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The Effects of Emergency Government Cash Transfers on Beliefs and Behaviours During the COVID Pandemic: Evidence from Brazil*

Fernanda L. Lopez de Leon[†] Bansi Malde[‡] Ben McQuillin[§]

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Abstract

This paper examines the impacts of emergency cash-transfers on individuals' social distancing behaviour and beliefs about COVID-19. We focus on the impacts of "Auxilio Emergencial" (AE): a large-scale cash-transfer in Brazil targeting low-income individuals who were unemployed or informally employed during the pandemic. To identify causal effects we exploit exogenous variation, arising from the AE design, in individuals' access to the cash-transfer programme. Using data from an online survey, our results suggest that eligibility to the emergency cash transfer led to a reduced likelihood of individuals contracting COVID-19, likely to have been driven by a reduction in working hours. Moreover, the cash transfer seems to have increased perceptions about the seriousness of coronavirus, while also exacerbating misconceptions about the pandemic. These findings indicate effects of emergency cash-transfers in determining individuals' narratives about a pandemic, in enabling social distancing and potentially in reducing the spread of the disease.

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[†]University of Kent. School of Economics. E-mail: f.de-leon@kent.ac.uk

[‡]University of Kent, School of Economics and Institute of Fiscal Studies. E-mail: b.k.malde@kent.ac.uk

[§]University of East Anglia, School of Economics. E-mail: b.mcquillin@uea.ac.uk

1 Introduction

During the recent coronavirus pandemic, cash-transfer programmes have been used across the globe to mitigate economic consequences of the public health emergency. Gentilini et al. (2021) identified 734 such measures, planned or implemented in 186 countries by May 2021, such that almost 17 percent of the world’s population had been covered by at least one COVID-related cash transfer payment. In this paper we examine the impact of one of the world’s largest emergency programmes – the Brazilian cash transfer “Auxílio Emergencial” – in terms of health outcomes, precautionary behaviour, beliefs and knowledge about the pandemic disease.

Brazil was the first country in Latin America to record cases of COVID-19 and in the first half of 2020 became the country with the second highest number of cases in the world (Barone et al. 2021). Auxílio Emergencial (AE) was implemented in April 2020: an emergency cash benefit providing support to low-income individuals, informal workers and the unemployed, and constituting an important insurance against unemployment during the pandemic. The scheme initially provided three monthly payments of R\$ 600 (approximately 60% of a monthly minimum salary¹) and was later extended to allow five such transfers plus subsequent lower amounts. By September 2020, 67 million individuals – more than 30% of the population – had received AE payments, making it the largest cash-transfer programme conducted in the country.

For the financially vulnerable during the pandemic the fear of hunger was sometimes cited as more pressing than fear of the disease. Without savings to cover income losses, social distancing was often unfeasible and precariously employed workers faced a stark trade-off between protecting their lives or their livelihoods. Here, we provide evidence that the AE programme in 2020 encouraged these individuals in Brazil to stay at home and reduced their likelihood of contracting coronavirus.

In addition, we examine the impacts of the emergency cash-transfer on beliefs and misconceptions about COVID-19. There were several and conflicting narratives around the new disease. In Brazil, an important feature of the pandemic was the minimisation of the health crisis by senior politicians, most notably President Bolsonaro (Ajzenman et al. 2023). On the other hand, the media aligned with the more cautious advice

¹The minimum salary in Brazil is the lowest amount that a firm can pay to an employee and reflects the minimum spending amount a person needs to survive per month. The amount is established by law and renewed yearly.

provided by health experts. Citizens therefore faced another choice – around who to believe about the dangers of COVID-19 – and there is an interesting possibility that the AE programme may have affected this choice also, through motivated beliefs (Bénabou and Tirole 2016, Golman et al. 2017). A channel that might be in play is cognitive dissonance (Harmon-Jones et al. 2009): the discomfort of maintaining contradictory ideas and behaviour, such as reconciling the belief that the COVID-19 virus is dangerous with an inability to practice social distancing. A narrative that minimizes the risks of the pandemic aligns better with behaviour that does not entail social distancing, and so individuals who cannot socially isolate may adjust their consumption and interpretation of information, or indeed avoid information, accordingly. We hypothesized that the AE programme, by alleviating financial constraints, may have steered recipients away from such a narrative.

Our data comes from an online survey collected during the first peak of the pandemic. We use a sample with 2,382 respondents drawn from a panel, maintained by a commercial research and polling specialist, that is representative of the population of internet and smartphone users in Brazil. We exploit two features of the AE cash-transfer programme that generate quasi-experimental variation in individuals’ access to the financial aid and allow us to identify effects of the programme. We draw a distinction between what may be regarded as *wealth* and *liquidity* effects of the transfer. A wealth effect here means an overall effect associated with becoming a recipient within the transfer programme, potentially encompassing anticipated payments as well as those that have already been received. A liquidity effect is associated specifically with having recently received a payment under the scheme. This distinction matters because most transfer programmes are paid in several instalments (Gentilini et al. 2021) or are prone to delays that may impinge on their effects.

To identify wealth effects, we focus on one of the eligibility criteria for AE. To receive the emergency cash transfer, individuals must live in a household that earns less than three minimum salaries. Current household income is itself determined by the pandemic, but we focus on a pre-determined income variable. In the survey we targeted participants whose household pre-pandemic income was close to this AE eligibility criteria, and then we elicited household incomes at a fine level. Our measure of pre-pandemic income, although noisy, predicts the use of the emergency cash-transfer, with those classified as below the eligibility threshold being by 10 to 22 percentage

points more likely to receive AE than those above.

We infer impacts of the emergency cash-transfer by using reduced form estimates of eligibility (or intention-to-treat estimates), comparing individuals' outcomes based on whether their pre-pandemic income exceeded or fell short of the three minimum salary cut-off. We focus our main analysis on a sample of individuals whose pre-pandemic income was very closely proximate to three minimum salaries (i.e., between 2.6 and 3.4 minimum salaries) and that are largely homogeneous in observable characteristics. The results are robust to regressions controlling for household income per capita and for an extensive set of demographic and economic covariates. As we show in the online Appendix, the findings also appear in uncontrolled means and when we further restrict to a matched sample. To increase the number of observations and the power of our estimates, we also replicate the analysis for an extended sample (i.e., a wider range of pre-pandemic incomes). In the Appendix, we report regression discontinuity results that largely support the main findings.

We find that individuals just below the income cut-off for AE eligibility were by 3-5 percentage points less likely to report to have contracted COVID and less likely to be tested for COVID-19 at early stages of the pandemic. The same group became likely to work fewer hours (2.2-3.6 fewer hours per week). We do not detect differences in other precautionary behaviour (other forms of social distancing or hygiene), and so it seems likely that the detected impacts of the emergency cash-transfer in preventing COVID contraction were driven by the decrease in labour supply.

Turning to the wealth effects of AE on coronavirus related beliefs, our results indicate that likely AE recipients became more inclined to regard COVID-19 as dangerous – consistent with dissonance effects. We also find weak evidence (when focusing on the extended sample) that AE recipients appeared more likely to trust the advice from the populist president. This is in line with previously studied relationships between government transfers and political loyalty (Zucco 2013, Manacorda et al. 2011, Bechtel and Hainmueller 2011).

Overall, our results point to unintended impacts of emergency cash-transfers that went beyond mitigating the financial shock of the pandemic or affecting financial health or food access. They are also likely to have affected individuals' perceptions about the seriousness of the pandemic and likelihood of contracting COVID-19.

We then test how the timing of AE payments affected beliefs, by restricting the

sample to AE beneficiaries. Monthly payments of AE were staggered, based largely on individuals’ month of birth. We therefore elicited month of birth in the survey, to access whether respondents would have received an AE instalment recently. We observe that the liquidity effects on beliefs conflict with the wealth effects, suggesting that the political trust gained is fluid. AE recipients that received an aid instalment within the last two weeks were significantly more likely to cite health experts as a trustworthy source of COVID information, to rank them above President Bolsonaro and hold fewer misconceptions about COVID-19.

Our work relates primarily to the literature on the impacts of cash-transfers in developing countries during the pandemic. One strand of work demonstrates large financial impacts of the COVID pandemic on the poor (Abay et al. 2023), and that pre-existing cash-transfer programmes had a significant role in mitigating poverty indicators: by decreasing the likelihood of hunger (Banerjee et al. 2020, Bottan et al. 2021), improving food security indicators (Aggarwal et al. 2022), and positively affecting individuals’ mental and physical health (Banerjee et al. 2020). The present paper complements this literature by considering a large-scale emergency cash-transfer programme in Brazil, implemented during the pandemic, and we focus more specifically on pandemic-related behaviours, beliefs and health outcomes.

Other related work has entailed new experiments initiated during the pandemic.² Karlan et al. (2022) implemented a series of cash-transfers in Ghana and find that these led to increases in expenditure on food and social distancing but had no effect on working hours. Stein et al. (2022) examined a one-off cash-transfer to selected households among South Sudanese refugees in a Ugandan settlement and detected improvements in food security and well-being. Brooks et al. (2022) provided a cash-transfer to female microenterprise owners in Kenya, and found that (contingent on the recipient already perceiving COVID as a risk) PPE spending and other precautionary management practices increased. In common with our paper, Londono-Velez and Querubin (2022) evaluate impacts of a new emergency programme (Columbia’s “Value Added Compensation Program”) but utilise a randomised controlled trial and focus on a sample living in extreme poverty. They find that cash-transfer recipients in fact became

²Karlan et al. (2022) present an exhaustive list of cash-transfer trials conducted during the pandemic and documented at the AEA RCT Registry, some remaining undocumented so far in any working paper, covering cash-transfer impacts on food consumption, financial behaviour, labour decisions, housing stability, mental health, labour force participation, loneliness, pro-social behaviour.

more likely to leave the house. In contrast to our findings, they do not detect significant impacts of the emergency cash-transfer on labour decisions or physical health. The AE programme in Brazil was more generous and had more frequent instalments than the programme analysed by Londono-Velez and Querubin, and in this current paper we investigate impacts on a broader set of outcomes, including beliefs and knowledge about COVID-19.

Further controlled randomised trials have focused on North American populations. Persaud et al. (2021) tested for the health effect of a one-time transfer (1000 Canadian Dollars), on individuals who self-reported as experiencing financial difficulties during the pandemic. Overall, they do not find that the transfer led to a reduction in COVID symptoms, though their results hint at an effect that may have been specific to participants who were 50 years or older. Jaroszewicz et al. (2022) examine a broader set of outcomes, again from a one-time cash-transfer amount (500 or 2000 US Dollars), and find negative effects on transfer recipients' mental well-being. Another RCT study (Jacob et al. 2022 and Pilkauskas et al. 2022) reports null effects of a similar transfer (1000 US Dollars) on mental health. The negative effects observed by Jaroszewicz et al. seemed to derive from an increase in the salience of needs, that could not then be met by the relatively modest transfer itself provided by the experimenters. We may therefore expect to observe different effects in a more generous, longer-term programme provided by government.

Menezes-Filho et al. (2021) and Razafindrakoto et al. (2021) provide descriptive studies of socio-economic indicators during the pandemic in Brazil. Menezes-Filho et al. analyse household panel data from the Brazilian Census Bureau (PNAD) and conclude that the AE Programme led to substantial reductions in poverty. They also document sharp reductions in labour force participation that they speculate to have been exacerbated by AE. Razafindrakoto et al. use municipality level aggregates and document that – whilst localities with the highest concentration of informal workers had the worst pandemic disease outcomes – COVID deaths appeared to be lower when such localities had greater uptake of AE.

Our paper also connects to a growing literature on understanding the more general determinants of pandemic-related behaviour and beliefs. Papageorge et al. (2021) find that higher income is associated with larger changes in individuals' self-protective behaviours during the pandemic. Other related work points to the role of age (Abel et

al. 2021, Bordalo et al. 2020), partisan differences (Allcott et al. 2020, Painter and Qiu 2021), gender (Galasso et al. 2020), media (Bursztyn 2020) and exposure to expert information (Akesson et al. 2022).

The paper is organized as follows. Section 2 explains the context and institutional background, and Section 3 describes the survey. Section 4 presents the results and concluding remarks are provided in Section 5.

2 Context and Institutional Background

2.1 Brazil during the pandemic

Brazil reported its first confirmed COVID-19 case on 25th February 2020, and its first COVID-related death on 17th March 2020. The AE programme was approved by the Brazilian Congress two weeks later. In early 2020 Brazil adopted only sporadic, short, and state-specific quarantines, but mandatory use of masks in public transport was implemented. It experienced the first peak of the pandemic around June-July 2020 and by early September, when we conducted our survey, Brazil had registered more than four million infections and more than 125,000 deaths, standing as the country with the second-highest toll in the world.

Other countries' earlier experience with COVID-19 was reported extensively by mainstream TV news in Brazil, with coverage highlighting the severity of the disease and the need for social distancing. However, this message conflicted with that of President Bolsonaro, which focused on the importance of preserving the economy. The president dismissed COVID-19 as a “flu”, argued that Brazilians had little to fear, and repeatedly clashed with national and international health experts.³

2.2 Auxilio Emergencial

Brazil's emergency aid programme – Auxilio Emergencial – was instituted by national law 13.982/2020 on 2nd April 2020. The AE programme provided cash transfers to informal workers, individual micro entrepreneurs (MEI), self-employed workers and the

³For example, in April 2020, President Bolsonaro fired the Minister of Health (Luiz Mandetta) over social distancing guidelines, and the next Minister of Health (Nelson Teich) resigned in May 2020 over disagreements with the president about social distancing and the use of hydroxychloroquine.

unemployed, with the stated goal of supporting low-income vulnerable individuals facing hardship during the coronavirus crisis and mitigating the impacts of negative income shocks due to the pandemic. The programme was initially intended to provide three monthly instalments of R\$ 600 (US\$ 115) per beneficiary (single mothers received R\$ 1,200), but was subsequently extended to allow five such instalments and then further lower monthly instalments into 2021.⁴

AE assistance was given to a named individual, and could be received by up to two members of the same family. To be eligible, an individual had to: (i) be over 18 years old, (ii) have no formal job or be currently unemployed, (iii) not be receiving social security, welfare benefits, unemployment insurance or other cash transfer programme benefits except “Bolsa-Família”⁵, and (iv) live in a low-income household, whose total monthly income is less than three minimum salaries, i.e. less than R\$ 3,135, or – for households of more than six – less than half the minimum salary per person, i.e. less than R\$ 522.50 per person.

In our analysis, to identify individuals that live in households likely to be eligible for AE, we used the total income rule, of three minimum salaries, in criteria (iv). Only 10 participants otherwise met the income criteria, i.e., through living in a household of more than six, and these respondents are excluded from the analysis.

We compare individuals whose household pre-pandemic income lies close to this criterion threshold. Therefore, we focus on the effect among the most affluent AE recipients: at the upper limit of income eligibility. For these recipients the AE monthly instalment corresponded to around 20% of their household monthly income.

The payment of AE instalments was implemented by Caixa Economica Federal, the largest state-owned bank in Latin America. Individuals that had applied and were enrolled on Cadastro Unico (CadUnico) – the main Brazilian welfare registry system⁶

⁴On June 30th, 2020, the government announced the extension of the programme for the payment of five monthly transfers of R\$ 600. Later the Government extended the aid to four additional monthly instalments of R\$ 300.

⁵Before Auxilio Emergencial, Bolsa-Família was the largest conditional cash transfer in Brazil, paying R\$ 89 to households earning less than R\$ 89 (US\$ 16) per capita (World Bank 2020). Bolsa-Família beneficiaries are poorer than the respondents of our survey, whose household per capita earnings are R\$ 1,333 (Table A1).

⁶CadUnico includes individuals living in households with income no greater than three minimum salaries (the same as the AE Programme) that wish to receive government benefits, such as “Bolsa Família” or “Programa Minha Casa, Minha Vida”. Individuals registered in CadUnico are a subset of AE beneficiaries: CadUnico had records of around 27 million individuals, while 67.2 million were reported to have received AE by August 2020. This discrepancy is because other programmes have

– received the AE aid automatically, using their existing Caixa Economica account. Individuals not registered in CadUnico had to apply online to receive the AE transfer, via either an Auxilio Emergencial app or the Caixa Economica Federal website (<https://auxilio.caixa.gov.br>). Individual applications went through a government-run triage and if approved the recipients used a “Caixa Tem” app to create an online savings account and receive the aid. By the end of May 2020 the AE app had been downloaded 81 million times and 107 million people had applied for the benefit (Barbosa et al. 2020).

3 The Survey

We conducted an online survey between 11th August and 2nd September 2020, using a consumer panel managed by Opinionbox – a research and polling specialist in Brazil.

To evaluate the impacts of AE, we used quotas for selecting participants such that household current income fell close to the eligibility upper limit of three minimum salaries. So 40% of participants were randomly drawn from panellists (all over 18 years old) in an income range of 2 to 3 minimum salaries, and 60% from panellists in a range of 3 to 5 minimum salaries. In total, 2,386 individuals answered the survey.

Participants were told the study was about “Current Issues in Brazilian Society”, that they were going to be asked about their experiences during the pandemic, and that the survey was anonymous. The survey (in English and in Portuguese) is included in the Kent Data Repository at University of Kent.⁷ The survey comprised closed-ended (mainly, multiple-choice) questions. Some questions addressed household and individual socio-economic and health pre-determined characteristics. We elicited month-of-birth, age, race, education, marital status, previous voting choices and lifestyle-activities prior to the pandemic, household composition and income. Further questions assessed respondents’ knowledge, beliefs and behaviour related to COVID, physical and mental health indicators, and financial and labour-market outcomes. We asked about use of AE – with the question, “Did you or someone else in your household receive an instalment

a lower income threshold than three minimum salaries for eligibility. As shown in Table A5, the CadUnico pre-registration is unlikely to be a confounder and this characteristic is also controlled in the regressions.

⁷<https://doi.org/10.22024/UniKent/01.01.468>

of Auxilio Emergencial?”⁸ – and about other government benefits. These questions were placed towards the end to avoid closer speculation about the purpose of the study affecting the responses (De Quidt et al. 2018, Mummolo and Peterson 2019).

3.1 Household Income

Within the survey, we asked questions to infer participants’ household income at a finer level. The first question was on household current monthly income, giving a choice of R\$ 150 (US\$ 27 or 0.14 minimum salaries) bandings in the range R\$2,100 to R\$5,100. These were the finest bands we could achieve (as advised by Opinionbox) whilst realistically accounting for participants’ likely uncertainty about household income, and allowing also for technological constraints on the number of alternatives offered. We later asked about whether and to what extent their household income had changed during the pandemic, and in the last question of the survey we asked about household income in February 2020 (before the pandemic). This time – to allow for gains and losses during the pandemic – participants selected a R\$ 400 (US\$ 72 or 0.38 minimum salaries) banding in the range R\$1,150 to R\$7,550. We conducted our analysis using this pre-pandemic income measure and restricting the sample: (i) to individuals in the bands immediately below and above 3 minimum salaries (R\$2,751 to R\$3,550), and (ii) to an extended sample of individuals whose pre-pandemic income was between two and five minimum salaries (R\$1,951 to R\$5,150). These samples comprise 612 and 1,912 individuals respectively. Figure A1 in the Appendix shows the histogram for this variable.

We checked for the reliability of the income answers above in several ways. We find that income predicts real outcomes, revealed in higher probabilities of use of AE for respondents that report income below R\$ 3,135. This is noticeable even when we restrict the sample for those that self-report to be very close to this cut-off, as shown in Figure 1. The survey was anonymous, and when we asked about their current incomes respondents were unaware that later questions would relate to government benefits. We did not find evidence that participants adjusted their past-income answers to pretend to have been “AE eligible”, by reporting a lower household income later in the survey. Moreover, for the pre-pandemic income question (which was the last question in the

⁸Therefore an individual is classified as an AE beneficiary if they live in a household that receives AE benefits, regardless of whether the respondent themselves is the recipient in the household.

survey) we provided the option, “I prefer not to say” and only 2.6% of respondents (or 63 respondents) chose this alternative.⁹

3.2 Main Outcomes

3.2.1 Beliefs, Perceived Severity and Misconceptions about COVID-19

Our hypothesis is that the cash transfer will – by alleviating financial constraints, enabling individuals to contemplate the health risks of COVID-19 and the advice of health experts – have affected individuals’ beliefs about the pandemic, media consumption and misconceptions about COVID. To investigate how and whether the emergency cash-transfer impacts perceptions, we collected three main set of outcomes.

We followed Belot et al. (2020) in assessing individuals’ perceptions about the severity of COVID. We asked three separate questions about the perceived chances of the respondent themselves, and two hypothetical individuals (a young woman and an old man), developing severe symptoms or dying if they contracted COVID.¹⁰ These were framed as follows:

“Suppose there are 100 people similar to you: same gender, age and health condition. If all these 100 people contract coronavirus, how many do you think would have severe symptoms or die?”

“Marta is 30 years old. Out of 100 people like Marta, if they contracted the coronavirus, how many do you believe would show severe symptoms or die?”

“Carlos is 65 years old. Out of 100 people like Carlos, if they contracted the coronavirus, how many do you believe would show severe symptoms or die?”

We evaluated individuals’ misconceptions about COVID with a quiz. Respondents were presented with twelve COVID-related statements about risk-factors, origins, transmission, treatment, and control of COVID (statements such as “There is a cure to

⁹In general, attrition does not seem to be an issue within the survey. Participants were allowed to skip sensitive questions by checking the alternative “Prefer not to say”. This option was rarely chosen, and we did not detect imbalances on this choice around the income cut-off.

¹⁰In pilots, we experimented with a scale of 0-1000 rather than a scale of 0-100. Respondents found the latter scale easier to understand than the former.

COVID-19 called hydroxychloroquine”)¹¹ and were asked to indicate which ones are true. The statements were compiled from the “Fake News” page of the Brazilian Health Ministry (<https://www.saude.gov.br/fakenews>), statements from the media, and recommendations from health experts. From the responses, we then constructed a “knowledge score” measured as the proportion of correct answers in the quiz and normalized.¹²

To investigate impacts on information consumption, we asked respondents about their sources of information on COVID (among news outlets, politicians, health experts, friends and family, and social media) and to identify their three most-trusted sources. During the pandemic there was a clear contrast between the messages conveyed by President Bolsonaro and by health experts. We constructed an indicator for whether health experts were trusted above President Bolsonaro when we could observe this preference.

3.2.2 Support for Self-Isolation with COVID Symptoms

We used a vignette to elicit respondents’ advocacy for self-isolation for a hypothetical low-income individual: Antonio, a street vendor who is the main earner in his household. Antonio earns one minimum salary and is currently exhibiting COVID symptoms. Respondents were then presented with various statements on how Antonio and other members of his household should behave and asked to select all the statements they agreed with. We combined their responses into two indicators: support for self-isolation (versus leaving the house to work) by Antonio himself and support for self-isolation by other members of Antonio’s household.

¹¹The other statements are: Smoking increases the risk of developing severe symptoms of coronavirus; Flu vaccination increases the risk of developing serious symptoms of coronavirus; Children cannot get COVID-19; The coronavirus that causes COVID-19 was manufactured in a laboratory in China; Wearing face masks or coverings can reduce the spread of the coronavirus; The use of gloves to handle money can prevent coronavirus infection; The coronavirus spreads more quickly indoors than outdoors, Bats spread the coronavirus so we should avoid them; COVID-19 can be spread by boxes and packages sent from other countries where it is present; People that do not have COVID symptoms can spread COVID; The Oswaldo Cruz Foundation (Fiocruz), from the Ministry of Health and AstraZeneca signed an agreement on 31 July for technology transfer and production of 30 million doses of the vaccine against COVID -19, in case their vaccine prove to be safe and effective.

¹²We also used another, similar question to construct a knowledge index about non-COVID current affairs in an analogous way. Participants were presented with twelve statements about other (true or false) events during the pandemic, relating to political, economic, sports and celebrity news.

3.2.3 Precautionary Behaviour and Labour Outcomes

We collected information on whether the respondent has engaged in several social and precautionary behaviours before the pandemic and in the last 7 days: went to the hairdresser, used public transport, attended or organised an extended family dinner, had a meal in a restaurant, went outside to work, visited elderly or vulnerable relatives, did online shopping, participated in social events, sanitised a cell phone, hugged or shook hands with people that do not live in the same household, went to the mall, smoked, and ate healthily. We constructed pre- and post- pandemic indexes, aggregating the answers above, following the methodology in Anderson (2008). In addition to these measures, we also document the frequency of visits to friends or family, and the number of times the person has washed his/her hands or used hand sanitiser in the day prior to the survey. We also collected information on respondents' current labour market outcomes – employment and weekly hours worked.

3.2.4 Physical and Mental Health

We constructed indicators for whether the respondent or any of their household members has had a serious case of COVID – where the individual was hospitalized or died – and indicators for whether the person had contracted COVID and/or had been tested for COVID. In addition to these indicators for physical health, we also measure mental health based on whether the respondent experienced any of the items included the DASS-21 scale (Lovibond and Lovibond 1995). From their responses, we created indicators for whether the person experienced any symptoms related to anxiety, depression, or stress in the previous week.

3.3 Sample

The data collection was administered by Opinionbox, using their own maintained panel of more than 150,000 respondents. The panel is representative of the population of internet and smartphone users in Brazil.¹³

¹³Opinionbox conducts a careful check on their panel of participants. When registering, participants answer a socio-economic survey, and this information is verified against their tax records. More information about Opinionbox and their procedure can be found at: https://www.opinionbox.com/wp-content/OPB_panel_book_2022.pdf

Our sample includes participants living in all Brazilian states. As shown in Table A1 in the Appendix, 57 percent are female and 55 percent are white. The average age is 38 and 48.2 percent voted for Bolsonaro in the last Presidential Election. The average pre-pandemic household income per capita is R\$ 1,333, compared to the national average of R\$ 1,439 in 2019 (IBGE). Among the sample, 26 percent had received some government benefit before the pandemic (Table A1) and 43 percent had received at least one AE cash transfer instalment (Table A3).

The sample does not aim to be representative of the broader national population, and it is focused on participants whose pre-pandemic household income was around three minimum salaries (the cut-off for eligibility). While we do not observe large differences in terms of gender, ethnicity or age,¹⁴ notably the average AE beneficiary in our sample is more affluent and educated than the average AE beneficiary in Brazil: 68 percent of participants in our sample have some college education in contrast to 14.8 percent in the population (IBGE PNAD). Notwithstanding, there is no indication that our respondents' views around COVID-related issues fail to align with those expressed in national polls. For example, by early 2021, in the general population 79 percent of Brazilians were afraid of contracting COVID, and 89 percent believed health experts (rather than politicians) should be the ones advising on the use of hydroxychloroquine as a treatment (Datafolha 2021). These patterns are consistent with respondents of our survey. Most respondents reported to have changed their behaviour due to the pandemic, by increasing social distancing (Table A2), and 91 percent reported to trust COVID-related advice from health experts above Bolsonaro. Table A3 in the Appendix provides summary statistics for all outcomes discussed in the text.

4 Results

In our first analyses we investigate the impacts of AE by focusing on the effect of (pre-pandemic) eligibility by the AE income criteria, or on the intention-to-treat effect of the AE Programme.

We use the fact that the proportion of AE recipients among individuals living in

¹⁴According to PNAD (Pesquisa Nacional por Amostra de Domicílios), a national representative survey including around 200,000 respondents, in August 2020, 52% of AE beneficiaries were female, 47% were white and the average age was 44 years old. In our sample, 58% of AE beneficiaries are female, 54% are white and the average age is 37.

households that used to earn less than 3 minimum salaries is significantly larger than their counterpart, as shown in Figure 1. For the narrowed sample (within one bandwidth of the cut-off), this proportion is 55 percent and 38 percent respectively. The effect of the pre-pandemic income criteria on AE use is robust to an extensive list of socio-economic controls as shown in Table A4 in the Appendix (estimated to be between 9.6 and 21.9 percentage points).

Figure 1

To quantify AE wealth effects, we estimate the following equation:

$$y_{is} = \alpha_1 + \lambda(hh_i \text{ earns less } 3ms)_i + \beta\mathbf{X}_i + \theta_s + \varepsilon_{is} \quad (1)$$

where y_{is} is the outcome of individual i in state s , \mathbf{X}_i contains a set of covariates, $(hh_i \text{ earns less } 3ms)$ is an indicator equal to one if the respondent lives in a household that earned less than three minimum salaries before the pandemic, θ_s are state of residence fixed effects and ε_{is} is a random error term. \mathbf{X}_i includes the variable pre-pandemic household income per capita and covariates for further pre-pandemic conditions: unemployment status, a behaviour index, indicators for whether received government benefits, and if was registered in Cadastro Unico. It also includes marital status, age, race, education, gender, whether the participant shares a household with somebody more than 60 years of age, if voted for Bolsonaro, number of individuals living in the household and a social desirability index. This is the baseline specification. We report robust standard errors.

The coefficient λ provides an estimate of the causal effects of the AE eligibility and reduced-form impacts of being eligible to AE or the intention-to-treat impact of the cash-transfer programme.

First, we estimate equation (1) for a group of individuals nearest the eligibility cut-off: those living in households with pre-pandemic earnings between 2.6-3.4 minimum salaries (RS\$ 2,751-3,550). This range encompasses the finest income categories in our survey that separate individuals living in households above and below three minimum salaries. In the main tables, we also report results for an extended sample (between 2-5 minimum salaries) to increase the number of observations.

We provide several robustness checks in the Appendix. Table A5 shows average characteristics based on AE income eligibility for this restricted sample. We find few significant differences between eligible and non-eligible. Out of thirty-seven pre-determined variables, seven are significantly different at the 10 percent level, and these are controlled in the baseline specification. In Table A6 we report results for the restricted sample without adding controls, and these are similar to the baseline specification (consistent with the hypothesis of random assignment). Moreover, we restrict this narrow sample and construct a matched sample, using a nonparametric coarsened exact matching (CEM) approach (Iacus, King, and Porro 2011, 2012), by selecting “above 3m.s.” (control) individuals with the same key covariates as the “below 3m.s.” (treatment) individuals. The regression results are reported in Table A6 and they are very similar to the ones presented in the text. In Table A7 we provide results correcting standard errors for multiple hypothesis testing for multiple outcomes and most of the impacts become non-significant. Lastly, we exploit the discontinuity in the AE eligibility criteria, by constructing and using a finer and normalised measure of the pre-pandemic income: in household per capita terms.¹⁵ In Table A8, we report reduced form estimates for the impact of AE using this alternative income variable, and a regression discontinuity framework.

Turning to the main results, in Table 1, we examine the effects on social distancing and precautionary behaviour. We find no significant impacts on the frequency of hand-washing (row 1) or of meeting with friends (row 2), or indeed on precautionary behaviours more generally. Following the methodology of Anderson (2008) we created a weighted average index capturing 14 (positively coded) further indicators of social distancing, healthy eating and non-smoking in the last week (detailed in section 3.2.3) and row 3 shows no significant impact on this variable (p-value>63%).¹⁶ However, we

¹⁵To circumvent the coarse nature of the income data, we constructed another variable Z , that conveys the normalised pre-pandemic income from the eligibility cut-off in per capita terms (i.e., Z equals pre-pandemic midpoint income minus 3,135, divided by the number of residents in the household) to use as a running variable in the regression discontinuity regressions. In the Appendix, we report estimates of coefficient δ in equation (2), in which $H(Z)$ is a linear trend, flexible on each side of the cut-off.

$$X_{is} = \alpha_2 + \delta(\text{hh earns less 3ms})_i + H(Z) + \beta\mathbf{X}_i + \theta_s + u_{is} \quad (2)$$

We use pre-determined bandwidths and optimal selected bandwidth following Cattaneo et. al (2020) to select the sample. We conducted standard validity tests results for RD design. These are reported in footnotes of Figure A2 and in Table A8.

¹⁶In Table A9 in the Appendix, we investigate the impacts separately for each of the index items,

do observe significant effects of AE eligibility in increasing the likelihood of reporting to be unemployed (by 5 to 8 percentage points, in row 4) and – for those who were employed – in decreasing individuals’ number of hours worked (by 2.7 to 3.6 hours per week, in row 5). In row 6, we consider number of working hours per week including unemployed in the sample (inputting zeros for their working hours), indicating an impact of reducing number of hours in a similar magnitude.¹⁷

These labour market effects are consistent with the large increase in inactivity in Brazil documented by Menezes-Filho et al. (2021), that may have been specific to Brazil and exacerbated by AE.¹⁸ It appears that the types of precautionary and social distancing behaviour that did not entail loss of income were being widely adopted regardless of AE eligibility, explaining the contrast of null impacts in rows [1]-[3] to rows [4]-[6]. For example (in the Appendix, Table A2), before the pandemic, 63% used to socialise with friends and 60% used to dine out in a usual week, and these fractions were 6% and 10% respectively during the pandemic.

In sum, the financial cushion of AE seems to have affected individuals’ decisions about whether to continue working, and how much paid work to undertake, during the pandemic.¹⁹

Table 1

In Table 2, we report the effects of AE eligibility on COVID infection. We find some evidence – statistically significant at the 10% level – of a reduction in individuals’ likelihood of reporting to have contracted COVID. The estimated coefficient of between

and we find suggestive evidence of an AE impact on increasing the chance of individuals taking public transport to go to hospital or for shopping, when focusing on the extended sample.

¹⁷The effect of including unemployed (i.e. those who were unemployed at the time of the survey) is ambiguous. When we include the unemployed we thereby include those who ceased being employed during the pandemic, for whom the reduction in hours worked is likely to have been greatest. On the other hand, those who were unemployed at the point of the survey may have been in large proportion already unemployed before the pandemic. For those, the reduction in hours worked would have been zero.

¹⁸Menezes-Filho et al. document sizable increase in transitions to labour market inactivity, and decreases in transitions to activity. They calculate probabilities of transition in the labour market between work, employment and inactivity, and compare the transitions between 2018-29 and 2019-20. In the pandemic period, among those employed there was a 9 percentage points increase in transitions to inactivity and among those unemployed, the fraction that remained unemployed increased by 23 percentage points.

¹⁹In Table A10 in the Appendix we show that these impacts were concentrated among the informally and self-employed, workers more likely to have flexibility to adjust their working hours.

3 and 6 percentage points is very large relative to the baseline proportion (around 15 percent) of self-reported COVID among AE non-eligible. However, we should note that the standard error is relatively large, and that individuals' perceptions may have been only loosely correlated with having actually contracted the disease. As this variable reflects individuals' *beliefs* (on having had COVID), it may also encompass an impact of AE on individuals' recollection and interpretation on their own health during the pandemic. Nonetheless, this effect is unlikely to be psychological only. We also find some evidence (column 2), corroborated in Appendix Tables A6 and A8, of a negative impact on individuals' likelihood of being tested for COVID-19.²⁰ At the time our survey was conducted, COVID tests were scarce in Brazil and were conducted mostly when individuals had severe symptoms,²¹ so the results in row 2 corroborate the interpretation of a physical effect. We do not however find an AE impact on the likelihood of a household member having developed serious symptoms or having died of the disease (row 3).

Table2

In Table 3, we explore evidence of an AE impact on mental health, based on whether the respondent experienced any of the anxiety, depression and stress indicators included the DASS-21 scale (Lovibond and Lovibond 1995). Overall, we do not find an impact, except it appears – and this is corroborated in Appendix Table A8 – the cash-transfer may have increased the likelihood of individuals showing signs of depression in the previous week. Reduced workplace engagement may have led to an increased incidence of depression.

Combining the findings in Tables 1-3, it seems likely that the channel for observable impacts of the cash-transfer was through labour supply. By allowing individuals to socially distance though undertaking less paid work, rather than changing their social patterns more generally, AE reduced their likelihood of contracting COVID.

Table3

²⁰In our sample, tested individuals were five times more likely to declare to have had COVID than non-tested individuals.

²¹Within the income bracket of our sample, less than 10% of Brazilians had taken a COVID test by November 2020 (IBGE, 2020).

In Table 4, we report impacts of AE income eligibility on COVID related knowledge and beliefs. We observe a negative impact of AE income eligibility on individuals’ performance in the COVID quiz (in the order of 0.16-0.18 standard deviations). We also find weak evidence (for the extended sample) that AE income eligibility led to an increased propensity to express trust in President Bolsonaro as a source of information about the disease. This perhaps suggests that the cash transfer caused, for recipients, an increased sense of loyalty towards the government, manifesting in a willingness to trust the President’s pronouncements generally, even where these promulgated misconceptions already listed as “fake news” by the Brazilian Health Ministry.²² On the other hand – though the President was “playing down” the health risks of COVID – we also find evidence that AE eligibility increased perceptions of the risks of hospitalisation or death. This may reflect a motivated belief. To the extent that AE eligibility enabled respondents to change their work patterns in order to reduce social contacts, it also allowed them to increase their estimate of the dangerousness of the disease itself.

Table 4

In Table 5 we examine heterogeneity in the impacts of AE by pre-existing support for the President: based on stated voting behaviour in the 2018 Presidential election. Ajzenman et al. (2023) have shown that the President’s rhetoric directly impacted social distancing behaviour among his supporters, and in our sample (shown by the averages in Table 5) Bolsonaro supporters hold more misconceptions about COVID-19 and perceive the health risks of coronavirus to be lower than non-Bolsonaro supporters.

Focusing on the narrower sample of respondents, in Panel A we report the previously examined AE impacts (λ from equation 1) by separate samples and in Panel B using both samples and reporting on the interaction term.²³ It is striking that several of the

²²In Table A11 in the Appendix, we investigate further by reporting effects on the frequency individuals look for COVID information in the media, preferred media sources and performance in a non-COVID quiz, and we do not detect any significant effect.

²³In Panel B, we estimate equation (3) and report estimates for ϕ_1 and ϕ_2 , where the variable $(hh_i \text{ earns less } 3ms) \times (votedBolsonaro)$ is an indicator for whether the respondent lives in a household that earned less than three minimum salaries before the pandemic and has voted for Bolsonaro in the 2018 Presidential election.

$$y_{is} = \alpha_3 + \phi_1(hh_i \text{ earns less } 3ms)_i + \phi_2(hh_i \text{ earns less } 3ms) \times (votedBolsonaro) + \beta \mathbf{X}_i + \theta_s + \omega_{is} \quad (3)$$

previously noted impacts of AE eligibility are specific to the sample of non-Presidential supporters. The impacts in increasing likelihood of unemployment and decreasing likelihood of contracting COVID and of testing for COVID are observed only among the sample of those who did not vote for the President. This may have been because the cash transfer facilitated a behaviour (of social distancing through reduced employment) that was desired by non-supporters but perceived as unnecessary among supporters of the President. Also, impacts in reduced performance in the COVID quiz, an increased likelihood of citing the President as a trusted source of information, and an increased likelihood of trusting the President above health experts are similarly observed only among those who had not previously voted for Bolsonaro. This may have been because a change in loyalty could only be induced where loyalty was not already directed towards the president.

On the other hand, there is no indication that the AE impacts through motivated beliefs – for example on perceived risks – are specific to non-Presidential supporters. Indeed, we observe an increased advocacy of staying at home if someone in a person’s household has COVID symptoms that seems to be specific to the group who voted for Bolsonaro.

Table 5

In our interpretation of the evidence in Tables 4 and 5 we have suggested two possible mechanisms, pulling in contrary directions: reciprocity effects, with an increase in loyalty towards President Bolsonaro (or towards the government) because of the cash-transfer, and the effect of (reduced) financial pressure to work in causing beliefs. To separate these potential drivers we conduct a further analysis only with AE beneficiaries. We exploit the fact that the monthly AE payment date was staggered and determined by individuals’ month-of-birth: information that was provided by respondents in the survey. Using the AE payment schedule, we could therefore identify whether an AE beneficiary had received her most recent AE instalment in the two weeks immediately prior to answering the survey. To circumvent measurement errors in this variable (i.e. from multiple individuals in the household receiving AE or from the respondents themselves not being the recipient in the household), we restricted the sample to AE beneficiaries that are sole adults in households. Such beneficiaries had on average already received 0.4 additional AE instalments in comparison to other

beneficiaries and therefore – whilst the overall wealth effect of AE (encompassing both realised and anticipated instalments) was the same for all beneficiaries – we might suppose that recipients of recent instalments at the time of the survey enjoyed a liquidity advantage. We use this variation to test a causal link between reduced liquidity pressure and COVID-related beliefs, regressing belief outcomes on an indicator for whether the respondent received AE within the last fourteen days.

The results are reported in Table 6. Column 1 shows the estimates without adding controls, column 2 shows results for the baseline specification, and column 3 adds to the baseline specification a control for the number of AE instalments already received.

Table 6

The results indicate that individuals that received AE within the last fourteen days performed better in the COVID quiz, by about 0.38 standard deviations. The results also indicate that individuals that received their AE instalment recently are less likely to mention President Bolsonaro among the most trusted sources for COVID information and were more likely to cite health experts. They are also significantly less likely to rank Bolsonaro above health experts as a trusted source of COVID information. However, whilst most of the coefficients in rows 5-7 are positive – i.e., in the direction of financial liquidity leading to increased perception of COVID risks – we do not find statistically significant effects for these outcomes.

Based on these results, it seems plausible that any reciprocity consequences of AE (an increase in loyalty towards the president, leading to credulity in the president’s views about COVID) would have been a wealth effect, or that political loyalty arises primarily from anticipated *upcoming* payments. A more recent transfer means that upcoming payments will be fewer or more distant. The liquidity effect of a recent transfer – as isolated in the analysis in Table 6 – reflected only (due to reduced financial pressure to work) in an increased willingness to attend to the opinions of experts and to perceive the disease as dangerous. If so, then this is an important finding because – though the former effect would have been particular to the political environment in Brazil – the latter effect could be supposed to be common to any other environment in which a pandemic-related cash-transfer programme is used.

5 Conclusion

In this paper, we examine impacts of “Auxilio Emergencial”, one of the largest emergency cash-transfers in the world. The programme targeted low-income individuals that were especially vulnerable to the financial shock of the pandemic: informal workers and the unemployed.

It is worth noting that our estimates for the impacts of the Brazilian emergency cash transfer (in Tables 1-5) are for the group on the upper income limit for AE eligibility, and our sample is more affluent than other cash-transfer studies (Bottan et al. 2021, Abay et al. 2023, Londono-Velez and Querubin 2022, Karlan et al. 2022). Within the first and second income deciles of the population, Auxilio Emergencial represented 93% and 53% of their monthly income during the pandemic (PNAD 2020)²⁴, while this fraction is much lower for those in our narrowed sample. In the Appendix, we report impacts on financial health outcomes and in our sample AE operated only in affecting the likelihood of individuals lending or borrowing (Table A12).

We have shown that the Brazilian emergency cash transfer programme seemed to affect individuals’ employment decisions. Individuals likely to have been eligible for the AE transfer worked fewer hours during the pandemic than others. This in turn may have improved health outcomes, as we detected significant impacts of AE eligibility in decreasing individuals’ likelihood of contracting or being tested for COVID. Our findings suggest that the AE benefits, aside from mitigating the direct welfare consequences of the negative income effects brought about by the pandemic, played a role in suppressing the pandemic itself by changing employment behaviours.

The existence and direction of this discovered effect is important because existing evidence for health effects has been mixed. For example, Karlan et al. (2022) and Londono-Velez and Querubin (2021) find that a cash-transfer had no effect in decreasing participants self-reported covid symptoms, whilst Banerjee et al. (2020) show effects of a cash-transfer in decreasing likelihoods of sickness and hospital visits. Previous studies of cash transfers during the pandemic have not generally observed any labour market effect and moreover Londono-Velez and Querubin found that cash-transfer recipients became more likely to leave the house.

It should be emphasised that our estimates are intention-to-treat effects, with rel-

²⁴https://www.gov.br/economia/pt-br/centrais-de-conteudo/publicacoes/notas-informativas/2020/nota_informativa_covid19_final.pdf

atively large standard errors, and cannot readily be translated to an estimation of magnitude for the effect of receiving the AE transfer itself. We cannot be certain that the effect of AE – as a perceived safety-net, affecting labour-supply decisions – was limited to those who actually took up the benefit. For example, the safety-net may have enabled an individual to eschew (COVID-risky) work opportunities that she would otherwise have accepted, before eventually taking up a COVID-safe work opportunity such that she did not end up using the benefit. Such effects would be likely to operate differently on individuals below or above the eligibility threshold. There is therefore no direct translation from our reported intention-to-treat effects to an estimated treatment effect.²⁵

The finding that access to the cash transfer discouraged work contrasts with evidence from impacts of cash transfers outside the pandemic around the world (Banerjee et al. 2017, Skoufias and Maro 2008) or in Brazil (Ribas 2018, de Brauw et al. 2015). We may suppose that the temporality of AE aid combined with its size – roughly 20% of household income – for individuals on the upper income limit of AE eligibility made it unlikely that the aid itself discouraged work except where there was a latent desire to self-isolate to avoid disease. This supposition is corroborated by the observation that the AE impacts in preventing COVID contraction and increasing the likelihood of unemployment are largely concentrated among non-Bolsonaro supporters. Within our sample, individuals that declared to have voted for President Bolsonaro were less likely to support quarantines or to practice social distancing, and had lower perceptions about the health risks of the pandemic.

We also explore whether the AE Programme led to a change in beliefs, and/or to a change in the sources for information that individuals chose to use or trust. One plausible channel seemed to be that – whilst the narrative that minimized the risks of coronavirus might be attractive to those for whom an imperative to earn a living made distancing infeasible – the AE programme would steer individuals towards trusting health experts and assimilating the mainstream scientific understanding of the dangers associated with the pandemic.

When we considered the overall wealth effect of the programme, individuals likely

²⁵If the effect of AE arose *only* through receipt of the transfer our coefficients could be translated to an estimated treatment effect by dividing by the difference proportions of AE recipients either side of the cut-off ($0.55-0.38=0.17$), but if it arose also through other channels then we would require some higher divisor.

to have been eligible for the transfer did show a heightened perception of the risks associated with COVID-19, but they also showed – if anything – less knowledge about the disease and an increased willingness to trust the populist president. So the hypothesised dissonance effect may have been offset by an extent to which the programme bought loyalty for and trust in a political incumbent who was hostile to many health experts’ opinions.

To remove the loyalty effect – focussing only on AE beneficiaries – we exploited the AE programme payment logistics to investigate how financial pressure itself affects coronavirus related beliefs. We found that AE recipients who received the aid instalment within the last two weeks held fewer misconceptions about COVID-19, and were significantly more likely to trust health experts on COVID-related advice and to rank them above President Bolsonaro. It is conceivable that the loyalty bought through the benefit is fickle: peaking in anticipation of an anticipated cash transfer and disappearing almost immediately after. However, if we presume that loyalty is driven by the overall wealth effect of eligibility, then our findings point to a financial liquidity effect such that the cash transfer encourages acceptance of mainstream science. In other circumstances, in which there may be a common message advanced by governments and by scientific leaders, we would expect the wealth and liquidity effects of an emergency cash transfer to align in helping to propagate trust in such a message conjointly.

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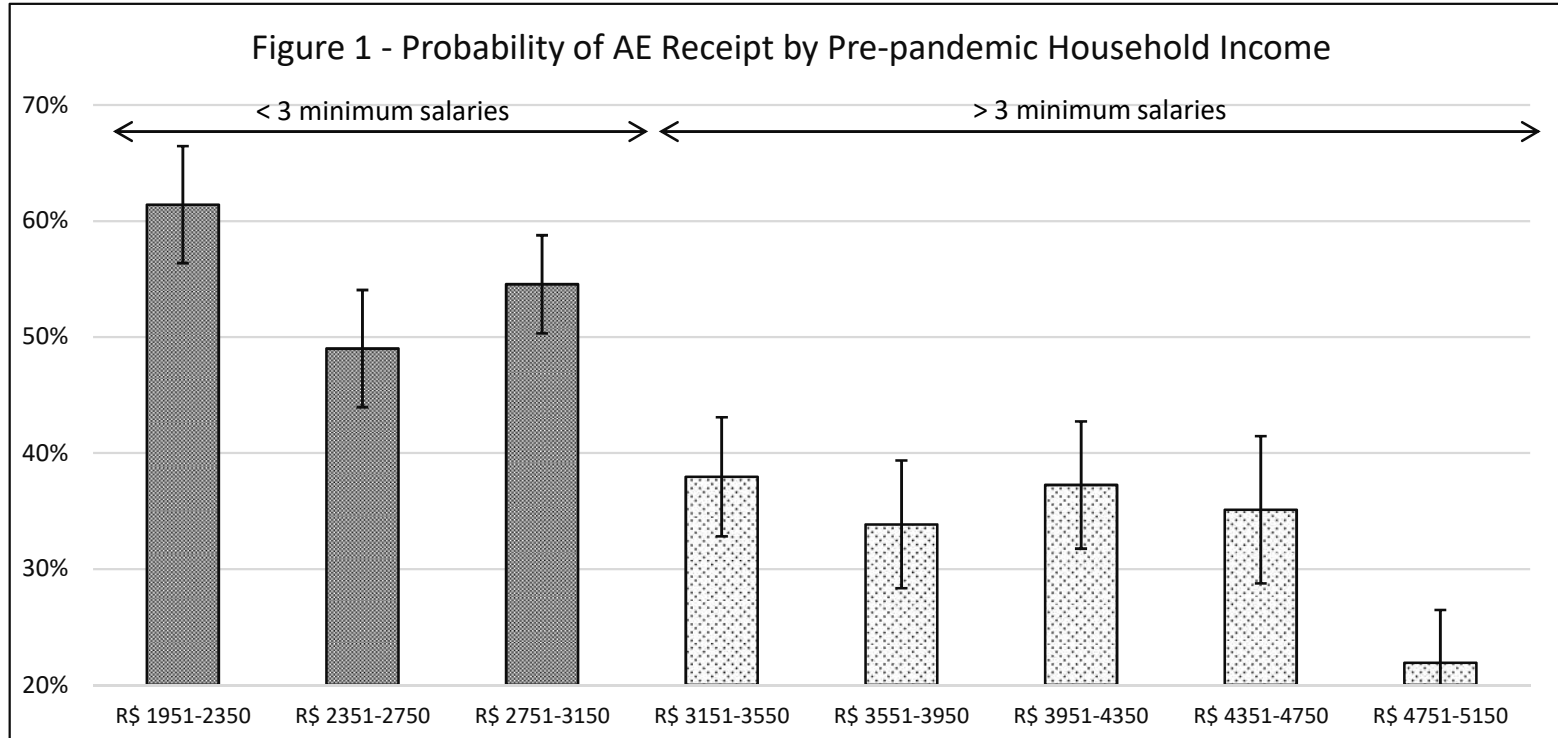
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Figure 1 - Probability of AE Use by Pre-pandemic Household Income



Note: Whiskers indicate 90 percent confidence intervals.

Table 1 - Impacts of Emergency Cash-Transfer on Precautionary Behavior

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<u>Outcomes:</u>						
[1] Number of times washed hands yesterday	5.24	-0.0231 [0.231]	526	5.29	-0.121 [0.146]	1,645
[2] Number of times visited friends in the last 2 weeks	1.19	-0.0424 [0.177]	528	1.16	-0.0718 [0.107]	1,650
[3] Precautionary behaviour index	-0.01	0.0116 [0.0351]	528	0.00	-0.0120 [0.0211]	1,650
[4] Currently unemployed	0.21	0.0503 [0.0310]	528	0.21	0.0798*** [0.0217]	1,650
[5] Current # hours worked per week (excluding unemployed)	32.92	-2.668** [1.307]	432	32.62	-3.621*** [0.870]	1,367
[6] Current # hours worked per week (including unemployed)	27.90	-2.222* [1.251]	528	28.27	-3.375*** [0.8608]	1650

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. The variable number of hours worked per week (including unemployment) inputs zero hours for the unemployed. The precautionary behaviour index aggregates several social and precautionary behaviors in the last 7 days (in Table A9), following the methodology in Anderson (2008). All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 - Impacts of Emergency Cash-Transfer on COVID-19 Infection

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<u>Outcomes:</u>						
Had COVID-19	0.152	-0.0562* [0.0298]	524	0.112	-0.0338* [0.0183]	1,640
Tested for COVID-19	0.320	-0.0583 [0.0436]	524	0.289	-0.0551** [0.0269]	1,640
Someone in the household had serious COVID (hospitalised or died)	0.010	0.0049 [0.0110]	469	0.014	0.0013 [0.0095]	1,476

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic.

All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 3 - Impacts of Emergency Cash-Transfer on Mental Health

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<u>Outcomes:</u>						
Had at least one mental health symptom (in the last week)	0.712	0.0064 [0.0421]	528	0.734	0.0177 [0.0262]	1,650
Had at least one symptom (in the last week) related to:						
Depression	0.458	0.0416 [0.0476]	528	0.486	0.0525* [0.0298]	1,650
Stress	0.610	0.0216 [0.0457]	528	0.619	0.0288 [0.0291]	1,650
Anxiety	0.331	0.0071 [0.0465]	528	0.356	-0.0074 [0.0293]	1,650

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Mental health outcomes are based on whether the respondent reported to have experienced any of the items included the DASS-21 scale (Lovibond and Lovibond 1995). All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 - Impacts of Emergency Cash-Transfer on Beliefs and Knowledge about COVID-19

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<u>Outcomes:</u>						
COVID quiz	0.035	-0.179* [0.0937]	528	0.158	-0.154*** [0.0584]	1,650
Cited among 3 trusted sources of COVID information:						
Pres Bolsonaro	0.127	0.0286 [0.0323]	528	0.092	0.0397** [0.0196]	1,650
Health experts	0.729	-0.0084 [0.0442]	528	0.743	-0.0291 [0.0265]	1,650
Trust Bolsonaro above health experts	0.102	-0.0049 [0.0335]	413	0.068	0.0234 [0.0189]	1,307
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:						
X="someone like me"	23.055	3.674 [2.667]	528	22.762	3.917** [1.757]	1,650
X=30-year old woman	17.924	2.250 [2.282]	528	17.822	2.718* [1.446]	1,650
X=65-year old man	38.314	1.294 [2.869]	528	37.462	3.206* [1.808]	1,650
Advocacy for a person to 'stay at home' (instead of working) if:						
The person has COVID symptoms	0.805	-0.0042 [0.0388]	528	0.811	-0.0075 [0.0240]	1,650
Someone else in their household has COVID symptoms	0.470	0.0443 [0.0484]	528	0.499	0.0082 [0.0303]	1,650

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Outcomes are explained in the text. All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 - Impacts of AE Eligibility by Vote in the Last Presidential Election

Sample:	Panel A						Panel B		
	Voted for Bolsonaro in 2018			Did not vote for Bolsonaro in 2018			Earned < 3 m.s.	All Earned < 3 m.s. X voted for Bolsonaro	n
	DV average where earned > 3 m.s.	Earned < 3 m.s.	n	DV average where earned > 3 m.s.	Earned < 3 m.s.	n			
Outcomes:									
Precautionary behaviour index	-0.10 [0.0534]	0.0763 [0.0494]	262	0.06 [0.0494]	-0.0545 [0.0494]	266	-0.0400 [0.0447]	0.101 [0.0638]	528
Currently unemployed	0.23 [0.0459]	-0.0061 [0.0459]	262	0.18 [0.0520]	0.138*** [0.0520]	266	0.101** [0.0467]	-0.0989 [0.0659]	528
Current # hours worked per week (excluding unemployed)	32.09 [2.013]	-3.666* [2.013]	216	33.46 [2.131]	-2.088 [2.131]	216	-2.114 [1.833]	-1.055 [2.650]	432
Current # hours worked per week (including unemployed)	26.64 [1.925]	-1.447 [1.925]	262	28.90 [1.904]	-2.251 [1.904]	266	-2.807 [1.746]	1.139 [2.553]	528
Had COVID-19	0.17 [0.0465]	0.0044 [0.0465]	261	0.13 [0.0390]	-0.0931** [0.0390]	263	-0.104*** [0.0392]	0.0934 [0.0586]	524
Tested for COVID-19	0.24 [0.0596]	0.0421 [0.0596]	261	0.39 [0.0715]	-0.175** [0.0715]	263	-0.179*** [0.0637]	0.235*** [0.0833]	524
Had at least one symptom (in the last week) related to:									
Depression	0.43 [0.0719]	0.0548 [0.0719]	262	0.52 [0.0723]	0.0055 [0.0723]	266	0.0355 [0.0658]	0.0120 [0.0915]	528
Stress	0.56 [0.0664]	0.0178 [0.0664]	262	0.66 [0.0716]	0.0000 [0.0716]	266	0.00796 [0.0640]	0.0266 [0.0881]	528
Anxiety	0.32 [0.0685]	0.0412 [0.0685]	262	0.36 [0.0702]	-0.0617 [0.0702]	266	-0.0201 [0.0652]	0.0530 [0.0884]	528
COVID quiz	-0.22 [0.140]	-0.172 [0.140]	262	0.36 [0.142]	-0.238* [0.142]	266	-0.276** [0.129]	0.190 [0.181]	528
Cited among 3 trusted sources of COVID information:									
Pres Bolsonaro	0.27 [0.0600]	0.0149 [0.0600]	262	0.00 [0.0216]	0.0517** [0.0216]	266	0.0647** [0.0260]	-0.0704 [0.0588]	528
Health experts	0.67 [0.0686]	0.0121 [0.0686]	262	0.78 [0.0641]	0.0208 [0.0641]	266	-0.0099 [0.0579]	0.0029 [0.0846]	528
Trust Bolsonaro above health experts	0.22 [0.0668]	-0.0650 [0.0668]	202	0.00 [0.0254]	0.0540** [0.0254]	211	0.0509* [0.0286]	-0.111* [0.0641]	413
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:									
X="someone like me"	14.79 [3.338]	3.713 [3.338]	262	31.85 [4.447]	1.512 [4.447]	266	1.875 [4.081]	3.505 [5.061]	528
X=30-year old woman	12.58 [2.747]	0.430 [2.747]	262	24.25 [3.577]	2.335 [3.577]	266	2.784 [3.406]	-1.042 [4.295]	528
X=65-year old man	31.04 [3.864]	-1.105 [3.864]	262	47.35 [4.686]	1.515 [4.686]	266	1.057 [4.287]	0.462 [5.683]	528
Advocacy for a person to 'stay at home' (instead of working) if:									
The person has COVID symptoms	0.78 [0.0620]	0.027 [0.0620]	262	0.88 [0.0501]	-0.0138 [0.0501]	266	-0.0416 [0.0474]	0.0892 [0.0726]	528
Someone else in their household has COVID symptoms	0.38 [0.0698]	0.137* [0.0698]	262	0.55 [0.0780]	-0.0643 [0.0780]	266	-0.0882 [0.0686]	0.258*** [0.0927]	528

The sample refers to individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. Each entry reports results from a separate regression. The reported coefficients in Panel A refer to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. The estimates for ϕ_1 and ϕ_2 from equation (3) are reported in Panel B. Outcomes are explained in the text. All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Regressions in Panel B also include an indicator for whether the respondent voted for Bolsonaro. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 6 - Liquidity Impacts of the Emergency Cash-Transfer on Beliefs and Knowledge about COVID-19

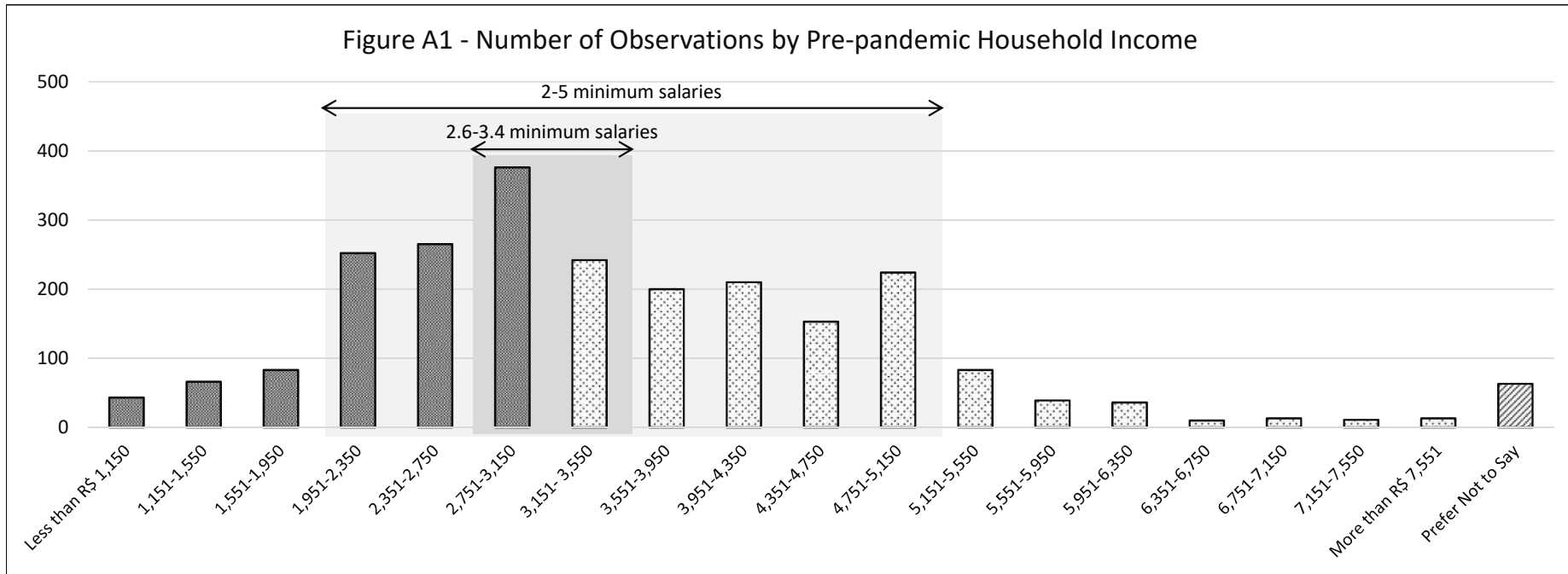
Outcomes:	DV average, AE installment not within last 14 days	[1]	[2]		[3]		<i>n</i>
		Coefficient	<i>n</i>	Coefficient	<i>n</i>	Coefficient	
[1] COVID quiz	-0.413	0.388** [0.156]	178	0.385** [0.184]	159	0.443** [0.191]	156
Cited among 3 trusted sources of COVID information:							
[2] Pres Bolsonaro	0.154	-0.0719 [0.0487]	178	-0.0970* [0.0578]	159	-0.101 [0.0608]	156
[3] Health experts	0.675	0.128* [0.0672]	178	0.0817 [0.0886]	159	0.0668 [0.0952]	156
[4] Trust Bolsonaro above health experts	0.133	-0.114*** [0.0407]	143	-0.109** [0.0528]	128	-0.108* [0.0567]	125
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:							
[5] X="someone like me"	29.62	-0.985 [4.736]	178	1.748 [4.864]	159	-0.887 [4.847]	156
[6] X=30-year old woman	22.55	2.256 [4.498]	178	4.770 [4.480]	159	4.491 [4.547]	156
[7] X=65-year old man	38.05	4.178 [4.967]	178	5.021 [5.485]	159	3.250 [5.729]	156
Advocacy for a person to 'stay at home' (instead of working) if:							
[8] The person has COVID symptoms	0.752	0.0184 [0.0674]	178	0.0291 [0.0778]	159	0.0342 [0.0838]	156
[9] Someone else in their household has COVID symptoms	0.444	-0.0510 [0.0780]	178	-0.0598 [0.0907]	159	-0.0488 [0.0985]	156
Controls:		No		Yes, baseline		Yes, baseline + num of AE installments received	

Note: The sample is restricted to AE beneficiaries that are the only adult in the household. Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the beneficiary received AE within the fourteen days immediately prior to answering the survey.

Baseline controls include state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

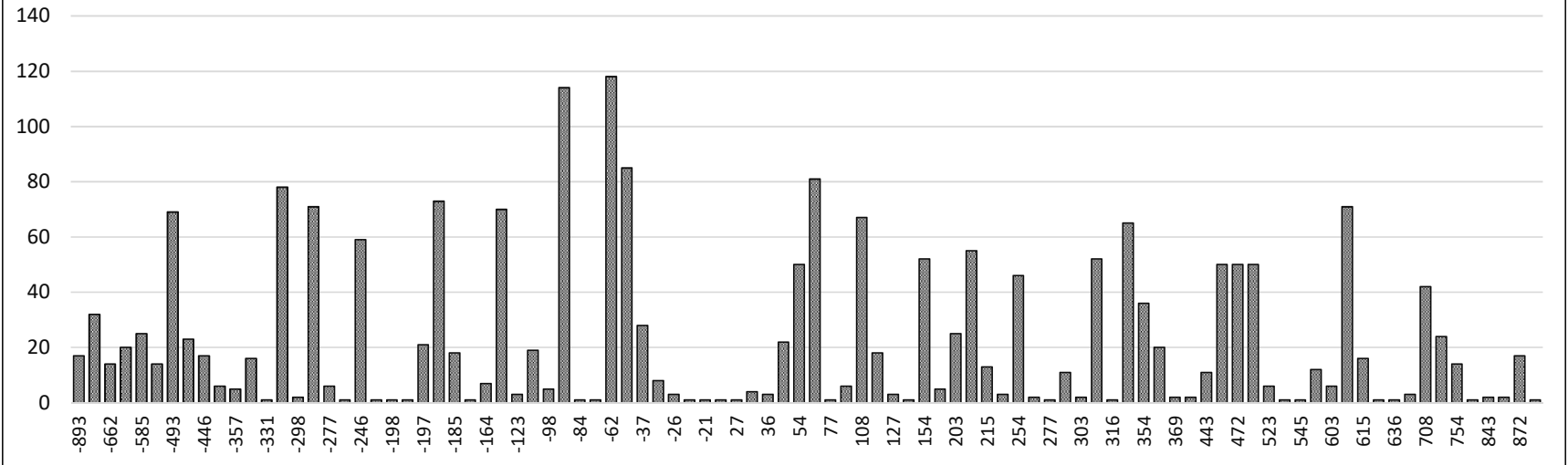
Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1 - Number of Observations by Pre-pandemic Household Income



Note: The household income cutoff for AE eligibility is three minimum salaries.

Figure A2 - Number of Observations: Variable Z (in RS\$)



Notes: The running variable is a normalised pre-pandemic household income with respect to the eligibility cut-off, in per capita terms (i.e. pre-pandemic midpoint income minus 3,135, divided by the number of residents in the household). We conducted the test for the null hypothesis of continuity of the density at the cutoff point 0 as proposed by Cattaneo et. al. (2020, 2018). The p-value is 0.9865.

Table A1 - Summary Statistics of Respondents Characteristics

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	mean	st. dev.	<i>n</i>	mean	st. dev.	<i>n</i>
Female	0.583	0.493	612	0.569	0.495	1,912
White	0.557	0.497	612	0.551	0.498	1,912
Age	37.5	10.2	612	37.8	9.9	1,912
Married or cohabiting	0.544	0.498	612	0.540	0.499	1,912
Voted Bolsonaro	0.494	0.500	583	0.487	0.500	1,820
College education	0.665	0.472	612	0.684	0.465	1,912
Live with elderly	0.177	0.382	609	0.193	0.395	1,902
Number of people in the household	3.078	1.175	612	3.085	1.204	1,912
Income per capita (in RS\$)	1,188	565	612	1,333	798	1,912
Received government benefits before pandemic	0.276	0.447	612	0.252	0.434	1,912
In Cadastro Unico before pandemic	0.315	0.465	556	0.308	0.462	1,735
Unemployed before pandemic	0.225	0.418	612	0.212	0.409	1,912

Note: All outcomes except age, number of people in the household and income per capita are binary variables valued at 0 or 1.

Table A2 - Averages of Respondents Outcomes – Precautionary Behavior Before and During Pandemic

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries				2-5 minimum salaries			
	Before pandemic	During pandemic	% change	<i>n</i>	Before pandemic	During pandemic	% change	<i>n</i>
<i>Outcomes:</i>								
<i>Practiced this behavior on an average week:</i>								
Went to the hairdresser or barber	0.49	0.16	-68%	612	0.51	0.18	-64%	1,912
Used public transport to go to work	0.38	0.14	-63%	612	0.38	0.15	-61%	1,912
Used public transport to go to the hospital or to shop	0.33	0.10	-68%	612	0.32	0.11	-67%	1,912
Used public transport for leisure	0.42	0.07	-83%	612	0.41	0.07	-83%	1,912
Attended a family dinner outside my home	0.59	0.12	-79%	612	0.55	0.13	-77%	1,912
Had lunch or dinner in a restaurant	0.62	0.09	-85%	612	0.62	0.10	-84%	1,912
Went out to work	0.61	0.41	-32%	612	0.60	0.41	-31%	1,912
Sanitized my cell phone	0.23	0.61	167%	612	0.20	0.59	193%	1,912
Visited elderly or vulnerable relatives	0.33	0.06	-82%	612	0.31	0.07	-78%	1,912
Hugged or shook hands with friends who don't live with me	0.77	0.12	-85%	612	0.76	0.11	-85%	1,912
Went to the shopping center	0.67	0.15	-77%	612	0.68	0.14	-80%	1,912
Met with my friends and participated in social events	0.67	0.05	-92%	612	0.65	0.06	-91%	1,912
Smoked cigarettes	0.10	0.08	-13%	612	0.08	0.08	0%	1,912
Kept a healthy diet	0.32	0.38	18%	612	0.32	0.39	24%	1,912

Note: Reported averages represent means of binary variables (valued at 0 or 1), valued at 1 for respondents who indicated they engaged in the listed activity "in a typical week, before the pandemic", and then "in the last week" (during the pandemic).

Table A3 - Summary statistics of outcomes

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	mean	st. dev.	<i>n</i>	mean	st. dev.	<i>n</i>
<i>Emergency Cash-Transfer</i>						
AE beneficiary	0.482	0.500	587	0.432	0.495	1,840
Number AE installments received (out of 3)	1.266	1.396	579	1.147	1.384	1,808
Number AE installments received (if AE beneficiary)	2.646	0.646	277	2.675	0.614	775
<i>Precautionary behaviour</i>						
Number of times washed hands yesterday	5.156	2.499	609	5.134	2.493	1,901
Number of times visited friends in the last 2 weeks	1.165	1.806	612	1.167	1.788	1,912
Precautionary behaviour index	0.008	0.354	612	-0.001	0.359	1,912
Currently unemployed	0.288	0.453	612	0.273	0.446	1,912
Current # hours worked per week (excluding unemployed)	30.593	14.230	499	30.874	14.316	1,580
Current # hours worked per week (including unemployed)	24.944	17.498	612	25.513	17.498	1,912
<i>Health</i>						
Had COVID-19	0.109	0.312	606	0.104	0.306	1,892
Tested for COVID-19	0.285	0.452	606	0.275	0.447	1,892
Someone in the household had serious COVID (hospitalised or died)	0.017	0.129	535	0.021	0.143	1,682
Had a mental health symptom (DASS-21) in the last week	0.735	0.442	612	0.744	0.437	1,912
Had at least one symptoms, in the last week						
Depression	0.508	0.500	612	0.506	0.500	1,912
Stress	0.636	0.482	612	0.625	0.484	1,912
Anxiety	0.348	0.477	612	0.368	0.482	1,912
<i>Beliefs and Knowledge about COVID-19</i>						
COVID quiz	-0.043	0.999	612	0.048	0.988	1,912
Cited among 3 trusted sources of COVID information						
Pres Bolsonaro	0.136	0.343	612	0.108	0.310	1,912
Health experts	0.711	0.454	612	0.730	0.444	1,912
Trust Bolsonaro above health experts	0.103	0.304	478	0.082	0.274	1,504
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:						
X="someone like me"	25.557	28.897	612	24.642	28.455	1,912
X=30-year old woman	20.165	24.146	612	19.339	23.934	1,912
X=65-year old man	40.507	31.095	612	39.218	30.324	1,912
Advocacy for a person to 'stay at home' (instead of working) if:						
The person has COVID symptoms	0.804	0.397	612	0.798	0.402	1,912
Someone else in their household has COVID symptoms	0.497	0.500	612	0.494	0.500	1,912

Note: Outcomes are explained in the text.

Table A4 - Effects of AE Income Eligibility on Chance of Receiving AE

	(1)	(2)	(3)	(4)
Mean (earned more than 3 m.s.)	0.330	0.330	0.379	0.379
Earned less than 3 m.s.	0.219*** [0.0227]	0.108*** [0.0290]	0.166*** [0.0417]	0.0962** [0.0470]
Observations	1,840	1,608	587	510
R-squared	0.048	0.199	0.026	0.209
Controls	No	Yes	No	Yes
Sample	2-5 m.s.	2-5 m.s.	2.6-3.4 m.s.	2.6-3.4 m.s.

Note: Each column reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Regressions in columns 2 and 4 include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A5 - Characteristics by Pre-pandemic household income - Averages

Sample (pre-pandemic household income)	2.6-3.0 minimum salaries (below AE income eligibility cut-off, R\$2751-R\$3150) [1]	3.0-3.4 minimum salaries (above AE income eligibility cut-off, R\$3151-R\$3550) [2]	mean test p-value ([1]=[2])
Female	0.588	0.576	0.779
Voted for Bolsonaro	0.488	0.505	0.691
White	0.553	0.564	0.802
Black	0.418	0.415	0.955
Other race	0.447	0.436	0.802
Primary incomplete	0.005	0.008	0.638
Primary complete	0.021	0.030	0.515
Secondary incomplete	0.016	0.008	0.428
Secondary complete	0.295	0.284	0.765
College incomplete	0.210	0.242	0.364
Has college	0.662	0.669	0.853
Postgraduate	0.093	0.106	0.604
Single	0.404	0.301	0.010
Married	0.431	0.504	0.076
Cohabiting	0.072	0.106	0.141
Divorced	0.066	0.076	0.646
Widower	0.019	0.008	0.311
Number of people in the household	3.101	3.042	0.548
Received government benefits (before the pandemic)	0.303	0.233	0.059
In Cadastro Unico	0.330	0.291	0.328
Behavior index (pre-pandemic)	-0.017	-0.006	0.731
Unemployed	0.261	0.169	0.009
18 to 24 years old	0.120	0.081	0.124
25 to 29 years old	0.181	0.148	0.296
30 to 39 years old	0.287	0.360	0.059
40 to 49 years old	0.205	0.250	0.191
Older than 50	0.207	0.161	0.154
Live with minor (younger than 18)	0.410	0.459	0.229
Live with elderly (older than 60)	0.207	0.129	0.013
Someone in the household has ... Diabetes	0.174	0.124	0.098
... Heart disease	0.342	0.295	0.225
... Asthma or respiratory disease	0.071	0.085	0.506
... Allergies	0.152	0.145	0.818
... Cancer	0.383	0.338	0.259
... Allergies	0.011	0.021	0.302
<i>n</i>	376	236	

Note: Columns 1 and 2 refer to individuals that indicated their pre-pandemic household income to be between R\$ 2,751- R\$ 3,150 (column 1), and between R\$ 3,151- R\$3,550 (column 2). The cut-off for AE income eligibility requires current household income to be less than R\$ 3,135 (or three minimum salaries).

Table A6 - Impacts of Emergency Cash-Transfer - Robustness Checks

Specification and sample	Baseline (same as column 1 tables in the text)			No controls		Matched sample and baseline specification		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<i>Precautionary behaviour</i>								
Number of times washed hands yesterday	5.24	-0.0231 [0.231]	526	-0.128 [0.206]	609	5.46	-0.0582 [0.253]	370
Number of times visited friends in the last 2 weeks	1.19	-0.0424 [0.177]	528	-0.0348 [0.152]	612	1.07	0.0402 [0.176]	371
Precautionary behaviour index	-0.01	0.0116 [0.0351]	528	0.0307 [0.0301]	612	-0.01	0.0333 [0.0397]	371
Currently unemployed	0.21	0.0503 [0.0310]	528	0.123*** [0.0361]	612	0.18	0.0505 [0.0343]	371
Current # hours worked per week (excluding unemployed)	32.92	-2.668** [1.307]	432	-3.883*** [1.255]	499	33.98	-2.891* [1.615]	310
Current # hours worked per week (including unemployed)	27.90	-2.222* [1.251]	528	-4.808*** [1.425]	612	29.32	-2.826* [1.581]	371
<i>Health</i>								
Had COVID-19	0.15	-0.0562* [0.0298]	524	-0.0688** [0.0276]	606	0.15	-0.0672** [0.0324]	370
Tested for COVID-19	0.32	-0.0583 [0.0436]	524	-0.0563 [0.0383]	606	0.31	-0.106** [0.0499]	370
Someone in the household had serious COVID	0.01	0.00487 [0.0110]	469	0.0118 [0.0105]	535	0.01	0.00440 [0.0125]	331
Had depression symptoms (last week)	0.46	0.0416 [0.0476]	528	0.0823** [0.0414]	612	0.47	0.0624 [0.0550]	371
Had stress symptoms (last week)	0.61	0.0216 [0.0457]	528	0.0414 [0.0402]	612	0.62	-0.0004 [0.0539]	371
Had anxiety symptoms (last week)	0.33	0.00705 [0.0465]	528	0.0285 [0.0394]	612	0.33	0.00682 [0.0543]	371
<i>Beliefs and Knowledge about COVID-19</i>								
COVID quiz (norm score)	0.03	-0.179* [0.0937]	528	-0.126 [0.0836]	612	0.14	-0.200** [0.0989]	371
Cited among 3 trusted sources of COVID information								
Pres Bolsonaro	0.13	0.0286 [0.0323]	528	0.0138 [0.0282]	612	0.15	0.0221 [0.0398]	371
Health experts	0.73	-0.00845 [0.0442]	528	-0.0293 [0.0374]	612	0.72	-0.0327 [0.0529]	371
Trust Bolsonaro above health experts	0.10	-0.00499 [0.0335]	413	0.0006 [0.0285]	478	0.13	-0.0073 [0.0414]	291
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:								
X="someone like me"	23.06	3.674 [2.667]	528	4.073* [2.393]	612	21.36	4.994* [2.995]	371
X=30-year old woman	17.92	2.250 [2.282]	528	3.648* [1.994]	612	18.01	4.350* [2.578]	371
X=65-year old man	38.31	1.294 [2.869]	528	3.569 [2.592]	612	38.43	3.221 [3.267]	371
Advocacy for a person to 'stay at home' (instead of working) if:								
The person has COVID symptoms	0.81	0.00419 [0.0388]	528	-0.002 [0.0330]	612	0.79	0.0061 [0.0455]	371
Someone else in their household has COVID symptoms	0.47	0.0443 [0.0484]	528	0.0430 [0.0415]	612	0.46	0.0506 [0.0579]	371

Note: The sample in columns 1 and 2 refers to individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. The sample in column 3 is a matched sample within individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. The matched sample was constructed using a nonparametric coarsened exact matching (CEM) approach (Iacus et al 2011, 2012), by selecting 'above 3m.s.' (control) individuals with same characteristics as the 'below 3m.s.' (treatment) individuals. These characteristics are gender, race, marital status, college education, living with elderly, pre-pandemic use of government benefits and CadUnico registration, unemployment status

Each entry reports results from a separate regression. The reported coefficients refer to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic.

The baseline specification includes controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, pre-pandemic precautionary behavior index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (we use a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A7- Correction for Multiple Hypotheses Testing (multiple outcomes)

Sample: Test:	p-value - Correction for multiplicity				
	Restricted (2.6-3.4 minimum salaries)		Matched sample (restricted)		Full (2-5 minimum salaries)
	<u>List, Shaikh, Xu</u>	<u>Anderson</u>	<u>List, Shaikh, Xu</u>	<u>Anderson</u>	<u>Anderson</u>
	[1]	[2]	[3]	[4]	[5]
<u>Outcomes:</u>					
Receive AE	0.000	0.399	0.035	0.296	0.001
Precautionary behaviour index	0.882	1.000	0.918	0.450	0.349
Currently unemployed	0.012	0.399	0.539	0.296	0.001
Current # hours worked per week (excluding unemployed)	0.021	0.399	0.244	0.296	0.001
Current # hours worked per week (including unemployed)	0.000	0.399	0.165	0.296	0.001
Had COVID-19	0.179	0.399	0.236	0.296	0.096
Tested for COVID-19	0.742	0.468	0.629	0.296	0.082
Had at least one symptom (in the last week) related to:					
Depression	0.479	0.706	0.706	0.397	0.102
Stress	0.920	1.000	0.954	0.962	0.183
Anxiety	0.907	1.000	0.998	0.962	0.416
COVID quiz	0.752	0.399	0.256	0.296	0.025
Cited among 3 trusted sources of COVID information:					
Pres Bolsonaro	0.922	0.706	0.997	0.627	0.082
Health experts	0.932	1.000	0.945	0.621	0.156
Trust Bolsonaro above health experts	0.983	1.000	0.990	0.962	0.155
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:					
X="someone like me"	0.650	0.468	0.300	0.296	0.065
X=30-year old woman	0.563	0.706	0.625	0.296	0.092
X=65-year old man	0.778	1.000	0.876	0.429	0.096
Advocacy for a person to 'stay at home' (instead of working) if:					
The person has COVID symptoms	0.999	1.000	0.989	0.962	0.416
Someone else in their household has COVID symptoms	0.901	0.706	0.879	0.450	0.416

Note: This table replicates the p-value for results in Tables 1-4 in the paper correcting for multiple outcomes. The sample in columns [1] and [2] refers to individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. The sample in columns [3] and [4] is a matched sample within individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. The matched sample was constructed using a nonparametric coarsened exact matching (CEM) approach (Iacus et al 2011, 2012), by selecting 'above 3m.s.' (control) individuals with same characteristics as the 'below 3m.s.' (treatment) individuals (as explained in Table A6). The sample in column [5] refers to individuals whose pre-pandemic household income is between 2-5 minimum salaries.

The results in columns [1] and [3] follow the methodology in List, Shaikh and Xu (2019). The results in columns [2], [4] and [5] refer to sharpened False Discovery Rate (FDR) q-values following the methodology in Anderson (2008) and correcting p-values from regressions using the baseline specification.

Table A8 - Reduced form AE Impacts - Regression Discontinuity Results (i)

	Panel A				Panel B			Panel C			
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	bandwidth (-/+)	n	Earned less than 3 m.s.	bandwidth (-/+)	n	DV average where earned > 3 m.s.	Earned less than 3 m.s.	bandwidth (-/+)	n
<i>Precautionary behaviour</i>											
Number of times washed hands yesterday	5.30	0.172 [0.275]	362	1,221	-0.0672 [0.242]	500	1,447	5.28	-0.149 [0.199]	900	1,788
Number of times visited friends in the last 2 weeks	1.19	0.0730 [0.208]	344	1,173	0.00930 [0.171]	500	1,454	1.15	-0.0580 [0.144]	900	1,796
Precautionary behaviour index	0.01	0.0308 [0.0477]	301	1,003	-0.0214 [0.0354]	500	1,454	0.01	0.00405 [0.0275]	900	1,796
Currently unemployed	0.24	0.0886** [0.0449]	335	1,114	0.0691* [0.0364]	500	1,454	0.22	0.0315 [0.0310]	900	1,796
Current # hours worked per week (excluding unemployed)	32.68	-1.833 [1.468]	467	1,082	-1.654 [1.394]	500	1,166	32.54	-2.397** [1.205]	900	1,460
Current # hours worked per week (including unemployed)	27.25	-4.090** [1.698]	337	1,114	-2.406* [1.386]	500	1,454	27.96	-2.125* [1.191]	900	1,796
<i>Health</i>											
Had COVID-19	0.13	-0.075 [0.0478]	247	879	-0.0283 [0.0308]	500	1,445	0.12	-0.0289 [0.0258]	900	1,783
Tested for COVID-19	0.29	-0.0420 [0.0503]	417	1,242	-0.0780* [0.0440]	500	1,445	0.29	-0.0707* [0.0367]	900	1,783
Someone in the household had serious COVID	0.02	0.008 [0.0175]	362	1,097	0.00199 [0.0149]	500	1,305	0.02	0.00416 [0.0128]	900	1,619
Had depression symptoms (last week)	0.46	0.108* [0.0622]	325	1,054	0.0958** [0.0484]	500	1,454	0.49	0.0691* [0.0402]	900	1,796
Had stress symptoms (last week)	0.61	0.009 [0.0535]	366	1,244	0.0193 [0.0470]	500	1,454	0.61	0.0202 [0.0389]	900	1,796
Had anxiety symptoms (last week)	0.36	0.0332 [0.0556]	355	1,222	0.0249 [0.0476]	500	1,454	0.36	-0.0171 [0.0394]	900	1,796

Optimal Bandwidth (Cattaneo et. al 2020)

Pre-determined

Note: Each entry reports results from a separate RD regression. The reported coefficient refers to estimates of δ in equation (2) and is an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. The running variable is a normalised pre-pandemic household income with respect to the eligibility cutoff, in per capita terms (i.e. pre-pandemic midpoint income minus 3,135, divided by the number of residents in the household). Panel A reports RD estimates using the bandwidth selection procedure of Cattaneo et. al. (2020).

All regressions include controls for the running variable fully interacted with an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Socio and demographic controls include indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Robust standard errors in brackets.

To test for selection effects, we estimate equation (2) using as dependent variable – the thirty-seven variables described in Table A5. For the sample in Panel B and Panel C (bandwidth R\$ 500 and R\$ 900), the estimates for coefficient δ were statistically significant for eight variables (e.g., education, age and previous use of government benefits). These characteristics are controlled in the RD regressions. *** p<0.01, ** p<0.05, * p<0.1

Table A8 - Reduced form AE impacts - Regression Discontinuity Results (ii)

	Panel A				Panel B			Panel C			
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	bandwidth (-/+)	<i>n</i>	Earned less than 3 m.s.	bandwidth (-/+)	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	bandwidth (-/+)	<i>n</i>
<i>Beliefs and Knowledge about COVID-19</i>											
COVID quiz (norm score)	0.13	-0.174 [0.118]	335	1,114	-0.163* [0.0955]	500	1,454	0.14	-0.121 [0.0776]	900	1,796
Cited among 3 trusted sources of COVID information											
Pres Bolsonaro	0.10	0.0292 [0.0371]	340	1,173	0.0497 [0.0309]	500	1,454	0.09	0.0322 [0.0252]	900	1,796
Health experts	0.74	-0.0182 [0.0569]	316	1,053	-0.00641 [0.0442]	500	1,454	0.75	0.0124 [0.0360]	900	1,796
Trust Bolsonaro above health experts	0.07	-0.00966 [0.0373]	379	991	0.00476 [0.0324]	500	1,153	0.07	0.00733 [0.0259]	900	1,428
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:											
X="someone like me"	23.79	5.564* [3.264]	397	1,251	4.850* [2.813]	500	1,454	23.53	5.128** [2.368]	900	1,796
X=30-year old woman	18.76	4.028 [2.804]	389	1,245	3.425 [2.405]	500	1,454	18.50	3.221 [1.997]	900	1,796
X=65-year old man	37.41	3.769 [3.455]	358	1,225	5.351* [2.969]	500	1,454	38.09	5.773** [2.471]	900	1,796
Advocacy for a person to 'stay at home' (instead of working) if:											
The person has COVID symptoms	0.83	0.0434 [0.0464]	332	1,114	0.0176 [0.0382]	500	1,454	0.81	-0.0217 [0.0323]	900	1,796
Someone else in their household has COVID symptoms	0.46	0.0491 [0.0802]	221	830	0.118** [0.0491]	500	1,454	0.50	0.0841** [0.0407]	900	1,796
	Optimal Bandwidth (Cattaneo et. al 2020)				Pre-determined						

Note: Each entry reports results from a separate RD regression. The reported coefficient refers to estimates of δ in equation (2) and is an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. The running variable is a normalised pre-pandemic household income with respect to the eligibility cutoff, in per capita terms (i.e. pre-pandemic midpoint income minus 3,135, divided by the number of residents in the household). Panel A reports RD estimates using the bandwidth selection procedure of Cattaneo et. al. (2020).

All regressions include controls for the running variable fully interacted with an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Socio and demographic controls include indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Robust standard errors in brackets.

To test for selection effects, we estimate equation (2) using as dependent variable – the thirty-seven variables described in Table A5. For the sample in Panel B and Panel C (bandwidth R\$ 500 and R\$ 900), the estimates for coefficient δ were statistically significant for eight variables (e.g., education, age and previous use of government benefits). These characteristics are controlled in the RD regressions. *** p<0.01, ** p<0.05, * p<0.1

Table A9 - Impacts of Emergency Cash-Transfer on Precautionary Behavior items

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<u>Outcomes:</u>						
<i>Practiced this behavior in the last week:</i>						
Went to the hairdresser or barber	0.203	-0.056 [0.0361]	528	0.207	-0.037 [0.0224]	1,650
Used public transport to go to work	0.148	0.000 [0.0360]	528	0.149	-0.006 [0.0223]	1,650
Used public transport to go to the hospital or to shop	0.081	0.040 [0.0298]	528	0.085	0.0497*** [0.0193]	1,650
Used public transport for leisure	0.089	-0.024 [0.0271]	528	0.060	0.013 [0.0159]	1,650
Attended a family dinner outside my home	0.140	-0.007 [0.0299]	528	0.147	-0.008 [0.0197]	1,650
Had lunch or dinner in a restaurant	0.081	0.030 [0.0307]	528	0.104	0.011 [0.0177]	1,650
Went out to work	0.453	-0.011 [0.0443]	528	0.446	-0.028 [0.0280]	1,650
Sanitized my cell phone	0.619	0.003 [0.0465]	528	0.594	0.009 [0.0294]	1,650
Visited elderly or vulnerable relatives	0.076	-0.017 [0.0216]	528	0.074	-0.001 [0.0151]	1,650
Hugged or shook hands with friends who don't live with me	0.123	-0.006 [0.0318]	528	0.125	-0.021 [0.0195]	1,650
Went to the shopping center	0.161	-0.012 [0.0340]	528	0.140	0.007 [0.0209]	1,650
Met with my friends and participated in social events	0.068	-0.025 [0.0255]	528	0.065	0.000 [0.0151]	1,650
Smoked cigarettes	0.097	-0.012 [0.0276]	528	0.084	0.000 [0.0168]	1,650
Kept a healthy diet	0.415	-0.0339 [0.0466]	528	0.430	-0.0362 [0.0293]	1,650

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10 - Impacts of AE Eligibility by Type of Job (before the pandemic)

Sample:	Panel A						Panel B		
	Informal worker or self-employed (before pandemic)						All		
	DV average where earned > 3 m.s.	Yes		No		Earned < 3 m.s.	(Earned < 3 m.s.) × (Inf. worker or self-employed)	n	
Earned < 3 m.s.		n	Earned < 3 m.s.	n					
<u>Outcomes:</u>									
Precautionary behaviour index	-0.13	0.105	171	0.03	-0.0190	339	0.0233	-0.0256	510
		[0.0724]			[0.0433]		[0.0381]	[0.0412]	
Currently unemployed	0.24	0.177**	171	0.20	0.0182	339	0.0357	0.0537	510
		[0.0771]			[0.0347]		[0.0353]	[0.0547]	
Current # hours worked per week (excluding unemployed)	31.44	-3.554	140	34.09	-1.902	276	-0.812	-5.045***	416
		[2.720]			[1.679]		[1.560]	[1.894]	
Current # hours worked per week (including unemployed)	25.81	-2.825	171	29.04	-1.504	339	-0.745	-3.596*	510
		[2.870]			[1.550]		[1.471]	[1.862]	
Had COVID-19	0.17	-0.0515	169	0.14	-0.0423	337	-0.0455	-0.0071	506
		[0.0555]			[0.0371]		[0.0341]	[0.0353]	
Tested for COVID-19	0.39	-0.186**	169	0.29	-0.0443	337	-0.0731	0.0113	506
		[0.0885]			[0.0536]		[0.0480]	[0.0549]	

Note: The sample refers to individuals whose pre-pandemic household income is between 2.6-3.4 minimum salaries. Each entry reports results from a separate regression.

The reported coefficients in Panel A refer to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (AE income eligibility cutoff) before the pandemic. The estimates for ϕ_1 and ϕ_2 from equation (3) are reported in Panel B, when replacing the indicator for whether the person has voted for Bolsonaro by an indicator for whether the participant was informal worker or self-employed before the pandemic

Outcomes are explained in the text. All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, pre-pandemic precautionary behavior index (explained in the text), indicators for education, marital status, gender, race, age, if lives with elderly, number of people living in the household, an indicator for whether the respondent voted for Bolsonaro and a social desirability index (we use a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets *** p<0.01, ** p<0.05, * p<0.1

Table A11 - Impacts of Emergency Cash-Transfer on Media Consumption

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
<i>Outcomes:</i>						
<i>How often do you look for COVID-19 information?</i>						
At least once a day	0.513	-0.0475 [0.0478]	528	0.489	-0.0168 [0.0302]	1,650
Less than once a week	0.169	0.0421 [0.0348]	528	0.185	0.0022 [0.0230]	1,650
number of sources of COVID-19 information	3.911	-0.0098 [0.232]	527	3.825	-0.0936 [0.144]	1,648
non-COVID quiz	-0.036	0.0510 [0.0974]	528	0.023	-0.0035 [0.0623]	1,650

Note: Each entry reports results from a separate regression. Participants were presented with twelve statements about other (true or false) events during the pandemic, relating to political, economic, sports and celebrity news. The non-COVID current affairs is the normalised performance in this quiz. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. All regressions include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses).

Robust standard errors in brackets. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table A12 – Impacts of Emergency Cash-Transfer on Financial Health

Sample (pre-pandemic household income)	2.6-3.4 minimum salaries			2-5 minimum salaries		
	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>	DV average where earned > 3 m.s.	Earned less than 3 m.s.	<i>n</i>
Outcomes:						
1. Used savings to cover day-to-day consumption	0.284	-0.00778 [0.0437]	528	0.283	0.00953 [0.0279]	1,650
2. Took loans from friends or family	0.076	0.0110 [0.0251]	528	0.067	0.0381** [0.0171]	1,650
3. Took loans from banks or other financial institutions	0.153	-0.0790** [0.0331]	528	0.116	-0.0305 [0.0195]	1,650
4. Spending on day-to-day consumption decreased	0.487	0.0245 [0.0481]	528	0.526	0.0125 [0.0305]	1,650
5. Reduced portions or skipped meals due to lack of money	0.081	-0.0239 [0.0275]	528	0.058	0.00617 [0.0169]	1,650
6. Reduced spending on entertainment or leisure activities due to lack of money	0.352	-0.0301 [0.0471]	528	0.349	0.00352 [0.0293]	1,650
7. Reduced spending on clothing and personal items due to lack of money	0.297	0.0140 [0.0449]	528	0.293	0.0266 [0.0282]	1,650
8. Did not settle some day-to-day bills (e.g., rent)	0.153	-0.0406 [0.0347]	528	0.117	0.00157 [0.0207]	1,650
9. Sold assets (e.g., cars, jewelry) to cover day-to-day consumption	0.042	-0.0239 [0.0175]	528	0.041	-0.0119 [0.0109]	1,650
10. Did an extra job	0.191	0.0586 [0.0418]	528	0.195	0.0091 [0.0251]	1,650
11. Deferred large expenses (e.g., car, refrigerator)	0.220	0.0214 [0.0413]	528	0.249	-0.0282 [0.0263]	1,650
12. Increased savings or paid off debts	0.123	-0.0228 [0.0299]	528	0.118	-0.0114 [0.0186]	1,650
13. Made loans to friends or family	0.042	-0.0353** [0.0179]	528	0.031	-0.0179* [0.0092]	1,650
14. Increased daily consumption	0.114	-0.0120 [0.0325]	528	0.112	0.00308 [0.0200]	1,650
15. Made large expenses (e.g., car, refrigerator)	0.013	-0.0021 [0.0133]	528	0.019	0.00669 [0.0084]	1,650

Note: Each entry reports results from a separate regression. The reported coefficient refers to an indicator for whether the respondent lives in a household that earned less than three minimum salaries (the AE income eligibility cut-off) before the pandemic. Controls include controls for state fixed effects and covariates for pre-pandemic conditions: household income per capita (mid-point of income range divided by the number of people living in the household), indicators for whether the participant has used government benefits, if the respondent was registered in Cadastro Unico, unemployment status, a pre-pandemic precautionary behaviour index (explained in the text), indicators for education, marital status, gender, race, age, if voted for Bolsonaro, if lives with elderly, number of people living in the household and a social desirability index (using a question asked in the British Election Study to measure respondents' tendencies to provide socially desirable responses). Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.