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CAN CASH TRANSFERS INCREASE SOCIAL DISTANCING DURING COVID-19? EVIDENCE FROM COLOMBIA

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Abstract

Governments around the world have used cash transfers to reduce human mobility during the current coronavirus pandemic, yet little is known about the effectiveness of this policy. We evaluate the impact of a large unconditional cash transfer program in Colombia using geolocated cellphone data and administrative records tracking the number of beneficiaries. We find that the program had an average null effect on mobility across the country but significantly reduced it in the largest cities. We test for different explanations and conclude that mobility responds relatively more to cash in places with high civic capital.

Keywords: cash transfers, social distancing, pandemic, civic capital.
JEL codes: H23, H53, H84, I31

Mobility data used in this paper were provided by the United Nations Development Program (UNDP) and GRANDATA, under the umbrella of UNDP's call for papers "Exploring impact and response to the COVID-19 pandemic in Latin America and the Caribbean using mobility data", to promote policy-oriented research on the COVID-19 pandemic effects in LAC. Findings, interpretations, and conclusions are from the authors and do not necessarily represent UNDP's views.

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¿PUEDEN LAS TRANSFERENCIAS MONETARIAS AUMENTAR EL DISTANCIAMIENTO SOCIAL DURANTE EL COVID-19? EVIDENCIA DE COLOMBIA[5]

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Resumen

Los gobiernos alrededor del mundo han usado las transferencias de dinero para reducir la movilidad humana durante la actual pandemia de coronavirus, pero sabemos poco sobre la efectividad de esta política. Evaluamos el impacto de un programa grande de transferencias no condicionadas en Colombia, usando datos georeferenciados de teléfonos móviles y registros administrativos que reportan el número de beneficiarios del programa. Encontramos que el programa tuvo un efecto promedio nulo sobre la movilidad en el país, pero la redujo significativamente en las ciudades más grandes. Corroboramos diferentes explicaciones y encontramos que la movilidad responde al efectivo relativamente más en los lugares con mayor capital cívico.

Palabras clave: Transferencias de dinero, distanciamiento social, pandemia, capital cívico.
Clasificación JEL: H23, H53, H84, I31

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1. Introduction

Governments around the world have implemented a wide number of policies to slow down the spread of the current coronavirus pandemic (Haushofer and Metcalf, 2020). Understanding the impact of these policies on infections has been a focus of research in real-time (Hsiang et al., 2020; Flaxman et al., 2020). Yet, many of the mediating factors explaining these impacts remain far from understood. A leading example is cash transfers, delivered both as financial help and as an incentive to decrease human mobility (Gentilini et al., 2020; Cejudo et al., 2020). Recent studies have found that social distancing policies can reduce mobility (Gupta et al., 2020; Asfaw, 2021) and contain the spread of the virus (e.g. Bonaccorsi et al. 2020; Kraemer et al. 2020). However, the impact of cash transfers on mobility remains empirically unclear (Banerjee et al., 2020).[9]

This paper presents one of the first estimates of the causal impact of cash transfers on human mobility patterns using data from a large Latin American country and a quasi-experimental research design. A few weeks after the first coronavirus case, the Colombian government successfully implemented a new unconditional cash transfer of US$43 monthly—18% of the minimum wage—reaching three million low-income households across the country. We use administrative data to track the number of beneficiaries over time, combined with georeferenced cellphone data, to estimate the impact of the cash transfers on mobility.[10] More precisely, we use census tracts as the unit of observation and a difference-in-differences design which leverages pre-pandemic mobility data and the different intensity of the cash transfer program across space and time.

Our main finding is that the roll out of cash transfers had an average null effect on mobility patterns. The average impact, however, masks significant differences across the Colombian territory. In particular, cash transfers appear to be effective in reducing mobility in large and dense urban areas where citizens have more access to the internet, higher levels of education, and more civic capital as measured by their electoral participation. These findings are robust to a battery of alternative specifications and remain similar when we estimate demanding specifications in which we non-parametrically control for the impact of other economic policies (e.g. lockdowns) and the corresponding differential compliance of citizens in different locations (e.g. municipality). To validate the identification assumption in our empirical strategy, we show the existence of similar mobility trends before the pandemic across tracts with different percentage of beneficiaries.

The higher impact of cash transfers in large cities informs a debate composed by at least three arguments. The first of these emphasizes that existing economic conditions such as the prevalence of poverty are able to explain compliance with policies that aim to reduce mobility during a pandemic (Wright et al., 2020). The second argues that partisanship is key to explain the effectiveness of these policies particularly in contexts of high polarization (Allcott et al., 2020). And the third suggests that "beliefs and social values" are the key explanation for the differential impact of these policies (Barrios et al., 2021). Despite the wide disparities in income and partisanship across Colombia, we fail to find evidence supporting

[9] Articles studying the impact of social distancing policies, mobility, and social interactions on the spread of the coronavirus disease include Courtemanche et al. (2020); Glogowsky et al. (2020); Engel et al. (2020); Mangrum and Niekamp (2020); Glaeser et al. (2020); Dave et al. (2020, 2021); Kuchler et al. (2021); Friedson et al. (2021).
[10] The use of cellphone data to track human mobility was pioneered by González et al. (2008). More recent articles include Couture et al. (2021); Peixoto et al. (2020); Pepe et al. (2020); Charoenwong et al. (2020), among others.

these variables as mediators of the findings. In contrast, the results are more consistent with civic capital being relatively more important.

The findings in this paper contribute to the literature studying the impact of cash transfers (Hanna and Olken, 2018; Handa et al., 2020), particularly during the coronavirus pandemic. Previous research studying the current crisis has found that cash transfers are effective in improving the well-being of beneficiaries, but their impact on mobility appears to be less clear (Banerjee et al., 2020; Londoño-Vélez and Querubín, 2021; Asfaw, 2021; Gallego et al., 2021). One limitation of existing studies is the use of self-reported mobility data or the focus on specific types of mobility associated to the workplace. In that sense, the main novelty in our empirical approach is the combination of data from a large cash transfer program with the use of georeferenced cellphone data to infer changes in mobility patterns. The cellphone data is arguably subject to fewer biases than surveys, covers a larger part of the population, and aggregates different types of mobility.

Beyond the pandemic, our results show that the effectiveness of monetary incentives (e.g. subsidies) to correct negative externalities and induce socially beneficial behaviors may be mediated by the moral values and levels of civic capital of individuals and communities. Consequently, in times of crisis, in places with lower levels of civic engagement, subsidies may be insufficient to incentivize compliance with collectively beneficial policies.

2. The new unconditional cash transfer program in Colombia

The first case of COVID-19 in Colombian territory was found on March 6 of 2020. Eleven days later, President Iván Duque appealed to Article 215 of the Constitution and declared a state of economic and social emergency in the country. As a consequence, a mandatory preventive isolation ("quarantine") period began in March 24, restricting human mobility and economic activity. Additionally, the Colombian government implemented a number of protection programs for affected households and companies. Cash transfers were one of the most important policies and aimed to ameliorate the negative consequences associated to restricted mobility, particularly among low-income households. Most cash transfer policies were built on existing social programs (e.g. *Familias en Acción*). Two new unconditional cash transfer programs were also introduced, *Compensación del IVA* and *Ingreso Solidario.*[11]

The Legislative Decree N. 518 of 2020, issued on April 4 of 2020, established the guidelines for the creation of *Ingreso Solidario.* More precisely, this policy was created as "an Unconditional Cash Transfer (UCT) aiming to mitigate the impacts of the COVID-19 emergency." In terms of the target population, the policy specified that the goal was to reach "low-income population without access to financial aid from other social programs" (DNP, 2020). The cash transfer was designed to be COP$ 160,000 per month, approximately US$ 43 or 18% of the minimum wage. The government estimated that these transfers would reach about 2.6 million low-income households.

To identify the de jure beneficiaries of the program the government used administrative data in three steps. In the first step, the National Planning Department (DNP for its acronym in

[11] Londoño-Vélez and Querubín (2021) and Gallego et al. (2021) evaluate the impacts on welfare of Compensación del IVA and *Ingreso Solidario*, respectively.

Spanish) built a master database of households from administrative records. Information about households came from the System for the Identification of Potential Beneficiaries, versions III and IV. This information allowed the government to identify and rank all households in the country by their income and vulnerability. In the second step, these data was used to identify households without access to other social programs and thus construct the target population. In the third step, the government used information from a bank-based program to divide the group of potential beneficiaries in two: banked and unbanked households. The latter distinction was important because the mechanism to deliver transfers and their timing was different for both groups.

The cash transfers were rolled out in three phases. The target population with an active bank account received the first transfer on April 7. Households without a bank account had access to the transfer a couple of weeks later, from April 22 onwards. In addition to these two initial phases, a third phase was implemented to reach the target population who for some reason did not have access to the transfers in the first two dates. There were 1.2 million beneficiaries in the first phase, 1.5 million in the second, and 360 thousand in the last one. The program has been progressively extended to the present.

3. Mobility data and beneficiaries

We construct a dataset of census tracts located in 1,068 municipalities of Colombia.[12] To ensure that our analysis focuses on households, we use information from the housing census and omit all tracts without households (e.g. commercial zones). After this restriction, we observe a total of 4,923 census tracts weekly, five weeks before and 21 weeks after the beginning of the cash transfer program we examine. To track the mobility of individuals in these areas, we use a mobility index derived from smartphone data. This index is particularly useful to measure the mobility patterns of individuals in urban areas (González et al., 2008). We infer the exposure of census tracts to the program using administrative data. We now describe each one of these variables.

1.1. Cellphone data and the mobility index

We use a mobility index that relies on smartphone data collected and processed by GRANDATA, a private company which uses anonymized first-party data to understand market and consumer dynamics. The smartphone data has been collected since the first week of March of 2020 and we analyze until the last week of August of that year.[13] It contains geolocated events which are derived from the use of smartphones. The data is at the user-level and the user's most frequent location is defined as his or her place of residence. All remaining geolocated events are then categorized as "out-of-home" events. The mobility index is simply the number of out-of-home events in a given geographic unit. We observe the index by census tract, the smallest administrative unit with mobility data, and collapse the daily data to the week level.

[12] There are a total of 1,123 municipalities in Colombia, but the mobility data is only available for 1,068 of these.
[13] As of September 1, the general lockdown in Colombia was ended, and therefore the mobility restrictions were widely relaxed.

This index measures the level of human mobility relative to the first week of March 2020, i.e. before the arrival of the pandemic to Colombia. More precisely, a value of zero in the index in week t means that human mobility was the same in week t than before the pandemic started. In contrast, a value of one means that there was a 100% increase in mobility. Negative values indicate decreases in mobility patterns with respect to the benchmark. To facilitate the interpretation of the analysis that follows, we multiply this indicator by 100. Additionally, we winsorize the index at the 99% level due to the existence of outliers in the right-hand side of the distribution. Table A.1 presents descriptive statistics of this variable, both before and after winsorization.

A novelty in our empirical analysis is the focus on municipality-level characteristics in driving the impact of the unconditional cash transfer program. Following Torres and Caicedo (2015), we classify a municipality as large if its population is greater than 100,000 inhabitants. This distinction is important because dense urban areas have been the most affected by the pandemic and it is in these locations where changes in urban mobility are expected to have a greater effect.

Figure 1 presents the evolution of the mobility index in three groups: all census tracts (panel A), census tracts in large municipalities (panel B), and census tracts in non-large municipalities (panel C). The x-axis measures the week since the beginning of the cash transfer program and the y-axis the mobility index. Panel A shows that after the arrival of the pandemic to Colombia there was a large drop in mobility followed by a gradual recovery. However, panels B and C show significant differences depending on the size of municipalities. These figures reveal that the fall in mobility is large and persistent in large and dense urban areas but never falls in other locations, suggesting that the impact of the pandemic and the response of citizens might be mediated by urban density and mobility patterns in large cities. We further analyze this pattern below.

1.2. The beneficiaries of cash transfers and covariates

We identify the beneficiaries of the program using administrative data from the National Planning Department. The data also allows us to geolocate beneficiaries in a census tract and construct the time-varying proportion of the census tract population that benefits from the program. On average, we calculate that 8-9 percent of people in a census tract benefitted from the program, with some census tracts having zero beneficiaries and some having 20% of households (see Table A.1).

We also use pre-pandemic data to characterize census tracts. In particular, we observe a multidimensional poverty index and a pandemic vulnerability index—information recently published by the National Administrative Department of Statistics—and the following characteristics from the 2018 population census: urbanization rates, geographic size, poverty rates, proportion of beneficiaries from other social programs, proportion of the population with access to the internet, and years of education. Finally, we also observe the percentage of people working in the formal sector of the economy and electoral data which we use to construct measures of ideology and civic capital. Table A.1 reports descriptive statistics for these additional variables.

4. Empirical strategy

To evaluate the impact of the cash transfer program on mobility patterns during the pandemic, we use a difference-in-differences design. In particular, we compare the mobility patterns of census tracts before and after the rollout of the program. Census tracts are differentially exposed to the cash transfers because of the different number of beneficiaries that live within them. Econometrically, we exploit the different exposure by estimating regression equations of the following form:

$$Mobility_{smdt} = \alpha_s + \alpha_{dt} + \beta Beneficiaries_{st} + \sum_{c \in X_s} \lambda'(c * \gamma_t) + \varepsilon_{smdt} \quad (1)$$

where "$Mobility_{smdt}$" is the mobility index of census tract *s*, located in municipality *m* of department *d*, and observed during week *t*. We always include fixed effects by census tract (α_s) and department-week (α_{dt}).[14] The treatment variable is denoted by "$Beneficiaries_{st}$" and measures the proportion of the population in census tract *s* benefitting from the program in week *t*. Recall that the program was implemented in three phases and thus the proportion of beneficiaries in the post-treatment period changes over time. The vector X_s denotes characteristics of census tracts before the beginning of the pandemic. The baseline specification includes population, geographic size, poverty levels, and the proportion of beneficiaries from other social programs.[15] Additional controls include measures of internet access, education, and vulnerability to a pandemic.[16] We interact these time-invariant characteristics with week fixed effects γ_t to account for predetermined trends in mobility patterns. Finally, ε_{smdt} is an error term clustered at the census tract level.

The coefficient of interest in equation (1) is denoted by β and captures the differential mobility in census tracts with a large share of beneficiaries. If $\beta < 0$, then we say that the program was successful in decreasing mobility. The identification of the causal impact of the program relies on the assumption of parallel trends across census tracts with varying degrees of exposure in the absence of the cash transfer program. Although essentially non-testable, we provide some empirical evidence that suggests this identification assumption is reasonable in our context. In particular, we test for differential trends *before* the rollout using the following dynamic model:

$$Mobility_{smdt} = \alpha_s + \alpha_{dt} + \sum_{j \in J} \beta_j (Beneficiaries_{st} * \gamma_t) + \sum_{c \in X_s} \lambda'(c * \gamma_t) + \varepsilon_{smdt} \quad (2)$$

where *J* includes all weeks in our sample except the week before the beginning of the program. In this case, the parameters are β_j and can be interpreted as the differential change in mobility in areas with a high proportion of beneficiaries—relative to areas with a low proportion—in week *j* using the beginning of the program as the benchmark. To test for parallel trends *before* the beginning of the program, we use the proportion of beneficiaries during the first week as a proxy for potential exposure before the rollout. We say that the

[14] The inclusion of department-week fixed effects allows us to compare census tracts within the same *department* over time. We also compare census tracts within the same *municipality* over time and reach similar conclusions.
[15] Controlling for access to other social programs, such as Familias en Accio´n or Comensación del IVA, is important given that, as part of the emergency response, there were additional transfers from these programs. In the robustness exercises, we also exclude from the analysis the municipalities that had other large cash transfer programs amid the pandemic, such as Bogota or Medellin.
[16] Controlling for both poverty and internet access is key given that GRANDATA measures mobility for people with internet access from their mobile phones. Naturally, having this type of telephone depends on household income, despite the fact that its prevalence is high in Colombia. In addition, according to the DANE's Quality of Life Survey, 86% of those who reported having access to the internet in 2019 said they did so from their mobile phones.

identification assumption is likely to hold if we observe parallel trends across areas differentially exposed before the implementation of the program.

The last part of our analysis tests for the role of municipal-level characteristics in driving the impact of the program. We use an augmented econometric model of the following form:

$$Mobility_{smdt} = \alpha_s + \alpha_{dt} + \beta_1 Benef_{st} + \beta_2 (Size_m * Benef_{sf}) + \sum_{c \in X_s} \lambda'(c * \gamma_t) + \varepsilon_{smdt} \quad (3)$$

where $Size_m$ is one of two measures of the municipality size: the natural logarithm of its population, or an indicator which takes the value of one in municipalities with more than 100,000 inhabitants. We use the same specification in equation (3) to test for potential mechanisms but replace the size variable by other predetermined characteristics of census tracts or municipalities.

5. The impact of cash transfers on mobility patterns

We present results in three parts. First, we find that the cash transfers had an average null effect on mobility patterns but a negative impact in large urban areas. Second, we show that these results constitute robust findings and present evidence to validate the assumptions in the empirical strategy. Third, we show that the reduced mobility from cash transfers in large urban areas appears to be mediated mainly by civic capital, while income and partisanship play no important role.

1.3. Changes in mobility and the importance of large urban areas

Table 1 presents estimates of equation (1). In all columns the dependent variable is the percentage change in mobility with respect to the week before the pandemic reached Colombia. We present results from four different specifications. The first uses tract and week fixed effects in addition to an interaction between baseline covariates and week fixed effects. The second adds the following pre-determined covariates—measured at the census tract level—interacted with week fixed effects: percentage of the population with a college degree, percentage of the population with access to the internet, and a pandemic vulnerability index.[17] All of these variables come from administrative records, and we call them "additional covariates." The third specification builds on the first one by adding department-week fixed effects, which effectively control for any policies or trends in any of the 33 departments in the country. The last specification is the most demanding one and includes municipality fixed effects interacted with an indicator for the period with active cash transfers. The latter controls for any policies that municipalities might have implemented during this period (e.g. lockdowns) and the corresponding differential compliance of citizens to these policies.

The results in columns 1-4 of Table 1 show that the cash transfers had an average null effect on mobility patterns when we look at all tracts in the country. Although mobility was lower everywhere after the pandemic, these columns suggest that the decrease was not larger in locations with more beneficiaries. Only column 2 suggests that the transfers reduced mobility but the estimate is small and not different from zero. Column 4 even suggests that,

[17] Access to the internet also indirectly controls for any measurement error in the dependent variable that might arise as a consequence of a low percentage of connections in the census tract. The pandemic vulnerability index was built from the prevalence of comorbidities associated with complications from COVID-19 and measures of poverty.

counterintuitively, mobility *increased* in tracts with more beneficiaries after the roll out of the transfers. In any case, the estimates in all these columns are small in magnitude. To see their economic size more easily, consider that the average tract had 9% of beneficiaries. Therefore, when the share of beneficiaries doubled, we observe changes in mobility that range from -0.6% (column 2) to 2% (column 4).

Motivated by the descriptive evidence in Figure 1, which suggested important differences between the largest municipalities and the rest of the country, we study a differential response of mobility mediated by the size of municipalities. Columns 5 through 8 of Table 1 report estimates of equation (3) using the logarithm of population interacted with the share of beneficiaries. Columns 9 through 12 repeat this empirical exercise but now using an indicator that takes the value of one for municipalities with more than 100,000 inhabitants, which includes the largest cities. In all these columns we find that the cash transfers are strongly associated to a larger decrease in mobility in large urban areas. For example, column 5 shows that an increase of one percent in the population of the municipality implies a 0.26% decrease in the marginal effect of the share of beneficiaries on mobility. Similarly, column 9 shows census tracts in major cities experienced a reduction of 0.95% in the marginal effect of cash transfers on mobility.

All in all, the evidence in Table 1 shows that the program had an average null effect on mobility patterns in all census tracts in the country but a negative impact on large urban areas where cities are located. This result is important because it is in large and dense cities where diseases are more likely to spread.[18] We now study the robustness of the results and the potential mechanisms to explain them.

Table 1. The impact of cash transfers on mobility patterns

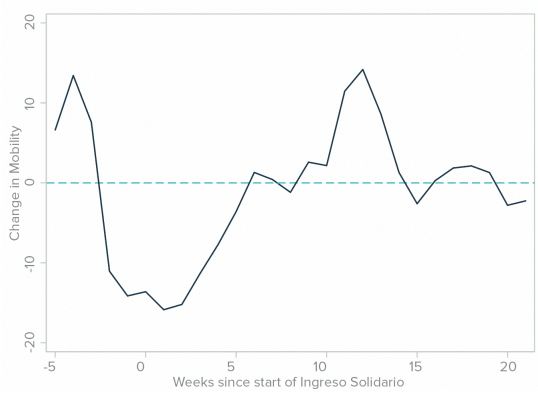
	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Share of beneficiaries (%)	0.00	-0.07	0.04	0.22	3.04***	1.90***	3.20***	2.41***	0.74***	0.35	0.79***	0.93***
	(0.20)	(0.20)	(0.24)	(0.20)	(0.41)	(0.53)	(0.49)	(0.81)	(0.21)	(0.26)	(0.25)	(0.33)
X Log population in municipality					-0.26***	-0.17***	-0.25***	-0.17***				
					(0.03)	(0.04)	(0.03)	(0.06)				
X Indicator municipality with > 100,000 inhab.									-0.95***	-0.55***	-0.90***	-0.79***
									(0.13)	(0.16)	(0.14)	(0.28)
Observations	104,636	104,636	104,620	104,636	104,636	104,636	104,620	104,636	104,636	104,636	104,620	104,636
Census tract fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X	X	X	X	X	X	X
Additional covariates X t		X				X				X		
Department-week fixed effects			X				X				X	
Municipality-after fixed effects				X				X				X
Average dep. variable	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from the main specification in equation (1) and the heterogeneous effects specification in equation (3). The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of *Ingreso Solidario*. Log Population is the natural logarithm of the municipality's population. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level. Source: Authors' calculations.

[18] Moreover, Figure A.4 and Table A.9 in the appendix show that there are non-linear effects, since the marginal effect of exposure to the program grows with the proportion of beneficiaries in the census tract.

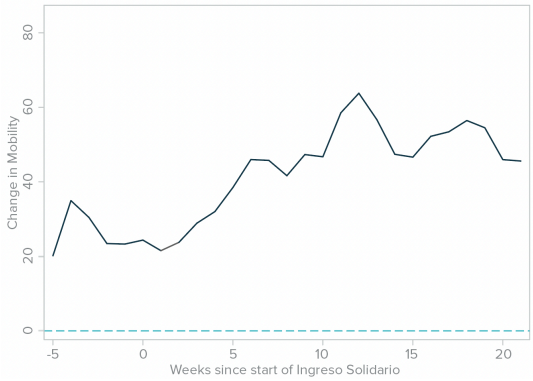
Figure 1. Mobility patterns before and after the cash transfers

A. All census tracts



B. Large municipalities

C. Non-large municipalities



Notes: Panels (a), (b), and (c) present the evolution of the (winsorized) mobility score for the full sample of urban sectors (Panel A), urban sectors in large and intermediate cities according to the 100,000 inhabitants' threshold (Panel B), and in the rest of the country. Mobility is measured as the percentage point change with respect to the benchmark date, March 2, 2020. Week 0 corresponds to the start of the Ingreso Solidario program. Week 0 corresponds to the start of the Ingreso Solidario program.

Source: Authors' calculations.

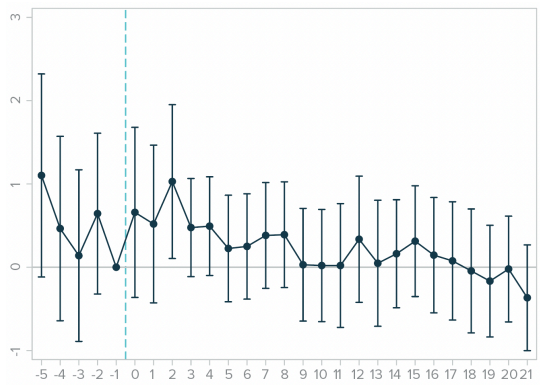
1.4. Robustness exercises and validity checks

We first test for the validity of our identification strategy. Locations with more beneficiaries had similar mobility trends than locations with fewer beneficiaries before the pandemic. This empirical finding is crucial for our empirical strategy because the causal interpretation of estimates rests on the assumption of parallel trends across tracts differentially exposed in the absence of the pandemic. To test for differential trends across census tracts we estimate the semi-parametric equation (2). Panel A in Figure 2 presents the results and reveals that trends were similar in the four weeks before the pandemic. This is, the share of beneficiaries has little predictive power of mobility trends during times without the cash transfers we examine. The *p*-value for the *F*-test of joint significance of these coefficients is 0.29. Overall, we interpret this as evidence supporting the identification assumption in our empirical strategy. The two remaining panels in the same figure present the same estimates for large municipalities using population (panel B) and the indicator for cities with more than 100,000 inhabitants (panel C). The results again reveal similar trends across census tracts differentially

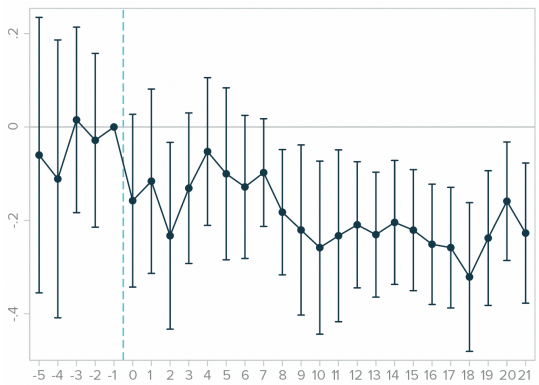
exposed to the program and hence support our identification assumption. The corresponding *p*-values for the *F*-tests of joint significance are 0.51 and 0.73 respectively. We can also see that the average decrease in mobility after the transfers appears to be immediate, persistent, and not driven by any particular week.

Figure 2. Changes in mobility patterns after the cash transfers

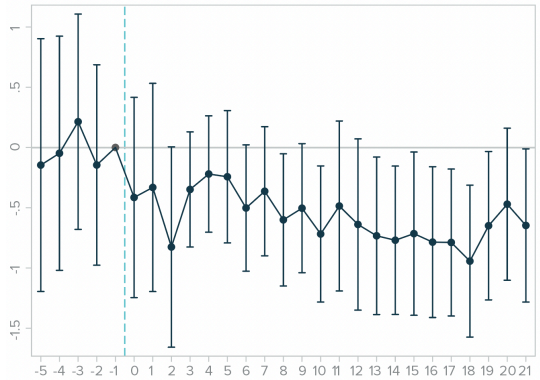
A. All census tracts



B. Interaction with population



C. Large urban areas



Notes: The y-axis measures changes in mobility patters with respect to the benchmark date, March 2, 2020. Week 0 corresponds to the start of the *Ingreso Solidario* program. Panels (a), (b), and (c) present the coefficients from our dynamic specification presented in equation (2) and for the heterogeneous treatment effects models. Panel A presents the results for the main specification with the full sample of urban sectors, Panel B for the heterogeneous effects model that uses the municipality population (in logs), and Panel C for the model that uses the major city indicator that considers a municipality intermediate or large if its population is greater than 100,000 inhabitants. We resent the point estimates of the regressions and the confidence of interval at the 95% level. Source: Authors' calculations.

In addition to the four specifications reported in Table 1, the results are also robust to other econometric models and definitions, each of which assesses potential confounders. Columns 1-3 in Table 2 exclude municipalities which were heavily affected by other transfer programs during the pandemic—i.e. Bogota, Medellin, Bucaramanga, Jamundí, and Tunja. Reassuringly, the null average impact and the importance of population remain robust results. In columns 4-6 we follow Bertrand et al. (2004) and collapsed the data to a period before and a period after the beginning of the program to deal with potential serial correlation in the dependent variable and the results remain very similar. In columns 7 and 8 and Figure A.1 we use an indicator for the 23 major cities—a set of cities chosen by the state to study labor

market trends—and results are again the same.19 The results are also similar if we exclude each one of the 33 departments, thus results are not driven by any particular region (Figure A.2). Similarly, we winsorized the dependent variable at the 98, 97, and 96% levels and find the same results (Table A.2).

Table 2. Robustness exercises

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)							
	Exclusion of cities with other major transfers			Collapse pre/post program (Bertrand et al. 2004)			Alternative measure of city size	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of beneficiaries (\%)	-0.31	3.29***	0.44*	-0.02	2.30***	0.67***	0.36*	0.50**
	(0.24)	(0.59)	(0.26)	(0.21)	(0.41)	(0.22)	(0.20)	(0.24)
X Log population in municipality		-0.29***			-0.25***			
		(0.04)			(0.03)			
X Indicator municipality > 100,000 inhab.			-0.86***			-0.91***		
			(0.14)			(0.12)		
X Indicator 23 major municipalities							-0.77***	-0.33**
							(0.12)	(0.14)
Observations	83,501	83,501	83,501	7,760	7,760	7,760	104,636	104,636
Census tract fixed effects	X	X	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X	X	X
Additional covariates X t								X
Average dep. variable	6.48	6.48	6.48	-0.38	-0.38	-0.38	-0.97	-0.97

Notes: This table presents the results from three different types of robustness tests. Columns 1-3 correspond to models in which the cities of Bogota, Medellin, Bucaramanga, Jamundi, and Tunja are excluded. Columns 4-6 collapse the data to pre and post *Ingreso Solidario* periods. Columns 7 and 8 report the results from heterogeneous effects models that use as alternative measure of city size an indicator of whether the municipality is one of the most important cities according to the DANE classification. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of *Ingreso Solidario*. Log Population is the natural logarithm of the municipality's population. Major City indicates if the municipality's population is greater than 100,000. 23 Major City indicates if the municipality in one of the 23 major cities according to DANE's classification. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Recent literature shows that in the presence of a staggered adoption of the treatment and heterogeneous effects over time and across units, estimates based on two-way fixed effects (TWFE) models may be biased (see Roth et al. (2022) and de Chaisemartin and D'Haultfoeuille (2022) for recent reviews). Different alternatives have been proposed to deal with this problem. To corroborate the robustness of our results to this type of designs, we follow the approaches of Callaway and Sant'Anna (2021), Borusyak et al. (2021), and de

[19] The 23 major cities are: Bogotá, Medellín, Cali, Barranquilla, Bucaramanga, Manizales, Pasto, Pereira, Cúcuta, Ibagué, Montería, Cartagena, Villavicencio, Sincelejo, Riohacha, Santa Marta, Valledupar, Florencia, Quibdó, Armenia, Neiva, Popayán, and Tunja. Similar results are encountered if we focus on the 13 most important cities.

Chaisemartin and D'Haultfoeuille (2020). In order to apply these methods, we use a dichotomous version of the treatment variable,20 under which a census tract is considered treated in week t if at least one household was a beneficiary of *Ingreso Solidario*.21 Figure A.3 presents the coefficients of event study models based on these alternative DID estimators. In general, we find no evidence of differential pre-trends across these models, while the effect of the treatment on mobility is null after the implementation of the program. In fact, Table A.3 reports the coefficients for the different specifications, reaffirming the null effect described above.22

Finally, in Table A.4 we use the double machine learning (lasso) methodology proposed by Belloni et al. (2014, 2016). In each model, a lasso estimator is used to select the group of covariates to be included. First, a regression of the outcome on all baseline and additional controls is estimated. Then, we estimate a regression of the treatment on the same group of variables. The final choice of controls is the union of the controls selected in these two regressions. As shown by Belloni et al. (2014), this methodology allows to appropriately select controls and thus improve the robustness of causal inference. Table A.4 shows that our results, both for the average treatment effect and for the heterogeneous effects by city size, are robust to this approach.

1.5. The importance education, the internet, and civic capital

What factors explain the differential impact of transfers on mobility in large and dense urban areas? The answer to this question is crucial to understand the context in which cash transfers could be the most effective to decrease the spread of a pandemic. Although we lack exogenous variation in important variables for a thorough empirical analysis, we provide suggestive evidence of the role of important economic and social mediators. In particular, we look at the role of four tract-level variables—poverty rates, vulnerability to the pandemic, share of the population with a college degree, and share with access to the internet—civic capital measured by the percentage of people in the municipality who voted in the 2018 presidential election (Putnam, 1993),23 and partisanship measured by the percentage of votes for the current incumbent president.

Three leading mediators have been emphasized by scholars: existing economic conditions, civic capital, and partisanship. Low-income areas might continue with their usual mobility patterns if cash transfers are too low to prevent citizens to work in order to meet their basic needs. When basic needs are satisfied and citizens have a sufficiently high level of civic capital—understood as "values and beliefs to pursue socially valuable activities" (Guiso et al., 2011)—they might interpret the transfers as an incentive to stay at home and comply (Barrios et al., 2021).24 The last explanation argues that people comply more with policies if these were implemented by a government of their preference (Allcott et al., 2020). Of course, it is

[20] The literature on DID models with staggered adoption for continuous treatment variables is not sufficiently developed, despite some recent work in that direction (see Callaway et al. (2021)).
[21] These results are robust to other ways of dichotomizing the treatment variable.
[22] In general, given the characteristics of these models and the approach we follow in this paper, it is not possible for us to estimate the heterogeneous effects of city size under these specifications.
[23] "Voting is the ultimate example of an activity that is privately costly but socially useful" (Barrios et al., 2021). The main advantage with respect to other measures of civic capital, is that it can be observed with precision. In our case, survey-based measures are not available at the census tract level.
[24] One example of these beliefs is trust, which has been shown to increase compliance in the context of the coron avirus pandemic (Brodeur et al., 2021; Chan et al., 2020).

possible that all explanations are empirically relevant at the same time and in that sense their relative importance is an open question.

The low-income explanation implies that places with high poverty or vulnerability should react less to the cash transfers (Wright et al., 2020). Columns 1 and 2 in Table 3 show that the impact of the transfers does not appear to be mediated by the poverty rate or the vulnerability to the pandemic measures. Furthermore, Table A.5 explores two alternative income-related channels. Columns 1 and 2 show that there are no heterogeneous effects at the level of labor formality, measured as the proportion of the population in the census tract that contributes to the social security system (available through administrative data, i.e. PILA). Analyzing this channel is important because the pandemic affected differentially formal and informal workers. Second, based on the occupations of household members, we construct a measure of exposure to the economic shock caused by COVID-19.25 Columns 3 and 4 show that the effect of transfers is not mediated by this measure either.

The civic capital mechanism implies that places with more electoral participation should react relatively more. The sign of coefficients in columns 3, 7, and 9 in Table 3 suggest that the transfers are able to reduce mobility in places where people have higher educational levels, more access to the internet, and higher levels of civic engagement as measured by their electoral participation.18 However, the effects of education and internet access are not robust to the inclusion of additional covariates, while the impact of civic capital remains significant. The last two columns in this table reveal that municipalities which supported the incumbent government in the past election display similar levels of response to the transfers than other places, suggesting a limited role for political partisanship. The latter results are in stark contrast with evidence from the United States which suggests partisanship played an important role during the administration of President Donald J. Trump (Allcott et al., 2020; Gadarian et al., 2020). In sum, the evidence in this table suggests that civic capital is likely to be more important as a mechanism than income and partisanship to explain the heterogeneous impact of cash transfers on social distancing.

Other changes in the labor market following the pandemic could also be important as mechanisms driving the impact of the cash transfers. In terms of data, we use administrative data to calculate the share of formal workers in the primary, secondary and tertiary sectors. According to official statistics, the secondary sector was the one most affected by the pandemic with a contraction of 17.5% in production during the first quarter after the arrival of the pandemic. The corresponding contractions in the primary and tertiary sectors were 9.8 and 10.3%. Table A.7 tests for the role of the composition of formal employment. Columns 3-4 reveal that only the secondary sector appears to be statistically important. This sector is mainly composed by industry and construction jobs, precisely the workers with fewer job alternatives because of mobility restrictions.

Which of all the mechanisms we tested are relatively more important? Table A.8 evaluates all relevant mediators jointly by estimating a full model with all variables as interactions. Civic capital remains the most relevant empirical factor in driving the differential response to cash transfers. This result shows that civic capital is not only important to understand why some people comply more than others with social distancing mandates during the pandemic (Barrios et al., 2021; Durante et al., 2021), but it also mediates the effect that monetary incentives have to promote collectively beneficial behavior.

[25] We note that all of these factors are positively correlated between themselves and are relatively more common in large urban areas. See the correlation analysis in Table A.6 for details.

Table 3. The role of socioeconomic variables as mediators

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Share of beneficiaries (%)	0.04	-0.20	0.02	-0.06	-0.48***	-0.169	-0.58***	-0.36**	-0.02	-0.13	-0.03	-0.06
	(0.18)	(0.16)	(0.19)	(0.20)	(0.17)	(0.16)	(0.17)	(0.16)	(0.19)	(0.19)	(0.20)	(0.20)
X Poverty rate [ct]	-0.08	0.18										
	(0.19)	(0.20)										
X COVID vulnerability [ct]			0.15	0.11								
			(0.10)	(0.09)								
X College degree [ct]					-0.30**	-0.15						
					(0.13)	(0.13)						
X Internet access [ct]							-0.31**	-0.25				
							(0.15)	(0.16)				
X Civic capital [m]									-0.21***	-0.20***		
									(0.08)	(0.08)		
X Voted incumbent [m]											0.11*	-0.03
											(0.06)	(0.06)
Observations	104,636	104,636	104,620	104,636	104,636	104,636	104,620	104,636	104,636	104,636	104,620	104,636
Census tract fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X	X	X	X	X	X	X
Additional covariates X t		X		X		X		X		X		X
Average dep. variable	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from heterogeneous effects models. [ct] denotes variables measured at the census-tract level and [m] variables measured at the municipality level. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of *Ingreso Solidario*. Poverty rate is the proportion of the population in the urban sector with multidimensional poverty (IPM) in 2018. COVID Vulnerability is the proportion of the population in the urban sector highly vulnerable to COVID-19 according to DANE's index. College degree is the proportion of the population in the urban sector with a college degree in 2018. Internet access is the proportion of the population in the urban sector with internet access in 2018. Civic capital is municipality level voter turnout in the first round of the 2018 presidential election. Voted incumbent is municipality level vote share for Iva´n Duque in the first round of the 2018 presidential election. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

6. Conclusion

We have shown that the impact of cash transfers on human mobility patterns depends on contextual factors that govern the level of compliance to policies that aim to decrease the spread of a disease during a pandemic. Colombia offered us a unique opportunity to measure both the roll out of a large cash transfer program combined with mobility data derived from cellphone information. The findings in this paper emphasize that, at least in the case of Colombia, the response of social distancing to the intensity of the program depends on the civic capital of the population and less so on the baseline economic conditions and the level of support for the current incumbent government. From a policy perspective, the lack of an impact of cash transfers in small locations suggest that local governments in these areas might need to diversify their strategies to fight negative externalities through monetary incentives, as the effects may be mediated by civic capital.

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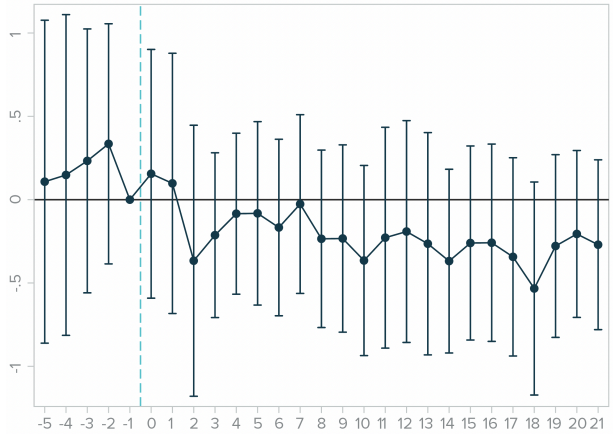
Appendix

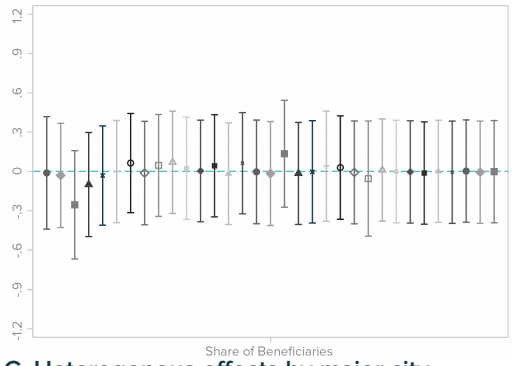
Figure A.1. Alternative measure of large urban areas



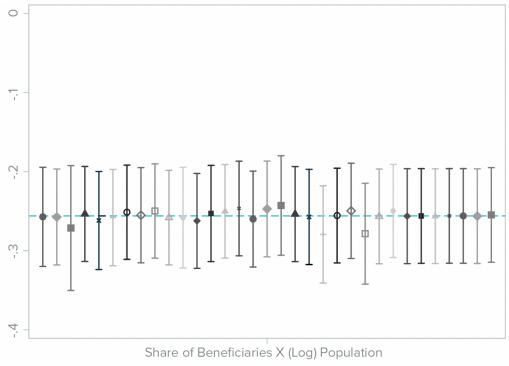
Notes: This figure presents the coefficients from our dynamic specification presented in equation (2) and for the heterogeneous treatment effects model that uses an alternative measure of city size. A municipality is considered a major city if it is one of the 23 most important cities according to the classification used by DANE in its labor market studies. We present the point estimates of the regressions and the confidence of interval at the 95% level. Source: Authors' calculations.

Figure A.2. Robustness to the exclusion of single departments

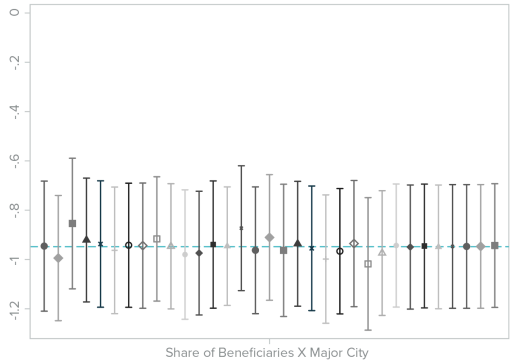
A. Main effects



B. Heterogeneous effects by population size



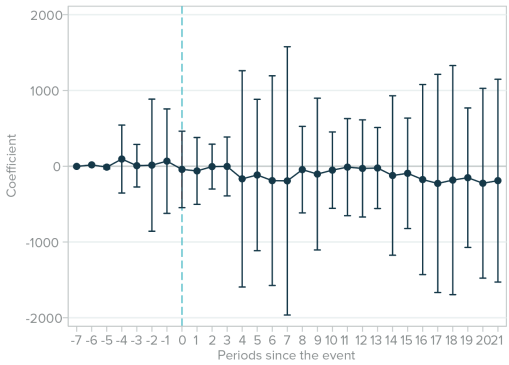
C. Heterogenous effects by major city



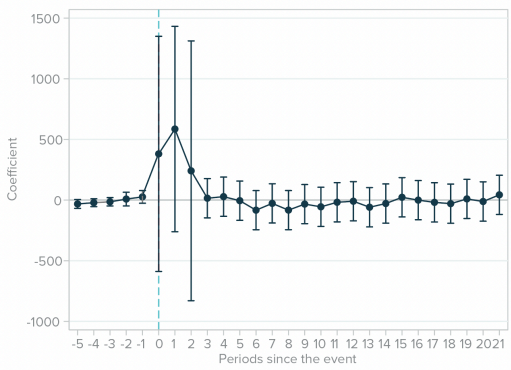
Notes: These figures report the coefficients of models based on (1) and (3), in which one of the 33 departments in Colombia are excluded at a time. Panel A shows the results for the main treatment effect of the share of beneficiaries on mobility. Panel B depicts the heterogeneous treatment effect by population size. Panel C the heterogenous treatment effect by the major city indicator. In all panels, the dashed horizontal line represents the estimated coefficient from the corresponding unrestricted model. We present the point estimates of the regressions and the confidence of interval at the 95% level. Source: Authors' calculations.

Figure A.3. Robustness to alternative staggered DID estimators

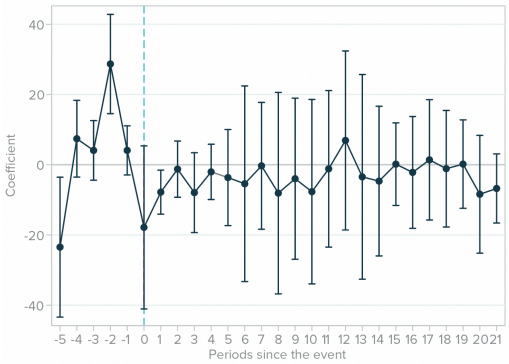
A. Callaway and Sant'Anna (2021).



B. Borusyak et al. (2021)

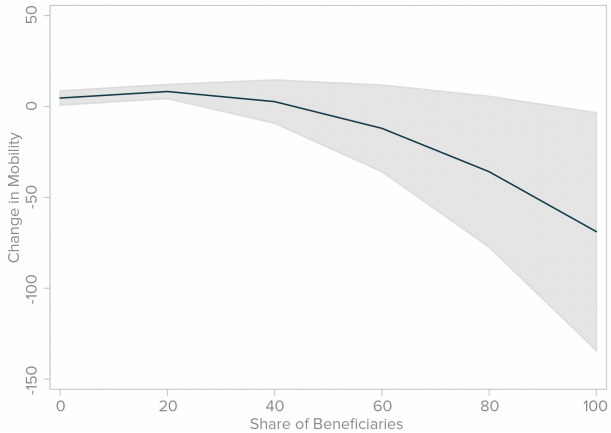


C. De Chaisemartin and D'Haultfoeuille (2020)



Notes: These figures report the coefficients of event study models based on alternative DID estimators that account for the staggered adoption of the treatment. Panel A presents the overall ATT following Callaway and Sant'Anna (2021). Panel B the imputation approach proposed by Borusyak et al. (2021). Panel C the model proposed by de Chaisemartin and D'Haultfoeuille (2020). In all models the treatment variable is a dummy indicating if at week t, the census tract had at least one beneficiary of Ingreso Solidario. We present the point estimates of the regressions and the confidence of interval at the 95% level. Standard errors are clustered and the census tract level. Source: Authors' calculations.

Figure A.4. Non-linear effects of *Ingreso Solidario*



Notes: This figure presents the results from a marginal effects estimation of a quadratic model of mobility on the share of beneficiaries. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Share of Beneficiares2 is the square of Share of Beneficiaries. Standard errors are clustered at the urban sector level. We present the point estimates of the regressions and the confidence of interval at the 95% level. Source: Authors' calculations.

Table A.1. Descriptive statistics

	Mean	Median	St. Dev.	p10	p90
	(1)	(2)	(3)	(4)	(5)
Panel A: Main variables					
Change in mobility	16.43	-21.59	380.05	-63.01	52
Change in mobility (winsorized)	-0.97	-21.59	101.77	-63.01	52
Share of beneficiaries (%)	8.51	7.63	7.81	0	18.69
Log municipality population	12.46	12.63	2.20	9.33	15.86
Indicator large municipality	0.67	1	0.47	0	1
Population	6,875	4,561	7,672	856	15,274
Panel B: Other variables					
Poverty index	18.9	15.03	14.65	3.71	39.9
Beneficiaries other programs (%)	20.13	19.85	15.04	2.25	34.66
Internet access (%)	0.41	0.42	0.26	0.08	0.76
College education (%)	0.18	0.14	0.12	0.04	0.37
Pandemic vulnerability index	20.8	14.42	22.3	0	53.4
Voted incumbent government (%)	0.40	0.37	0.14	0.26	0.6
Electoral participation (%)	0.55	0.55	0.09	0.42	0.65
Census tracts	4,923				

Notes: This table presents descriptive statistics for the variables used in the empirical analysis in the paper. Large municipalities are defined as those with more than 100,000 inhabitants.

Source: Authors' calculations.

Table A.2. Robustness to different winsorization thresholds

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)								
	98% Winsorization			97% Winsorization			96% Winsorization		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of beneficiaries (%)	0.13	2.44***	0.73***	0.18	2.16***	0.72***	0.21*	1.95***	0.69***
	(0.15)	(0.30)	(0.15)	(0.13)	(0.26)	(0.13)	(0.11)	(0.23)	(0.12)
X Log population in municipality		-0.20***			-0.17***			-0.15***	
		(0.02)			(0.02)			(0.02)	
X Indicator municipality with >100,000 inhab.			-0.78***			-0.68***			-0.62***
			(0.09)			(0.08)			(0.07)
Observations	104,636	104,636	104,636	104,636	104,636	104,636	104,636	104,636	104,636
Census tract fixed effects	X	X	X	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X	X	X	X
Average dep. var.	-6.02	6.02	6.02	-9.04	-9.04	-9.04	-11.03	-11.03	-11.03

Notes: This table presents the results from a robustness exercise in which different thresholds are used upon winsorizing the outcome variable. Columns 1- 3 correspond to models for a 98% threshold, 4-6 for a 97% threshold, while 7-9 for a 96% threshold. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Major City indicates if the municipality's population is greater than 100,000 inhabitants. Log Population is the natural logarithm of the municipality's population. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.3. Alternative staggered DID estimators

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)			
	(1)	(2)	(3)	(4)
Panel A: Callaway and Sant'Anna (2021)				
X Indicator of at least one beneficiary	-12.52	-90.93	-109.59	-114.27
	(61.88)	(314.66)	(399.70)	(414.67)
Panel B: Borusyak et al. (2021)				
X Indicator of at least one beneficiary	0.47	124.14*	39.09	39.84
	(59.02)	(69.38)	(87.82)	(95.21)
Panel C: de Chaisemartin and D'Haultfoeuille (2020)				
X Indicator of at least one beneficiary	-16.76	-16.76	-17.85	-16.58
	(11.64)	(11.64)	(11.85)	(10.69)
Baseline covariates	X	X	X	X
Additional covariates		X		
Department fixed effects			X	
Municipality fixed effects				X
Average dep. var.	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from alternative DID models for designs with staggered adoption of the treatment. Panel A presents the overall ATT following Callaway and Sant'Anna (2021). Panel B the imputation approach proposed by Borusyak et al. (2021). Panel C the model proposed by de Chaisemartin and D'Haultfoeuille (2020). The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. The treatment variable is a dummy indicating if at week t, the census tract had at least one beneficiary of Ingreso Solidario. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.4. Robustness to double lasso selection of covariates

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of beneficiaries (%)	-0.17	0.01	-0.16	2.00***	2.30***	2.19***	0.34	0.48*	0.29
	(0.20)	(0.21)	(0.21)	(0.53)	(0.58)	(0.63)	(0.26)	(0.27)	(0.28)
X Log population in municipality				-0.18***	-0.18***	-0.19***			
				(0.04)	(0.04)	(0.04)			
X Indicator municipality with $>$100,000 inhab.							-0.58***	-0.60***	-0.57***
							(0.16)	(0.16)	(0.18)
Observations	104,636	104,620	104,636	104,636	104,620	104,636	104,636	104,620	104,636
Census tract fixed effects	X	X	X	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X	X	X	X
Department-week fixed effects		X			X			X	
Municipality-after fixed effects			X			X			X
Average dep. var.	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from models following the post-double-selection methodology proposed by Belloni 2014, Belloni 2016. In each model, a lasso estimator is used to select the covariates. First, a regression of the outcome on all baseline and additional controls is estimated. Then, we estimate a regression of the treatment on the same group of controls. The final choice of controls is the union of the controls selected in these two regressions. Controls used in the lasso models are urban sector population, size, poverty index, the proportion of beneficiaries from other social programs, internet access, education, and vulnerability to COVID-19. The outcome in all final models is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Major City indicates if the municipality's population is greater than 100,000 inhabitants. Log Population is the natural logarithm of the municipality's population. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.5. Additional economic mediators

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)			
	(1)	(2)	(3)	(4)
Share of beneficiaries (%)	-0.251	-0.251	0.706*	0.672*
	(0.218)	(0.218)	(0.390)	(0.403)
X Share of formal workers	-0.125	-0.125		
	(0.147)	(0.147)		
X Economic shock			-0.280	-0.275
			(0.185)	(0.195)
Observations	101223	101223	101221	101221
Census tract fixed effects	X	X	X	X
Week (t) fixed effects	X	X	X	X
Baseline covariates X t	X	X	X	X
Additional covariates X t		X		X
Average dep. var.	-0.97	-0.97	-0.97	-0.97

Note: This table presents the results from heterogeneous effects models. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Share of formal workers is the proportion of the population in the census tract that contributes to the social security system. Economic shock results from calculating the share of census tract workers in each economic sector, multiplying it by the change in the output in that sector during quarters 2 and 3 of 2020, and aggregating it for all sectors. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.6. Correlation between variables measuring mechanisms

	Poverty Rate	COVID Vulnerability	College Degree	Internet Access	Civic Capital	Voted Incumbent	Primary Sector	Secondary Sector	Tertiary Sector
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Indicator municipality with $>$100,000 inhab.	-14.54***	-4.94***	0.14***	0.36***	0.07***	-0.12***	-0.01***	0.01***	0.06***
	(0.39)	(0.73)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	4,923	4,923	4,923	4,923	4,923	4,923	3,753	3,753	3,753

Notes: This table presents the results from pre-treatment cross-sectional OLS regressions. Robust standard errors are presented in parenthesis. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.7. Primary, secondary, and tertiary sectors

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of beneficiaries (\%)	-0.08	-0.13	-0.13	-0.20	0.21	0.08
	(0.20)	(0.20)	(0.21)	(0.20)	(0.41)	(0.45)
X Share of workers in primary sector	0.05	-0.03				
	(0.11)	(0.10)				
X Share of workers in secondary sector			-0.29***	-0.26***		
			(0.10)	(0.10)		
X Share of workers in tertiary sector					-0.21	-0.41*
					(0.21)	(0.24)
Observations	101,223	101,223	101,223	101,223	101,223	101,223
Census tract fixed effects	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X
Additional covariates X t		X		X		X
Average dep. var.	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from heterogeneous effects models. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Primary, Secondary, and Tertiary Sectors correspond of the share of urban sector workers in each of these economic sectors. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.8. Testing for mechanisms simultaneously

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Share of beneficiaries (\%)	0.35	0.31	-0.01	0.25	0.19	-0.19
	(0.26)	(0.25)	(0.26)	(0.25)	(0.26)	(0.27)
X Indicator municipality t[m] with $>$100,000 inhab.	-0.55***	-0.54***	-0.35*	-0.49***	-0.49***	-0.28
	(0.16)	(0.17)	(0.19)	(0.16)	(0.16)	(0.19)
X College degree t[m]		-0.05				-0.08
		(0.14)				(0.13)
X Internet access t[m]			-0.14			-0.05
			(0.18)			(0.19)
X Civic capital t[m]				-0.17**		-0.15*
				(0.08)		(0.08)
X Share of workers t[m]					-0.20**	-0.08
					(0.10)	(0.10)
Observations	104,636	104,636	104,636	104,636	101,223	101,223
Census tract fixed effects	X	X	X	X	X	X
Week (t) fixed effects	X	X	X	X	X	X
Baseline covariates X t	X	X	X	X	X	X
Additional covariates X t	X	X	X	X	X	X
Average dep. var.	-0.97	-0.97	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from heterogeneous effects models. [ct] denotes variables measured at the census-tract level and [m] variables measured at the municipality level. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Major City indicates if the municipality's population is greater than 100,000 inhabitants. College degree is the proportion of the population in the urban sector with a college degree in 2018. Internet access is the proportion of the population in the urban sector with internet access in 2018. Civic capital is municipality level voter turnout in the first round of the 2018 presidential election. Secondary sector is the share of workers in the secondary sector. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.

Table A.9. Non-linear effects of *Ingreso Solidario*

	Dependent variable: Change in mobility with respect to the pre-pandemic situation (%)			
	(1)	(2)	(3)	(4)
Share of beneficiaries (%)	0.41	0.03	0.41	0.48**
	(0.34)	(0.27)	(0.26)	(0.24)
Share of Beneficiares (squared)	-0.01***	-0.00	-0.01***	-0.01**
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	104,636	104,636	104,620	104,636
Census tract fixed effects	X	X	X	X
Week (t) fixed effects	X	X	X	X
Baseline covariates X t	X	X	X	X
Additional covariates X t		X		
Department-week fixed effects			X	
Municipality-after fixed effects				X
Average dep. var.	-0.97	-0.97	-0.97	-0.97

Notes: This table presents the results from heterogeneous effects models. The outcome in all regressions is the (winsorized) change in mobility compared to March 2, 2020. Share of Beneficiaries is the time-varying number of beneficiaries per capita of Ingreso Solidario. Share of Beneficiares2 is the square of Share of Beneficiaries. Baseline controls include urban sector population, size, poverty index, and the proportion of beneficiaries from other social programs, all of them interacted with week fixed effects. Additional controls include urban sector measures for internet access, education, and vulnerability to COVID-19, also interacted with week fixed effects. Standard errors are clustered at the urban sector level. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Source: Authors' calculations.