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Ana Carolina Marinato de Resende

Money in the Bank, Health on the Mend: An investigation of the Emergency Aid’s effect on health demand in Brazil.

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Orientador: Prof. Dr. Naércio Aquino Menezes Filho

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Às mulheres da minha família,
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Abstract

This study aims to estimate the Emergency Aid program's effect on healthcare demand in Brazil, focusing on the reasons for seeking healthcare and the heterogeneous impacts on isolated and less developed communities. Using a difference-in-differences methodology, the study leverages the nearly random distribution of EA payments based on birth months, allowing comparison between eligible and non-eligible groups.

Findings indicate that the EA cash payment causes an average decrease of 6 hospitalizations for municipalities. Urban areas experienced decreased hospitalizations and rural areas saw increases, while highly developed municipalities showed a more pronounced negative effect. Specific health conditions responded differently: Covid-19 and eye-related hospitalizations decreased, while those for cardiovascular and mental health issues increased.

Overall, the EA was shown to effectively promote social isolation, reducing hospitalization rates, but with varied impacts based on municipality characteristics and health conditions. Institutional quality and population density influenced these outcomes, reflecting local governments' capacity to manage aid and pandemic measures effectively. The findings underscore the importance of considering socioeconomic and geographic disparities in policy implementation, offering valuable guidance for designing equitable and impactful aid programs in future crises.

Keywords: Impact Evaluation; Emergency Aid; Income shock; Cash Transfer; Health Demand; Covid-19 Pandemic.

Resumo

Este estudo tem como objetivo estimar o efeito do programa de Auxílio Emergencial na demanda por cuidados de saúde no Brasil, com foco nas razões para a procura por cuidados de saúde e nos impactos heterogêneos em comunidades isoladas e menos desenvolvidas. Utilizando uma metodologia de diferenças-em-diferenças, o estudo aproveita a distribuição quase aleatória dos pagamentos do AE com base nos meses de nascimento, permitindo a comparação entre grupos elegíveis e não elegíveis.

Os resultados indicam que o pagamento em dinheiro do AE causa uma queda média de 6 hospitalizações sobre os municípios. As áreas urbanas experimentaram uma redução nas hospitalizações, enquanto as áreas rurais viram um aumento, e os municípios altamente desenvolvidos mostraram um efeito negativo mais pronunciado. Condições de saúde específicas responderam de maneira diferente: hospitalizações relacionadas à Covid-19 e problemas oculares diminuíram, enquanto aquelas por questões cardiovasculares e de saúde mental aumentaram.

No geral, o AE demonstrou promover efetivamente o isolamento social, reduzindo as taxas de hospitalização, mas com impactos variados dependendo das características dos municípios e das condições de saúde. A qualidade institucional e a densidade populacional influenciaram esses resultados, refletindo a capacidade dos governos locais de gerenciar a ajuda e as medidas pandêmicas de forma eficaz. Os achados ressaltam a importância de considerar disparidades socioeconômicas e geográficas na implementação de políticas, oferecendo orientações valiosas para a criação de programas de auxílio equitativos e impactantes em crises futuras.

Palavras-chave: Avaliação de Impacto; Auxílio Emergencial; Choque de Renda; Transferência de Renda; Demanda por saúde; Pandemia de Covid-19.

Resumen

Este estudio tiene como objetivo estimar el efecto del programa de Ayuda de Emergencia en la demanda de atención médica en Brasil, centrándose en las razones para buscar atención médica y en los impactos heterogéneos en comunidades aisladas y menos desarrolladas. Utilizando una metodología de diferencias en diferencias, el estudio aprovecha la distribución casi aleatoria de los pagos de AE según los meses de nacimiento, lo que permite la comparación entre grupos elegibles y no elegibles.

Los resultados indican que el pago en efectivo del AE disminuyó las hospitalizaciones por municipio en 6, en promedio. Las áreas urbanas experimentaron una reducción en las hospitalizaciones, mientras que las áreas rurales vieron un aumento, y los municipios altamente desarrollados mostraron un efecto negativo más pronunciado. Las condiciones de salud específicas respondieron de manera diferente: las hospitalizaciones relacionadas con la Covid-19 y los problemas oculares disminuyeron, mientras que aquellas por cuestiones cardiovasculares y de salud mental aumentaron.

En general, se demostró que el AE promovió efectivamente el aislamiento social, reduciendo las tasas de hospitalización, pero con impactos variados dependiendo de las características de los municipios y las condiciones de salud. La calidad institucional y la densidad poblacional influyeron en estos resultados, reflejando la capacidad de los gobiernos locales para gestionar la ayuda y las medidas pandémicas de manera efectiva. Los hallazgos resaltan la importancia de considerar las disparidades socioeconómicas y geográficas en la implementación de políticas, ofreciendo orientación valiosa para diseñar programas de ayuda equitativos y efectivos en futuras crisis.

Palabras clave: Evaluación de Impacto; Ayuda de Emergencia; Choque de Renta; Transferencia de Renta; Demanda de Salud; Pandemia de Covid-19.

Tables

Graphs

Figures

Contents

1 Introduction

The Covid-19 pandemic led to a series of economic and public health crises across the world, requiring rapid and effective responses from governments. The threat to public health led to measures to contain the spread of the disease, which affected the economy immensely by having millions of people stay at home unable to work, businesses to shut down and commerce to halt. Many governments chose disease containment strategies in conjunction with economic aid programs.

One of these economic aids widely used around the world was some form of cash transfer sent directly to those who needed it. The main role of these cash transfers was to mitigate the negative economic effects of the pandemic such as job loss, salary cuts and being unable to work due to social isolation. It also served as an incentive for social isolation, especially in places like Brazil where around 37% of the working population had informal jobs when the pandemic started (IBGE, 2024).

The Brazilian Emergency Aid program was one of the most important mitigation methods implemented during the Covid-19 pandemic in the country, as it reached millions of unemployed and economically vulnerable people.

In this dissertation I aim to estimate the impact of the Brazilian Emergency Aid program on the beneficiaries' demand for healthcare. I am focusing particularly on the causes for seeking healthcare and the heterogenous impact on more isolated and less developed communities.

This can be achieved using the nearly random distribution system for the stipend granted by the program, as it allows direct comparison of groups who received it to groups who didn't. With a difference-in-differences approach, it is possible to estimate how receiving the cash transfer could alter someone's search for healthcare.

My analysis reveals the Emergency Aid had a significant negative impact on healthcare demand, albeit with varying direction and strength across different specifications. For urbanization levels, in particular, as rural municipalities encountered a positive effect on hospitalizations whereas more urban municipalities saw negative effects. In turn, the level of socioeconomic development seemed to influence the magnitude of the financial assistance's impact, with a more pronounced effect observed in highly developed municipalities. The same is true for the different health conditions that cause hospitalizations. Notably, there was a decrease in hospitalizations related to Covid and eye issues and no effect on accidents leading to hospitalizations. While visits to the hospital caused by cardiovascular issues and mental health afflictions experienced an increase.

Through this endeavor I aim to contribute to two main strands of literature. Firstly, the literature exploring the impact of income on health demand includes numerous studies that use exogenous sources of income to isolate the true effect of income on health. These studies typically examine the mechanisms underlying the causal relationship, such as the alleviation of liquidity constraints and changes in consumption behavior. By analyzing different causes for hospitalization, I will be able to examine the mechanisms driving the relationship between the Emergency Aid and variations in hospitalizations.

Moreover, in the literature examining the effect of emergency aid programs during the pandemic, there is a lack of studies addressing one of the primary reasons behind their implementation: promoting social distancing. By focusing on their effect over hospital visits, I can provide insight into how these programs may influence individuals' behaviors towards social distancing during a pandemic. In addition, by examining the effects across socioeconomic and urbanization levels, I aim to deepen understanding into how these programs may affect vulnerable populations.

The structure of the paper is divided into 7 sections beyond this introduction. Section 2 looks at the context behind the implementation of the Emergency Aid program, the COVID-19 pandemic. Section 3 has a review of the relevant literature for this study. Section 4 describes data sources, details the information extracted and database construction. Section 5 details the methodology used, and Section 6 describes the results. While Section 7 has a discussion on heterogeneities and the possible mechanisms behind the results found and Section 8 concludes the paper with final considerations.

2 Context

2.1 Covid-19 Pandemic in Brazil

In Brazil the pandemic had a large impact overall, causing 710.174 Covid-19 related deaths and 38.521.738 Covid-19 infections by March 2024. The disease spread quickly, surpassing 1 million cases by the end of June 2020, just four months after the first case was detected in late February. This swift spread caused strain on the public health system, which became heavily focused on testing and treating Covid-19 cases, thereby limiting its capacity to assist those with other health issues and overcrowding critical care units. (GLOBO, 2020c).

This prompted government efforts to contain the spread of the airborne disease and alleviate the burden on the public health system. By mid-March 2020, some state governments, like the state government of São Paulo, issued a lockdown, permitting only essential services such as healthcare to remain open and restricting people’s movements to necessary activities like obtaining food (SÃO PAULO, 2020).

Along with the global economic crisis, social distance measures affected Brazilian society in a myriad of ways, namely by preventing people from working and keeping children from attending school. While there’s some evidence of a negative impact on children and teenager’s learning measures (BARROS ET AL., 2021), job loss and reduced working hours disproportionately affected low-income informal workers who couldn’t work remotely, leading to an increase in poverty and inequality. (MASRI et al, 2021; REIS et al, 2022)

Concurrently, there were various aid programs aimed at helping to mitigate the economic impact of the pandemic while encouraging social distancing, such as credit programs, unemployment insurance extension and cash transfers.

2.2 Emergency Aid Program

The Emergency Aid (EA) program was one of the stimulus initiatives created by the Brazilian government in April 2020[1] in an attempt to mitigate the economic and public health crises caused by the pandemic. It provided an unconditional cash transfer primarily to

[1] Via complementary Law 13.982 (BRASIL, 2020a)

unemployed individuals and informal workers. Its main objective was to supplement people's income and encourage them to stay home instead of going out to work.

In order to be eligible the unemployed person or informal worker was required to be at least 18 years of age, couldn't be a beneficiary of other federal assistance programs[2] (with the exception of Bolsa Família[3]), and needed either monthly family per capita income to be no higher than half of the minimum wage at the time (R$ 522,50) or total family income no higher than three times the minimum wage (R$ 3.135).

The EA granted a monthly stipend of R$600 generally, and an increased amount of R$1200 for single mothers who were the heads of their households. Each family could have up to two recipients, meaning female single-parent households could get up to R$1800 and other households up to R$1200. Bolsa Família beneficiaries received the higher amount between the two programs.

Initially, there were plans for three monthly payments, spanning from April to June. However, as the pandemic persisted and the crisis worsened, the payments were extended twice in 2020. First, for an additional two installments of the same value (R$600), and later to four installments of half the original value (R$300) (BRASIL, 2020c). In April 2021, another round of payments was initiated, this time at about a third of the original value with only a fraction of the previous beneficiaries eligible to receive it (BRASIL, 2021). By the end of 2020, the program reached 63.89 million people, totaling R$ 280.82 billion in expenses (CGU, 2024).

To qualify for the stipend payment, individuals who were not already beneficiaries of Bolsa Família or registered in the national social assistance registry Cadastro Único[4], were required to sign up either on a website or through a mobile app. Additionally, they needed to have accounts with national banks Banco do Brasil or Caixa Econômica. Despite the accessibility of online registration, many prospective beneficiaries sought assistance at physical Caixa Econômica locations (GLOBO, 2020b). This phenomenon could be attributed to the unreliable internet access in Brazil, particularly in poor and rural areas, potentially due to technological illiteracy or the high cost of internet services (IBGE, 2023). Furthermore, since a significant number of registrants lacked bank accounts, the government facilitated the opening

[2] Other assistance programs include social security, unemployment insurance and other cash transfer programs.
[3] Bolsa Família is Brazil's main conditional cash transfer program, aimed at reducing poverty and inequality by providing financial assistance to low-income families contingent upon conditions related to healthcare and education. There is widespread evidence of its positive effects on poverty, health, and education.
[4] Cadastro Único is a national centralized registry of low-income households and individuals across Brazil, primarily used to identify and target beneficiaries for various social welfare programs, including Bolsa Família.

of digital bank accounts with Caixa Econômica for approximately 30 million individuals (MDS, 2020b).

Starting in early April, the initial round of payments was made prioritizing individuals previously registered in Cadastro Único, single mothers and informal workers. These payments were directly deposited into bank accounts, allowing it to be used to pay bills and transfer funds to other accounts. By the end of April, the government established a withdrawal schedule for cash payments, potentially taking into account individuals who faced challenges accessing or navigating digital banking platforms. In order to avoid agglomerations, the schedule, outlined in **Table 1** below, categorized recipients into cohorts based on their birth month and assigned specifics payment collection days for each cohort to visit a Caixa Econômica. Subsequent rounds of payments were distributed concurrently, resulting in various groups receiving different rounds of payments simultaneously.

Table 1- EA Payment Bank Withdrawal Schedule

Birth Month Cohort	Day of Withdrawal
January & February	04/27
March & April	04/28
May & June	04/29
July & August	04/30
September & October	05/04
November & December	05/05

Source: Adapted from BRASIL (2020b).

This schedule delineating the first round of payments is the key to my identification strategy. The quasi-random allocation of cash payments allows me to conduct a comparative analysis among birth cohorts, distinguishing between those who had access to the Emergency Aid payments and those who did not at a specific moment in time. My focus will be on examining disparities in healthcare demand, particularly hospitalizations, between individuals who potentially experienced the income shock from the cash payment and those who did not.

This comparative analysis aims to shed light on the potential effects of the EA on liquidity constraints and consumption patterns among beneficiaries. Given the challenges related to technology and banking accessibility in Brazil, it is plausible to argue that marginalized populations, who typically face more pronounced limitations in these areas, are more likely to seek out cash payments, especially during times of crisis such as a pandemic.

3 Literature Review

3.1 Effect of Income Shocks on Health

There is extensive literature examining the causal impact of income on health, often utilizing income shocks as exogenous variation of income to provide more precise estimations of this relationship. Generally, studies indicate a clear positive relationship between income and health (LINDAHL, 2005; FRIJTERS, 2005), as well as between income and mental health (APOUEY; CLARK, 2015; LINDAHL, 2002; GARDNER; OSWALD, 2007).

Nonetheless, the traditional association between income and health may not hold true for health demand. Central to my investigation, hospitalizations not only reflect a demand for health services but also indicate the necessity to address health conditions. As existing studies propose that higher income levels are associated with increased demand for health services, it may indicate it is also associated with poor health outcomes. This phenomenon can be understood through different mechanisms.

Gross and Tobacman (2013) present evidence suggesting that an income shock, followed by a surge in risky consumption, can lead to heightened demand for emergency health services related to substance abuse. More generally, an income increase has been linked to the consumption of unhealthy substances such as drugs and alcohol, resulting in adverse health effects including accidents, cardiovascular disease and substance abuse (PHILLIPS et al., 1999; EVANS; MOORE, 2012; CASTRO, 2022; RUHM, 2000).

Conversely, it is believed that rising income levels can alleviate liquidity constraints that hinder individuals from accessing health goods and services, thereby increasing their demand for such resources. Both Gross et al.(2022) and Lyngse (2020) argue that an increase in consumption of health products like medication, prompted by an income shock, signals a reduction of liquidity constraints. Additionally, Belchior and Gomes (2021) contend that the estimated increase in Covid-19 related hospital visits, spurred by the Brazilian Emergency Aid (EA), is attributable to the alleviation of a liquidity constraint, particularly evident in credit-constrained areas.

In order to contribute with the literature exploring the effects of income shocks on health, this study will examine the different health conditions that motivate hospital visits. This approach will offer a more comprehensive understanding of the potential mechanisms underlying the link between the Emergency Aid and the demand for healthcare services.

3.2 Impact Evaluations of Pandemic Cash Transfer Programs

The widespread adoption of cash transfer programs during the pandemic inspired numerous studies investigating their impact on various outcomes. Many of these studies focused on the direct effects of the main purpose behind financial aid programs, income supplementation. Positive effects were found for pandemic financial aid programs over poverty in the US (HAN et al., 2020) and over financial resilience and food security in Bolivia (BOTTAN et al.,2020). Conversely, findings on the effect of cash transfers on consumer spending in the United States have been conflicting (CASADO et al., 2020; COIBION et al., 2020). Notably, evidence also suggest negative effects of the Brazilian Emergency Aid (EA) on inequality and extreme poverty (MASRI et al, 2021; CARVALHO et al, 2021; MENEZES-FILHO et al, 2021).

However, another objective of the cash transfer aid programs implemented during the pandemic was to encourage social distancing, aiming to prevent individuals from needing to venture out for work thus mitigating the spread of Covid-19 in large gatherings. Studies inspecting effects of aid programs on social distancing are limited, with some estimating the impact of the Brazilian EA on labor market participation (MENEZES-FILHO et al, 2021; LEVY; MENEZES-FILHO 2022) and some finding opposing effects on the demand for health services for different emergency aid programs (BANERJEE et al., 2020; BELCHIOR; GOMES, 2022; TOSSOU et al. 2021)

Complementing these findings, the present study aims to further investigate how an emergency cash transfer program may affect an individual’s inclination to socially distance, particularly by focusing on the different reasons behind hospital visits. While Belchior and Gomes (2022) estimate the EA has a positive effect on Covid-related hospital visits, this may not hold true for different conditions, especially considering the consistent governmental concern and discussion in Brazilian media at the time regarding the overcrowding of hospitals (GLOBO, 2020c).

4 Data

The main data source being used for this impact estimation is the Hospitalization System of Information (SIH/SUS), a comprehensive database of daily hospitalization records of Brazil's public health system, the Unified Health System (SUS)[5]. The database also includes information on private health facilities with services funded by SUS.

I used the individual level microdata available on the DATASUS data extraction website[6]. As my main analysis is restricted to the beginning of the Emergency Aid distribution during the pandemic in Brazil, I only extracted the data for the months of April and May of 2020.

The information taken from the SIH/SUS database for the main estimation consists of:

a) The number of hospitalizations per day and municipality;
b) Hospitalized individual's municipality of residence;
c) Hospitalized individual's birth month;
d) Date of individual's hospitalization and;
e) Principal cause of individual's hospitalization.

In addition, the hospitalized individuals' demographic characteristics such as age, sex, race and education level were used to analyze and select my cohorts of interest.

As I am seeking to estimate the effect of the Emergency Aid on hospitalizations, my dependent variable will be number of hospitalizations. Seeing as SIH/SUS stores daily hospitalization records, each observation on the database represents a single hospitalization. Therefore, by adding the number of observations for each day I obtain the number of hospitalizations per day. My independent variables will be indicator variables representing: a) my cohorts of interest, defined by birth month, and; b) the periods in which those cohorts may or may not have had access to the Emergency Aid, determined here by the dates of hospitalization.

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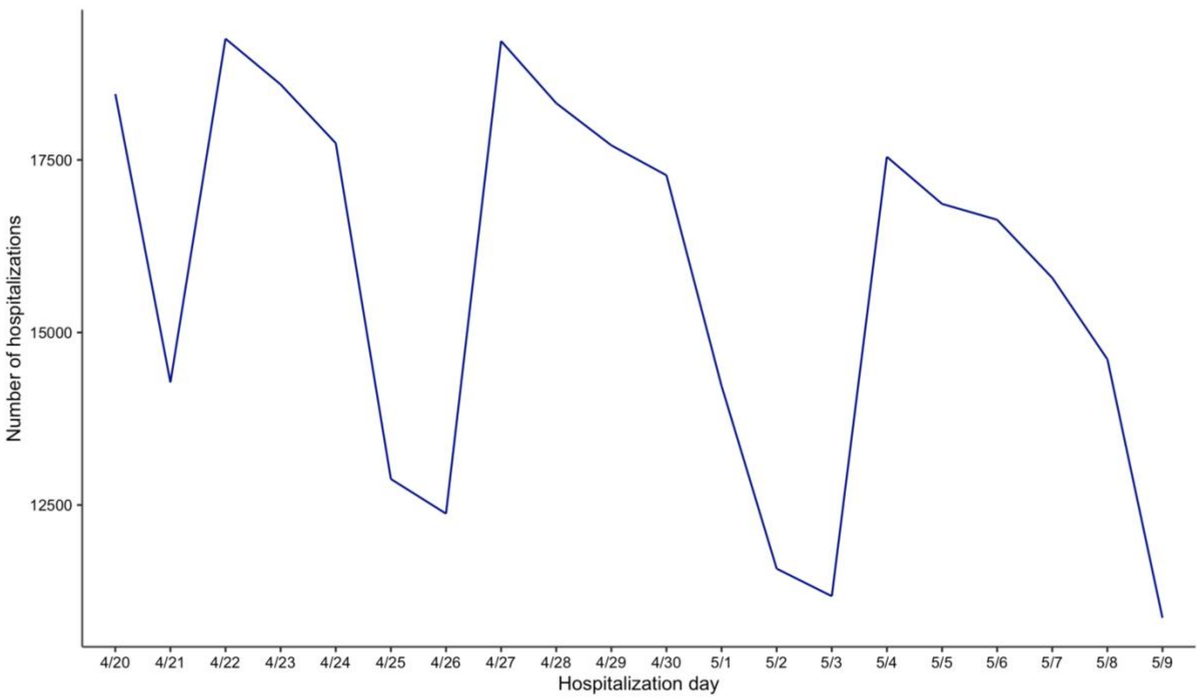
[5] Additionally, I used supplemental data with more information about ICDs and municipalities, as well as some extra documentation about the coding of the database. Available at: https://pcdas.icict.fiocruz.br/conjunto-de-dados/sistema-de-informacoes-hospitalares-do-sus-sihsus/documentacao/; https://www.ipea.gov.br/atlasviolencia/arquivos/artigos/9058-sistemahospitalar.pdf; https://pcdas.icict.fiocruz.br/conjunto-de-dados/sistema-de-informacoes-de-mortalidade-sim/dicionario-de-variaveis/;

[6] BRASIL. Ministério da Saúde. DATASUS. Transferência de Arquivos. SIHSUS – Sistema de Informações Hospitalares do SUS: 2020. Disponível em: https://datasus.saude.gov.br/transferencia-de-arquivos/ Acesso em: 24 nov. 2023.

There were important adjustments made to the original SIH/SUS database in order to achieve the final database. At first the database's sample was restricted to hospitalization days around the EA payment schedule, April 20th to May 10th. Moreover, considering the eligibility criteria for the EA transfer, all underage individuals were removed (17 years old and younger).

Graph 1 below illustrates the average number of municipal hospitalizations for each day of the period of interest. The dips in the hospitalization data correspond to weekends and holidays (such as April 21st), when it is likely there is less data collection activity. This reduction could stem from fewer individuals visiting hospitals or hospital staff primarily entering data on regular workdays. The consistent peaks in hospitalizations immediately following these dips suggest that the latter explanation is more plausible.

Graph 1 – Average number of Hospitalizations per day



Source: Made by author using 2020 SIH/SUS individual-level data.

To ensure satisfactory variation in hospitalization numbers, it was important to calculate them both by day and municipality. Thus, the original individual-level database was aggregated by municipalities. However, this left a database that did not cover all Brazilian municipalities, as it included only those with at least one hospital visit in that particular period.

In order to accurately measure differences between cohorts and periods, it was crucial to include municipalities with zero hospitalizations in the period of interest for the chosen cohorts. Given that one of the aims of this dissertation is to estimate whether the EA caused beneficiaries to stay at home or if it gave them an incentive to seek hospital care, it is essential to include instances with zero hospitalizations. Therefore, for each period and cohort combination, all municipalities without any records of hospitalization were added with the hospitalization count set to zero. This process resulted in a final database comprising 23,232 municipal-level observations, including all Brazilian municipalities across the different cohorts and periods analyzed.

To analyze the heterogeneity of the impact based on hospitalization causes, the primary diagnosis variable was categorized into the most aggregate categories of ICD codes, leaving me with 21 possible disease "groups". The individual-level database was then aggregated by municipality, period, cohort, and ICD disease groups.

Following the same approach of the main analysis, the number of observations by municipality, period, cohort, and disease groups was calculated to create the dependent variable. Successively, municipalities with zero hospitalizations were included for each combination of period, cohort, and disease group, ensuring all cases were represented in the dataset. Consequently, the database expanded to 487,872 observations, detailing 21 ICD codes for each municipality, cohort, and period.

Subsequently, additional municipal-level databases were integrated into the main database. The 2022 Brazilian Demographic Census[7] provided the municipal population number, used as weight in the regression estimations. For added heterogeneity analysis, municipal GDP per capita from the System of National Accounts[8] were converted into quartile values.[9] Finally, the municipal degrees of urbanization, from IBGE's Rural and Urban classifications[10], where divided into three levels of urbanization – low, moderate, and high.[11]

Considering a municipality's GDP per capita can help separate the effect of the policy on populations with varying socioeconomic levels, allowing for an inspection of lower socioeconomic regions where the cost of living is smaller but there may be a greater need for aid during a pandemic. Similarly, analyzing the differential impact of the cash transfers for

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[7] IBGE (2022a).
[8] IBGE (2022b).
[9] The first quartile is between R$4,921 and R$11,605, the second quartile is between R$11,605 and R$20,405, the third quartile is between R$20,405 and R$34,211, while the fourth quartile is between R$34,111 and R$590,595.
[10] IBGE (2017).
[11] Urbanization levels are defined by the percentage of urban population of each municipality, low being under 50%, moderate between 50% and 75% and high 75% and over.

beneficiaries living within different levels of urbanization can help distill how the policy may have reached regions with less access to technology and/or health services.

5 Methodology

To accurately gauge the impact of a public policy or program on specific outcome, it is crucial to discern the direct influence of that policy, excluding external factors. In this case, to evaluate the influence an emergency aid cash transfer program may have on health demand, it is imperative to filter out factors that could affect both demand for health and the receipt of the Emergency Aid. For instance, the cohort accessing the EA payment could be inherently more likely to seek out government services, therefore being more likely to both participate in the emergency aid program and demand more public health services. This would create a selection bias in the impact estimation, as the group who chooses to participate would be innately different from the group that chooses not to.

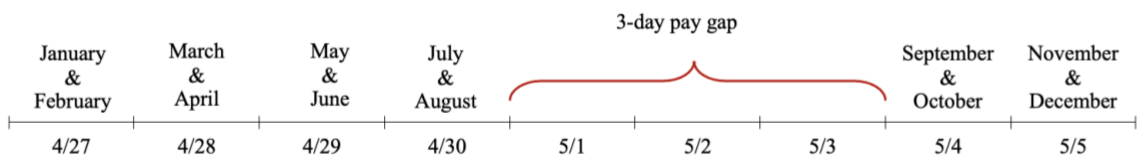
Ideally, I would be able to observe the same individual both with and without the program to accurately assess their behavior in each scenario, thus determining the program's true impact. However, it is impossible to simultaneously observe someone in two distinct states – they are either enrolled in the program or not. Generally, this dilemma leads to the estimation of a "counterfactual" individual who is not part of the program but shares enough similarities with someone who is, to the extent that comparing them would mirror the scenario of observing the same person both within and outside of the program concurrently.

Various methods can be employed to identify suitable counterfactuals for individuals benefiting from a policy or program. The ideal for impact assessment of a public policy involves random selection of participants (the treatment group) and non-participants (the control group). Randomization helps isolate the policy's causal effect by ensuring that participants aren't self-selected into the program, consequently avoiding inherent differences between the treatment and control groups, and eliminating selection bias. If selection criteria are non-random – for example based on factors like age – comparing treatment and control groups may lead me to compare groups with different age compositions and potentially varying health behavior patterns, affecting the reliability of my impact assessment.

When randomized selection is not feasible due to monetary or time restrictions, or if the program is already in effect, other methods can be used to define a counterfactual group. Utilizing the government's Emergency Aid payment schedule for the initial round of payments, as outlined in Table 1, I leverage the cohort allocation based on birth month to delineate my counterfactual group. Since the month of one's birth is unrelated to differing behavior in health service demand, this selection criteria can be considered nearly randomized.

As the six cohorts have access to EA payment on different days, the number of treated cohorts gets incremented by one additional cohort each day. However, there is an exception in the period preceding the last two cohorts' payday. Illustrated in **Figure 1** below, this period consists of a three day interval between May 1st and May 3rd (or, a 3-day pay gap), when no additional cohorts are afforded access to EA payments, ergo, there are no additions to the number of treated cohorts. This 3-day pay gap in the schedule extends the divide between the first four cohorts granted access to the EA and the last two, thereby widening the timeframe for scrutinizing behavioral disparities between those with and without access to EA payments. Therefore, my main comparison will be between the cohorts eligible to receive the EA before and after this gap (specifically before and after May 3rd).

Figure 1 – Emergency Aid Payment Timeline



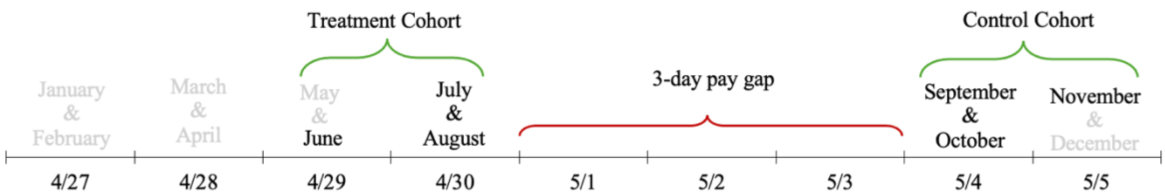
Source: Made by author based on BRASIL (2020b).

5.1 Treatment and Control Cohorts

Since the SIH/SUS hospitalization database does not have information on who actually received an Emergency Aid cash payment on the scheduled day, my definition of the treated cohorts represents those who were eligible to receive the EA, regardless of whether they actually received it or not. The same applies to the control cohorts. Because I also lack information on who did not receive the EA, my control cohorts consist of those who were not eligible to receive the cash payments during the period of interest. In this context, the cohorts are comprised of hospitalized individuals whose birth months coincide with those specified in the withdrawal payment schedule.

Considering this, I selected the appropriate treatment and control cohorts by looking for the most similar hospitalized cohorts, comparing those with potential paydays before and after the three-day payment gap. Whilst all cohorts were mapped out based on hospitalized individual's birth month, I conducted tests to identify the cohorts that were most suitable for comparison. After testing different combinations for treatment and control groups, I found that the hospitalized cohorts most similar to each other during the period of interest were those born in the months of June, July and August (treatment group) and those born in the months of September, October and November (control group). The groups and where they fall in the payment schedule timeline can be seen in **Figure 2** below.

Figure 2 – Emergency Aid Payment Timeline – Treatment and Control Cohorts



Source: Made by author based on BRASIL (2020b).

Their similarities can be corroborated in **Table 2** below, where some observable characteristics registered in the hospitalization database are shown side-by-side. The average proportions for sex, education and race are all shown to be nearly identical and their differences not statistically significant. While the averages in population of young and middle-aged people, while appearing to be very close, are shown to be statistically different between the cohorts[12]. Additionally, the proportion of the cohort populations within Brazil and their size (N) within the hospitalizations database appear to be the same.

Table 2 – Socioeconomic Characteristics by Cohort for Pre-EA Period: Average and t-tests

Characteristics	Cohorts		t-statistic
	Treatment (June-July-August)	Control (September-October-November)	
Women	0,62	0,63	0,52
Illiterate	0,05	0,05	-0,07
			(continua)

[12] This statistically significant difference may be due to the size of the sample, which would not jeopardize my estimation.

Characteristics	Cohorts		t-statistic
	Treatment (June-July-August)	Control (September-October-November)	
High School Education	0,49	0,49	0,15
Nonwhite	0,56	0,56	0,21
Black & Brown	0,53	0,53	0,28
Young (18-29)	0,30	0,29	-2,85
Adult (30-39)	0,18	0,18	0,86
Middle Aged (40-59)	0,23	0,24	3,20
Elderly (60+)	0,30	0,30	-0,83
Brazilian Population (%)	0,24	0,24	
N	85637	84056	

Source: Elaborated by author using SIH/SUS individual-level data and the 2019 PNAD-Contínua (IBGE, 2024) for Brazilian population proportion by cohort)

Note: N reflects the size of the cohort populations in the SIH/SUS individual-level database from April 19th to May 10th.

This limited the sample of my analysis to hospitalized individuals whose birth months are June, July, August, September, October, and November. Focusing the analysis on these cohorts with months that are closer together may help to control for non-observable characteristics which could affect how one responds to receiving the EA.

Nonetheless, there is still a possibility that non-observable characteristics may influence the cohorts' health demand behavior, causing a bias in my impact estimate. To mitigate that possible bias, I will utilize a difference-in-differences approach to calculate the differences in the cohorts' health demand behaviors that might be due to the EA payment.

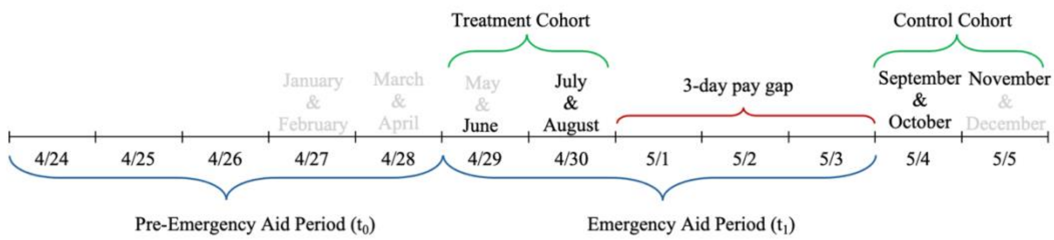
5.2 Period of Interest

As my main assessment is a difference-in-differences estimation comparing health demand between cohorts at two moments in time, it is also essential to determine the periods of interest. Since I am investigating hospitalizations before and after the potential receipt of the EA, the date of hospitalization will designate my periods of interest. Using date of hospitalization from the original SIH/SUS database, an indicator variable was created to reflect periods before and after the cohorts could potentially receive the EA.

Knowing the cohorts of interest makes defining the Emergency Aid payment period (after the potential EA receipt) easy, as it is the period when the treatment cohort (June-July-August) is eligible to receive the cash transfer while the control cohort (September-October-November) is not yet eligible to do the same. Ergo, the EA period (t_1), starts on the day the Emergency Aid payment becomes available for the treatment cohort on April 29th and ends on May 3rd, the day before the EA becomes available for the control cohort. Looking at this period in time will permit a comparison of behavior between the similar cohorts at a time when only one of them is eligible to receive the EA, leading to an estimation of how potentially receiving the payment could alter the treatment cohort's health demand.

Because the period between the treatment and control cohort's potentially receiving the cash transfer is five days, the period pre-EA (before potential EA receipt for the treatment cohort) was also restricted to five days. Accordingly, the pre-EA period starts on April 24th and ends on April 28th, the day before the treatment cohort is eligible for the EA. Figure 3 below illustrates the periods chosen, how they relate to cohort paydays and where they fall on the EA withdrawal payment timeline.

Figure 3 – Emergency Aid Payment Timeline: Cohorts & Periods of Interest



Source: Made by author based on BRASIL (2020b).

5.3 Difference-in-differences model

The difference-in-differences model estimates 2 differences at the same time, between the cohorts and between the different time periods. Taking the difference in the outcome of interest before and after the EA payments for both cohorts, then calculating the difference between the cohorts' time differences. The equation below shows the difference-in-differences estimator, where the first difference for the outcome calculated is the average time difference for the treatment cohort, and the second difference calculated is the average time difference for the control cohort.

$$\beta_{DiD} = \{E[T_i = 1, t = post] - E[T_i = 1, t = pre]\} - \{E[T_i = 0, t = post] - E[T_i = 0, t = pre]\} \tag{1}$$

Doing this controls for time invariant differences between the cohorts that could affect their behavior towards the outcome. It also controls for time trends both cohorts may be experiencing at the same time that could affect the outcome, such as the Covid-19 pandemic which may have affected people's behavior towards demand for health services in either direction.

In order to calculate a statistically relevant estimate, I will use an Ordinary Least Squares (OLS) regression, which will give a better idea of how statistically significant the number found is. The equation below demonstrates how the OLS model was specified:

$$Y_{mct} = \alpha + \beta_1 Cohort_T + \beta_2 Period_{t_1} + \beta_3 Cohort_T \times Period_{t_1} + \gamma_m + \varepsilon_{mct} \tag{2}$$

Let Y_{mct} be the number of hospitalizations for municipality m, cohort c and period t, and let $Cohort_T$ be an indicator variable equal to 1 for the Treatment cohort (June-July-August) and equal to zero for the Control cohort (September-October-November). Likewise, let $Period_{t1}$ be a dummy equal to one for the period after the Treatment cohort receives the Emergency Aid (April 29th until May 3rd), while the period before (April 24th until April 28th) is marked with a zero. γ_m denominates municipality fixed-effects, ε_{mct} is the error term and α is the intercept.

The model for investigating heterogeneity and different causes of hospitalization is slightly different:

$$Y_{mctj} = \alpha + \beta_1 Cohort\ _T + \beta_2 Period_{t_1} + \beta_3 Cohort_T \times Period_{t_1} + \gamma_m + \varepsilon_{mctj} \tag{3}$$

Where j indicates the level of the heterogeneity variable or number of ICD code being analyzed. For GDP per capita that includes 4 quantiles of municipal GDPs, whereas for urbanization degrees[13] that includes 3 levels of urbanization- low, moderate, and high. While for hospitalization causes, j goes through the highest level of aggregate ICD codes 1 to 22.

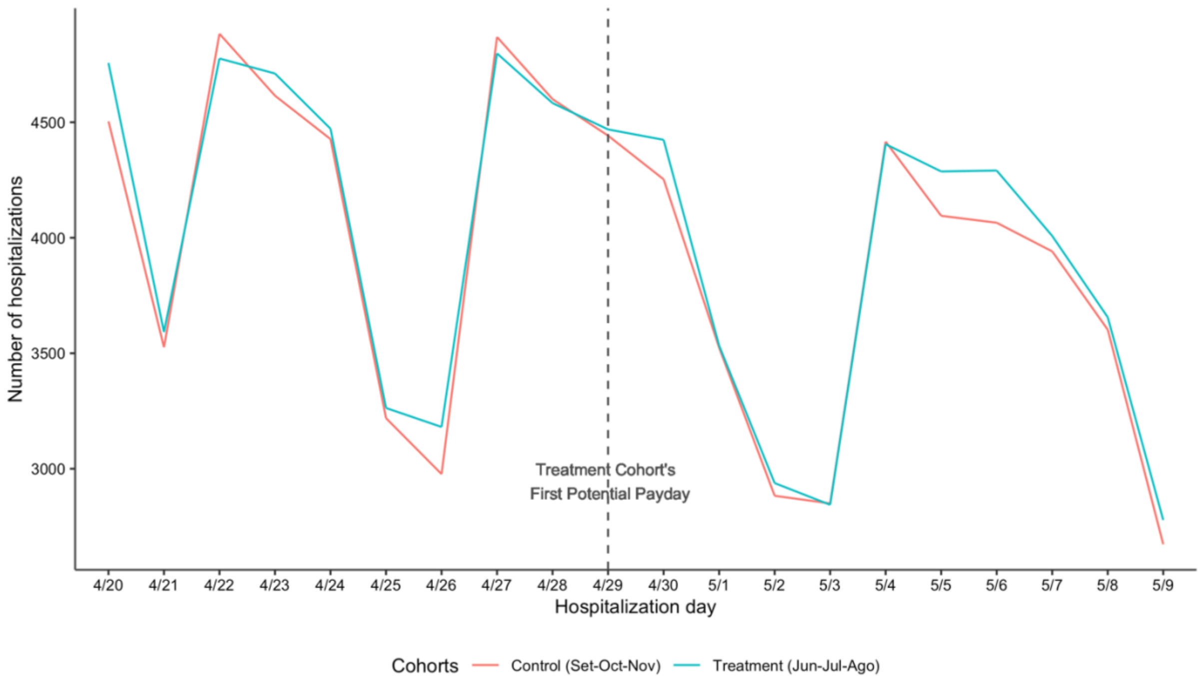
All specifications of the regression use municipal population as weight, which controls for differences in effect that may be due to the difference in population size for each municipality. Additionally, municipal fixed-effects were added in order to control for time invariant non-observable heterogeneities between municipalities that can affect how people in these municipalities interact with the treatment and the outcome.

Three hypotheses must hold true for difference-in-differences to produce the best estimates. First, the treatment and control cohorts must have parallel trends, meaning the outcome of interest must have a similar evolution in time for both cohorts before the start of the program. This signals that the progression of the outcome variable for the control cohort corresponds to the trajectory of the outcome variable for the treatment cohort in the absence of the program. Namely, if the treatment cohort was not eligible to receive the EA payment, that is how their healthcare demand would advance.

The parallel time trends between the cohorts can be seen in **Graph 1** below, particularly before the dotted line marking the first potential payday for the Treatment Cohort.

[13] Degree of urbanization consists of the percentage of the population living in urban areas. A low degree has under 50% of urban population, moderate degree is between 50% and 75% of urban population and high is 75% and above urban population.

Graph 2– Hospitalization time trend - April 20th to May 9th: Treatment vs. Control Cohorts



Source: Made by author using 2020 SIH/SIS individual-level data.
Note: The dips in the hospitalization data represent weekends and holidays (in the case of April 21st).

The second hypothesis is that the composition of the treatment and control groups, such as socioeconomic characteristics, should not change significantly between the periods before and after the program. Seeing as this study is looking at daily hospitalizations and a period in time that spans 10 days, it is difficult to imagine the cohorts' compositions could change in such a short amount of time.

Lastly, for the third hypothesis to hold neither cohort should be affected significantly by changes that may occur after the program. Considering the short period being investigated, for this to be false there would need to be a very significant event that affected both cohorts within 10 days. There is no evidence of such event happening after the EA payments were made.

Holding these assumptions as true, estimating the coefficient of the interaction between the cohort and period indicators, $\widehat{\beta_2}$, would give me the average treatment effect, or the average effect of the Emergency Aid on health demand. Nonetheless, as my treatment and control groups are defined by month of birth, representing those eligible to be in the treatment and control cohorts, the effect found in this estimation will reflect the Emergency Aid's impact on the intention to treat.

6 Results

The result of my primary model is shown in Table 3 below, its coefficients reporting different things. The intercept shows that average hospital visits for the control cohort during the period that I designated as pre-EA, when neither of my cohorts could have received the EA cash transfer, was of 8.23. The treated cohort coefficient represents the difference between the cohorts in the pre-EA period, revealing that before the EA receiving period, the treatment cohort had an average of 0.65 additional hospital visits than the control cohort.

Table 3 – Effects of the EA on hospitalizations

Dependent variable: Number of hospitalizations	
Treatment effect (Treated Cohort X EA period)	**-5.99***** (0.53)
Treated Cohort (born in June, July or August)	0.65* (0.37)
EA period (April 29 to May 3)	-8.11*** (0.37)
Control average pre-EA	8.23 (12.84)
N	23,232
R^2	1.00
Adjusted R^2	0.99
F-Statistic	623.02

Note: (i) * p<0.1, **p<0.05, ***p<0.01.
(ii) In parenthesis are the standard errors for each estimate.

Conversely, the EA period coefficient gives me the difference in hospital visits for the control cohort between the pre-EA and EA periods, indicating that the control cohort had a decrease in hospital visits of 8.11 from one period to the next. Lastly, the coefficient for the interaction term represents the difference-in-difference between the cohorts and between before and after periods, which gives me the causal impact of being provided with the financial assistance. Therefore, it can be concluded that potentially obtaining the EA cash transfer leads to an average reduction of 6 hospital visits for each municipality.

Despite the main trend in hospitalizations for the EA period being negative for both cohorts, the treatment cohort shows an additional drop of about 6 in average hospitalizations. This finding can be interpreted as the average effect of being granted the Emergency Aid payments in cash. Importantly, as the cohorts are defined by eligibility for treatment and not actual treatment, the estimates reflect the impact of the intention to treat.

6.1 Robustness

In order to provide some robustness to my main impact estimate, I realized the same regression as above changing one parameter specification.

The periods of interest in my main specification span 5 days, which is less than a whole week, possibly leading to each of the periods having a different composition of hospitalization trends. The different compositions could happen if one of the periods had days with unusual hospitalization trends and the other did not. This includes weekends and holidays, that tend to have lulls in hospital activity, as well as Mondays, which tend to have post-weekend peaks. While both the 5-day pre-EA and EA periods include weekends, holidays are only present in the EA period (May 1st) and Mondays only appear in the pre-EA period.

The different composition of the periods being compared could affect the number of hospitalizations they will have, the holiday causing the EA period to have a day with less hospitalizations than usual and the Monday causing the pre-EA period to have more hospitalization than usual. This could mean the comparison of hospitalizations between the 5-day periods would bias my estimate, possibly giving me a false negative impact estimate.

Changing my periods of interest from 5 to 7 days allows to me compare cohorts in week-long periods, with a weekend, a holiday, and a Monday in each, balancing the composition of the periods. The results in Table 4 shows that the model with better balanced period compositions revealed a negative effect of bigger magnitude than the primary model, suggesting that the differential compositions of the periods in my main specification are not the cause of the negative effect found.

Table 4 – Effects of the EA on hospitalizations: Robustness

Dependent variable: Number of hospitalizations	
Treatment effect (Treated Cohort X EA period)	**-7.07***** (0.41)
Treated Cohort (born in June, July, or August)	2.65*** (0.29)
EA period (April 29 to May 3)	-2.44*** (0.29)
Control average pre-EA	6.91 (9.99)
7-day periods	x
N	23,232
R^2	1.00
Adjusted R^2	0.99
F-Statistic	2,129.92

Note: (i) The 7-day periods are: Pre-EA from Apr. 20th to Apr. 26th and EA period from Apr. 27th to May 3rd
(ii)* p<0.1, **p<0.05, ***p<0.01.
(iii) In parenthesis are the standard errors for each estimate.

7 Heterogeneities

7.1 Urbanization and Economic Development

When analyzing municipalities with different levels of urban population, the effect of the Emergency Aid is not as straightforward.

Table 5 demonstrates that for municipalities with low levels of urbanization, receiving the emergency cash stipend had a positive effect on hospitalization, while for highly urbanized municipalities it had the opposite impact, reducing the average number of hospitalizations by a magnitude even bigger than the general impact.

Table 5 – Effects of the EA on hospitalizations/demand for medical care: Urbanization and GDP per capita

Dependent variable: Number of hospitalizations							
	Urbanization			GDP per capita (Quartiles)			
	Low	Moderate	High	1st	2nd	3rd	4th
Treatment effect	**0.17*** **	**0.05**	**-8.26*** **	**0.04**	**-0.51*** **	**-0.51**	**-12.92*** **
(Treated Cohort X EA period)	(0.06)	(0.12)	(1.13)	(0.11)	(0.19)	(0.36)	(1.48)
Treated Cohort	0.08*	-0.00	0.87	-0.11	0.54***	1.98***	0.06
(born in June, July or August)	(1.02)	(0.08)	(0.80)	(0.08)	(0.13)	(0.26)	(1.05)
EA period	-0.34***	-0.48***	-10.96***	-0.47***	-0.70***	-4.87***	-14.78***
(April 29 to May 3)	(0.04)	(0.08)	(0.80)	(0.08)	(0.13)	(0.36)	(1.05)
Control average pre-EA	0.34	3.23***	19.11*	1.03	0.70	4.57	14.09
	(1.02)	(1.10)	(11.10)	(0.69)	(2.47)	(4.64)	(27.96)
N	10,112	5,800	6,348	5,624	5,592	5,540	5,524
R^2	0.76	0.82	1.00	0.85	0.96	0.99	1.00
Adjusted R^2	0.68	0.76	0.99	0.80	0.95	0.99	0.99
F-Statistic	9.53	13.88	610.76	16.76	69.94	301.13	614.64

Note: (i) Urbanization levels are defined by the percentage of urbanization of each municipality, low being under 50%, moderate between 50% and 75% and high 75% and over.
(ii) GDP quartiles are defined as follows: the first quartile is between R$4,921 and R$11,605, the second quartile is between R$11,605 and R$20,405, the third quartile is between R$20,405 and R$34,211, while the fourth quartile is between R$34,111 and R$590,595.
(iii) * p<0.1, **p<0.05, ***p<0.01.
(iv) In parenthesis are the standard errors for each estimate.

Comparing municipalities with different economic development levels, a similar trend can be seen. The effect for municipalities in the lowest quartile of GDP per capita seems to be positive, but it is also not statistically significant. In contrast, while for the other quartiles the effect appears to be negative, it is evident the biggest effect can be encountered for municipalities in the highest quartile of GDP per capita, with a magnitude that is also bigger than the general effect found.

7.2 Causes of hospitalization

As previously mentioned, the causes of hospitalization were categorized using the most aggregate form of the International Classification of Diseases, which created 22 disease groups, 21 of which are present in the SIH/SUS database[14]. In addition, I created a group using 2 of the main ICDs that were being used to designate Covid-19 related hospitalizations at the time[15] and are present in the database. Table 6 below shows the regression results for 9 of the 21 disease groups as well as the Covid group I created. It also includes standardized treatment effect estimates, which allows for comparison of effect magnitude between the hospitalization causes while accounting for differences in value range[16].

Table 6 – Effects of the EA on hospitalizations/demand for medical care by Causes of Hospitalization: Level and Standard Deviation

Dependent variable: Hospitalizations									
	Causes by disease group								
	Covid	Respiratory	Infections and parasites	Tumors	Eye related	Accidents	Cardiovascular	Blood and immunity	Mental health
Treatment effect	**-2.44***** (0.08)	**-2.36***** (0.10)	**-2.59***** (0.09)	**-0.52***** (0.09)	**-0.33***** (0.02)	**0.15** (0.10)	**0.69***** (0.09)	**0.11***** (0.02)	**0.14***** (0.04)

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[14] Group 20 “External causes of morbity and mortality” is only used to classify causes of death.
[15] The two main ICDs present in the dataset that were recommended by the World Health Organization (WHO, 2020) and adopted by the Brazilian government (BRASIL, 2020) to identify Covid-related hospitalizations/deaths in the beginning of the pandemic are B342 and U049.
[16] Standardized coefficients express the average change in standard deviations of an outcome variable associated with a 1 standard deviation change in a predictor variable.

(continua)

Dependent variable: Hospitalizations									
	Causes by disease group								
	Covid	Respiratory	Infections and parasites	Tumors	Eye related	Accidents	Cardiovascular	Blood and immunity	Mental health
Treated Cohort	1.87***	0.66***	1.53***	0.43***	0.19***	-0.80***	-0.52***	-0.20***	-0.25***
	(0.06)	(0.07)	(0.06)	(0.06)	(0.01)	(0.07)	(0.07)	(0.01)	(0.03)
EA period	1.38***	0.62***	1,55***	-1.35***	-0.02*	-2.07***	-2.20***	-0.10***	-0.34***
	(0.06)	(0.07)	(0.06)	(0.06)	(0.01)	(0.07)	(0.07)	(0.01)	(0.03)
Intercept	-1.02	0.20	-0.14	1.09	0.00	1.40	1.44	0.12	0.26
	(2.01)	(2.32)	(2.19)	(2.22)	(0.45)	(2.45)	(2.30)	(0.37)	(1.05)
	-0.38***	-0.67***	-0.33***	-0.17***	-0.74***	0.04	0.17***	0.23***	0.10***
Treatment effect (SD)	(0.01)	(0.03)	(0.01)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	(0.03)
N	23,232	23,232	23,232	23,232	23,232	23,232	23,232	23,232	23,232
n	1,154	3,115	3,191	2,461	217	3,893	4,046	533	1,045
R^2	1.00	0.98	1.00	0.97	0.83	0.98	0.98	0.89	0.94
Adjusted R^2	0.99	0.97	1.00	0.96	0.78	0.97	0.98	0.85	0.92
F-Statistic	748.72	135.53	856.46	96.55	14.84	125.37	168.75	24.16	49.86

Note: (i) While N represents the observations in the final sample with municipalities that had zero hospitalizations, n shows the number of municipalities that had a positive number of hospitalizations for each ICD.
(ii) * p<0.1, **p<0.05, ***p<0.01.
(iii) In parenthesis are the standard errors for each estimate.

At a quick glance it is clear the negative impact of the EA on health demand does not hold for all causes of hospitalization. The Covid coefficient refers to the codes intended for official use during hospital admissions for COVID-19 infections. However, during the initial stages of the pandemic in Brazil, which began with the detection of the first case in March 2020, hospitals were likely not fully equipped to implement the government-mandated codes or accurately distinguish Covid-19 cases from other viral illnesses, as the underreporting of deaths from Covid-19 may corroborate (Orellana et al, 2023). Accordingly, the categories identifying respiratory diseases[17] as well as infections and parasites[18] (a group that includes one of the official Covid-19 ICDs) should also partly reflect hospitalizations caused by Covid-19.

[17] Respiratory category includes pneumonia, flu, acute infections and chronic diseases of airways.
[18] Infections and parasites category includes intestinal issues, tuberculosis, STDs, viral hemorrhagic fever, viral hepatitis, HIV, bacterial and zoonotic illnesses.

Consistent with the broader impact assessment, the three statistically significant Covid-related coefficients indicate that the Emergency Aid had an adverse effect on Covid-related hospitalizations. However, contrary to the pattern observed in the general model, the coefficient for the Emergency Aid period for all three Covid-related groups implies that the group not eligible to receive the Emergency Aid experienced an increase of Covid-related hospitalizations during the 5-day interval when only the treatment group had received the aid. This implies that the Emergency Aid may have had a greater impact on hospitalizations from Covid than on overall hospitalizations. Additionally, the standardized estimates suggest the biggest effect among the Covid groups, and second biggest amongst all disease groups, is for the respiratory diseases.

With trends closer to the general model, the other disease groups for which the EA caused a decrease in hospitalizations are tumors and eye related issues[19]. While hospitalization related to tumors saw a significant downward shift, the standard deviation estimates show that hospitalizations related to eye issues saw the most substantial decline for all disease groups due to the EA.

On the other hand, the pandemic financial assistance led to a rise in hospitalizations for illnesses that fall under the categories of cardiovascular diseases[20], mental health disorders[21], as well as blood and immunity issues[22]. Analysis of standard deviation estimates reveals that the income shock had the largest positive impact on hospitalizations caused both by blood and immunity issues, followed by significant impacts on hospitalizations due to cardiovascular diseases and mental health conditions. This indicates the added income of the Emergency Aid led people to seek more help with health issues within these disease groups.

Notably, the estimate for hospitalizations due to accidents is not statistically significant, suggesting that the Emergency Aid had no effect on the number of accidents resulting in hospitalizations.

[19] Eye related category includes glaucoma, blindness, optical muscle and nerve issues.
[20] Cardiovascular category includes nerve disorders, heart attacks, strokes, thrombosis and hypertension.
[21] Mental health category includes organic and psychoactive created mental disorders, schizophrenia, mood disorders and stress-related disorders.
[22] Blood and immunity category includes anemia, coagulation defects, blood diseases and disorders that compromise the immune system.

7.3 Discussion

Though the general trend for both cohorts is negative, the Emergency Aid has had a significant impact on hospital visits, causing municipalities to have, on average, 6 less visits for the 5-day EA period. To better understand the mechanisms behind the effect of the Emergency Aid on hospital visits, I examine the heterogenous impacts by development level, urban population percentage and hospitalization causes.

Whereas rural and urban areas saw effects in opposite directions, the level of economic development of municipalities seems to have affected the magnitude of the EA impact. According to Pereira and Nakabashi (2011), disparities in institutional quality can explain significant differences in GDP levels between municipalities. Based on this premise, municipalities with higher levels of GDP per capita would have better institutions, which could be reflected in how each government handled the pandemic and/or how the Emergency Aid distribution. Coupled with the differences in population density of rural and urban areas, this could explain my results.

Rural areas are defined by IBGE (2023) partly by low population density, and urban areas by higher population density[23]. In densely populated areas, there may have been greater government-led encouragement for social distancing during the pandemic, with significant focus on discouraging hospital visits for non-emergency reasons. Therefore, there was great incentive for people in urban areas to stay at home, which may have contributed to the negative impact of the EA in urban municipalities.

In the same vein, the low population density of rural areas may have caused local government to worry less about the overcrowding of hospitals and the encouragement of social distancing. In addition, living in rural areas further from urban centers could mean less regular access to medical services, either due to distance from hospitals, cost of transportation, and/or inability to cease work to go to a doctor. Ergo, the EA stipend could have freed rural recipients to stop working and take care of their health.

[23] Low population density areas typically distinguished by landscape modifications primarily due to human activities associated with agriculture and other forms of economic use, which may include small, urbanized clusters and/or natural fragments.

7.4 Mechanisms

Examining the health conditions that lead to hospitalization during the period of analysis, it is possible to explore various mechanisms that may underlie the EA payment's influence on healthcare demand. Acknowledging that promoting social isolation was one of the main goals of the Emergency Aid, the adverse effect it had on hospital visits generally serves as evidence of that objective being achieved.

Coupled with the negative impact on Covid-related hospitalizations, the upward trend in Covid hospitalization among those who were not eligible to receive the EA provides further validation of this evidence, implying that recipients of the cash transfer were able to avoid Covid contamination by staying home and not having to work outside. This assumption is reinforced by the fact that the periods before and after potentially receiving the EA span 5 days, which corresponds to the Covid incubation period. This suggests that after receiving the EA and remaining at home thereafter, beneficiaries were less likely to contract Covid, whereas non-beneficiaries, lacking the same incentive to stay at home, continued to work and faced a higher risk of contracting the virus.

In additional backing to the encouragement of social isolation, the effects on hospitalizations due to eye and tumor related conditions were also negative. Given the government's dissuasion of hospital visits for non-emergency issues, the negative impact on issues less likely to be emergencies like eye related problems sustains the belief that the EA caused increased social isolation. Similarly, the effect on visits due to tumors suggests individuals with tumor-related conditions sought medical care less frequently, perhaps due to concerns about contracting Covid.

Decisively, the most compelling evidence for the EA fostering greater social isolation is the lack of a significant impact on hospitalizations resulting from accidents. Unlike the existing literature, I found no evidence of a causal link between the income shock from the financial aid and accidents.

Prior research has also associated the income shock and accidents relationship to excessive consumption of unhealthy items due to the increase in income (Phillips et al., 1999; Evans and Moore, 2012; Castro, 2022; Ruhm, 2000). Though the mechanism of consumption does not seem to be at play in regards to hospitalizations due to accidents, the estimates found for other disease groups may imply otherwise.

In particular, the positive effect found for hospitalizations stemming from cardiovascular diseases signals consumption may play a role in the hospitalization-income shock relationship. The literature on the consumption mechanism indicates a connection between cardiovascular issues, such as heart attacks and/or strokes, and the overconsumption of unhealthy items following an income shock. Hence, it is possible the higher number of hospitalizations due to cardiovascular problems caused by the Emergency Aid is driven by consumption.

Comparing the effects on disease groups related to higher consumption of unhealthy items, it can be inferred that even though the consumption mechanism could be behind the increased hospitalizations for cardiovascular diseases, that is not true for hospitalizations caused by accidents. This seems to insinuate that although the EA beneficiaries may have engaged in unhealthy consumption behaviors, their adherence to social isolation may have prevented them from experiencing more accidents.

Another significant mechanism discussed in the literature regarding the impact of income shocks on health is liquidity constraint. The positive effects detected for disease groups such as mental health issues and blood and immunity problems suggest that the EA may have alleviated liquidity constraints by enabling recipients to forgo work and access medical treatment. During the pandemic, there was a surge in mental health issues attributed to heightened social isolation and stress induced by the global situation, exacerbating existing conditions and giving rise to new ones among previously unaffected individuals (CAMPOS et al., 2020). Consequently, it stands to reason that hospitalizations related to these issues would increase, particularly as individuals gained financial relief and had more time to prioritize self-care. Likewise, individuals prone to hypertension, present in the disease group for blood and immunity conditions, experiencing elevated stress levels, may have sought medical assistance upon receiving financial aid.

The category of blood and immunity issues encompasses not only hypertension but also those who are immunocompromised, both considered high risk for Covid-19 infection. The rise in hospitalizations within this group could also be interpreted as misclassified Covid cases, as a patient initially admitted for conditions such as high-blood pressure or immunity issues may later find they were experiencing complications from Covid. However, this interpretation is contradicted by the observed effect of the Covid-19 variables, as the Emergency Aid significantly reduced hospitalizations related to Covid-19.

8 Conclusion

This study has provided a comprehensive analysis of the impact of Emergency Aid (EA) on hospitalizations during the Covid-19 pandemic, exploring differences in municipality location, development, and patient health conditions. The findings suggest that while the EA had an overall negative effect on hospitalization rates, indicating its efficacy in promoting social isolation—a crucial measure in combating the spread of the virus—there were notable variations in its impact across different municipalities and health conditions.

Specifically, disparities in impact magnitude and direction based on municipalities' development and urbanization level underscore the significance of institutional quality and population density in shaping the outcomes of the EA. Institutional quality may have influenced the potential that the financial aid had to cause an impact, reflecting the capacity of local governments to manage the pandemic and administer aid effectively.

Whereas the urban-rural contrast in population density could have dictated local governments' emphasis on social distancing as a measure of Covid containment, distinctively affecting how individuals of these areas reacted to the EA's income shock as related to healthcare demand.

Furthermore, the examination of these differences across various health conditions has illuminated the protection effect and the underlying mechanisms of consumption and liquidity constraints in the relationship between the income shock provided by the EA and healthcare demand. Adverse effects on Covid-related hospital visits provide strong evidence of the EA's success in incentivizing social distancing and creating a protection effect, allowing beneficiaries to protect themselves from contracting Covid. This is even more apparent by the contrasting growing trend in Covid hospitalizations for the control group.

In addition to negative effects on hospitalizations connected to Covid and other non-emergency issues, the absence of effects on hospitalizations stemming from accidents provides compelling substantiation of a social distancing effect. Paired with evidence of a consumption mechanism suggesting unhealthy consumption behaviors, typically associated to an increase in accidents, the null effect for accidents indicates the EA caused such adherence to social isolation that it resulted in fewer accidents.

Relatedly, the presence of a consumption mechanism is implied by the observed rise in hospitalizations for cardiovascular diseases. In this scenario, the influx of cash from the Emergency Aid likely prompted consumption of unhealthy substances, resulting in an uptick in heart attacks and strokes.

Finally, the positive effects of the EA on mental health as well as blood and immunity conditions underscore the significant role played by the liquidity constraint mechanism. Stress-related conditions within these categories could have been intensified by the pandemic and left untreated due to work commitments, only permitted to be addressed once the financial assistance alleviated their liquidity constraints and reduced the need to work.

These insights contribute to the literature through our understanding of the effectiveness of social assistance programs during times of crisis, while also emphasizing the significance of socioeconomic and geographic disparities in policy implementation. Moreover, they furnish evidence supporting the presence of both consumption and liquidity constraint mechanisms in the relationship between income shocks and health demand.

Moving forward, policymakers should consider these nuances in designing and implementing future aid programs to ensure equitable and impactful support for vulnerable populations in times of need.

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