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# The effects of emergency government cash transfers on beliefs and behaviours during the COVID pandemic: Evidence from Brazil<sup>☆</sup>

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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 "Auxílio 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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## 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.

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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 salary<sup>1</sup>) 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 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 2382 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

<sup>1</sup> The 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.

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.<sup>2</sup> 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, Londoño-Vélez and Querubín (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 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 Londoño-Vélez and Querubín, 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 recipients' mental well-being. Another RCT study (Jacob et al., 2022; 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 et al., 2020) and exposure to expert information (Akeson 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.

<sup>2</sup> Karlan 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.

## 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.<sup>3</sup>

### 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 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\$ 1200), but was subsequently extended to allow five such instalments and then further lower monthly instalments into 2021.<sup>4</sup>

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"<sup>5</sup>, and (iv) live in a low-income household, whose total monthly income is less than three minimum salaries, i.e. less than R\$ 3135, 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 system<sup>6</sup> – 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://www.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.

<sup>3</sup> For 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.

<sup>4</sup> On 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.

<sup>5</sup> Before 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\$ 1333 (Table A1).

<sup>6</sup> CadUnico 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 Familia" 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 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.



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, 2386 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. 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 of Auxilio Emergencial?”<sup>7</sup> – 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\$ 2100 to R\$ 5100. 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\$ 1150 to R\$ 7550. 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\$ 2751 to R\$ 3550), and (ii) to an extended sample of individuals whose pre-pandemic income was between two and five minimum salaries (R\$ 1951 to R\$ 5150). These samples comprise 612 and 1912 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\$ 3135. 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 Fig. 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 survey) we provided the option, “I prefer not to say” and only 2.6% of respondents (or 63 respondents) chose this alternative.<sup>8</sup>

### 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.<sup>9</sup> 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?”

<sup>7</sup> Therefore 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.

<sup>8</sup> In 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.

<sup>9</sup> In 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.

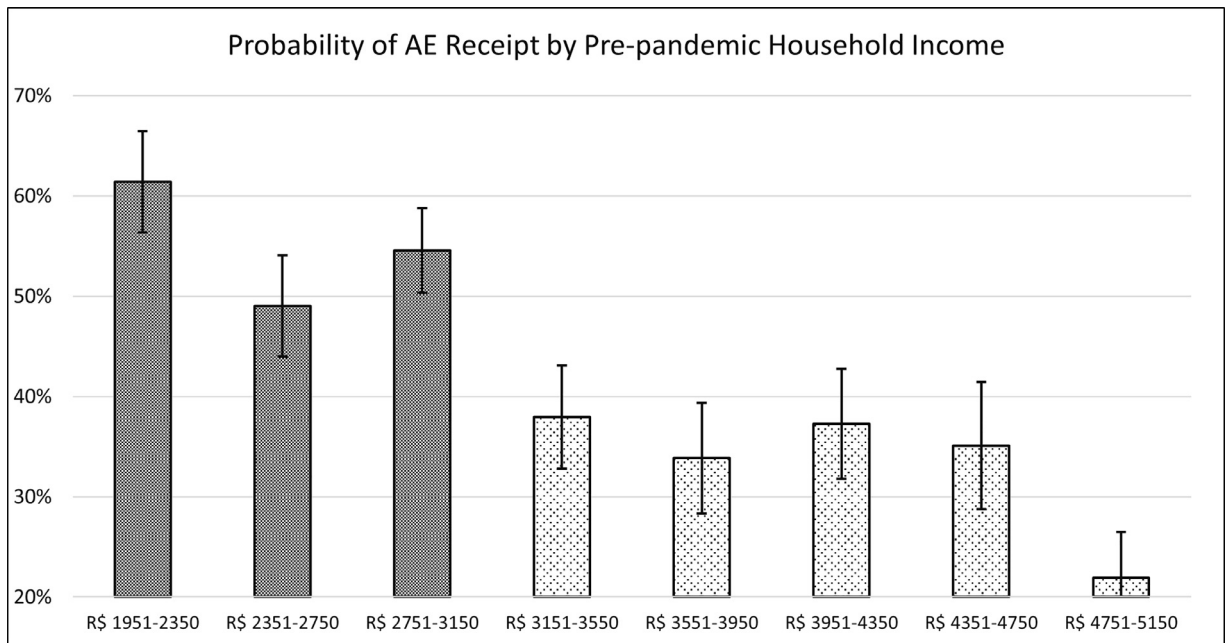


Fig. 1. Probability of AE Use by Pre-pandemic Household Income. Note: Whiskers indicate 90 percent confidence intervals.

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 COVID-19 called hydroxychloroquine")<sup>10</sup> 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.<sup>11</sup>

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.

### 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

<sup>10</sup> The 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.

<sup>11</sup> We 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.



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.<sup>12</sup>

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\$ 1333, compared to the national average of R\$ 1439 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,<sup>13</sup> 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, 2020). 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 households that used to earn less than 3 minimum salaries is significantly larger than their counterpart, as shown in Fig. 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).

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

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

where  $y_{is}$  is the outcome of individual  $i$  in state  $s$ ,  $\mathbf{X}_i$  contains a set of covariates,  $(hh \text{ earns less } 3ms)_i$  is an indicator equal to one if the respondent lives in a household that earned less than three minimum salaries before the pandemic,  $\theta_s$  are state of residence fixed effects and  $\varepsilon_{is}$  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.

<sup>12</sup> Opinionbox 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](https://www.opinionbox.com/wp-content/OPB_panel_book_2022.pdf)

<sup>13</sup> According 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.

**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.	n	DV average where earned >3 m.s.	Earned less than 3 m.s.	n
<b>Outcomes:</b>						
[1] Number of times washed hands yesterday	5.24	-0.0231 [0.231]	526	5.29	-0.121 [0.146]	1645
[2] Number of times visited friends in the last 2 weeks	1.19	-0.0424 [0.177]	528	1.16	-0.0718 [0.107]	1650
[3] Precautionary behaviour index	-0.01	0.0116 [0.0351]	528	0.00	-0.0120 [0.0211]	1650
[4] Currently unemployed	0.21	0.0503 [0.0310]	528	0.21	0.0798*** [0.0217]	1650
[5] Current # hours worked per week (excluding unemployed)	32.92	-2.668** [1.307]	432	32.62	-3.621*** [0.870]	1367
[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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

The coefficient  $\lambda$  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 Eq. (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 (R\$ 2751-3550). 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 et al., 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.<sup>14</sup> 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%).<sup>15</sup> However, we 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

<sup>14</sup> To 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  $\delta$  in Eq. (2), in which  $H(Z)$  is a linear trend, flexible on each side of the cut-off.

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

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.

<sup>15</sup> In Table A9 in the Appendix, we investigate the impacts separately for each of the index items, 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.

**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.	n	DV average where earned >3 m.s.	Earned less than 3 m.s.	n
Outcomes:						
Had COVID-19	0.152	-0.0562* [0.0298]	524	0.112	-0.0338* [0.0183]	1640
Tested for COVID-19	0.320	-0.0583 [0.0436]	524	0.289	-0.0551** [0.0269]	1640
Someone in the household had serious COVID (hospitalised or died)	0.010	0.0049 [0.0110]	469	0.014	0.0013 [0.0095]	1476

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. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

3.6 h 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.<sup>16</sup>

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.<sup>17</sup> 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.<sup>18</sup>

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 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.<sup>19</sup> At the time our survey was conducted, COVID tests were scarce in Brazil and were conducted mostly when individuals had severe symptoms,<sup>20</sup> 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).

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.

<sup>16</sup> The 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.

<sup>17</sup> Menezes-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 2018-20 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.

<sup>18</sup> In 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.

<sup>19</sup> In our sample, tested individuals were five times more likely to declare to have had COVID than non-tested individuals.

<sup>20</sup> Within the income bracket of our sample, less than 10% of Brazilians had taken a COVID test by November 2020 (IBGE, 2020).

**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.	n	DV average where earned 3 m.s.	Earned less than 3 m.s.	n
Outcomes:						
Had at least one mental health symptom (in the last week)	0.712	0.0064	528	0.734	0.0177	1650
		[0.0421]			[0.0262]	
Had at least one symptom (in the last week) related to:						
Depression	0.458	0.0416	528	0.486	0.0525*	1650
		[0.0476]			[0.0298]	
Stress	0.610	0.0216	528	0.619	0.0288	1650
		[0.0457]			[0.0291]	
Anxiety	0.331	0.0071	528	0.356	-0.0074	1650
		[0.0465]			[0.0293]	

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. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

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.

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.<sup>21</sup> 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.

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 ( $\lambda$  from Eq. (1)) by separate samples and in Panel B using both samples and reporting on the interaction term.<sup>22</sup> It is striking that several of the 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.

<sup>21</sup> In 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.

<sup>22</sup> In Panel B, we estimate Eq. (3) and report estimates for  $\phi_1$  and  $\phi_2$ , 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.

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

**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>
Outcomes:						
COVID quiz	0.035	-0.179* [0.0937]	528	0.158	-0.154*** [0.0584]	1650
Cited among 3 trusted sources of COVID information:						
Pres Bolsonaro	0.127	0.0286 [0.0323]	528	0.092	0.0397** [0.0196]	1650
Health experts	0.729	-0.0084 [0.0442]	528	0.743	-0.0291 [0.0265]	1650
Trust Bolsonaro above health experts	0.102	-0.0049 [0.0335]	413	0.068	0.0234 [0.0189]	1307
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]	1650
X=30-year old woman	17.924	2.250 [2.282]	528	17.822	2.718* [1.446]	1650
X=65-year old man	38.314	1.294 [2.869]	528	37.462	3.206* [1.808]	1650
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]	1650
Someone else in their household has COVID symptoms	0.470	0.0443 [0.0484]	528	0.499	0.0082 [0.0303]	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. 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. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

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.

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.

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.

**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			All		
	DV average where earned >3 m.s.	Earned <3 m.s.	n	DV average where earned >3 m.s.	Earned <3 m.s.	n	Earned <3 m.s.	Earned <3 m.s. X voted for Bolsonaro	n
Outcomes:									
Precautionary behaviour index	-0.10	0.0763	262	0.06	-0.0545	266	-0.0400	0.101	528
		[0.0534]			[0.0494]		[0.0447]	[0.0638]	
Currently unemployed	0.23	-0.0061	262	0.18	0.138***	266	0.101**	-0.0989	528
		[0.0459]			[0.0520]		[0.0467]	[0.0659]	
Current # hours worked per week (ex. unemployed)	32.09	-3.666*	216	33.46	-2.088	216	-2.114	-1.055	432
		[2.013]			[2.131]		[1.833]	[2.650]	
Current # hours worked per week (inc. unemployed)	26.64	-1.447	262	28.90	-2.251	266	-2.807	1.139	528
		[1.925]			[1.904]		[1.746]	[2.553]	
Had COVID-19	0.17	0.0044	261	0.13	-0.0931**	263	-0.104***	0.0934	524
		[0.0465]			[0.0390]		[0.0392]	[0.0586]	
Tested for COVID-19	0.24	0.0421	261	0.39	-0.175**	263	-0.179***	0.235***	524
		[0.0596]			[0.0715]		[0.0637]	[0.0833]	
Had at least one symptom (in the last week) related to:									
Depression	0.43	0.0548	262	0.52	0.0055	266	0.0355	0.0120	528
		[0.0719]			[0.0723]		[0.0658]	[0.0915]	
Stress	0.56	0.0178	262	0.66	0.0000	266	0.00796	0.0266	528
		[0.0664]			[0.0716]		[0.0640]	[0.0881]	
Anxiety	0.32	0.0412	262	0.36	-0.0617	266	-0.0201	0.0530	528
		[0.0685]			[0.0702]		[0.0652]	[0.0884]	
COVID quiz	-0.22	-0.172	262	0.36	-0.238*	266	-0.276**	0.190	528
		[0.140]			[0.142]		[0.129]	[0.181]	
Cited among 3 trusted sources of COVID information:									
Pres Bolsonaro	0.27	0.0149	262	0.00	0.0517**	266	0.0647**	-0.0704	528
		[0.0600]			[0.0216]		[0.0260]	[0.0588]	
Health experts	0.67	0.0121	262	0.78	0.0208	266	-0.0099	0.0029	528
		[0.0686]			[0.0641]		[0.0579]	[0.0846]	
Trust Bolsonaro above health experts	0.22	-0.0650	202	0.00	0.0540**	211	0.0509*	-0.111*	413
		[0.0668]			[0.0254]		[0.0286]	[0.0641]	
Estimated probability 'X' dies or is hospitalised if contracts COVID-19: X="someone like me"	14.79	3.713	262	31.85	1.512	266	1.875	3.505	528
		[3.338]			[4.447]		[4.081]	[5.061]	
X=30-year old woman	12.58	0.430	262	24.25	2.335	266	2.784	-1.042	528
		[2.747]			[3.577]		[3.406]	[4.295]	
X=65-year old man	31.04	-1.105	262	47.35	1.515	266	1.057	0.462	528
		[3.864]			[4.686]		[4.287]	[5.683]	
Advocacy for a person to 'stay at home' (instead of working) if:									
The person has COVID symptoms	0.78	0.027	262	0.88	-0.0138	266	-0.0416	0.0892	528
		[0.0620]			[0.0501]		[0.0474]	[0.0726]	
Someone else in household has COVID symptoms	0.38	0.137*	262	0.55	-0.0643	266	-0.0882	0.258***	528
		[0.0698]			[0.0780]		[0.0686]	[0.0927]	

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 (the AE income eligibility cut-off) before the pandemic. The estimates for  $\phi_1$  and  $\phi_2$  from Eq. (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.

		DV average, AE installment not within last 14 days	[ 1 ]		[ 2 ]		[ 3 ]	
			Received AE installment within last 14 days					
			Coefficient	n	Coefficient	n	Coefficient	n
Outcomes:								
[1]	COVID quiz	-0.413	0.388**	178	0.385**	159	0.443**	156
			[0.156]		[0.184]		[0.191]	
Cited among 3 trusted sources of COVID information:								
[2]	Pres Bolsonaro	0.154	-0.0719	178	-0.0970*	159	-0.101	156
			[0.0487]		[0.0578]		[0.0608]	
[3]	Health experts	0.675	0.128*	178	0.0817	159	0.0668	156
			[0.0672]		[0.0886]		[0.0952]	
[4]	Trust Bolsonaro above health experts	0.133	-0.114***	143	-0.109**	128	-0.108*	125
			[0.0407]		[0.0528]		[0.0567]	
Estimated probability 'X' dies or is hospitalised if contracts COVID-19:								
[5]	X="someone like me"	29.62	-0.985	178	1.748	159	-0.887	156
			[4.736]		[4.864]		[4.847]	
[6]	X=30-year old woman	22.55	2.256	178	4.770	159	4.491	156
			[4.498]		[4.480]		[4.547]	
[7]	X=65-year old man	38.05	4.178	178	5.021	159	3.250	156
			[4.967]		[5.485]		[5.729]	
Advocacy for a person to 'stay at home' (instead of working) if:								
[8]	The person has COVID symptoms	0.752	0.0184	178	0.0291	159	0.0342	156
			[0.0674]		[0.0778]		[0.0838]	
[9]	Someone else in household has COVID symptoms	0.444	-0.0510	178	-0.0598	159	-0.0488	156
			[0.0780]		[0.0907]		[0.0985]	
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 Único, 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. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

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; Londoño Vélez and Querubín, 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)<sup>23</sup>, 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 [Londoño-Vélez and Querubín \(2022\)](#) 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 [Londoño-Vélez and Querubín](#) 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 relatively 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.<sup>24</sup>

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](#)) or in Brazil ([Ribas and Soares, 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 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.

## Data availability

The data, code and survey (in English and in Portuguese) are in the Kent Data Repository at University of Kent: <https://doi.org/10.22024/UniKent/01.01.468>

<sup>23</sup> [https://www.gov.br/economia/pt-br/centrais-de-conteudo/publicacoes/notas-informativas/2020/nota\\_informativa\\_covid19\\_final.pdf](https://www.gov.br/economia/pt-br/centrais-de-conteudo/publicacoes/notas-informativas/2020/nota_informativa_covid19_final.pdf)

<sup>24</sup> If 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.

## Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.jebo.2023.01.006](https://doi.org/10.1016/j.jebo.2023.01.006).

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