effects exist, but are too small to be detected. In the next section, we will show that the centers specifically designed to deliver substance abuse treatment (CAPSs AD) reduced mortality by alcoholic liver diseases.

Our results indicate that public policies aimed at providing community mental

health care can be effective at reducing hospital admissions due to mental illness without increasing mortality outcomes. In our context, the reduction in hospitalizations was driven by individuals with schizophrenia and related disorders, who are usually high users of inpatient services (Madianos and Economou, 1999). This is consistent with our previous results, which indicated a frequent usage of Psychosocial care services among schizophrenic individuals and pointed to an increase in the drugs dispensed to treat such disorder. Additionally, our results were driven by the reduction of long-stay hospitalizations, rather than sporadic inpatient admittance. Therefore, it seems that CAPSs shifted health care for severe mental disorders from the inpatient level to the community. This can be seen as a positive result of the policy since researchers indicate that community mental health care provides more humane treatment for patients. Moreover, this kind of treatment is usually cheaper. However, a recurrent concern is that the increased presence in the community of severe mentally ill persons that, otherwise, would be hospitalized may positively affect local criminality. We investigate this in the next section..

5.3 Homicides

We now assess whether CAPSs affected homicide rates. Figure 11 presents the results on mortality by assault. Estimates indicate that before CAPSs' introduction, treated and control municipalities had very similar trends in homicide rates. Then, the creation of CAPSs is associated with an increase in homicides. Considering the specification without controls, we find that one year after the CAPSs' establishment, homicide rate increased by 0.16 points in treated areas compared to control municipalities, or by 8 percent compared to a pre-CAPS mean within the treated of 1.9 deaths per 10,000 people. Treatment effects rise to 0.36 (18 percent) by the fifth year. Estimated effects are marginally smaller when we control for state-specific trends (0.23, or 12 per cent, by year-5). Further including controls does not change much our results.

Under the assumption that the only channel by which CAPSs affect mortality by assault is through de-hospitalization, the ratio between the CAPSs' effects on homicides and the CAPSs' effects on hospital admissions due to mental illness estimates the impacts of de-hospitalization on homicides induced by CAPSs. Considering the specification with controls and state-specific trends, the average effects of CAPSs on

mental health hospitalizations is -0.71 (s.e. 0.22), while the effects on homicides is 0.12 (s.e. 0.04). This indicates that every 10 de-hospitalizations per 100,000 inhabitants generates approximately 1.7 homicides. Such a ratio remains nearly the same (19%) if we focus exclusively on admissions due to schizophrenia.²⁰

This estimate is quantitatively similar to evidence found in the literature, based on follow-ups of discharged individuals from mental hospitalizations. Using U.S. data and following mental patients during their first year after discharge from the hospital, [Steadman et al. \(1998\)](#) found that the 1-year aggregate prevalence of violence among them varied between 20 and 40 percent, depending upon on the diagnoses.²¹ Similarly, [Belfrage \(1998\)](#) found rate of 40% criminality among individuals with schizophrenia, affective psychosis or paranoia, in a ten-year follow-up of patients who were discharged from mental hospitals in Sweden. Our estimate is also consistent with papers reporting exclusively violent crimes. Using Israeli data, [Fleischman et al. \(2014\)](#) found that among 3,187 discharged schizophrenic patients, 656 (20%) were later convicted by at least one crime, with 73% of them (480) being involved in a violent crime. Based on a Swedish sample, [Fazel et al. \(2009\)](#) show that the proportion of individuals with schizophrenia committing violent crimes (in a post diagnosis period) is 13.2%. Similarly, [Link et al. \(1992\)](#) report that among former and new hospital mental health patients in the U.S., the proportion of individuals hurting someone badly is 17 and 19 percent, respectively. Our evidence is also in line with several other researches reporting the prevalence of violent behavior in samples of severe mentally ill. Using data from the Australia, Sweden, U.K., and U.S., studies report similar rates, in the 20 – 40 percent range ([Swanson et al., 2006, 2004](#); [Wallace et al., 2004](#); [Belfrage, 1994](#); [Hodgins et al., 2007](#); [Brekke et al., 2001](#); [Monahan et al., 2001](#); [Hodgins et al., 2007](#)).

Our estimate could also be consistent with the elevated rate of victimization experienced by persons with severe mental illness reported by the literature ([Walsh et al., 2003](#); [Silver et al., 2005](#); [Hodgins et al., 2007](#); [Teplin et al., 2005](#)). Using U.K. data, [Walsh et al. \(2003\)](#) show that the prevalence of violent victimization among schizophrenic patients is 17 percent. If this is the main driver of our result, we should expect a similar

²⁰The average effect of CAPSs on hospitalization due to schizophrenia (0.62, s.e. 15) is nearly identical to the average effect on overall hospitalization rate.

²¹They also show that over the course of the year violence decreased for some individuals, but not for those with a diagnosis of major mental disorder (schizophrenia and other psychoses), who did not also have a diagnosis of substance abuse.

characterization of the individuals who drive the effects on both hospital admissions and homicides. To shed light on this possibility, we break down our dependent variables by individual characteristics available in both datasets: age and gender. Table A3 in the Appendix presents such heterogeneous results for homicides (panel a), hospitalizations due to mental disorders (panel b), and hospitalizations due to schizophrenia (panel c). In the first column, we present our main results. Then, we first restrict the analysis to male homicides/hospitalizations (column 2). Later, we break down the outcomes based on two age categories: 15 – 39 and more than 40 (columns 3 and 4). Finally, within the age bin 15 – 39, we again restrict the analyses only to men. We present the average effects on all these dis-aggregations.

Table A3, column (2), points to the prevalence of men as victims of homicide (96% of the overall effects). Such composition mimics variability from the data, which consists basically of male mortality (90%). This is also true for the hospitalization results. However, we see that 65% of the decrease in hospital admissions due to mental disorders is related to male hospitalizations. The respective proportion for schizophrenia hospitalizations is 59%. Therefore, a significant share of our results is explained by a decrease in hospital admissions of women, not represented in the mortality data. The major difference, though, relates to heterogeneous effects by age bin. The effect on homicide is entirely driven by the death of people between 15 and 39 years old. Differently, for hospital admissions the heterogeneous effects based on both age categories (Age 15 – 39 and Age > 39) are statistically significant and contribute quite similarly to the decrease of hospitalizations. By restricting the analysis by age and gender, we can see that the increase in homicides following the CAPSs' introduction is fundamentally driven by violence against prime-aged men (96%). While CAPSs affected negatively mental hospitalizations of prime-aged men, this explains only one third of the CAPSs' effects on de-hospitalization. Therefore, it is unlikely that increased victimization is the main mechanism behind our results on homicides.

Overall, we find a steady and robust increase in homicide rate after the roll-out of CAPSs across the Brazilian municipalities. As previously discussed, our results may indicate that a significant share of mentally ill individuals not institutionalized end up getting involved in homicides. This is consistent with the pattern of our results on hospital admissions. First, the results were driven by long-stay hospitalizations,

which may have an incapacitation effect. Second, the results were more pronounced among patients with a severe mental disorder – schizophrenia – usually associated with violent behavior. Still, the average effect of CAPSs on homicides is just modest: -0.14 deaths by 10,000 people, or 7 percent relative to the pre-CAPS mean.²² This is also consistent with studies from the literature, which highlight that although persons with mental disorders are at increased risk of committing violent crime, the proportion of total violence attributed to this group is quite small (e.g., [Walsh et al. \(2002\)](#)).

6 Heterogeneity by CAPSs' Types

As previously commented, most of the CAPSs opened in Brazil are of the smallest type (CAPS I). Hence, the results presented so far are driven by the effect this specific type has on outcomes. Next, we analyze potential heterogeneities over the CAPSs' types.

6.1 CAPSs I and II: Replicating Previous Results

CAPSs II offer the same type of care from CAPSs I. They are simply target for larger municipalities, and thus have greater teams. Therefore, we do not expect major changes in this case, except there is significant heterogeneity according to population size. CAPSs III additionally deliver night care for patients needing monitoring. But, as very few municipalities implemented a CAPS III, we do not have enough variation to estimate its effects. Therefore, we will only explore variation coming from the adoption of CAPSs I and CAPSs II. Only 5 per cent of the cities that implemented a CAPS I have also implemented other centers. For CAPS II, though, some larger municipalities additionally adopted other types of CAPSs. To explore variation coming only from the creation of CAPSs II, we adjust all our specifications for the implementation of other centers. Dropping the cities that received more than one type of CAPSs leads to very similar results.

Figure [A.6](#) presents the effects of CAPSs I and CAPSs II on overall and long-stay mental health hospitalizations. For both CAPSs' types the treatment effects are negative, although the dynamic of the results is different for overall admissions (panels

²²When put in perspective to externalities from other Brazilian policies on homicides – such as the trade liberalization ([Dix-Carneiro et al., 2018](#)) and the transition of a market from legal to illegal ([Chimeli and Soares, 2017](#)) – our results have much lower magnitude.

a and c). For CAPSs I, the reduction in hospital admissions is immediate: -0.7 by 10,000 people, or 6 per cent in comparison to the baseline mean. One year later, treatment effects are a bit higher ($0.77 - 0.91$, depending upon on the specification). After that, treatment effects begin to decrease toward zero. For CAPSs II, treatment effects are lower in the beginning of the policy but are monotonically increasing over time. By year-5, the reduction in hospitalization reaches, according to the specification with controls and state-specific trends, 1.2 points, or 7.5 per cent in comparison to the baseline. In both cases, the decline in hospitalizations is driven by long-stay admissions (panels b and d). For CAPSs I, though, such composition is much more pronounced.

Figure A.7 presents the results dis-aggregated by cause and depicts a pattern already presented in the previous section. The policy significantly reduced hospitalizations due to schizophrenia. Additionally, results are stable over time in all specifications we consider. Summarizing the effects by the average of the event-times effects, we find that the reduction ranges from 0.4 to 0.7 points, depending upon on the specification, for both types of CAPSs. The main difference between the effects of CAPSs I and II relates to hospitalizations due to mood disorders. For CAPSs I, we find a precisely estimated negative effect only for one year after the CAPSs' introduction. For CAPSs II, though, point estimates are systematically negative for all the event-times after the beginning of the policy. Considering the specification with controls and state-specific trends, the average of the effects indicates an overall reduction of 0.4 (s.e. 0.17) hospitalizations per 10,000 people, or 17 per cent in comparison to a baseline rate of 2.4. Finally, from panel (e) we realize that the attenuation of the effects of CAPSs I on overall mental health hospitalizations is driven by hospitalization due to substance abuse, which seem to increase some years after the CAPSs' introduction. For CAPSs II, the effects are flat over time. As smaller CAPSs may not offer treatment for substance abuse disorders, they may increase inpatient care by referring patients to hospitals.

As CAPSs I and II offer similar type of care and decreased mental hospitalization rates, we should expect, based on previous results, that both treatment variations should be associated with an increase in homicide rates. Figure A.8 indeed shows this is the case. The estimates for CAPSs I basically replicate our previous results (panel a). The point estimates are statistically different from zero from year-1 and then slightly increase over time. The average of the effects for the period 0-5 indicates an increase

of 0.09 (s.e. 0.04) homicides per 10,000 people considering the specification with state-specific trends and controls, where the average rate for the pre-CAPS I period is 1.8. For CAPSs II, the dynamic of the effects is very similar. We find that one year after the introduction of CAPSs II, homicide rate increased by 0.18 points, or by 7 per cent compared to a pre-CAPS II mean within the treated of 2.5 deaths per 10,000 people. Treatment effects rise to 0.26 (10 per cent) by the fifth year. The average dynamic treatment effect equals 0.19 (s.e. 0.06). The point estimates are nearly the same in all three specifications. These evidence provide further robustness for our previous results on homicide rates. In particular, we found similar results for different sets of treated municipalities that implemented psychosocial care centers that had similar effects on mental health hospitalization rates.

6.2 CAPSs AD: Substance Abuse Treatment

Now, we discuss a major difference between the different types of CAPSs, related to the treatment of alcohol and drug abuse. The Psychosocial Care Centers Alcohol and Drugs (CAPSs AD) are specific for such purpose. We showed in Figure A.5 that, overall, very few procedures made in CAPSs are related to psychoactive substance abuse, in comparison to other mental disorders. However, when one looks specifically to CAPSs AD, the pattern is totally different. On average, 133 outpatient services (per 10,000 inhabitants) related to substance abuse are delivered in these centers by year (see Figure A.9). Therefore, one may expect different results for this type of CAPS on substance-abuse-related outcomes. Due to the low number of municipalities getting a CAPS AD III, we only evaluate CAPSs AD. Because nearly all municipalities that opened a CAPS AD have also implemented other types of CAPSs, we cannot discard such treated cities. Still, to capture variation coming only from the implementation of CAPSs AD we control for the adoption of other centers.

To analyze whether, concerning substance abuse treatment, the implementation of CAPSs AD caused a shift from hospital care to community-based care, Figure A.10 examines the effects of CAPSs AD on hospital admissions. Most point-estimates are negative, but none is significant. The average effect over the event-times equals -0.39 (s.e. 0.36), low compared to the pre-CAPS mean of 7.4 hospital admissions. Alternatively, the provision of community services to treat substance abuse may have reduced

admissions. Still, part of this effect was mitigated as CAPSs AD can also refer patients to realize part of the treatment in hospital settings. We cannot separate these two forces using our data.

Figure 12 presents our main results: the effects of CAPSs AD on substance-abuse-related deaths. Panels (a) and (b) presents variables that constitute deaths of despair: intoxication of alcohol and drugs (intentional – suicide, or not – overdose), and alcoholic liver disease. In panel (c) we present the effects on deaths with primary cause coded as psychoactive substance abuse disorders. We found significant results for alcoholic liver disease (panel b). Estimates indicate that before the establishment of CAPSs AD, treated and control municipalities had very similar trends in the rates of deaths caused by alcoholic liver disease. Then, the creation of CAPSs AD is associated with a decrease on deaths caused by this condition. Treatment effects are monotonically increasing over time, becoming statistically different from zero from year-2: -0.06 deaths by 10,000 people, or 12 per cent compared to the average in the pre-CAPS period (main specification). By year-5 treatment effects rise to 0.09 points (18 per cent). Appendix Figure A.11 presents the results when we consider the more broad definition of alcohol-related mortality. In this case, the point-estimates are higher in magnitude, but treatment effects are the same in relative terms. Therefore, community-based treatment for substance abuse disorders, as measured by the creation of CAPSs AD, does reduce deaths by alcoholic liver disease. This is particularly important for contexts like ours where, differently from the U.S., deaths by alcoholic liver disease are much more common than other drug-induced deaths.

Independent of the disease stage, abstinence from alcohol is the cornerstone of care management among individuals with alcohol-related liver disease. For end-stage liver disease, liver transplantation is one of the only treatments available. If, somehow, liver transplantation became more accessible in the Brazilian public health system parallel the opening of the CAPSs AD, this could be behind our results. However, we show in Appendix Figure A.12 that the rates of liver transplantations did not change following implementation of CAPSs AD (panel a), neither when we analyze transplants performed only in persons with alcoholic liver disease (panel b). It is unlikely that this is a relevant channel in our context.

Finally, we re-estimate the results on deaths by alcoholic liver disease using re-

stricted samples. As CAPSs AD are target only to larger cities, it may not be appropriate to use the whole pool of control units, which is mostly composed of smaller cities. Figure A.13 presents the average effects over the non-negative event-times when we restrict the sample to municipalities above the 10th, 25th, 50th, and 90th percentile of the population distribution. For municipalities in the last decile of the distribution, for example, we are left with 279 never-treated municipalities (out of 5,161) and 270 treated cities (out of 315). Overall, the results remain remarkably stable across the different samples, both when analyze only deaths coded as alcoholic liver disease (panel a) or consider the more broader definition for alcohol-related mortality (panel b).

7 Robustness Checks

7.1 Alternative Specifications

Since homicides are relatively rare events, data may be very noisy in smaller cities, and results may be affected by possibly spurious outliers. To check whether this seems to be a concern, we adopt a few alternative specifications. Appendix Figure A.14 present such results. In one of the specifications, we place more weight into municipalities with a larger population to check whether there are relevant heterogeneous effects by population size (panel (a)). In panel (b) we discard municipalities bellow the 10th percentile of the population size distribution. In another specification, we restrict the sample to municipalities reporting positive counts of homicides in all years. (panel (c)). Finally, we winsorize data by limiting extreme values to the 5th and 95th percentiles (panel (d)). All the results obtained from these alternative specifications are quantitatively similar to our main results. As deaths due to alcoholic liver diseases are also rare events, Appendix Figure A.15 depicts a similar exercise that confirms the stability of the results under alternative specifications.

7.2 Placebo Outcomes

One concern regarding the effects we find is that the presence of CAPS could be related to other programs or socioeconomic shocks that might also affect our main outcomes. Indeed, Brazil experienced a period of significant economic growth during the 2000s

and, concomitantly, saw other important public policies like the *Bolsa Família* and the Family Health Programs be created or expand substantially. However, none of the other public policies in place during the period were directed specifically at mental health and economic shocks will not only affect mental health indicators. Hence, we can use the proposed estimation strategy to run placebo tests on other outcomes aside from mental health indicators and homicides, but related to these other policies and economic conditions, to assess whether the effects we find are due to confounders or not.

Appendix Figure A.16 presents the CAPSs' effects on placebo outcomes. Panels (a) and (b) depict that results on economic and socio-economic indicators measured by GDP per capita and PBF spending per capita.²³ We also investigate a few health outcomes. Previous research has shown that other Brazilian health policies expanded or implemented during the 2000s significantly affected inputs in the production function of infant health and infant mortality (Carrillo and Feres, 2019; Bhalotra et al., 2019; Macinko et al., 2007). Then, panel (c) investigates the CAPSs' effects on infant mortality. Following Malta et al. (2007), we also select avoidable causes of deaths due to interventions of the Brazilian Health System that do not include mental health complications. In particular, we evaluate vaccine-preventable diseases like Tuberculosis and Hepatitis B, and infectious diseases such as HIV/AIDS and influenza (panel d). Previous research has also shown that other Brazilian health policies significantly affected hospitalizations and mortality rates caused by sensitive conditions to primary care (Rocha and Soares, 2010; Fontes et al., 2018; Hone et al., 2020). Finally, in panel (e), we check whether the pattern of our results on homicides could be explained by local shocks on violent causes of death reflecting, for example, increased urbanization. To do so, we investigate another major cause of violent death: car accidents. Appendix Figures A.17 and A.18 repeat similar exercises but exploiting variation only from the establishment of CAPSs II and CAPSs AD, respectively.

Overall, we do not find any correlation between CAPSs' presence and the aforementioned variables, which further suggests that the effects we find are indeed due to the CAPS.

²³PBF spending should be based on the number of poor families in each municipality.

7.3 Compositional Effects

A potential challenge to the interpretation of our estimates, especially the duration effects, relates to compositional effects. For 35 per cent of our treated units, the data contain less than five post-intervention years. Under selective treatment timing, the dynamic effects from early-treated cities may not be representative for the longer-run effects from municipalities with missing post-CAPS data. In Appendix B, we re-estimate a subset of our results using a restricted treated-sample that includes only municipalities with at least five years of exposure to treatment. All our main results remain remarkably similar when we explore this alternative composition of treated cohorts. Estimates still indicate that CAPS increased utilization of mental health outpatient care, reduced hospitalization rates due to mental illness, but increased homicide rates. These last results are also seen for CAPSs II. Finally, CAPSs AD reduced mortality rates due to alcoholic liver disease. Taken together, evidence presented in Appendix B indicates that compositional effects do not explain our results.

7.4 Longer Event-Study

In our main exercises, we trimmed event-time plots at +5 and -5 to mitigate major changes in group composition. More distant events would explore variation from fewer municipalities. To show that there is no arbitrariness behind this choice, in the Appendix C we present our main results for an extended event study going from -8 to +8. Although for some outcomes, the estimates are noisy for more distant periods, the dynamics of the results remain the same in the long run.

8 Conclusion

This paper studies the Brazilian psychiatric reform, which reorganized mental health care provision by the public system building a network of community-based services centered on Psychosocial Care Centers (CAPSs). To identify the causal effects of this reform, we exploit municipality-level variation in the CAPSs' establishment. We find that the reform increased access and utilization of ambulatory mental health care and reduced hospitalizations due to mental and behavioral disorders. Those reductions

were more pronounced for long-stay admissions and among patients with schizophrenia. We additionally show that the centers delivering substance abuse treatment reduced deaths due to alcoholic liver diseases. Despite those positive effects, we also find an increase in homicide rates, potentially caused by the incapacitation effects long-stay hospitalizations have on 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 many countries facing significant deficits in the provision of mental health treatments ([WHO, 2013](#)) and countries developing or revising deinstitutionalization interventions, which, according to the [UN \(2020\)](#), should be a priority following the Covid-19 pandemic.

Our results support the view there are significant trade-offs to be considered when choosing the optimal delivery of mental healthcare ([Lamb, 2015](#)). While it is outside the scope of this paper to uncover the pitfalls of community care compared with other forms of care, there are a few particularities of our context worth mentioning. The deinstitutionalization process in Brazil occurred late and still needs investment. Less than three percent of Brazilian municipalities have access to mental health centers that serve the population 24 hours a day and have outpatient beds and crisis intervention services. However, such infrastructure is seen as fundamental to reducing violence by people with serious mental illness in the community ([Thorncroft and Tansella, 2013](#); [Lamb and Weinberger, 2005](#); [Dvoskin and Steadman, 1994](#)). Therefore, incorporating intensive care models into the community settings, as some countries have recently done ([Adamou, 2005](#)), can be a way of improving policy outcomes of mental-health reforms. Evaluating these interventions is left to future work.

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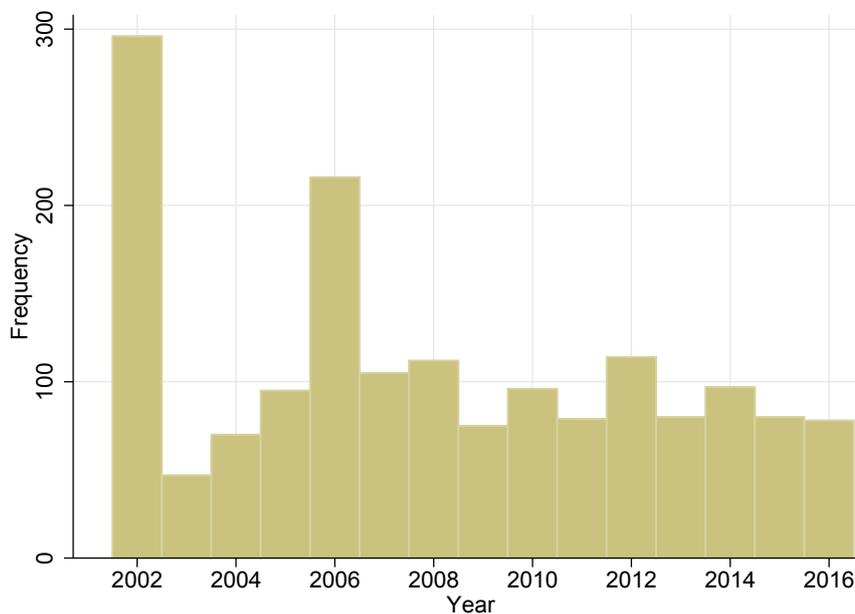
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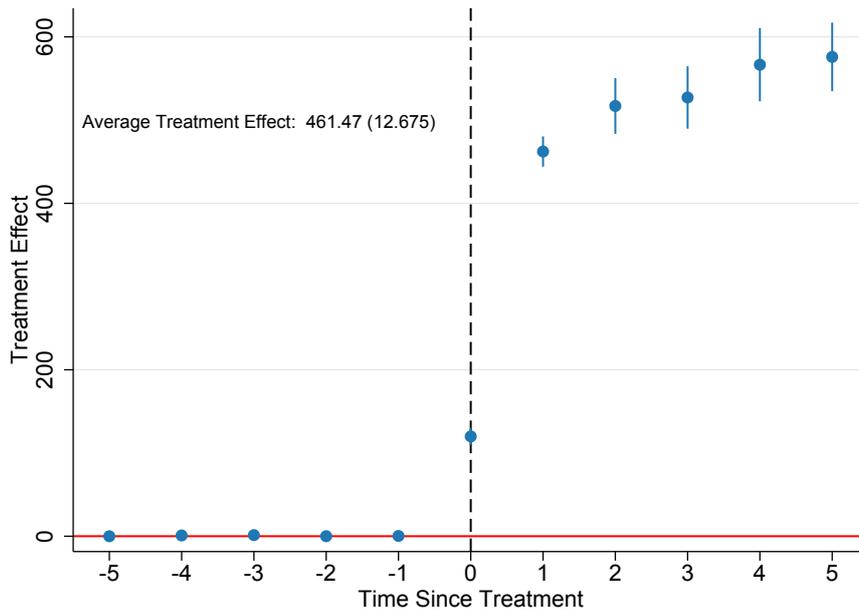
Tables and Figures

Figure 1: Number of municipalities receiving a CAPS (of all types) by year



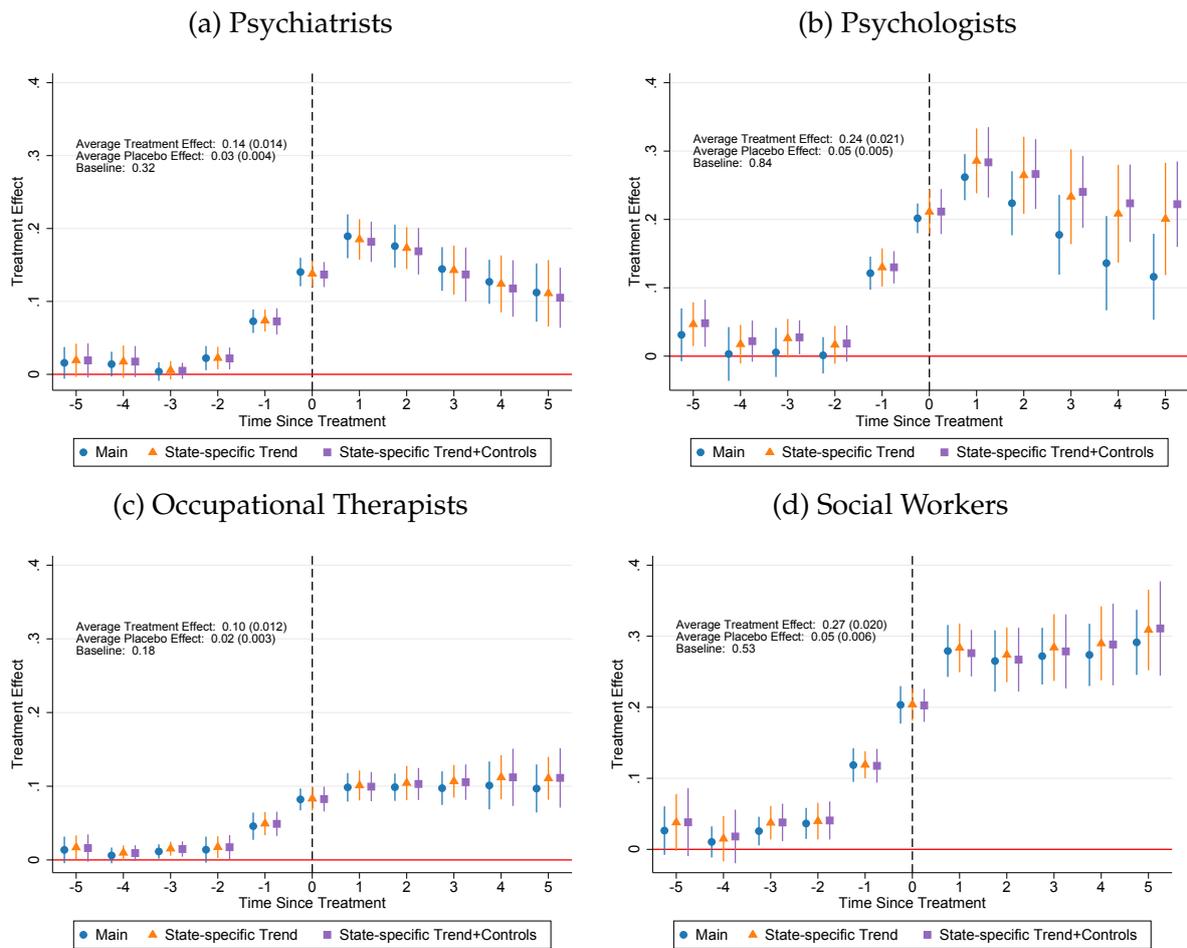
Notes: This graph plots the number of municipalities receiving a CAPS for the first time from 2002 to 2016. A number of CAPSs were created before the period and were accredited in 2002, explaining the spike in 2002. For the remaining CAPSs, the accreditation coincides with opening. This data show the first date of accreditation, or date of CAPSs' opening for the vast majority.

Figure 2: Makers of compliance: psychosocial care procedures – 02-16



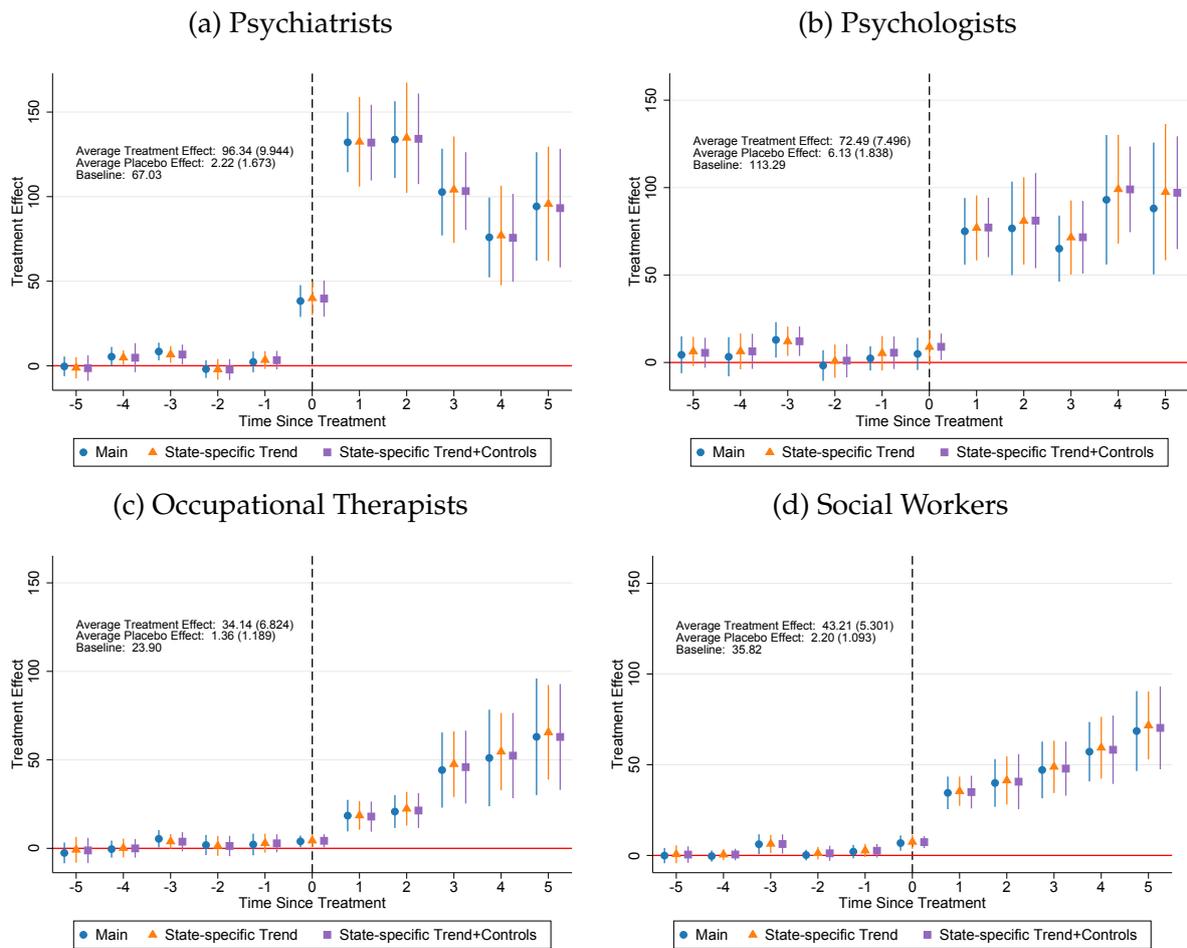
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered boot-strap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of psychosocial care procedures, which are the ambulatory services provided at the CAPSs. The Average Treatment Effect computes a simple average of the instantaneous and dynamic effects. In parenthesis, standard errors computed with a municipality-level clustered boot-strap.

Figure 3: Effects of CAPSs on mental health practitioners – 05-16



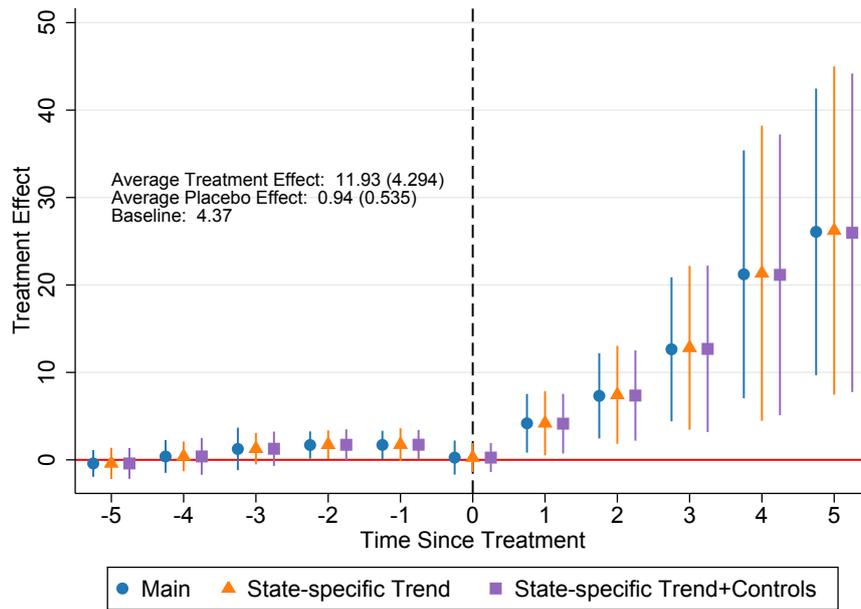
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of mental health practitioners. Results for the three different specifications considered are shown, as indicated in the graph. In the first specification, we do not include any controls. In the second specification, controls include a set of state \times year indicators. In the third specification, it further include municipality GDP per capita, PBF spending per capita, and a series of indicators for age-by-gender population bins. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 4: Effects of CAPSs on outpatient care by mental health specialists – 08-16



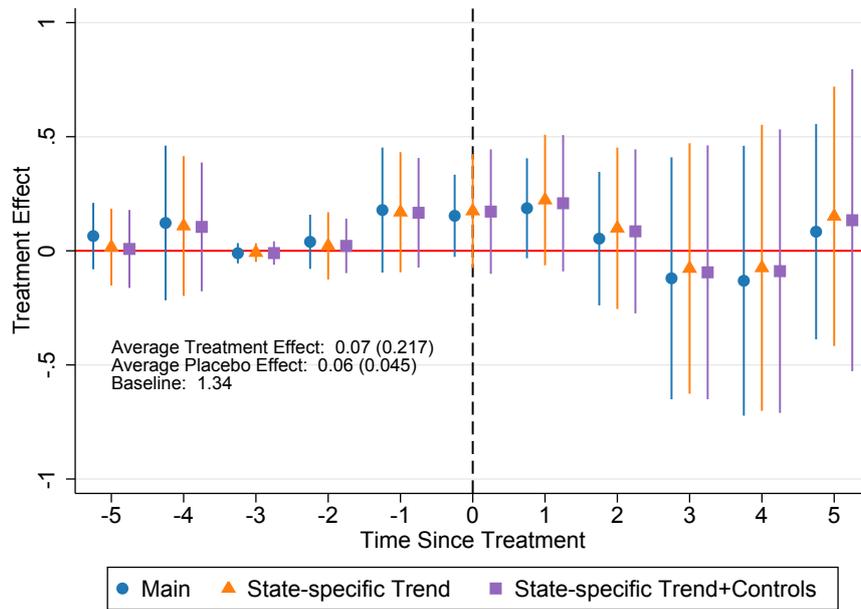
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of ambulatory procedures made by mental health providers. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 5: Effects of CAPSs on dispensed antipsychotic drugs – 02-16



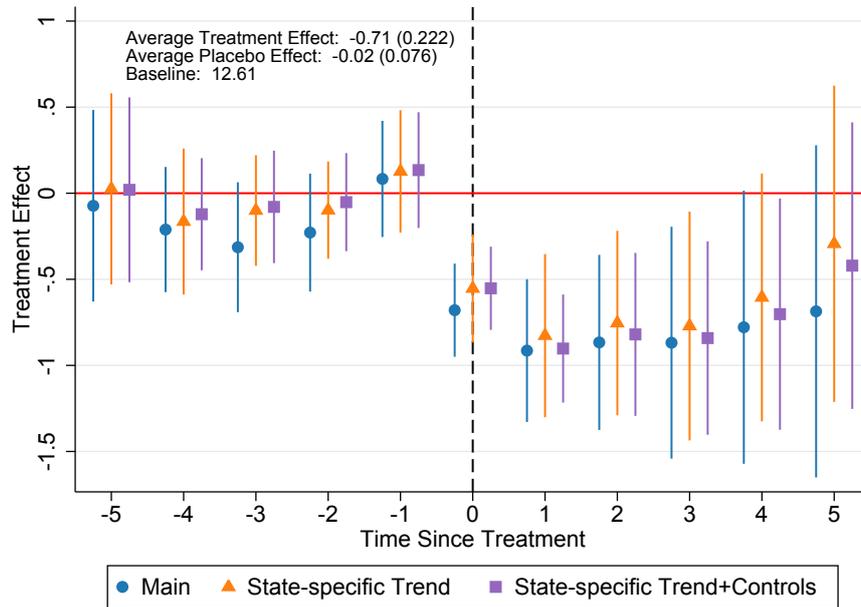
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of dispensed antipsychotic drugs. These are drugs delivered at the ambulatory level for patients, especially with schizophrenia and other psychotic disorders, for home use. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 6: Effects of CAPSs on psychiatric beds (05-16)



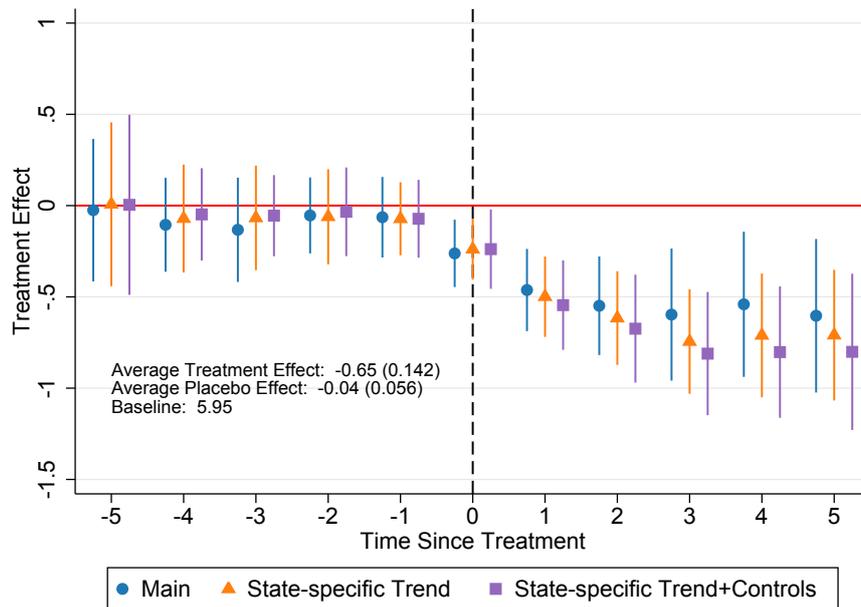
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of psychiatric beds. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 7: Effects of CAPSs on hospitalizations due to mental and behavioral disorders
 – 02-16



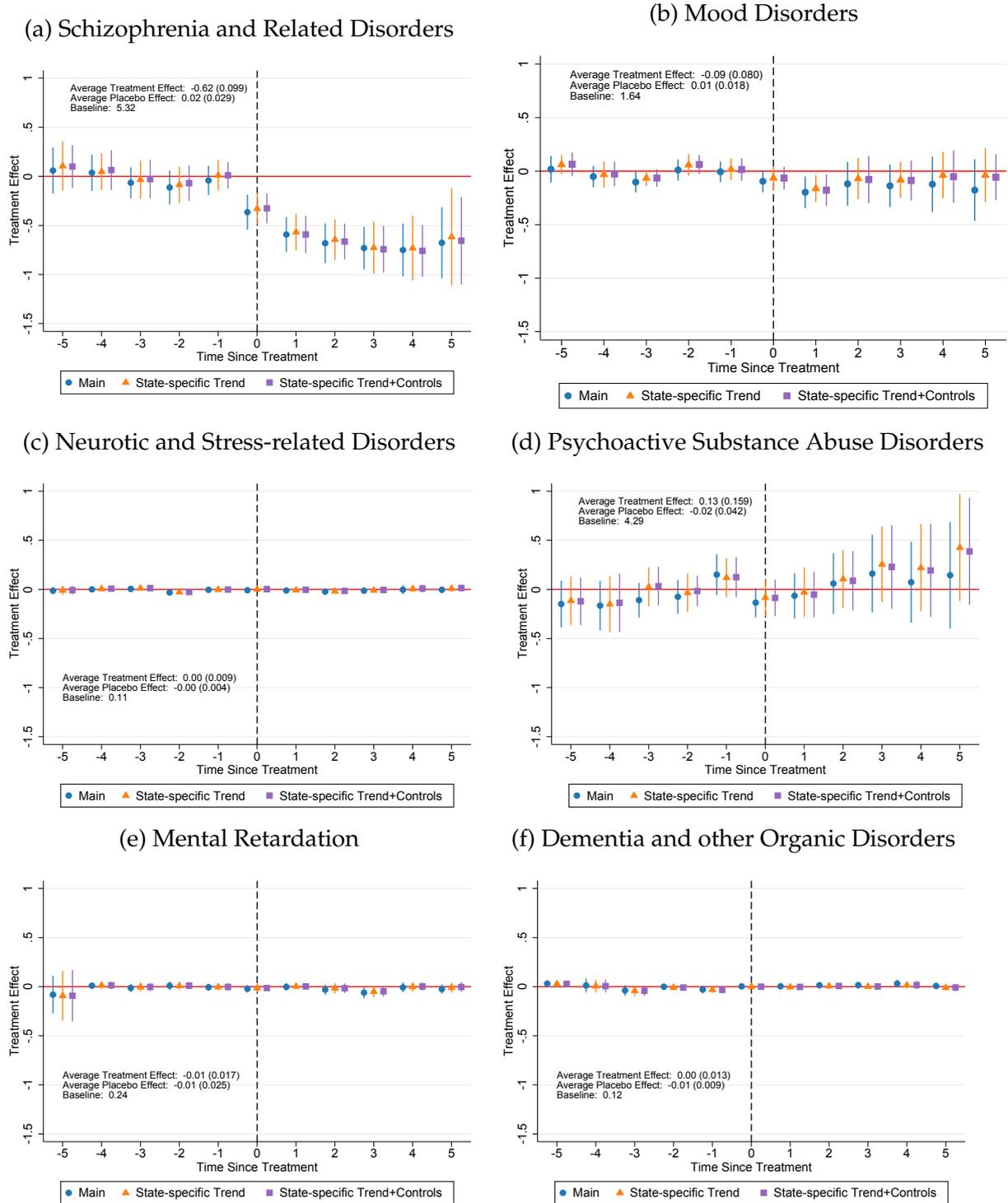
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs’ effects on the rate of hospital admissions due to mental and behavioral disorders (ICD-10 F00-F99). The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 8: Effects of CAPSs on long-stay hospitalizations due to mental and behavioral disorders (>30 days) – 02-16



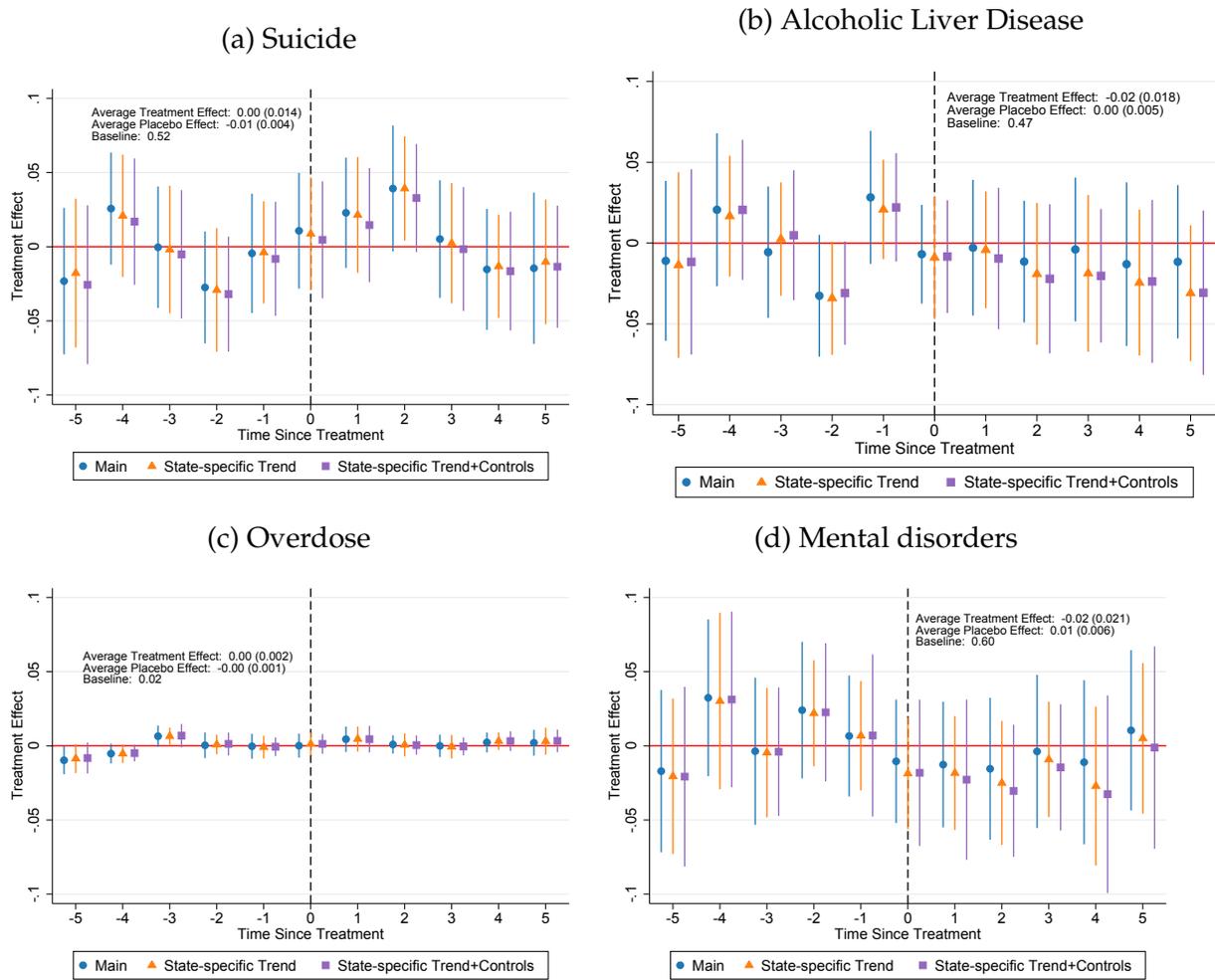
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of long-stay hospital admissions due to mental and behavioral disorders (ICD-10 F00-F99). Long-stay hospitalizations are defined as those in which the patient is hospitalized for more than 30 days. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 9: Effects of CAPSs on hospitalization by cause – 02-16



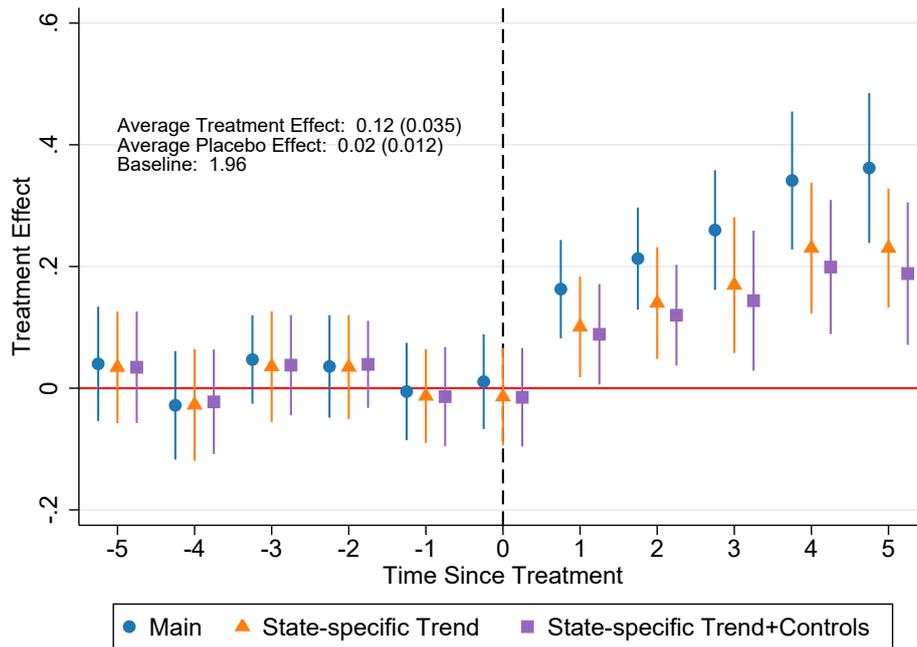
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), mood disorders (F30-F39), substance abuse disorders (F10-F19), mental retardation (F70-79), and organic disorders (F00-F09). The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 10: Effects of CAPSs on mortality by cause – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of mortality due to m suicide (X60-X84), alcoholic liver disease (K70, K73-K74), overdose (X40-X45, Y10-Y15, Y45, Y47, Y49), and mental disorders (F00-F99). The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

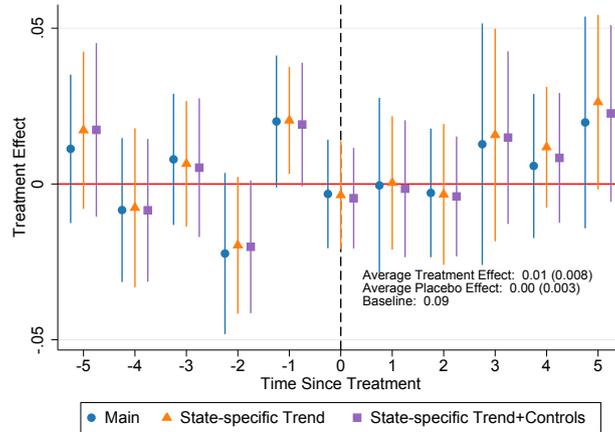
Figure 11: Effects of CAPSs on homicides – 02-16



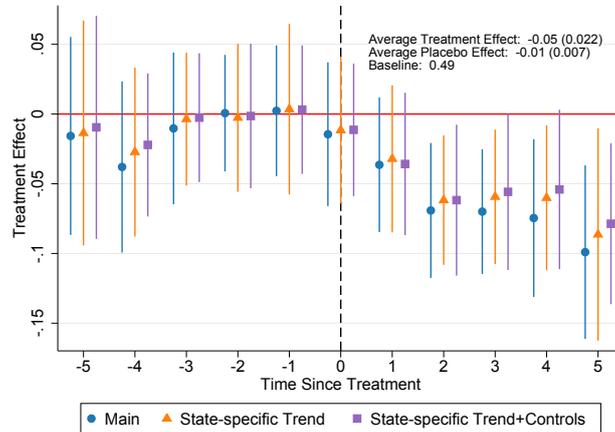
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of homicide (X85-Y09). The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure 12: Effects of CAPSs AD on substance-abuse-related deaths – 02-16

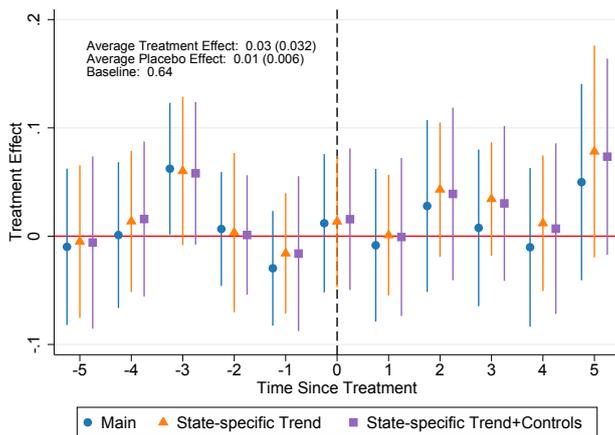
(a) Self-inflicted Poisoning and Overdose



(b) Alcoholic Liver Disease



(c) Psychoactive Substance Abuse Disorders



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS AD effects on the rate of deaths caused by self-inflicted poisoning and overdose (X60-X69, X40-X45, Y10-Y15, Y45, Y47, Y49), alcoholic liver disease (K70), and psychoactive substance abuse disorders (F10-F19). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Table 1: Descriptive Statistics – baseline year (2002, except where noted)

	All	Treated	Never Treated
<i>Hospitalizations (per 10,000 pop.)</i>			
Mental and behavioral disorders	14.89	13.90	14.89
Schizophrenia	6.59	6.46	6.54
Mood Disorders	1.70	1.50	1.73
Stress-related disorders	0.10	0.12	0.09
Psychoactive substance abuse	4.79	4.31	4.82
Mental retardation	0.22	0.21	0.20
Dementia	0.19	0.21	0.18
Others	1.29	1.10	1.32
<i>Mortality (per 10,000 pop.)</i>			
Suicide	0.56	0.45	0.60
Overdose	0.01	0.01	0.01
Alcoholic and chronic liver diseases	0.74	0.77	0.70
Mental and behavioral disorders	0.40	0.39	0.40
Homicide	1.29	1.70	1.06
<i>Outpatient Care (per 10,000 pop.)</i>			
By psychiatrists (2008)	30.84	45.44	28.47
By psychologists (2008)	77.56	67.05	79.27
By social workers (2008)	29.03	23.38	29.95
By occupational therapists (2008)	19.85	18.78	20.03
Antipsychotic drugs	0.77	0.55	0.16
<i>Mental Health Facilities (per 10,000 pop.)</i>			
Psychiatrists (2006)	0.21	0.30	0.14
Psychologists (2006)	0.86	0.65	0.89
Social workers (2006)	0.59	0.47	0.60
Occupational therapists (2006)	0.11	0.14	0.07

To be continued

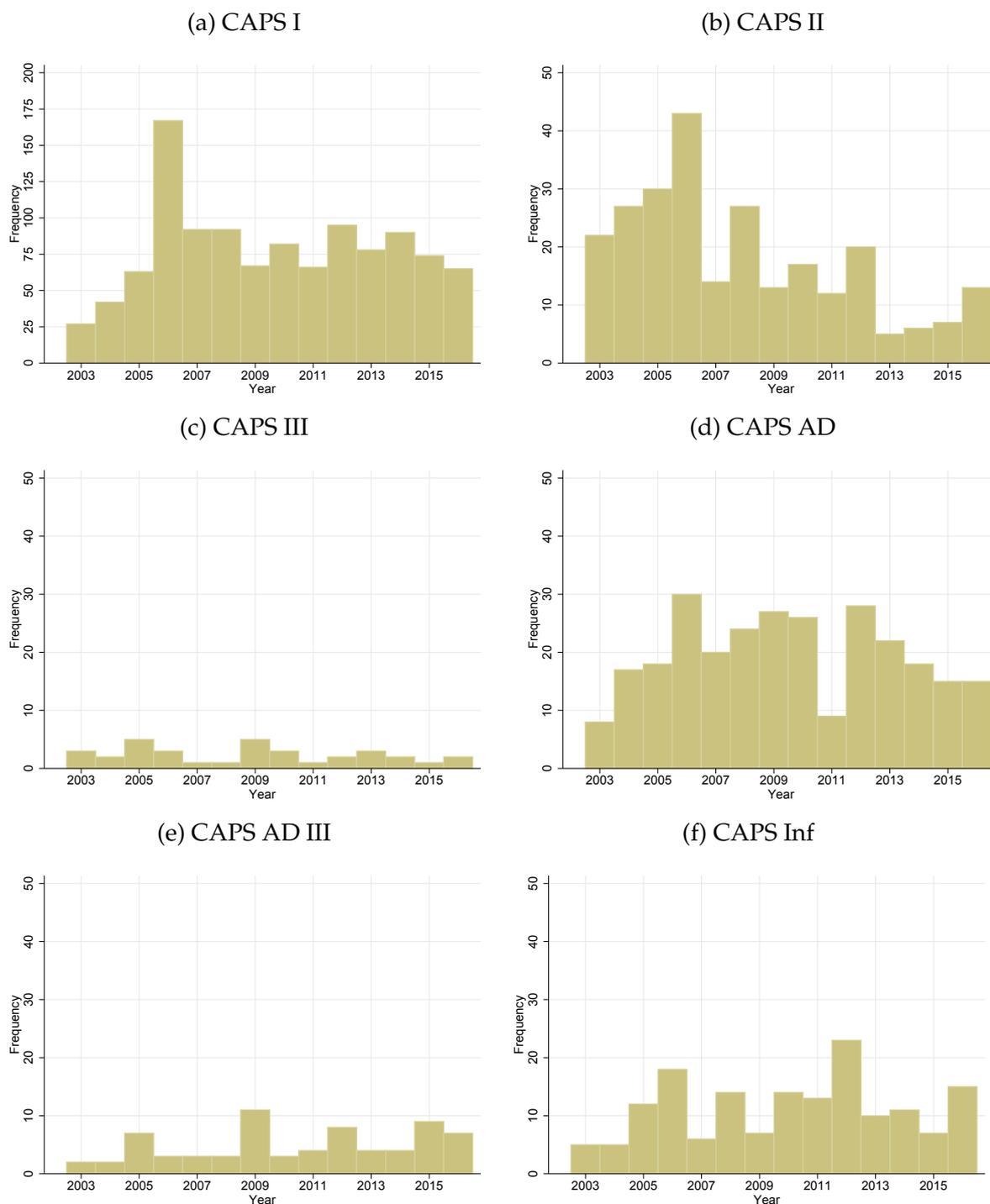
Table 1 (continued)

	All	Treated	Never Treated
Psychiatric beds (2006)	0.85	1.57	0.28
<i>Municipalities' Characteristics</i>			
Number of municipalities	5180	1344	3836
Population	31007	43884	9437
Men	0.51	0.50	0.51
Age 10–19	0.19	0.20	0.19
Age 40–49	0.12	0.12	0.12
Age 70–79	0.04	0.03	0.04
PBF per capita	4.02	4.08	4.05
GDP per capita	2.26	2.28	2.22

Notes: All tabulations refer to the baseline year (2002), except where noted. Treated includes the cohorts of municipalities that implemented a CAPS for the first time in the period 2003-2016. Men, Age 10–19, Age 40–49, and Age 70–79 represent the fraction of the population that are men, and the fraction within each age bin (10-19, 40-49, and 70-79).

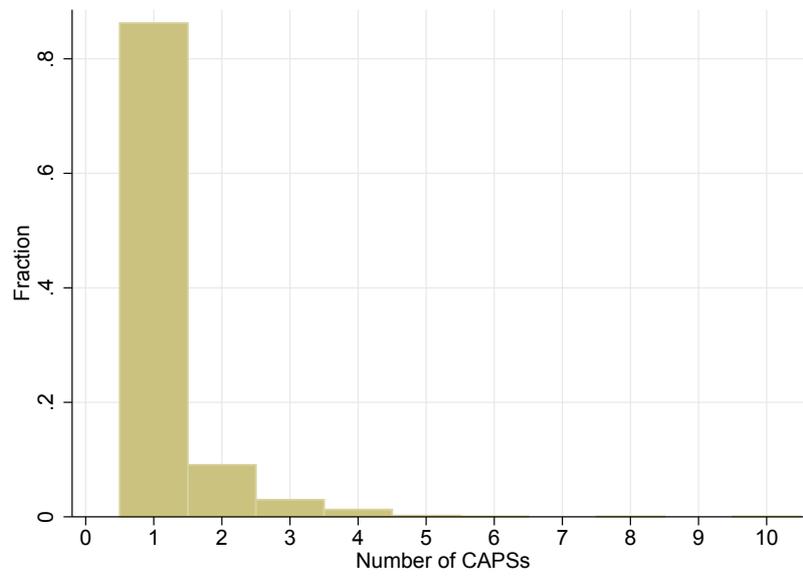
A Additional Figures and Tables

Figure A.1: Number of municipalities adopting CAPSs (by CAPSs' types) by year



Notes: These graphs plot the number of municipalities receiving a CAPS for the first time, by CAPSs' type, from 2003 to 2016. Due to the discrepancy between the number of municipalities receiving a CAPS I and other types, panel (a) uses a different scale. We omit 2002 as we cannot distinguish municipalities that got a CAPS in 2002 from those that got earlier. This data show the first date of accreditation, or date of CAPSs' opening for the vast majority.

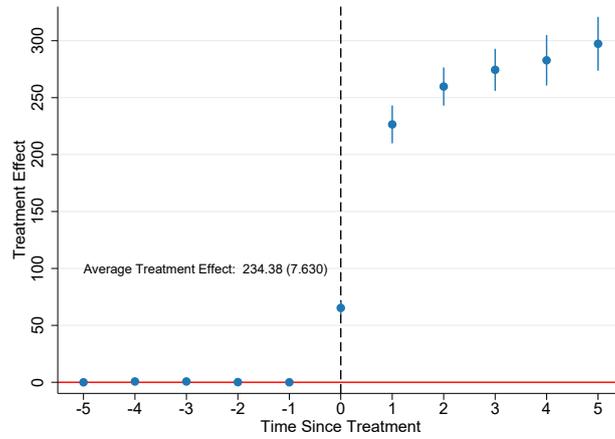
Figure A.2: Fraction of treated municipalities by the number of adopted CAPSs



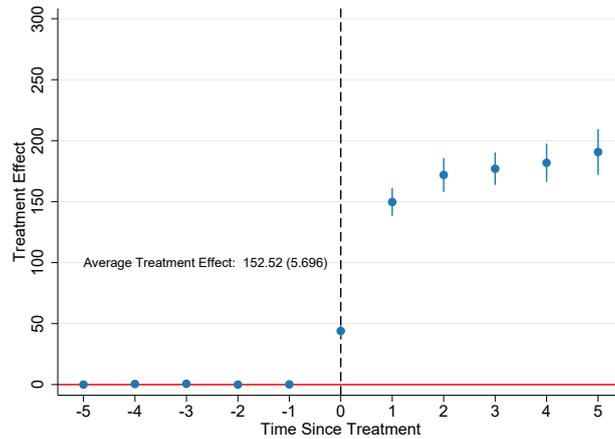
Notes: Among the municipalities that have adopted a CAPS starting in 2003, this graph plots the fraction by the number of opened centers.

Figure A.3: Markers of compliance by type of care– 02-12

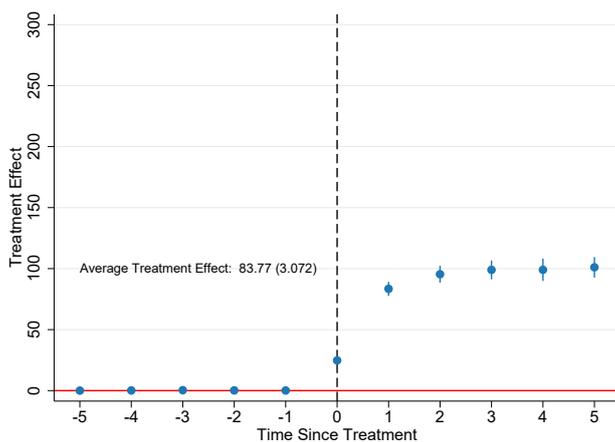
(a) Non-intensive care (monthly visits)



(b) Semi-intensive care (weekly visits)

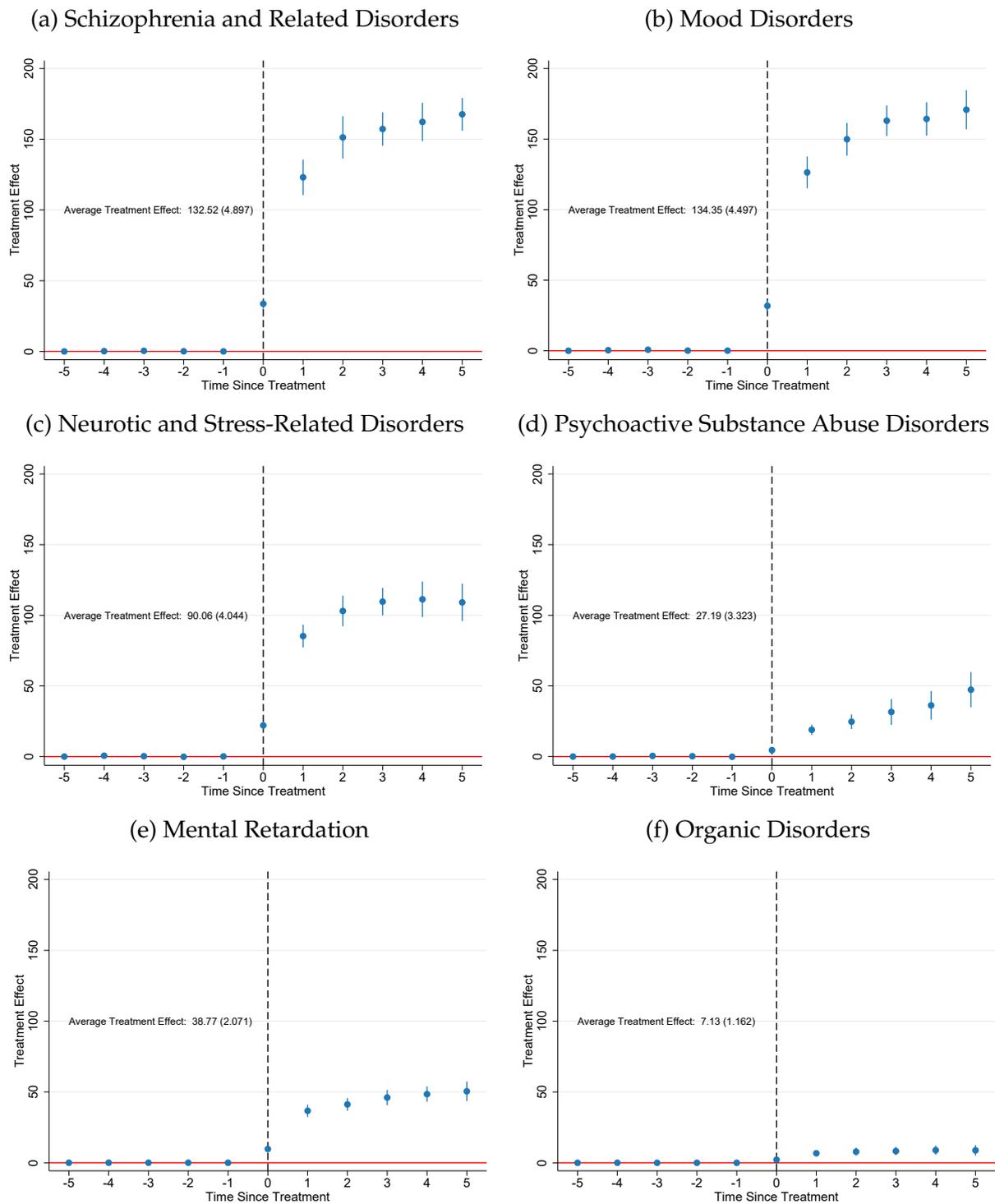


(c) Intensive care (daily visits)



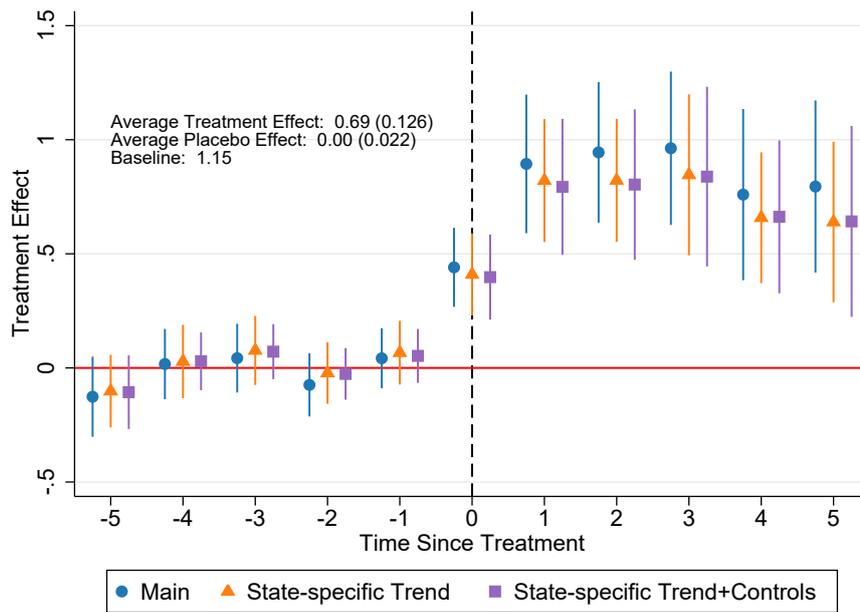
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of psychosocial care procedures by type of care. Until 2012, the provided care were labeled as non-intensive – for patients needing monthly assistance, semi-intensive – for weekly visits, and intensive – for patients with almost daily care. The Average Treatment Effect computes a simple average of the instantaneous and dynamic effects. In parenthesis, standard errors computed with a municipality-level clustered bootstrap.

Figure A.4: Markers of compliance by groups of cause– 02-12



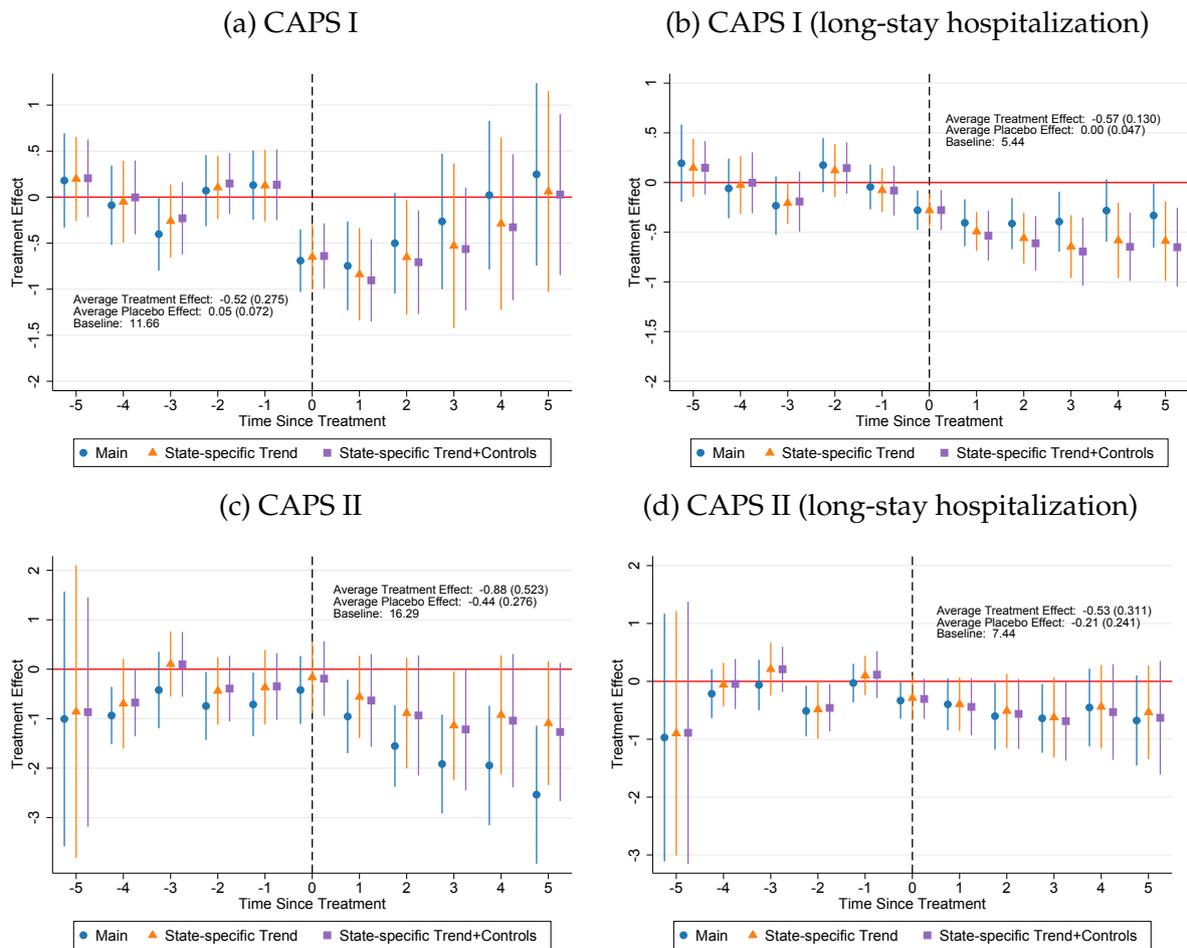
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of psychosocial care procedures by groups of cause. The Average Treatment Effect computes a simple average of the instantaneous and dynamic effects. In parenthesis, standard errors computed with a municipality-level clustered bootstrap.

Figure A.5: Effects of CAPSs on therapeutic workshops – 02-16



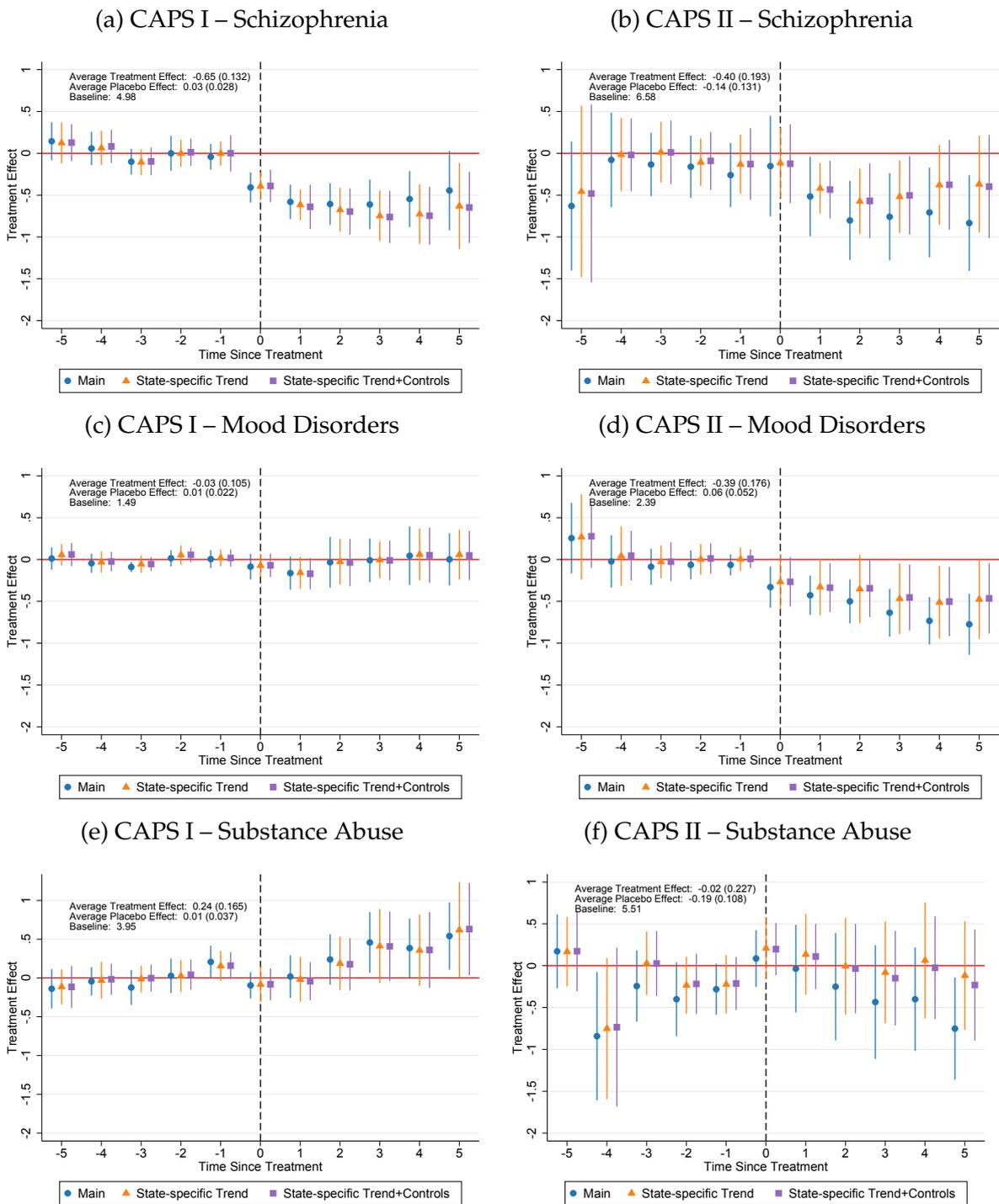
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of therapeutic workshops. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.6: Effects of CAPSs I and CAPS II on hospital admissions by mental health and behavior disorders – 02-16



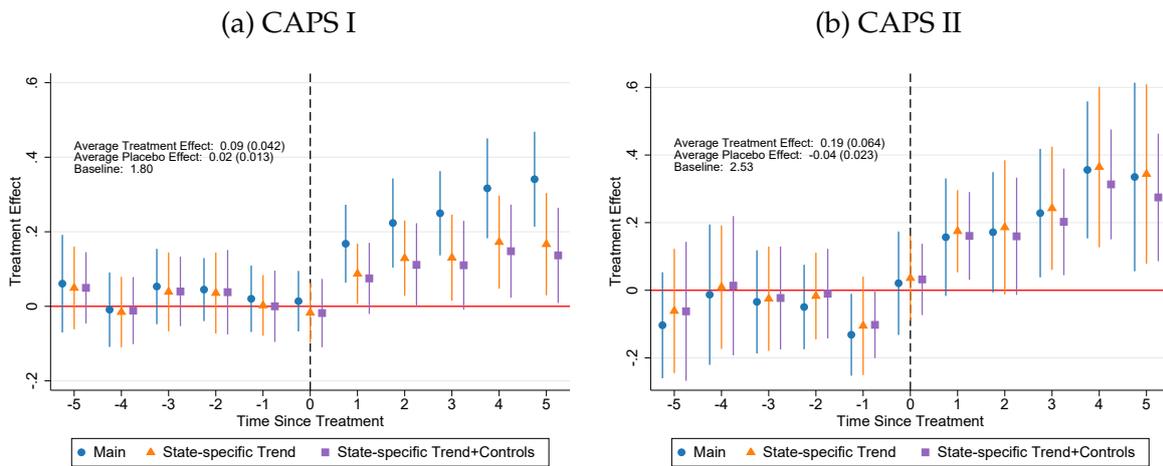
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the Effects of CAPSs I (panel a) and CAPS II (panel b) on the rate of hospital admissions due to mental and behavioral disorders (codes F00-F99). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.7: Effects of CAPSs I and CAPS II on hospital admissions by cause – 02-16



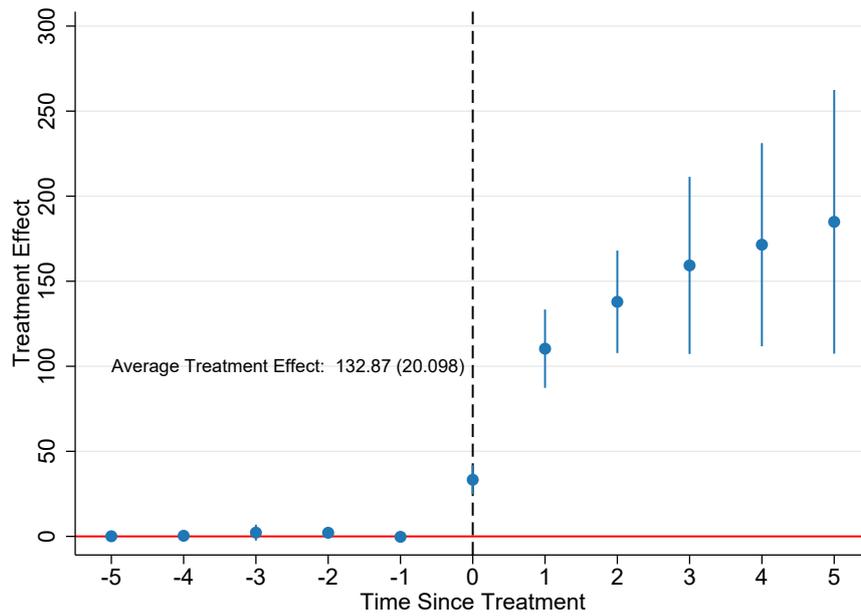
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the Effects of CAPSs I and II on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), mood disorders (F30-F39), and substance abuse disorders (F10-F19). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.8: Effects of CAPSs I and CAPSs II on homicide rate – 02-16



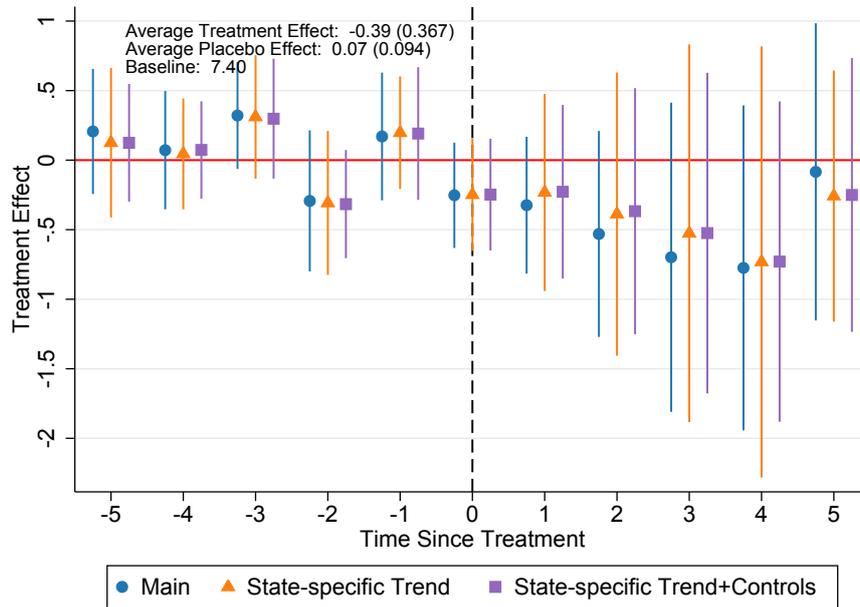
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the Effects of CAPSs I (panel a) and CAPS II (panel b) on homicide rates. The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.9: Effects of CAPSs AD on psychosocial care procedures related to psychoactive substance abuse disorders – 02-12



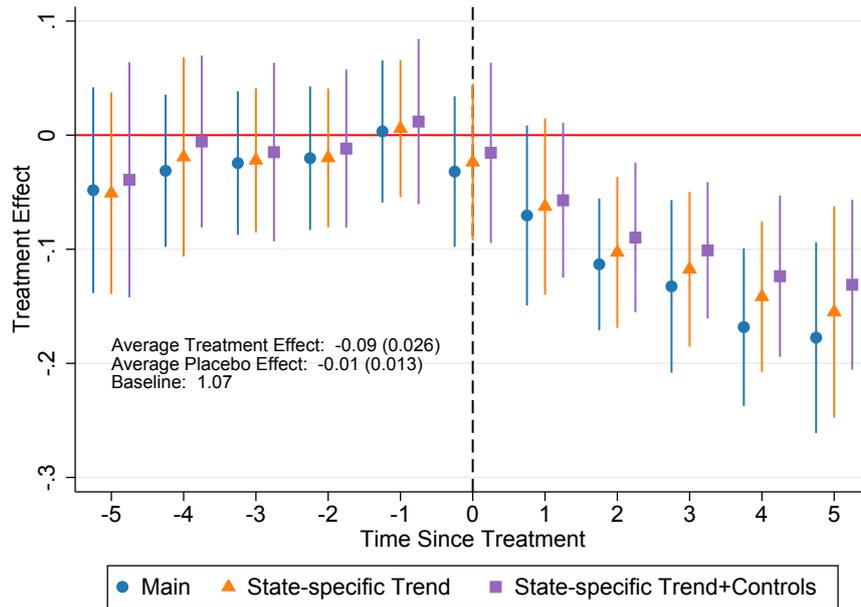
This figure plots 95% confidence-intervals computed with a municipality-level clustered boot-strap and dynamic and placebo DID estimators for the CAPS AD effects on the rate of psychosocial care procedures related to psychoactive substance abuse disorders. The Average Treatment Effect computes a simple average of the instantaneous and dynamic effects. In parenthesis, standard errors computed with a municipality-level clustered boot-strap.

Figure A.10: Effects of CAPSs AD on hospitalizations due to psychoactive substance abuse disorders – 02-16



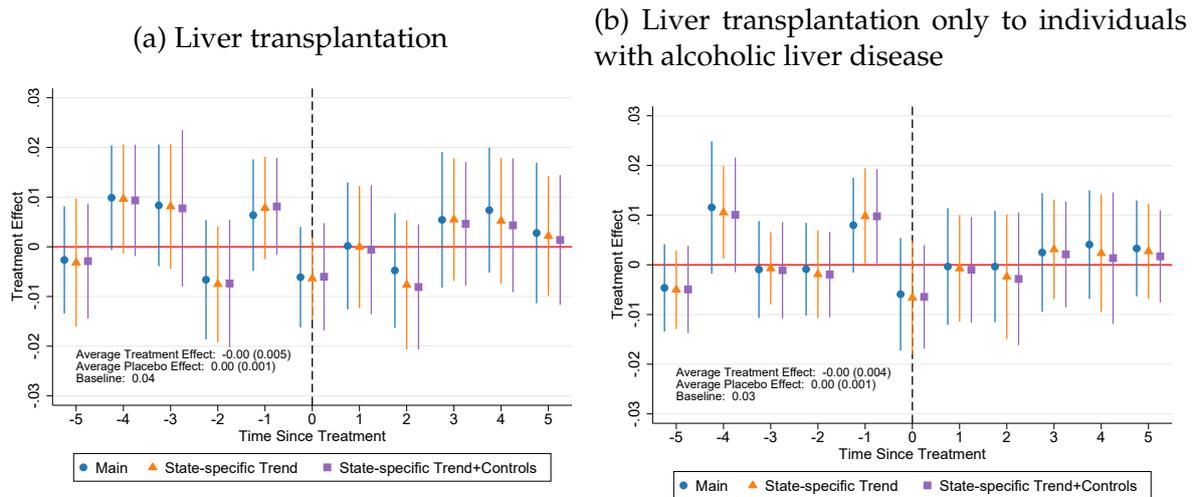
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS AD effects on the rate of hospital admissions due to psychoactive substance abuse disorders (codes F10-F19). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.11: Effects of CAPSs AD on deaths due to alcoholic and unspecified sources of chronic liver diseases – 02-16



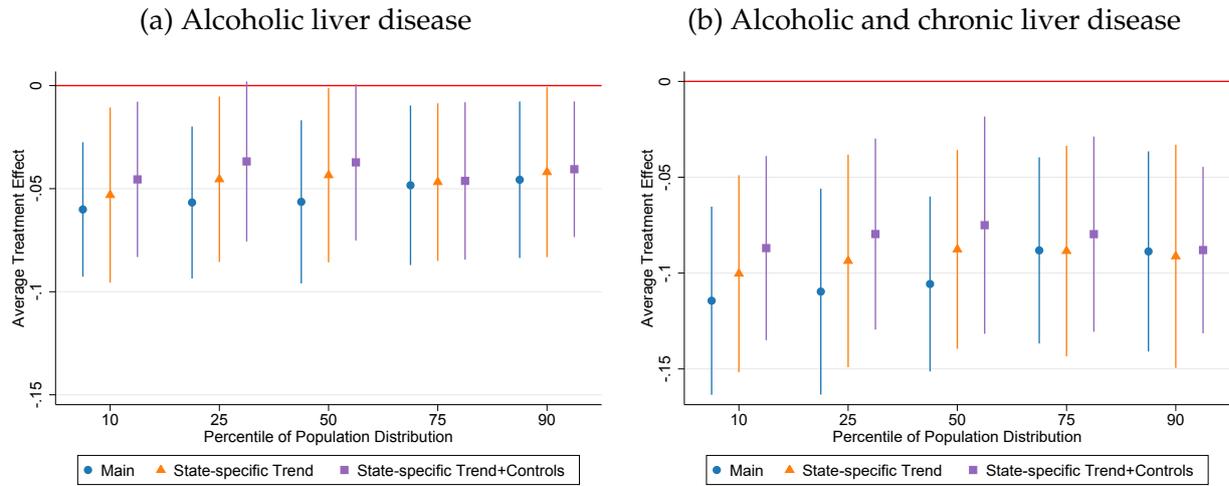
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS AD effects on mortality rates due to alcoholic and unspecified sources of chronic liver diseases (codes K70, K73, K74). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.12: Effects of CAPSs AD on liver transplantation rate – 02-16



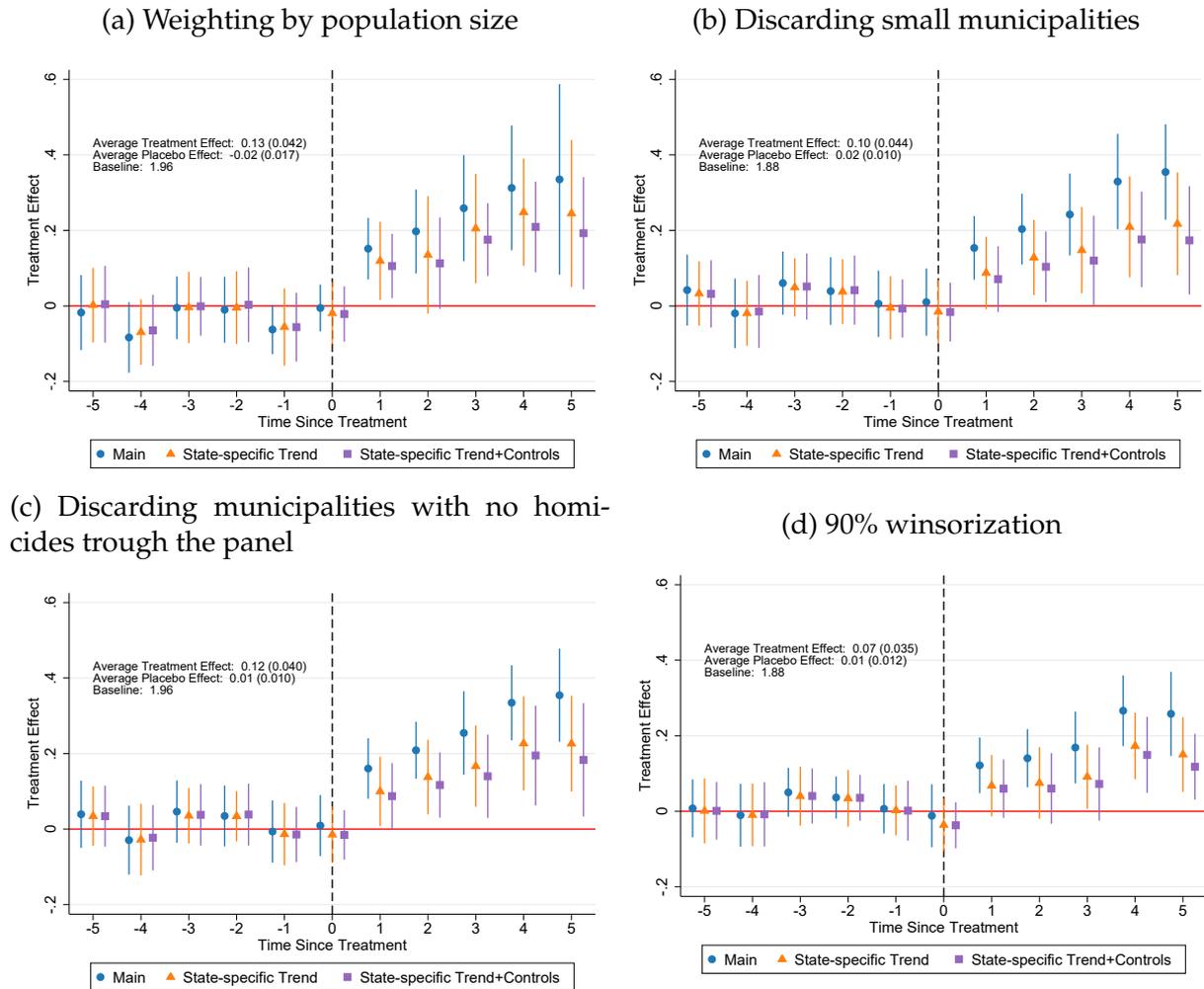
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the Effects of CAPSs AD on liver transplantation rate (panel a) and on the rate of liver transplantation performed in individuals with alcoholic liver disease (panel b). The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. For the third specification, the average effects over the non-placebo even-times are the following. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.13: Average effects of CAPSs AD on the rate of deaths caused by alcoholic liver disease – 02-16



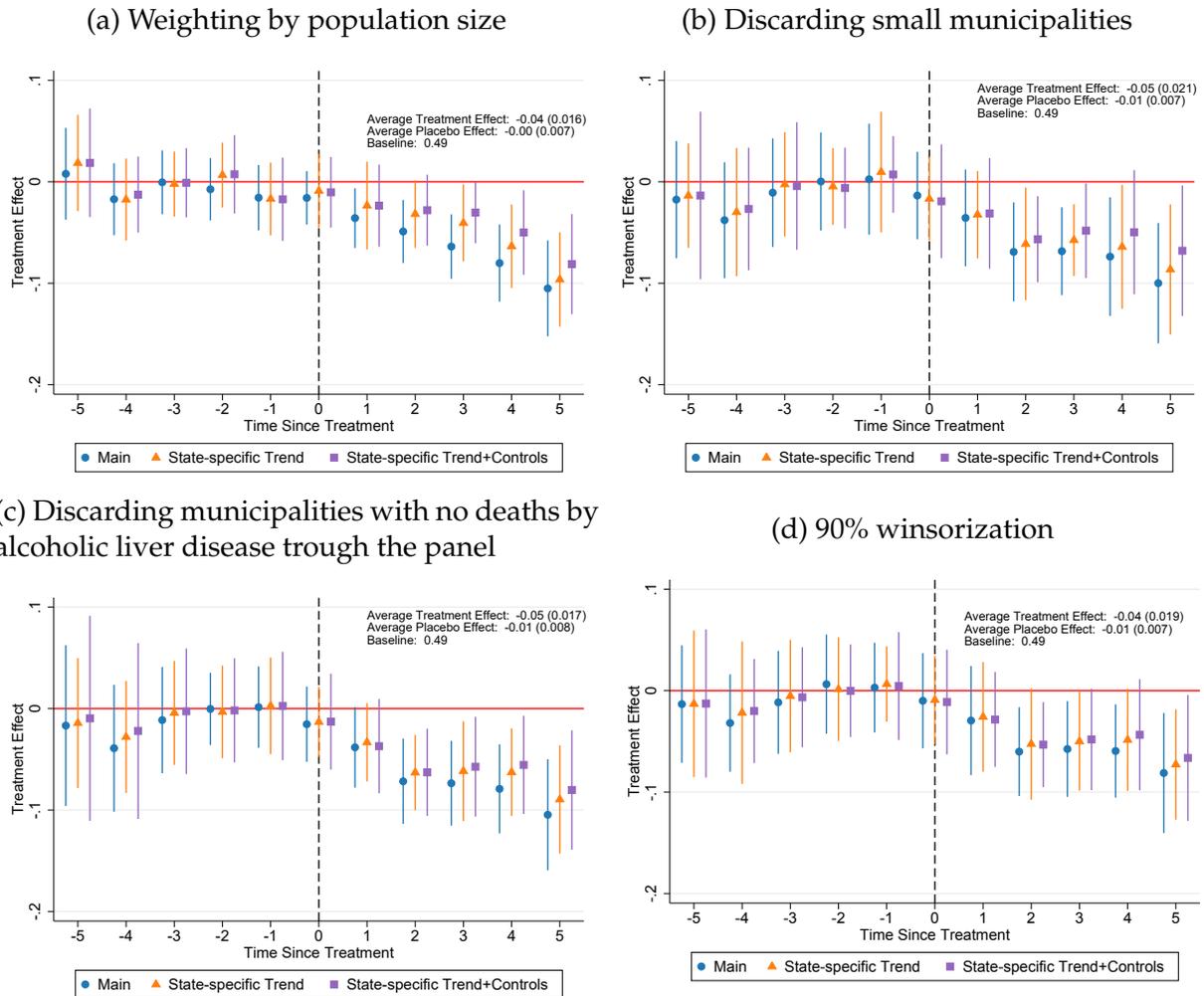
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and the average treatment effect of CAPSs AD on deaths due to alcoholic liver disease (panel a; code K70) and alcoholic and unspecified sources of chronic liver diseases (panel b; codes K70, K73, K74). We present the results for different sample restrictions. In particular, we restrict the sample to municipalities above the 10th, 25th, 50th, 75th, and 90th percentile of the population distribution. Average treatment effect is defined by the average of the effects for the non-negative event-times. The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center.

Figure A.14: Effects of CAPSs on homicide - 02-12



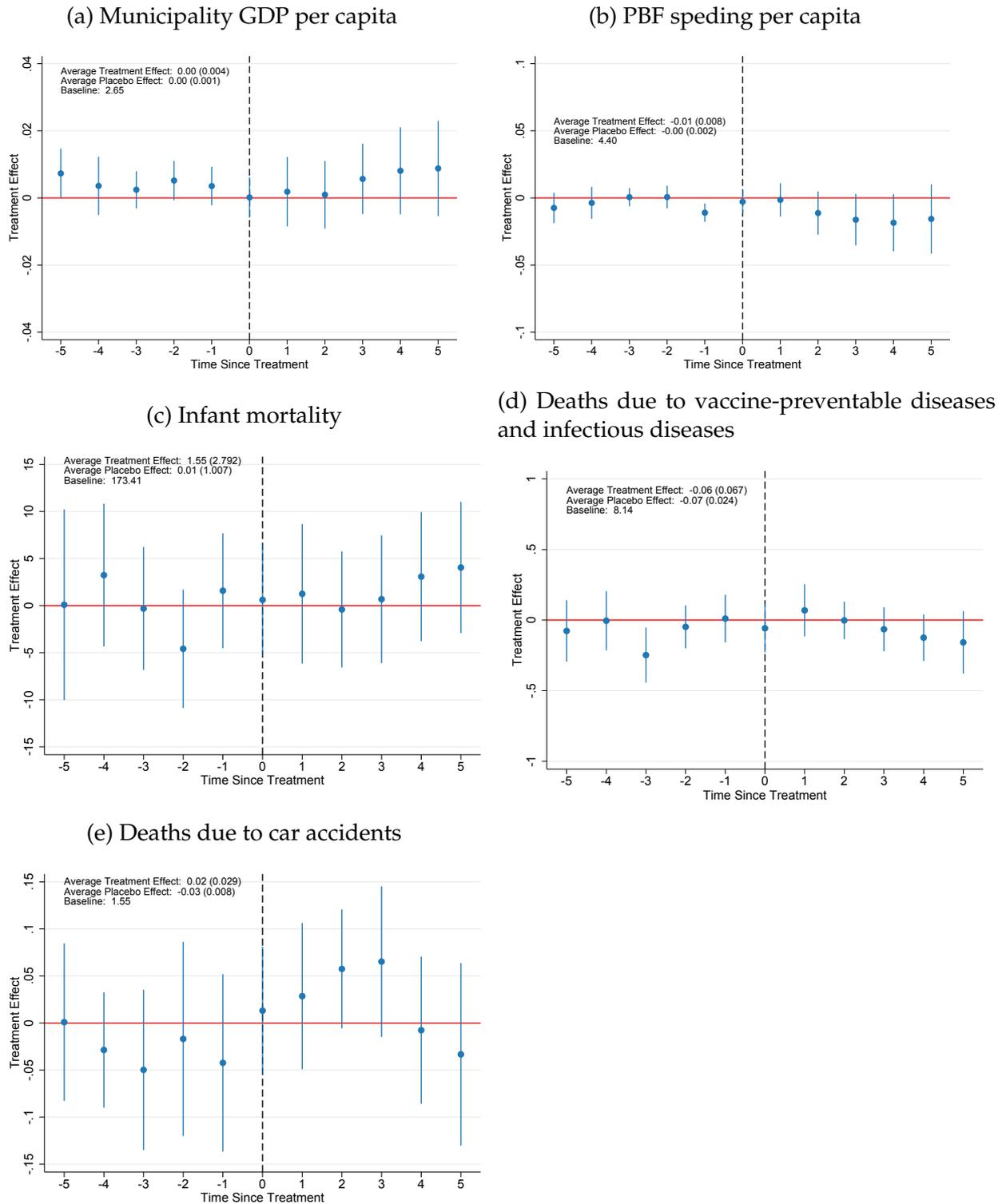
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on homicides, for different specifications. In panel a, we present estimates weighted by the baseline population size; in panel b, we discard municipalities below the 10th percentile of the population size distribution; in panel c, we restrict the sample to municipalities reporting positive counts of homicides rates in all years; and in panel d, we set data below the 5th percentile to the 5th percentile, and set data above the 95th percentile to the 95th percentile. Regarding covariates, the same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.15: Effects of CAPSs AD on the rate of deaths caused by alcoholic liver disease- 02-12



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of deaths caused by alcoholic liver disease (code K70), for different specifications. In panel a, we present estimates weighted by the baseline population size; in panel b, we discard municipalities below the 10th percentile of the population size distribution; in panel c, we restrict the sample to municipalities reporting positive counts of deaths by alcoholic liver disease in all years; and in panel d, we set data below the 5th percentile to the 5th percentile, and set data above the 95th percentile to the 95th percentile. Regarding covariates, the same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

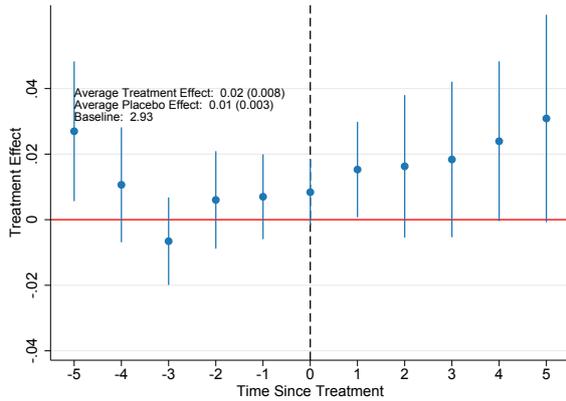
Figure A.16: Effects of CAPSs on placebo outcomes – 02-16



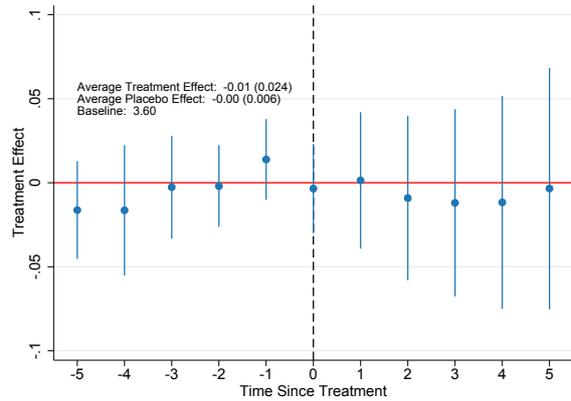
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the effects of CAPSs on municipality GDP per capita (panel a) and PBF spending per capita (panel b), infant mortality (panel b), deaths due to vaccine-preventable diseases and infectious diseases (panel d), and deaths due to car accidents (panel e). The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.17: Effects of CAPSs II on placebo outcomes – 02-16

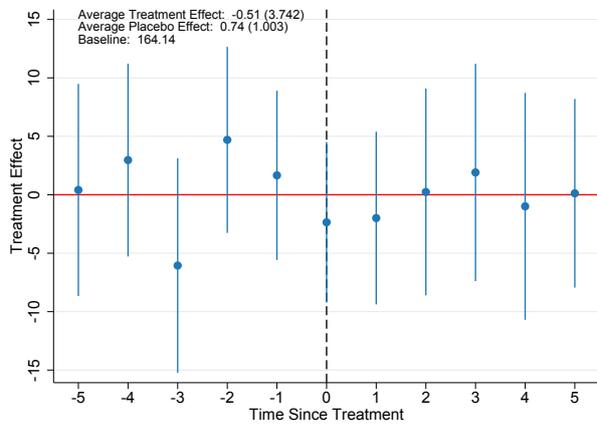
(a) Municipality GDP per capita



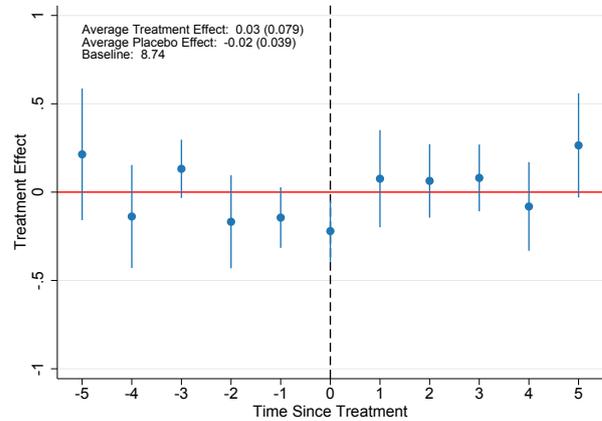
(b) PBF spending per capita



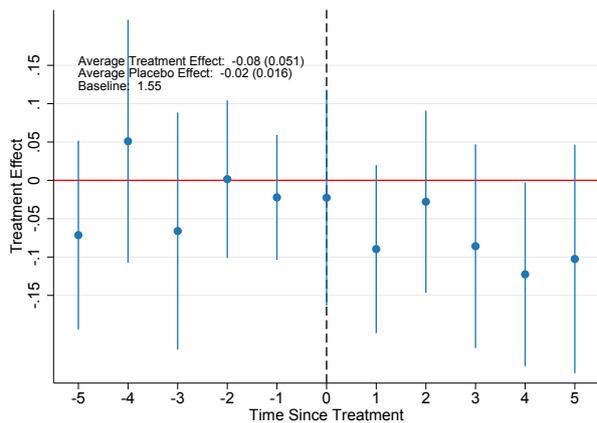
(c) Infant mortality



(d) Deaths due to vaccine-preventable diseases and infectious diseases

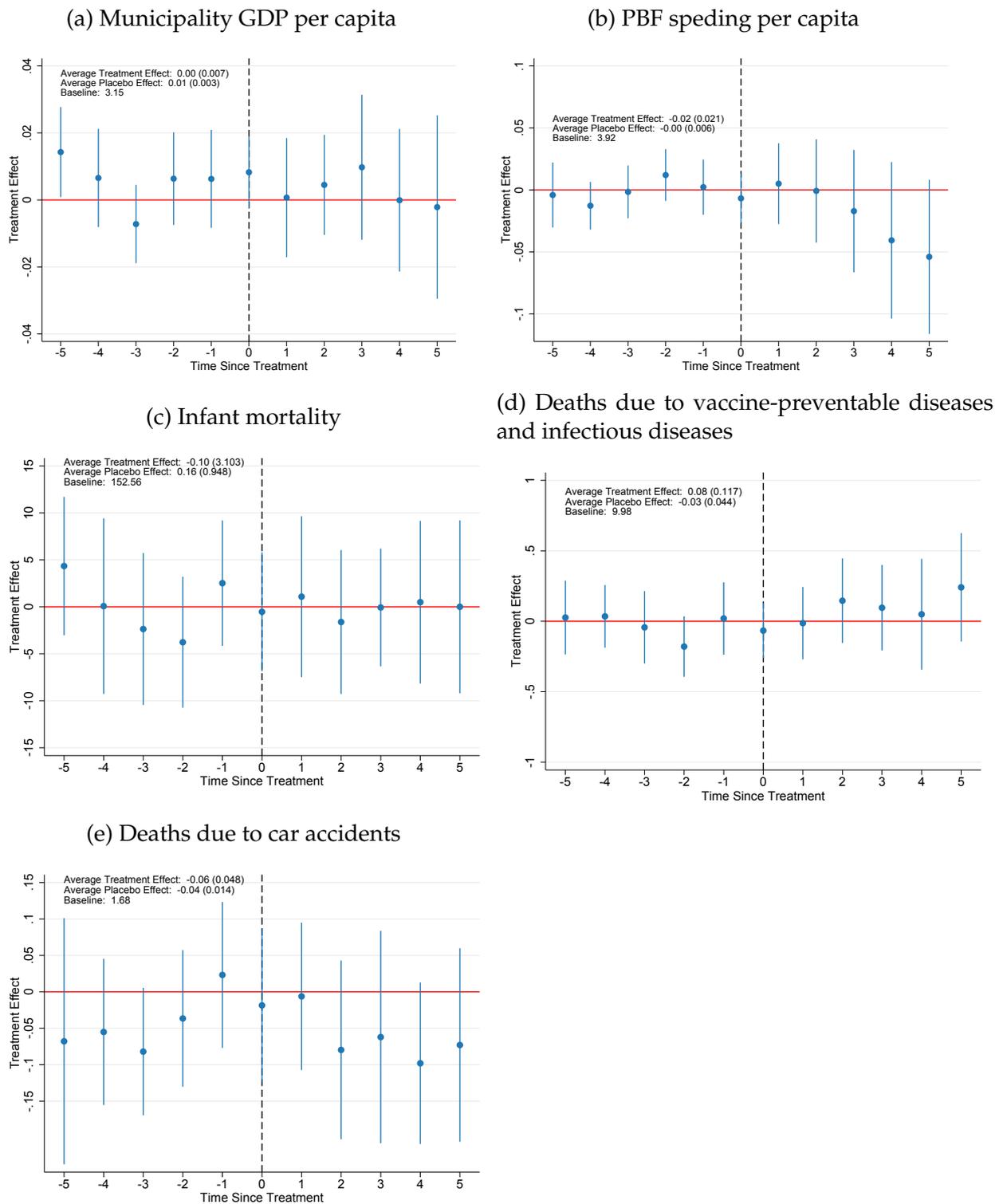


(e) Deaths due to car accidents



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the effects of CAPSs II on municipality GDP per capita (panel a) and PBF spending per capita (panel b), infant mortality (panel c), deaths due to vaccine-preventable diseases and infectious diseases (panel d), and deaths due to car accidents (panel e). The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure A.18: Effects of CAPSs AD on placebo outcomes – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the effects of CAPSs AD on municipality GDP per capita (panel a) and PBF spending per capita (panel b), infant mortality (panel b), deaths due to vaccine-preventable diseases and infectious diseases (panel d), and deaths due to car accidents (panel e). The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Table A1: Hazard estimation of probability of receiving a CAPS (marginal effects)

	(1)	(2)	(3)
<i>A. Lagged variables (per 10,000 people)</i>			
Δ_{-1} Hosp: Mental & Behavioral Disorders	0.00008 (0.00008)		
Δ_{-1} Deaths: Mental & Behavioral Disorders	-0.00006 (0.00051)		
Δ_{-1} Deaths of Despair	0.00015 (0.00042)		
Δ_{-1} Homicides	-0.00008 (0.00034)		
Δ_{-1} arcsinh(GDP per capita)	0.00143 (0.00457)		
Δ_{-2} Hosp: Mental & Behavioral Disorders		-0.00002 (0.00009)	
Δ_{-2} Deaths: Mental & Behavioral Disorders		0.00034 (0.00054)	
Δ_{-2} Deaths of Despair		-0.00023 (0.00045)	
Δ_{-2} Homicides		0.00036 (0.00036)	
Δ_{-2} arcsinh(GDP per capita)		0.00315 (0.00486)	
Δ_{-3} Hosp: Mental & Behavioral Disorders			-0.00009 (0.00009)
Δ_{-3} Deaths: Mental & Behavioral Disorders			0.00000 (0.00053)
Δ_{-3} Deaths of Despair			-0.00005 (0.00044)
Δ_{-3} Homicides			0.00025 (0.00036)
Δ_{-3} arcsinh(GDP per capita)			0.00598 (0.00475)
<i>B. Variables at baseline</i>			

Table A1 – *Continued*

	(1)	(2)	(3)
Theil Index (2000)	0.06141 (0.00562)	0.06020 (0.00601)	0.04973 (0.00610)
Share Illiterate (2000)	-0.02865 (0.01405)	-0.02497 (0.01479)	-0.01717 (0.01483)
Share Poor (2000)	-0.04439 (0.00892)	-0.04626 (0.00941)	-0.03486 (0.00928)
Share Rural (2000)	-0.06102 (0.00445)	-0.05955 (0.00467)	-0.05721 (0.00477)
Hosp: Mental & Behavioral Disorders (2003)	0.00005 (0.00004)	0.00005 (0.00005)	0.00002 (0.00005)
Deaths: Mental & Behavioral Disorders (2003)	-0.00006 (0.00086)	-0.00008 (0.00090)	-0.00018 (0.00089)
Deaths of Despair (2003)	0.00074 (0.00063)	0.00081 (0.00066)	0.00093 (0.00064)
Homicides (2003)	0.00241 (0.00037)	0.00249 (0.00040)	0.00204 (0.00040)
arcsinh(GDP per capita) (2003)	-0.00044 (0.00139)	-0.00097 (0.00150)	0.00021 (0.00149)
Observations	53,096	48,045	43,089
State FE	Yes	Yes	Yes
Time Polynomial Degree	5	5	5

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Marginal effects of the hazard estimation of probability of receiving a CAPS. In this sample, covering the period from 2003 to 2016, units appear in the data until they receive a CAPS and, after that, they leave the sample. Each specification considers a different lagged difference of the main variables of interest and control for their baseline values, as well as the baseline values of other variables of interest as indicated in the table. A logit model is estimated and the reported marginal effects are taken at the average of each variable. Observations at the municipality level.

Table A2: Groups of Mortality and Morbidity Causes and Associated ICD-10 Codes

Group	ICD-10 Codes
Mental and behavioral disorders	F00-F09: Organic, including symptomatic, mental disorders
	F10-F19: Mental and behavioral disorders due to psychoactive substance abuse
	F20-F29: Schizophrenia, schizotypal and delusional disorders
	F30-F39: Mood disorders, including major depressive disorder and bipolar disorder
	F40-F48: Neurotic, stress-related and somatoform disorders
	F50-F59: Behavioral syndromes associated with physiological disturbances and physical factors
	F60-F69: Disorders of adult personality and behavior
	F70-F79: Mental retardation
	F80-F89: Disorders of psychological development
	F90-F98: Behavioral and emotional disorders with onset usually occurring in childhood and adolescence
F99: Unspecified mental disorder	
Homicide	X85-Y09: Assault, excluding injuries due to legal intervention and operations of war
Deaths of Despair	X40-X45, Y10-Y15, Y45, Y47, Y49: Alcoholic poisoning and overdose of prescription and illegal drugs
	X60-X84: Suicide
	K70: Alcoholic liver disease
	K73, K74: Unspecified chronic liver disease

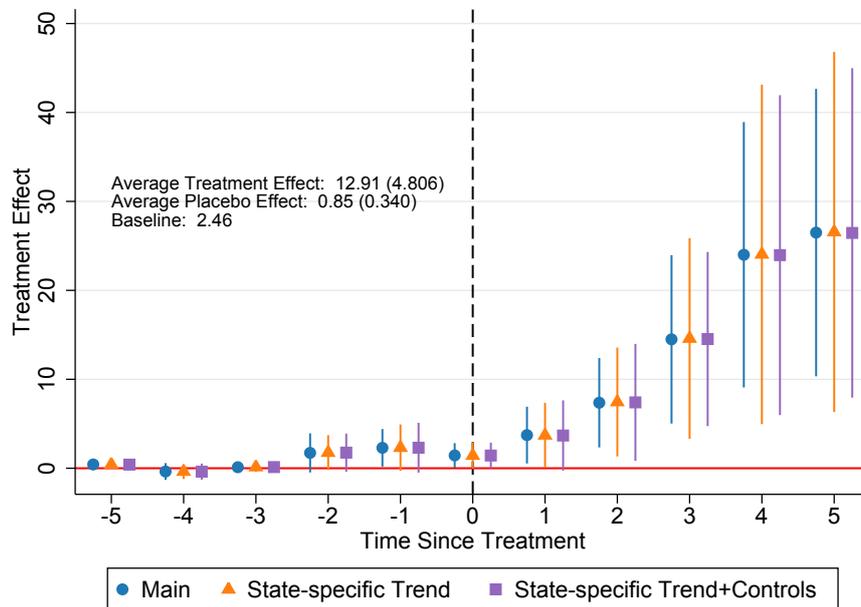
Table A3: Heterogeneous CAPSs' effects on homicide rates and hospital admissions

	Overall (1)	Male (2)	Age 15-39 (3)	Age > 39 (4)	Male & Age 15-39 (5)
<i>Panel (a): Homicides</i>					
Average Effects	0.120 (0.04)	0.116 (0.04)	0.120 (0.03)	-0.010 (0.01)	0.116 (0.02)
Pre-CAPS mean of the outcome	1.96	1.76	1.37	0.45	1.25
<i>Panel (b): Hosp. by Mental Disorders</i>					
Average Effects	-0.71 (0.22)	-0.459 (0.21)	-0.340 (0.14)	-0.350 (0.12)	-0.236 (0.12)
Pre-CAPS mean of the outcome	12.61	8.44	6.77	5.73	4.66
<i>Panel (c): Hosp. by Schizophrenia</i>					
Average Effects	-0.624 (0.10)	-0.364 (0.11)	-0.328 (0.08)	-0.294 (0.05)	-0.209 (0.05)
Pre-CAPS mean of the outcome	5.32	3.24	2.81	2.47	1.91

Note: This table reports the average effects of CAPSs on homicide rates (panel a; codes X85-Y09), hospital admissions by mental and behavioral disorders (panel b; codes F00-F99), and hospital admissions by schizophrenia (panel c; codes F20-F29). The average effects are defined by the average of the DID estimators for the non-negative event-times. Standard errors in parenthesis are computed using a municipality-level clustered bootstrap. Column 1 presents the results on overall mortality and admission rates. Column 2 considers only male homicides/admissions. Later, we break down the outcomes based on two age categories: 15-39 and more than 40 (columns 3 and 4, respectively). Finally, within the age bin 15-39, we again restrict the outcome only to men (column 5).

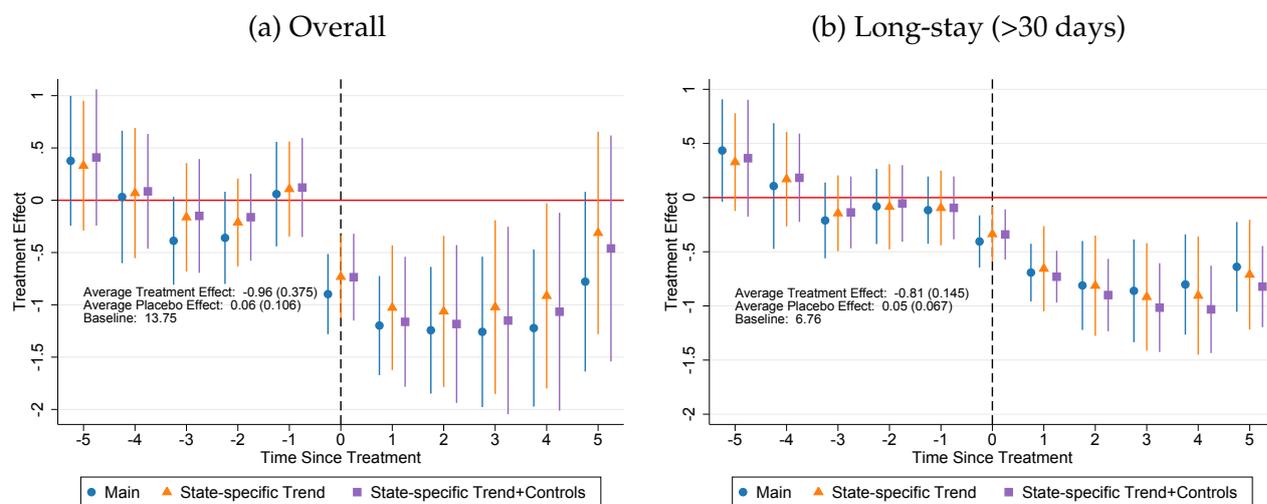
B Compositional Effects

Figure B.1: Effects of CAPSs on dispensed antipsychotic drugs – 02-16



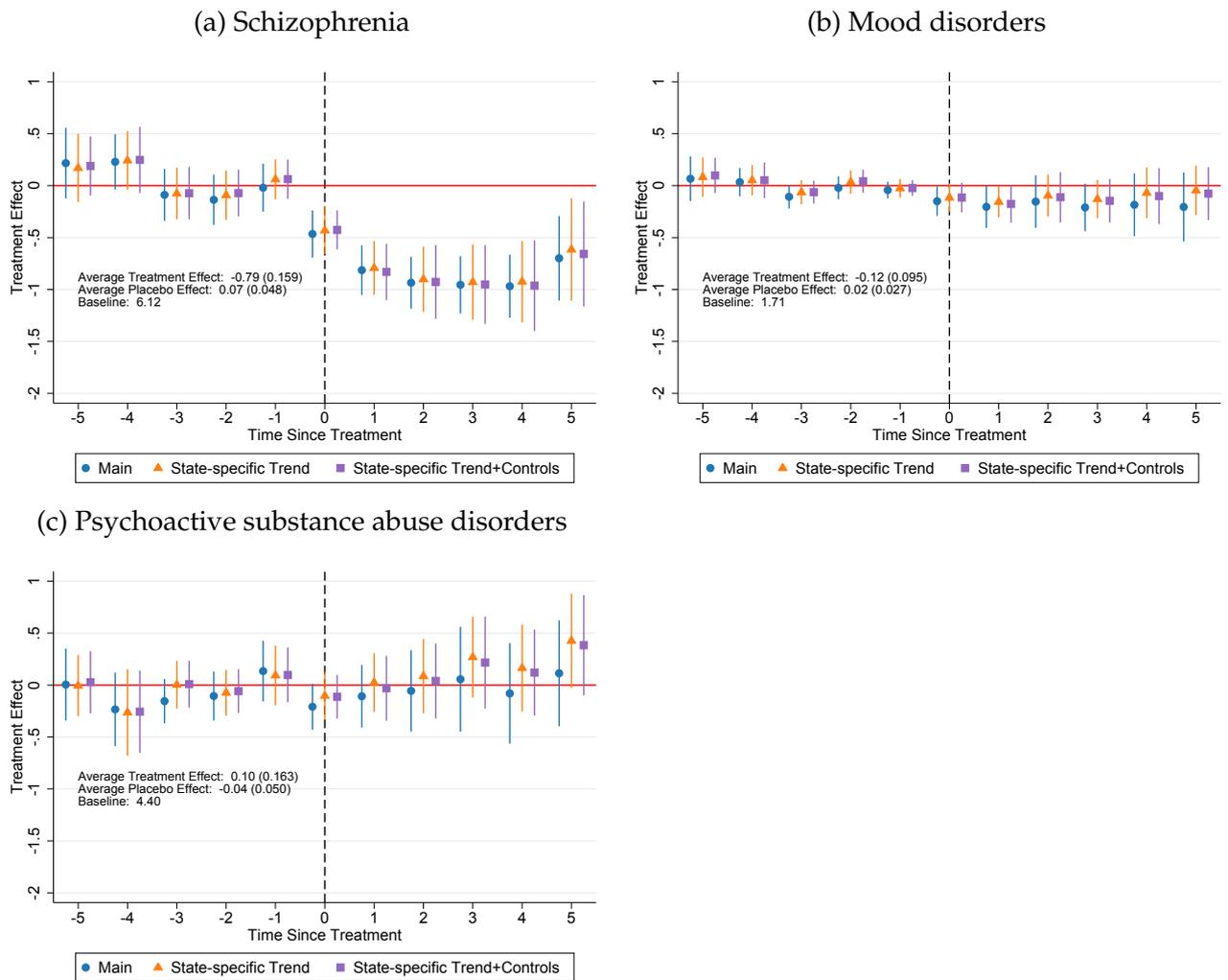
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of dispensed antipsychotic drugs. These are drugs delivered at the ambulatory level for patients, especially with schizophrenia and other psychotic disorders, for home use. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.2: Effects of CAPSs on hospitalization due to mental and behavioral disorders
 – 02-16



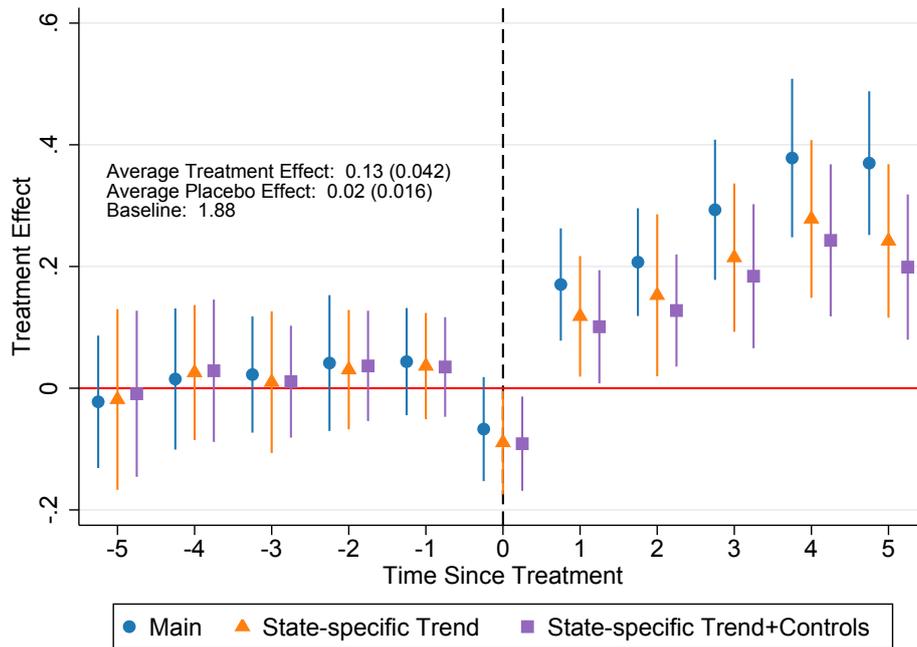
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of hospital admissions due to mental and behavioral disorders (codes F00-F99). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.3: Effects of CAPSs on hospitalization due to mental and behavioral disorders by cause – 02-16



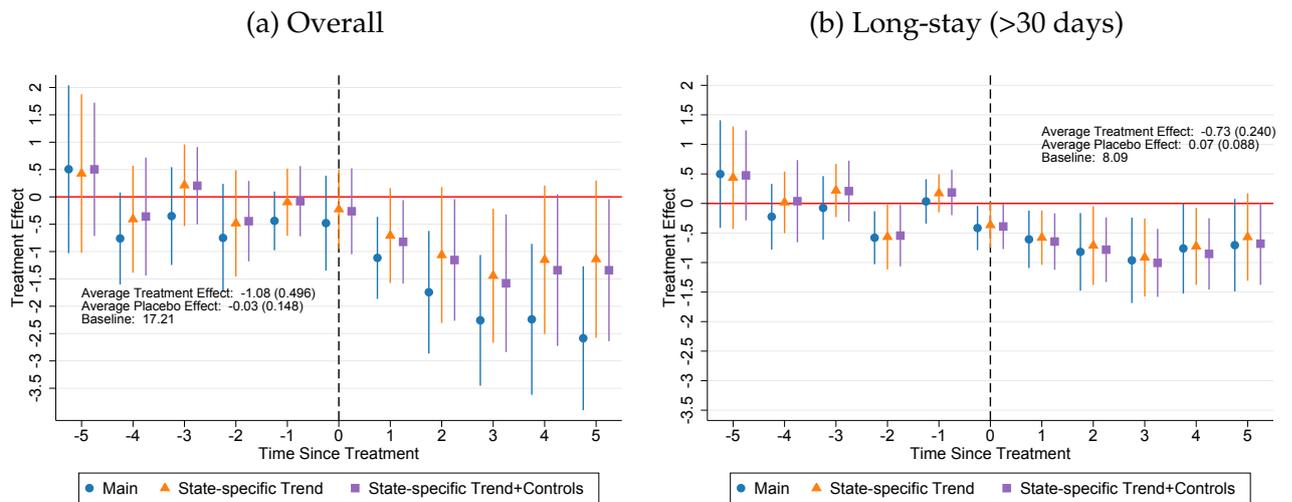
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), and mood disorders (F30-F39). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.4: Effects of CAPSs on homicides – 02-16



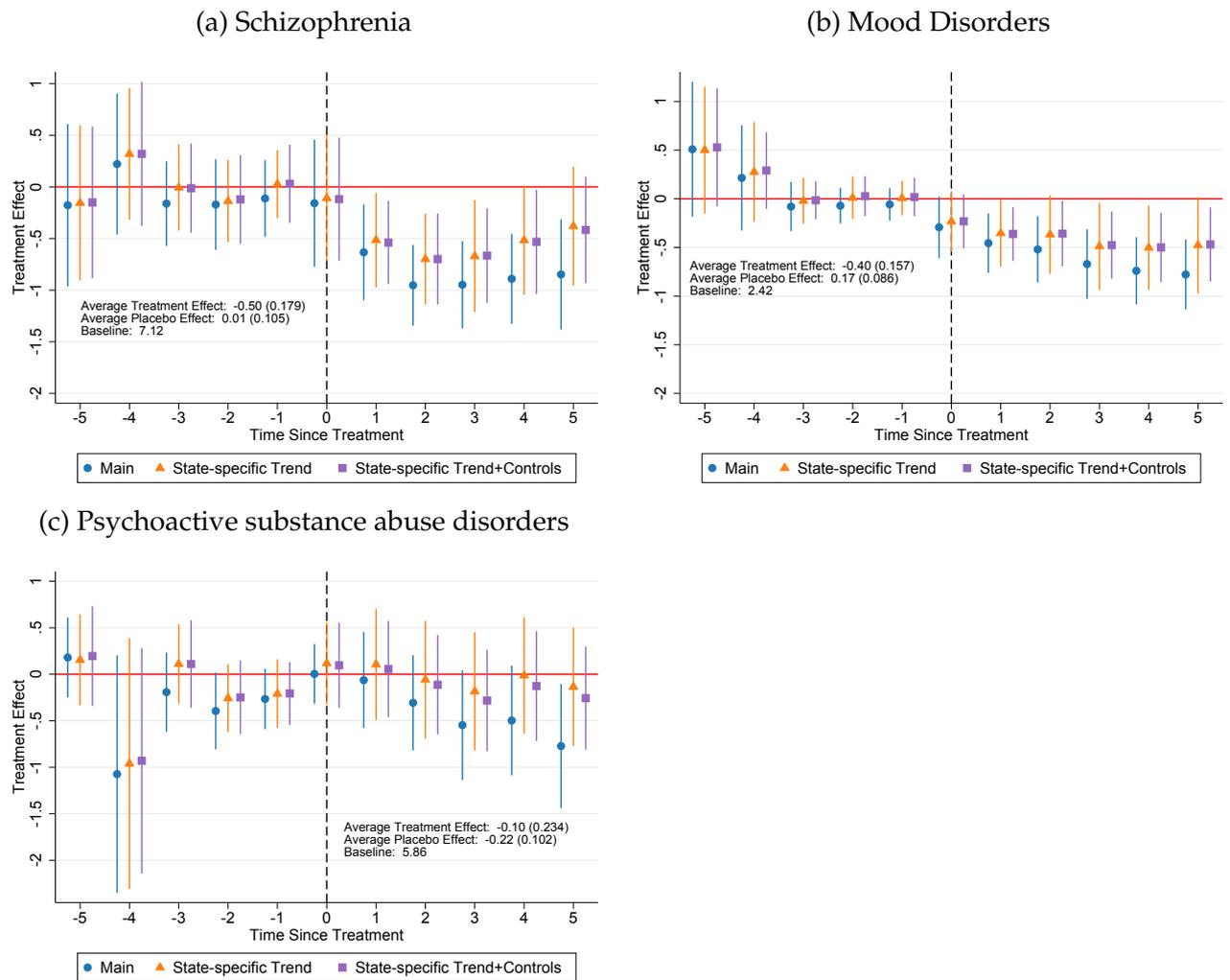
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of homicide (X85-Y09). Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.5: Effects of CAPSs II on hospitalization due to mental and behavioral disorders – 02-16



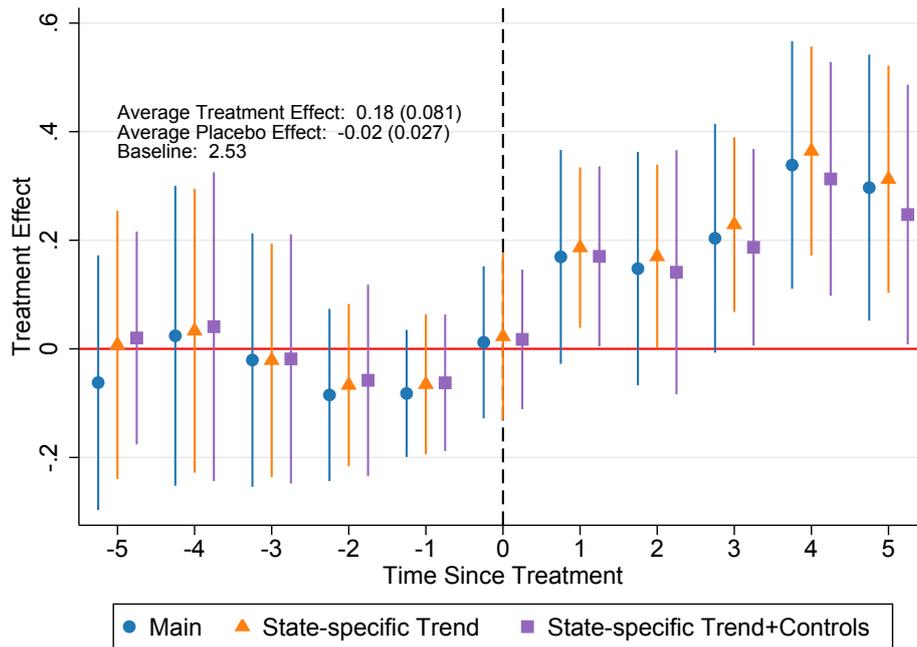
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), and mood disorders (F30-F39). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.6: Effects of CAPSs II on hospitalization due to mental and behavioral disorders by cause – 02-16



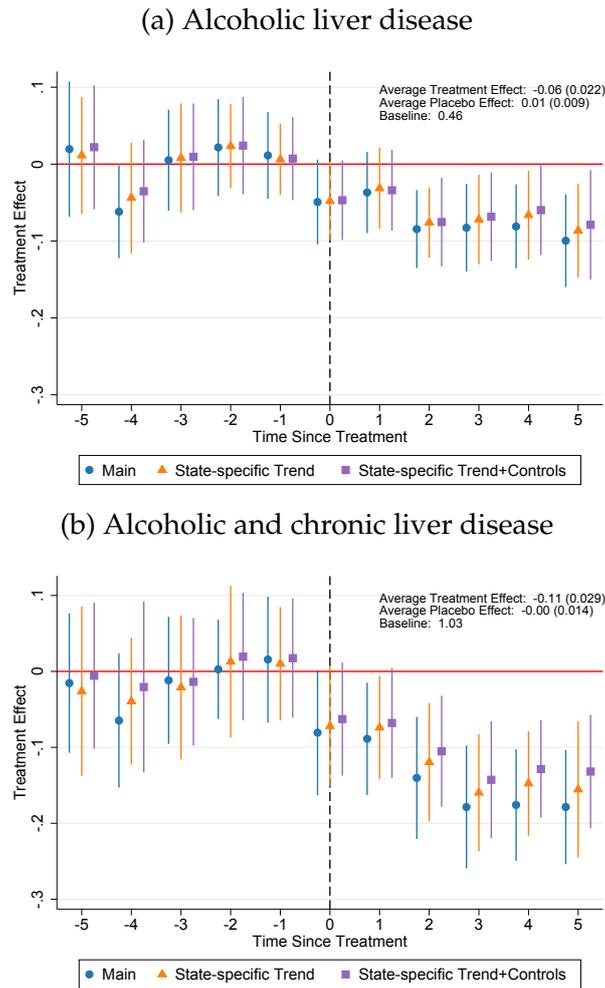
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), and mood disorders (F30-F39). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure B.7: Effects of CAPSs II on homicides – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of homicide (X85-Y09). Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

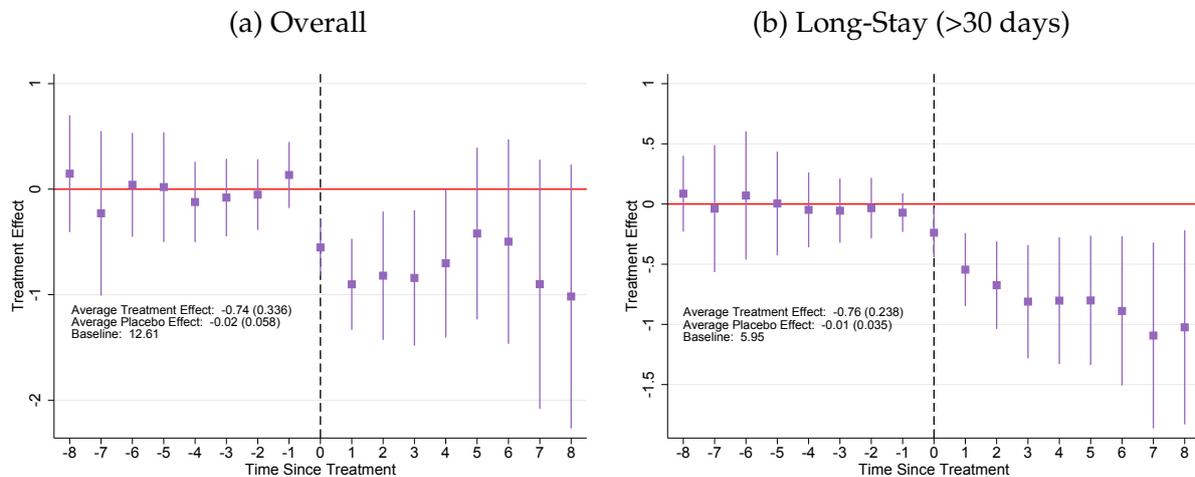
Figure B.8: Effects of CAPSs AD on hospitalization and mortality rates due to alcoholic liver disease – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS AD effects on deaths due to alcoholic liver disease (panel a; code K70) and alcoholic and unspecified sources of chronic liver diseases (panel b; code K70, K73, K74). Here we use a restricted sample of municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

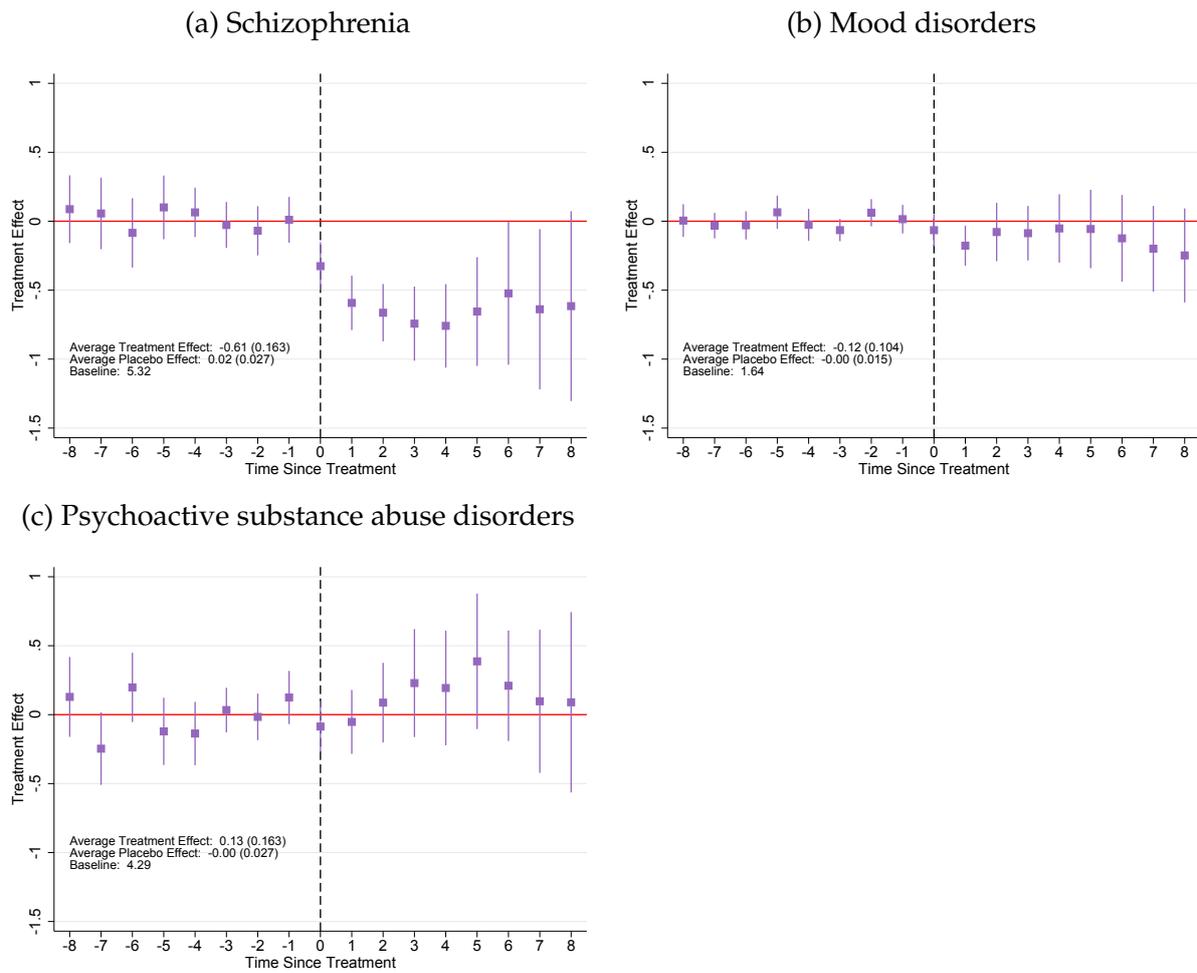
C Longer Event-Study

Figure C.1: Effects of CAPSs on hospitalization due to mental and behavioral disorders – 02-16



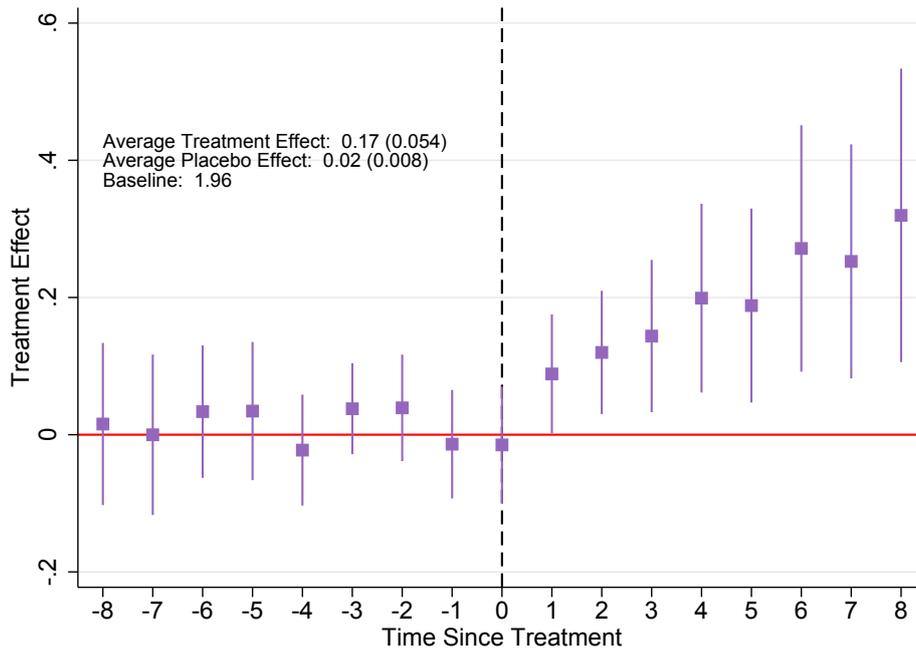
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), mood disorders (F30-F39), and psychoactive substance abuse disorders (F10-F19). Specifications include state-specific trends, municipality GDP per capita (transformed by inverse hyperbolic sine), PBF spending per capita, and a series of indicators for age-by-gender population bins. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.2: Effects of CAPSs on outpatient care by mental health specialists – 08-16



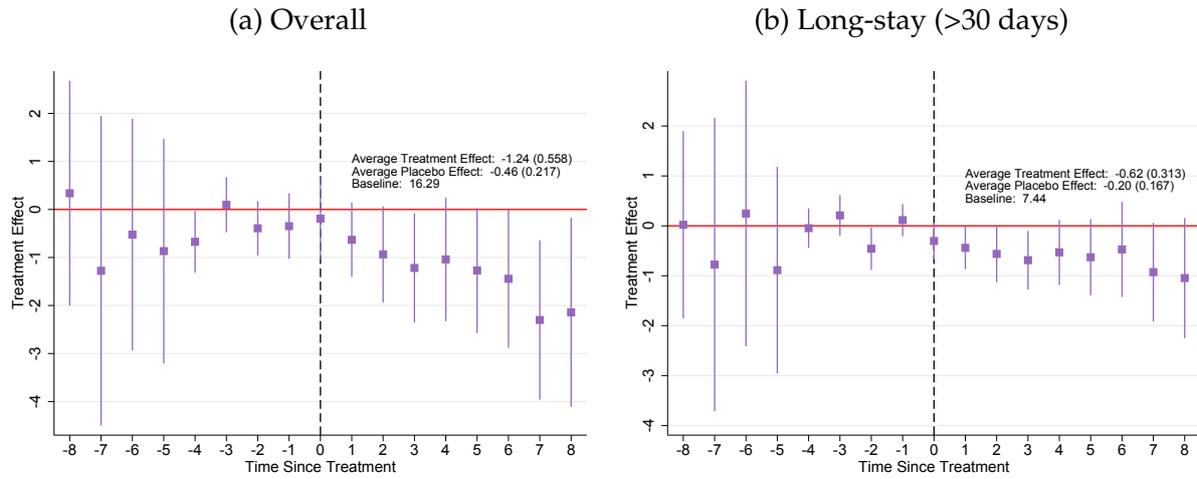
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), mood disorders (F30-F39), and psychoactive substance abuse disorders (F10-F19). Specifications include state-specific trends, municipality GDP per capita (transformed by inverse hyperbolic sine), PBF spending per capita, and a series of indicators for age-by-gender population bins. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.3: Effects of CAPSs on homicides – 02-16



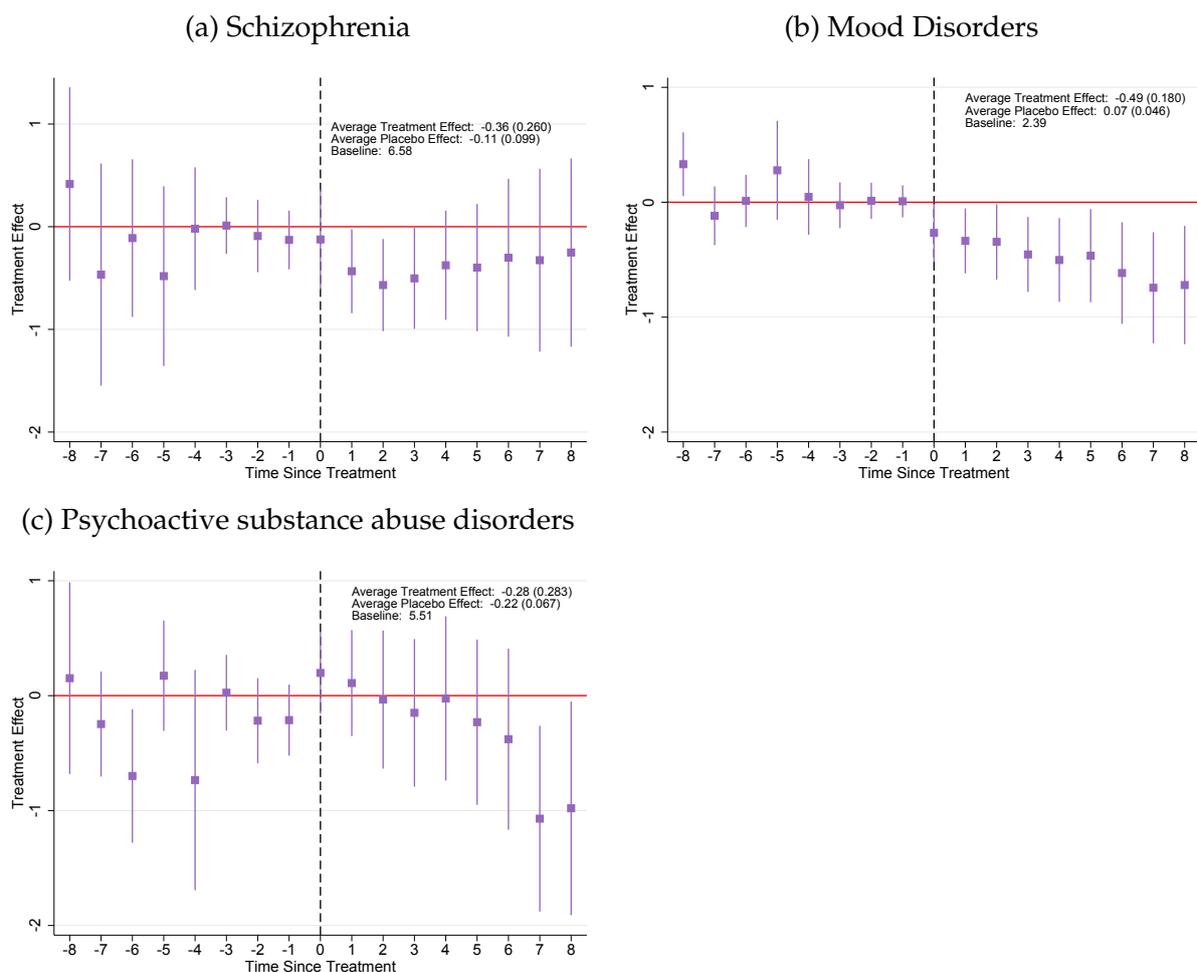
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPSs' effects on the rate of homicide (X85-Y09). Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.4: Effects of CAPSs II on hospitalization due to mental and behavioral disorders – 02-16



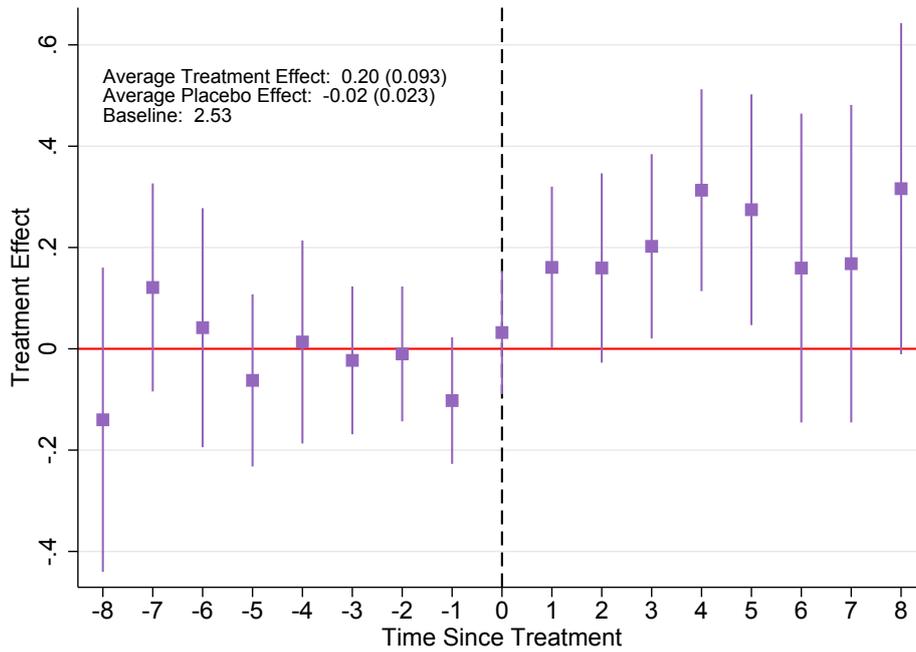
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of hospital admissions due to mental and behavioral disorders (F00-F99). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.5: Effects of CAPSs II on outpatient care by mental health specialists – 08-16



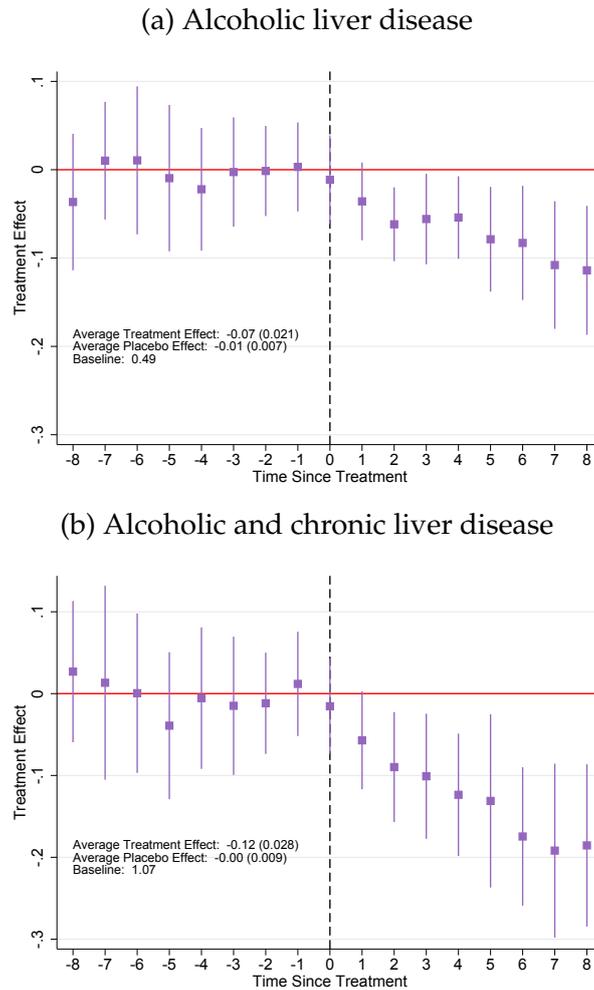
Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of hospital admissions due to mental and behavioral disorders by groups of cause: schizophrenia and related disorders (F20-F29), and mood disorders (F30-F39). The same specifications from Figure 3 also apply here. Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.6: Effects of CAPSs II on homicides – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS II effects on the rate of homicide (X85-Y09). Here we restrict the treated sample to municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.

Figure C.7: Effects of CAPS AD on hospitalization and mortality rates due to alcoholic liver disease – 02-16



Notes: This figure plots 95% confidence-intervals computed with a municipality-level clustered bootstrap and dynamic and placebo DID estimators for the CAPS AD effects on deaths due to alcoholic liver disease (panel a; code K70) and alcoholic and unspecified sources of chronic liver diseases (panel b; code K70, K73, K74). Here we use a restricted sample of municipalities with at least five non-missing post-CAPS data. The same specifications from Figure 3 also apply here. We additionally control for the introduction of any other type of center. The Average Treatment Effect and the Average Placebo Effect compute a simple average of the non-placebo and placebo effects, respectively, considering the specification with state-specific trends and controls. In parenthesis, standard errors computed with a municipality-level clustered bootstrap. Baseline indicates the sample mean values for the treated in the pre-CAPS period.