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ABSTRACT

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We study the impact of a COVID-19 relief program on compliance with confinement measures in Italy, the early epicenter of the pandemic. We match information on the allocation of funds across Italian municipalities with data tracking citizens' movements drawn from mobile devices and vehicles' navigation systems, anonymized and aggregated at the municipality level. To assess the role of the program, we exploit a sharp kink schedule in the allocation of funds as a function of past income differentials that generated random treatment assignment in a neighborhood of the threshold point. We find robust evidence that, after the introduction of the program, mobility decreased with the amount of transfers. The impact is economically sizeable and resists bandwidth changes, with stronger effects holding in the proximity of the cut-off and the coefficient stabilizing with distance from the threshold. A battery of placebo tests supports the interpretation of results. Our evidence suggests that authorities could leverage targeted relief programs to nudge compliance with emergency measures at a relatively modest cost.

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1 Introduction

Compliance with social distancing measures requires civic-mindedness, law enforcement, and the capacity to satisfy basic needs from home. Stay-at-home orders, in particular, can be unbearably unfair to people who can neither work from home nor afford food delivery. Economically vulnerable individuals face the most challenging difficulties in coping with lockdown rules and have more substantial incentives to go outside (Wright et al., 2020).

Inadequate or unfair policies weaken the social contract between citizens and the state, encouraging agents to withdraw their cooperation (Feld and Frey, 2007; Besley, 2020). In a pandemic crisis, the belief that the policy response is unsustainable may discourage compliance with emergency measures, resulting in the worsening of the epidemiological situation. Relief programs mitigating the pandemic economic disruption could foster the observance of social distancing mandates by limiting the mobility needs of targeted groups and nurturing the public's belief that the crisis management is adequate and fair. Understanding how compensation for economic losses relates to respect of lockdown rules may help designing policy responses that foster community resilience to adverse shocks.

In this paper, we study how a relief program affected citizens' compliance with confinement measures in Italy, the early epicenter of the pandemic in Europe. On March 30, the Italian government announced an aid scheme to support economically vulnerable groups through the distribution of food stamps. To assess the program's impact on compliance, we combine information on the allocation of the program's resources across Italian municipalities with data tracking citizens' movements through mobile devices and vehicles' navigation systems, drawn from City Analytics by Enel X. As social distancing requires renouncing unnecessary movements, we follow the literature and use human mobility as a proxy for compliance (e.g., Allcott et al., 2020; Bargain and Aminjonov, 2020b; Barrios et al., 2020; Durante et al., 2020; Egorov et al. (2020)). The granularity of the data allows us to measure the observance of restrictions at a highest possible level of disaggregation for Italy.

To address endogeneity, we exploit the kink design in the allocation of funds as a function of past income differentials that generated a random treatment assignment in a neighborhood of the threshold point. Authorities partitioned the program's budget, 400 million euros ($\simeq$ 485 million dollars), into two quotas. Eighty percent was distributed to municipalities proportionally to their population. The remaining amount was allocated as a function of the difference between municipal and national per capita income in 2017,

weighted by the population share. We first illustrate the sharp kink schedule in the distribution of funds. In a neighborhood of the threshold point, access to funds relied on the random deviation of the municipality's per capita income from the national level in 2017. Standard tests support the validity of the assumptions by showing no manipulation of the assignment variable at the kink. In the neighborhood of the threshold point, all covariates are balanced and there are no changes in slope or spurious kinks due to nonlinearities. A -€1000 per capita deviation from the cut-off ($\simeq$-1216$) determines an increase in municipality transfers of €0.58 multiplied by the population size, to be distributed to the targeted group of beneficiaries. We then assess the impact of the relief program on compliance through a Regression Kink Design (RKD).

We find robust evidence that, after the introduction of the program, mobility decreased with the amount of transfers received by each municipality. The effect is statistically significant at the 0.05 level and economically sizable. In the week of the policy announcement, the transfers cause a drop in mobility of 3 percentage points from the baseline level observed before the pandemic crisis (between January 13 and February 16, 2020). Given an average drop of 60 percentage points in the same week, the increase in transfers determined by a -€1000 per capita deviation from the threshold point causes a decrease in mobility of 5%. Two weeks later, the impact is still negative, statistically significant, and sizable, with the increase in transfers leading to a reduction in mobility of roughly 3.5 percentage points. The decline in mobility persists for approximately two more weeks. Standard and more recently developed RKD robustness checks show that our results resist irrespective of the bandwidth choice, with stronger effects holding in the proximity of the cut-off and the coefficient stabilizing with distance from the threshold. All results hold after controlling for the drop in mobility observed in the first week of the lockdown. The effect is also robust to including municipality-level controls for demographic and geographic characteristics, the share of essential workers, social capital, and weather conditions. We show that municipalities' features that may affect mobility are smooth in a neighborhood of the threshold point, suggesting that the relationship between transfers and compliance is not confounded by other potential drivers of citizens' movements. A placebo analysis in the spirit of the permutation test of Ganong and Jäger (2018) supports the causal interpretation of results.

The size of the effect suggests that more than one mechanism may have been at work in keeping citizens at home. The aid program probably reduced the mobility needs and the marginal utility of breaking lockdown restrictions for the targeted group. However, it may also have encouraged compliance in a larger population by improving the fairness

of the COVID-19 policy response and reinforcing the "contract" between citizens and the institutions (Feld and Frey, 2007; Besley, 2020).

Our work connects to several strands of literature. A growing body of research analyzes the cultural and behavioral determinants of social distancing such as civic capital (Barrios et al., 2020; Durante et al., 2020), partisanship (Allcott et al., 2020; Barrios and Hochberg, 2020; Simonov et al., 2020), public role models (Abel and Brown, 2020; Ajzenman et al., 2020), ethnic diversity (Egorov et al., 2020), economic preferences (Müller and Rau, 2020), social trust (Bargain and Aminjonov, 2020b), trust in institutions (Watanabe and Tomoyoshi, 2020), and expectations (Briscese et al., 2020). Our results add to this field by suggesting that fiscal policies may also play a role and authorities could leverage targeted relief programs to nudge compliance at a relatively modest cost. Encouraging compliance is a crucial aspect of the COVID-19 policy response, as people's willingness to follow the rules is a fundamental driver of resilience to the pandemic (e.g., Flaxman et al., 2020; Greenston and Nigam, 2020).

Social sciences have studied the drivers of compliance behavior way before the coronavirus crisis. Enlightenment thinkers like Locke (1690) and Rousseau (1762) argue that citizens accept obligations in return for benevolent government. The public economics literature suggests that people's willingness to comply with rules largely depends on their beliefs about the efficiency and fairness of public policies as if a contract with institutions was in force (Tyler, 1990; Smith, 1992; Murphy and Tyler, 2008; Hallsworth et al., 2017). If a government fails to deliver fair policies, then citizens can withdraw their cooperation (Feld and Frey, 2007; Besley, 2020). For example, Hallsworth et al. (2017) show that if taxpayers believe that the government does not spend their taxes well, they may want to reciprocate by not entirely declaring their income. Our results offer support to these views by suggesting that even a small improvement in the fairness of the COVID-19 policy response is associated with a significant increase in compliance with confinement measures. The size of the effect we detect through the RKD suggests that the impact of the aid program on mobility may extend beyond the targeted group.

We also contribute to the COVID economics literature by analyzing the outcomes of policies intended to mitigate the pandemic economic disruption. Previous research addresses the impact of fiscal support for payroll and fixed costs (Alstadsæter et al., 2020) and tracks U.S. consumers' response to the COVID-19 fiscal stimulus (Bayer et al., 2020; Coibion et al., 2020; Kaplan et al., 2020). Green and Loualiche (2021) exploit nonlinearity in the award of federal grants to U.S. state governments to estimate the state and local

employment response to federal stimulus. In a paper close in spirit to ours, Fetzer (2020) analyzes the impact of a program stabilizing the public's demand for dining out on the spreading of the viral disease. Areas that benefited from the program most also saw a remarkable increase in the emergence of new infection clusters, probably due to lesser social distancing. Our evidence adds to this literature by directly addressing the stimulus' impact on social distancing. Our results complement Fetzer's finding that stimulating the demand for the hospitality sector can worsen the epidemiological situation. Targeting relief programs at economically disadvantaged groups may potentially exert an opposite effect through the reduction in mobility.

Finally, we contribute to studies analyzing the economic outcomes of food stamps provision. Previous work assessed the impact of food stamps on crime (Tuttle, 2019), consumption (Hoynes and Schanzenbach, 2009), immigrants' labor supply (East, 2018), traffic fatalities (Cotti et al., 2016), and neonatal health (Almond et al., 2011). We add to this research by studying how an aid program providing food stamps to economically disadvantaged groups relates to compliance with emergency measures in a public health crisis.

The remainder of the paper is organized as follows. Section 2 provides context and describes our data. Section 3 shows the kink schedule in the Italian government's relief program and illustrates our empirical strategy. In Section 4, we present and discuss our results. Section 5 concludes.

2   Context and data

In this section, we first provide some background (Section 2.1). We then illustrate the sharp kink schedule in the food relief program (2.2). Finally, we describe our mobility data (2.3).

2.1   Background

On March 9, 2020, Italy was the first Western democracy to impose a national lockdown, requiring the population's confinement at home. Authorities closed schools, restaurants, and non-essential shops and banned any outdoor activity, including walking far from home. Citizens could only leave their houses for a handful of reasons— for example, to go to the supermarket or the pharmacy—and needed to carry a document stating the reason they

Figure 1: Timeline of the outbreak in Italy

were outside.[1] Despite these measures, Italy rapidly surpassed China as the country with the highest death toll from the novel coronavirus disease, becoming the epicenter of a shifting pandemic. Figure 1 illustrates the timeline of the outbreak in Italy.

Though useful in flattening the contagion curve (Amuedo-Dorantes et al., 2020; Flaxman et al., 2020), confinement measures were *de facto* the most substantial suppression of constitutional rights in the history of the Republic, undermined the incumbent government's popularity (Fazio et al., 2021), and threatened the social contract between citizens and the state. Lockdowns triggered economic hardship for millions of people, with unskilled workers and the poor bearing the heaviest toll. Mongey et al. (2020) show that workers with less ability to work from home suffered higher unemployment increases and were less able to comply with stay-at-home orders. Palomino et al. (2020) find sizable changes in poverty and inequality across Europe, with Southern countries witnessing the higher rise and increases varying with the duration and intensity of social distancing mea-

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[1] Authorities allowed police officers a discretionary power to assess the residents' statements and fine transgressors €200 (≃228$). On March 21, the government tightened the lockdown by shutting down all non-necessary businesses and industries. Given that authorities had caught more than 100,000 people outside for no good reason or lying on their forms, on March 25, the Italian Prime Minister announced an increase in lockdown fines up to €3000 (≃3400$).

sures. The authors estimate an average increase in the headcount poverty index from 4.9 to 9.4 percentage points and a mean loss rate between 10% and 16.2% for the working poor.

Colussi (2020) documents that industrial production fell by almost 30% in Italy and GDP contracted by 4.7% in March 2020 due to lockdown measures. In April, the country's industrial production further contracted by 19.1% relative to March. The welfare system and the suspension of layoffs delayed the impact of the crisis on the labor market.[2] The unemployment rate fell by 11,1% in March 2020 and continued decreasing in April due to the massive increase in economically inactive people. Requests for subsidies for temporary reductions of hours worked (*Cassa Integrazione Guadagni*) increased by about 2,953% with respect to April 2019. According to the Italian National Institute for Social Security (*Istituto Nazionale della Previdenza Sociale*, INPS), 51% of Italian firms, accounting for 40% of the private sector employment, adopted short-time work schemes in March and April 2020. Their employees reduced the hours worked by about 90% and experienced a 27% loss in their gross monthly wage (Bovini et al., 2020). Figari and Fiorio (2020) show that 41% of those who suffered from earning losses belonged to one-earner households: for them, the temporary shutdown of their activities caused the loss of the primary income source. As a result, the demand for food and financial aid dramatically increased. Only in Rome, parishes and their outreach centers distributed 600% more food supplies compared to the same period in 2019. Caritas (2020) reports that 35.3% of people who turned to parish outreach centers had never sought their assistance before. According to the Diocesan Caritas office in Rome, over 83% of outreach centers were assisting new poor.[3] These figures confirm the national trend, albeit perhaps to a worse degree (Caritas, 2020).

Towards the end of March 2020, tension mounted in more impoverished areas, with police patrolling supermarkets following a series of thefts. Newspapers reported a dramatic increase in petty crimes such as assaults on grocery stores and attacks on food delivery riders.[4]

On March 30, the Italian Prime Minister announced new relief measures paying in

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[2] On March 17, 2020, the Italian Government issued the *Cure Italy Decree* that comprised a series of interventions to support workers and firms hit by the lockdown. The Decree introduces the suspension of tax deadlines for economic activities and extends the layoff freeze to the end of March 2021.

[3] The increase was persistent and lasted throughout 2020. In Rome, the Diocesan Caritas office reports that the three Caritas soup kitchens assisting the poorest saw a 50% increase from April to June (Caritas, 2020).

[4] See, for instance: https://www.theguardian.com/world/2020/mar/29/italy-sets-aside-400m-for-food-vouchers-as-social-unrest-mounts.

advance €4.3 billion (≃5.2$ billion) to the municipal solidarity fund and allocating 400 additional million (≃ 485 million dollars) to an emergency relief program providing food stamps to economically disadvantaged groups.[5] The government stressed the intention to levy these measures to make the COVID-19 policy response fairer, create a buffer against the increase in poverty, and halt social unrest. Our empirical analysis focuses on the 400 million food aid program as the authorities allocated part of the funds following a kink design.[6] The Civil Protection Department partitioned the program's budget, €400 million (≃485$ million), into two quotas. Eighty percent (*Quota* A) was distributed to municipalities proportionally to their population. The remaining amount (*Quota* B) was allocated as a function of the difference between the municipality and national per capita income in 2017, weighted by the population share.[7]

Municipalities were responsible for identifying the pool of beneficiaries, defining the value of the vouchers, and distributing them to purchase food and relief goods. Households most likely affected by the economic consequences of the lockdown and economically vulnerable individuals were given a priority in the access to the aid program. Local authorities coordinated with nonprofit organizations to deliver the food at home to a limited pool of residents in a particular state of need. The other beneficiaries of the program received food stamps to be used for purchasing food an essential goods.

2.2 Policy environment

In this section, we describe the kink schedule in the distribution of the program's funds. To reconstruct the allocation mechanism, we combine information about the amount received by each municipality under the two quotas, provided by the Civil Protection Department, with data on 2017 income taxes aggregated at the municipality level, provided by the Department of Finance.[8]

We compute the municipal income per capita, I_m, as the municipality's total taxable income in 2017 divided by the municipal population. The national income, I_N, is the sum

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[5] The Municipal Solidarity Fund was established in 2012. It is fed through municipal taxes and serves to support municipalities under public finance distress. The advance payment established by the government is an ordinary procedure that takes place every year.

[6] All the other relief programs allocated funds uniformly across targeted groups. For a summary, see, for example https://www.bruegel.org/publications/datasets/covid-national-dataset/.

[7] The decree of the Civil Protection Department's head establishing the allocation of funds is available at the url: https://www.trovanorme.salute.gov.it/norme/dettaglioAtto?id=73781.

[8] Aggregate municipal income tax data are available from the Department of Finance at https://www1.finanze.gov.it/finanze3/pagina_dichiarazioni/dichiarazioni.php.

of taxable incomes in all municipalities in 2017 divided by the Italian resident population. According to the decree of the Civil Protection Department's head, each municipality m should receive the following amount under the two quotas, A and B:

$$Q^*_{A,m} = \frac{pop_m}{pop_N} Q_{A,tot} \tag{1}$$

$$Q^*_{B,m} = \begin{cases} \delta^* \Delta I_m \dfrac{pop_m}{pop_N} Q_{B,tot} & \text{if } (I_N - I_m) > -\alpha \\ 0, & \text{otherwise} \end{cases} \tag{2}$$

where, pop_m is the resident population in municipality m, pop_N is the total population resident in Italian municipalities, ΔI_m is equal to $(I_N + \alpha - I_m)$. $Q_{A,tot}$ and $Q_{B,tot}$ is the total budget of the aid program.[9] Parameter δ is a rescaling factor ensuring that $\sum_m Q^*_{B,m} = Q_{B,tot}$, implying:

$$\delta = \left(\sum_m \left(\Delta I_m \frac{pop_m}{pop_N} \mathbf{1}_{\{(I_N - I_m) > -\alpha\}} \right) \right)^{-1} \tag{3}$$
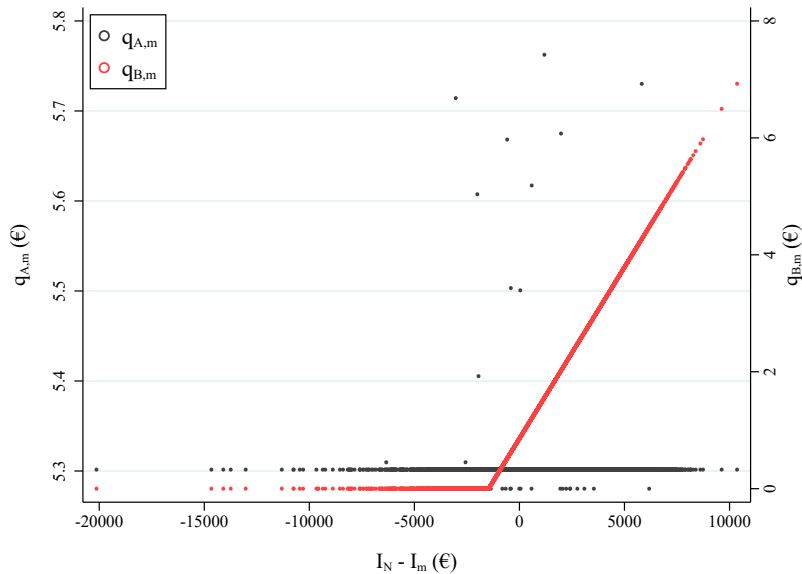
Finally, parameter α allows flexibility in determining the cut-off, which is neither clearly indicated in the decree nor in any related official document. Equations 1 and 2 can be equivalently written in per capita terms as:

$$q^*_{A,m} = \frac{Q_{A,tot}}{pop_N} \tag{4}$$

$$q^*_{B,m} = \begin{cases} \delta \Delta I_m \dfrac{Q_{B,tot}}{pop_N} & \text{if } (I_N - I_m) > -\alpha \\ 0, & \text{otherwise} \end{cases} \tag{5}$$

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[9] Note that pop_N is different from the total population in Italy (pop_{IT}), since municipalities in regions with special constitution (Aosta Valley, Trentino Alto Adige and Friuli Venezia Giulia) received different support regime.

Figure 2: Testing the relationship between $q_{A,m}$, $q_{B,m}$ and $(I_N - I_m)$
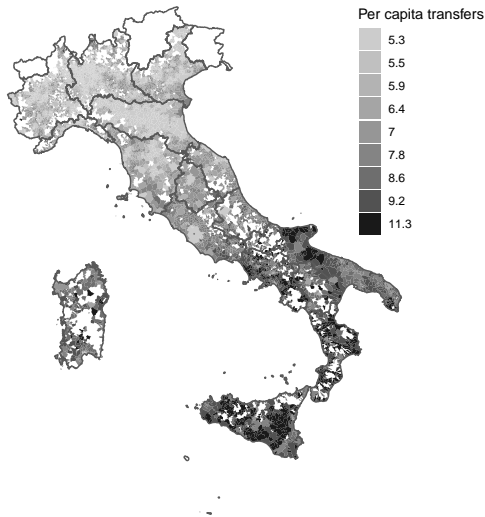

Note: the graph shows the per capita transfers computed as the weighted difference $(I_N - I_m)$. Each dot is a municipality under the two quotas: $q_{A,m}$ and $q_{B,m}$ in black and red, respectively.

where it is clear that $q^*_{A,m}$ should be fixed and identical across municipalities, while $q^*_{B,m}$ should be equal to the national per capita rescaled by a constant and a factor that depends on the municipal relative position in the distribution of taxable incomes.

In Figure 2, we plot the empirical relationship between the actual values of $q_{A,m}$ and $q_{B,m}$ (i.e. the per capita transfers to each municipality under the two quotas) and the computed difference $(I_N - I_m)$. In this way, we check the validity of equations 4 and 5 derived from the decree. As expected, the graph shows that component A is independent on the difference between the national and the municipality per capita income. As prescribed by equation 4, $q_{A,m}$ is virtually identical to €5.3 per capita for all municipalities, with a few tiny deviations.

The kink schedule identified by equation 5 is sharp. $q_{B,m}$ is equal to zero until the difference $(I_N - I_m)$ is smaller than €-1465.6, which empirically defines the parameter $\alpha = 1465.6$, and has a deterministic slope thereafter. Having defined the α parameter, we also compute δ according to equation 3, which delivers a value of 0.000425. If equations 3 and 5 are both correct, this value can also be obtained by regressing $q_{B,m}$ on $\Delta I_m \left(Q_{B,tot}/pop_N \right)$

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Figure 3: Distribution of the total amount of transfer across municipalities

using observations on the right of the cut-off point, i.e. the municipalities for which $q_{B,m} >$ 0. Figure A.1 in Appendix shows the estimated line and the estimated δ, which is identical to the one computed using equation 3. As expected due to the deterministic kink schedule, the value of the R^2 of the regression is equal to 1.

Figure 3 illustrates the distribution of total per capita transfers (i.e. accounting for both "Quota A" and "Quota B") across Italian municipalities. Shades of grey illustrate the heterogeneity in the distribution of funds, with darker areas receiving higher amounts. White areas cover the three autonomous regions that benefited from different aid schemes and are therefore excluded from the analysis. Since funds are allocated based on past municipal incomes weighted by the population, transfers are higher in the South of Italy than the North.

2.3 Mobility

Compliance with confinement measures depends on a variety of factors and cannot be taken for granted. Healthy people may not fully perceive the severity of the crisis, and infected individuals derive no personal benefit from following the rules (Barrios et al., 2020). As social distancing requires renouncing to unnecessary movements and staying at home as much as possible, the literature has used indicators of mobility to measure compliance at
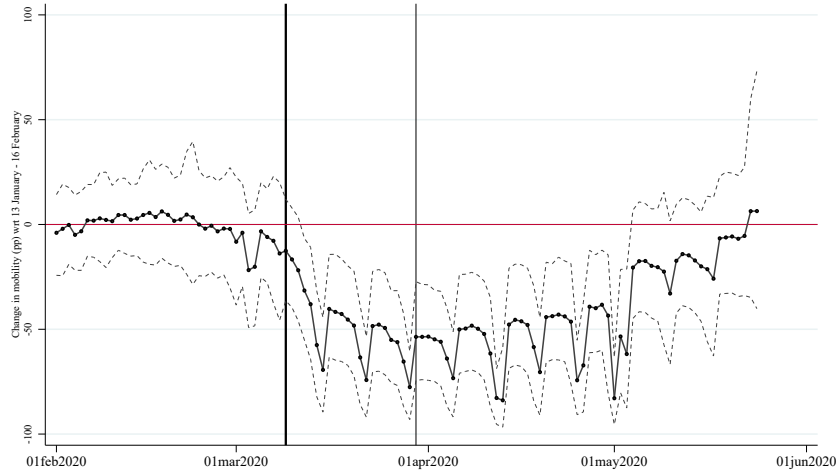
the city (Ajzenman et al., 2020; Egorov et al., 2020), province (Durante et al., 2020), region (Bargain and Aminjonov, 2020b), or U.S. county level (Allcott et al., 2020; Barrios et al., 2020; Simonov et al., 2020). Previous studies on Italian mobility during the lockdown employed province-level indicators drawn from Teralytics (Durante et al., 2020), Cuebiq Inc. (Pepe et al., 2020), or Google Mobility Reports (Caselli et al., 2020).

To measure compliance, we follow the literature and exploit data from Enel X to build an indicator of mobility at the municipality level. Enel X data rely on the geolocation system of mobile and vehicles' navigation devices to capture individual movements and aggregate them at the municipal-daily level.

Mobility is expressed as the percent deviation of the ratio between citizens' movements and the municipal population from the baseline level observed before the pandemic crisis from January 13 to February 16, 2020. Figure 4 illustrates the change in mobility between February 1 and May 31. Each dot represents the average daily change in mobility across Italian municipalities. The two dotted lines represent the daily mobility trends observed in the 10th and the 90th percentile of the national distribution of mobility.

The plot shows that mobility rapidly decreased with the beginning of the lockdown on March 9 (thick solid line). A further decline occurred after March 21, when the gov-

Figure 4: Average daily change in mobility in Italy

Note: The graph shows the pattern of the mobility index over the period February 1-June 1. The thicker vertical line represents the beginning of the lockdown while the thin solid line indicates the policy announcement. Source: our elaboration on Enel X data.

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ernment tightened confinement measures by shutting down all non-necessary businesses and industries. The lockdown formally ended on May 4, but pre-crisis levels were reached only towards the end of May. The series also follows clear weekly patterns, with working days showing higher mobility and downward peaks representing the substantially smaller movements on the weekends.

3 Empirical strategy

In this section, we first describe our regression kink design for the assessment of the aid program's impact on mobility across the Italian municipalities (3.1). Then, we describe our estimation sample and the covariates we control for in the empirical analysis (3.2).
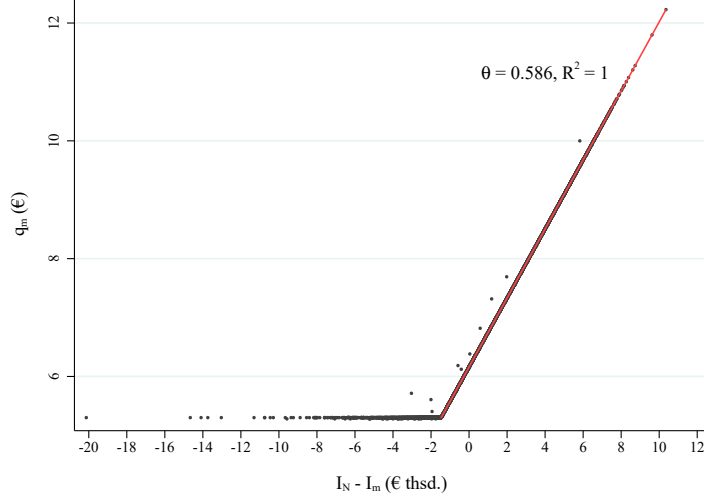
3.1 The kink design

The per capita amount received by each municipality under the aid program is the sum of the two components $q_{A,m}^*$ and $q_{B,m}^*$ described in Section 2.2 (equations (4) and (5)). The relationship between transfers and the difference between municipal and national per capita income is summarized in Figure 5. The plot appears as a flat line until the cut-off point. Above this value, the per capita amount of transfers linearly and deterministically increases with the forcing variable.

We exploit the exogenous change in slope at the cut-off point (kink) to identify the causal effect of transfers on compliance with stay-at-home orders, as measured by the mobility index described in section 2.3. Since the per capita amount of transfers is a linear function of municipal income above the cut-off point, an OLS estimate of the relationship between mobility and transfers would likely be biased, because municipal income may be correlated with unobservable determinants of mobility.

To solve this potential endogeneity issue, we adopt the standard *Regression Kink Design* (RKD) firstly introduced by Nielsen et al. (2010), and more recently developed by Card et al. (2015) and Simonsen et al. (2016). The key intuition is that if the amount of transfers affects compliance, then the relationship between the outcome and the forcing variable will also show a change in slope at the cut-off point. The identifying assumption is that in a neighborhood of the cut-off point, the change in the amount of transfers induced by the kink is as good as random.

Figure 5: Allocation mechanism

Note: The empirical relationship between transfers and the difference between the municipal and the national per capita income.

Define as τ the ratio of the discontinuity in derivatives at the cut-off point in the outcomes Y divided by the discontinuity in the amount of benefits received by each municipality:

$$\tau = \frac{\lim_{r \to c+} \left.\frac{\partial \mathbb{E}[Y|R=r]}{\partial r}\right|_{r=c} - \lim_{r \to c-} \left.\frac{\partial \mathbb{E}[Y|R=r]}{\partial r}\right|_{r=c}}{\lim_{r \to c+} Q'(r) - \lim_{r \to c-} Q'(r)}, \tag{6}$$

where $Q(.)$ is a function that maps the amount of benefits received and the value of the running variable R. As showed by Card et al. (2015), the quantity described in (6) identifies the "treatment-on-the-treated" parameter introduced by Florens et al. (2008) or, equivalently, the "local average response" parameter introduced by Altonji and Matzkin (2005) at the threshold point c, i.e. the extent to which the outcome Y will vary in response to a unit variation in the amount of the received treatment.

In our case, the quantity at the denominator of equation (6) comes from the deterministic function $Q(.)$ and we do not need to estimate it, i.e., we are in a sharp RKD. This quantity is equal to 0.586, i.e. the θ parameter in Figure 5. On the other hand, the numerator can be identified by the coefficient β of the following equation:

$$Y_{wm} = \alpha + \beta[Z_m \times (R_m - c)] + \gamma f(R_m - c) + \varepsilon_{wm}, \tag{7}$$

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where Y_{wm} is the mobility index of municipality m in week w, $Z_m = \mathbf{1}_{\{R_m \geq c\}}$ and $f(R_m - c)$ includes a polynomial function of the running variable and their interactions with Z_m.

We estimate equation (7) using the optimal bandwidth proposed by Calonico et al. (2019) (CCT hereafter) separately for each week. We use a triangular kernel and a linear polynomial. Since nonlinearity in the relationship between the outcome and the forcing variable might generate a spurious kink at the the cut-off point we show that similar results are obtained using a second order polynomial.

3.2 The estimation sample

We start with 7,257 municipalities in the 17 (out of 20) Italian regions for which it was possible to match the data on the aid program's transfers with municipality tax records.[10] As mentioned in Section 2.2, we use aggregated municipal tax records to compute the forcing variable we employ to estimate the kink.

We aggregate daily mobility data for each municipality at the weekly level. We keep only municipality-week observations for which the daily mobility index is non-missing for the entire week. We also drop provinces where all municipalities are either all above or all below the cut-off of the kink design. After putting together the municipality-week panel, the final sample consists of 32,238 municipality-week observations for 3,582 municipalities observed between the 10th week (March 9– 15) and the 18th week 18 (May 4-18).

Figure 6 illustrates the geographical distribution of municipalities above and below the cut-off point. The left/right panel shows the municipalities included/excluded in our main sample. Figure A.2 displays the density of the forcing variable for the provinces included and excluded from the sample separately.

Finally, we complement our dataset with a set of municipality-level covariates that could affect mobility but have no relationship with the kink, as confirmed by the results of the balancing test. We use ISTAT's 2011 Census data to control for municipalities' population and degree of urbanization, as people living in small towns may have to travel higher distances to reach the workplace or satisfy basic needs.

Since the essential activities that remained open during the lockdown are a crucial source of mobility, we also control for the number of active firms and the number of workers

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[10] All municipalities in the autonomous regions of Aosta Valley, Trentino Alto Adige, and Friuli Venezia Giulia benefited from a different aid program with no kink schedule. See details on https://www.gazzettaufficiale.it/eli/id/2020/03/30/20A01942/sg.

Figure 6: Selected sample

(a) Included   (b) Excluded

Note: Right and left figures display the municipalities included and excluded from our main sample of analysis. The excluded provinces are those where all municipalities are either all above or all below the cut-off point. Excluded municipalities are also shown in Figure A.2.

in 3-digits NACE categories using data from the 2018 ISTAT's register of Active Enterprises (ASIA - Enterprises). Specifically, we compute an indicator of firm density as the number of firms divided by the municipality's surface. We also build an indicator of the share of essential workers over the population. We define the essential NACE categories following the classification proposed by Di Porto et al. (2020) for Italy.

Compliance with lockdown rules also is a matter of social capital (Barrios et al., 2020; Borgonovi and Andrieu, 2020; Durante et al., 2020). Healthy people may not properly understand the risks of contagion, and infected individuals with mild symptoms do not derive any personal benefit from staying at home (Barrios et al., 2020). Besides law enforcement and the fear of contagion, people comply as long as they care about the community's welfare and expect that most others will do the same. Therefore, compliance reflects the tendency to internalize externalities as a matter of civic-mindedness and believe that most people can be trusted. As pointed out by Barrios et al. (2020), this combination of "values and beliefs that help a group overcome the free-rider problem" is what Guiso et al. (2010) define as civic capital and the literature commonly labels as social capital (Putnam et al., 1993).

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We use the ISTAT's 2011 census of nonprofit institutions to construct a municipality-level indicator of social capital that captures the per capita number of Putnam-type associations, including civil and human rights, civil protection, environmental, international cooperation and solidarity, philanthropic, social cohesion, and social service organizations.[11]

Finally, we control for the change in mobility during the first week of the lockdown, which provides the baseline level of compliance observed in each municipality.

4   Results

We first present our results about the impact of the relief program on compliance across Italian municipalities (Section 4.1). We then provide a battery of robustness and placebo tests to support the causal interpretation of results (4.2). Finally, we briefly discuss our findings (4.3).

4.1   Main results

In this section, we assess the program's impact from the week of its announcement (the 13th week of 2020) to the end of stay-at-home orders (May 4, 2020, i.e., the 18th week).
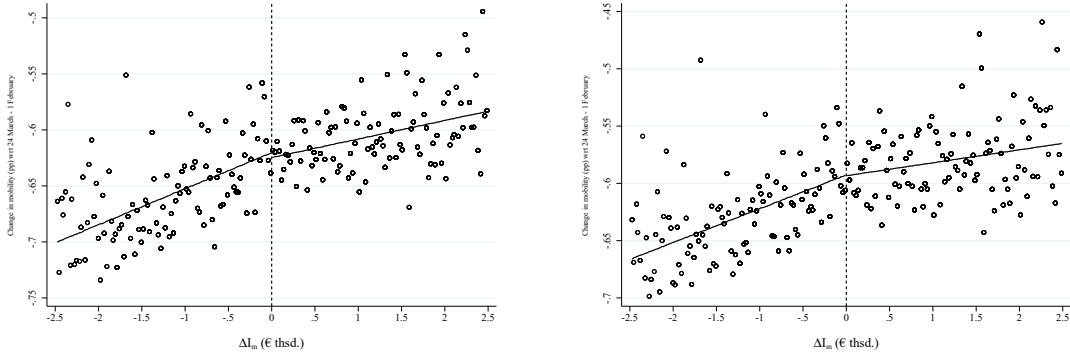
As our empirical strategy exploits the kink schedule in the allocation of funds (illustrated in Figure 5), we first show the existence of a kink in the relationship between the assignment variable and mobility in Figure 7. Using the change in slope in the assignment rule (its first derivative), we identify the treatment effect precisely in a neighborhood of the cut-off point. The negative change in slope suggests that the treatment negatively affects mobility across municipalities. On the right of the cut-off, mobility plateaus around -60 percentage points.

By contrast, the plot reveals a positive relationship on the left, implying that poorer municipalities are on average less compliant with confinement measures.

As suggested by (Card et al., 2015), there are two testable necessary and sufficient conditions for the validity of the RKD. The first is the smoothness condition of the density of the forcing variable. Evidence of bunching of observations around the cut-off point could

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[11] The density of non-profit organizations is one of the oldest and more consolidated indicators of social capital. First proposed by Putnam et al. (1993), it was extensively adopted in the economics literature (e.g. Knack and Keefer, 1997 and Guiso et al., 2016). Putnam et al. (1993) credits the organizations we consider in the analysis with the ability to instill in their members habits of cooperation, solidarity, and public-spiritedness as opposed to the so-called Olson-type organizations that mostly have redistributive goals.

Figure 7: Program's impact on mobility

(a) Week 13   (b) Week 15

Note: Figure shows the kink in the relationship between the assignment variable and mobility in the 13th and 15th week.

signal endogenous sorting and cast doubts about the validity of the design. In our context, the forcing variable is a function of the distribution of municipal incomes in 2017, more than two years before the beginning of the pandemic. Therefore, it is implausible to observe endogenous sorting around the threshold. Figure 8 provides evidence of smoothness in

Figure 8: Smoothness of the conditional density of the forcing variable

Note: The figure shows the density of the forcing variable in a neighborhood of the cut-off point using the graphical test proposed by McCrary (2008).

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Table 1: Smoothness of pre-determined covariates

Dependent variable	Polynomial order	
	Linear	*Quadratic*
	(1)	(2)
Population	3024	1853
	(3938.087)	(8618.253)
Municipality size	-1.516	-2.565
	(6.5784)	(12.0341)
Dummy urban area	-0.0278	-0.0165
	(0.0638)	(0.1303)
Firm density	8.791	5.374
	(5.6603)	(14.4248)
The share of non-essential workers per capita	-0.00750	-0.00960
	(0.0105)	(0.0208)
The density of non-profit organizations	-0.0331	-0.0763
	(0.0632)	(0.1311)
The change in mobility over the first week of the lockdown	-0.0150	-0.0236
	(0.0154)	(0.0258)

Note: The table shows tests for the presence of changes of the slope in the conditional expectation of covariates determined before the treatment assignment. The test is performed using both a linear and quadratic polynomial (columns 1 and 2 respectively). For each kink regression the bandwidth is computed according to CCT.

Robust standard errors in parentheses. * $p<.10$ ** $p<.05$ *** $p<.01$.
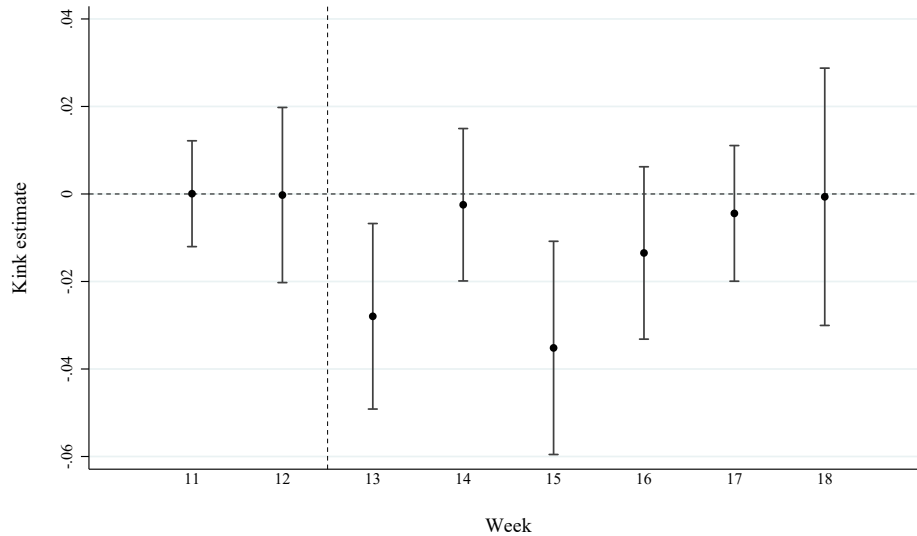
the conditional distribution of the forcing variable around the cut-off point based on the graphical test proposed by McCrary (2008).

The second testable condition requires the conditional distribution of the pre-determined covariates to be smooth around the cut-off point. To test for the smoothness of the first derivative of covariates' conditional expectation functions, we estimate separate kink regressions for each covariate using both a linear and a quadratic polynomial. The results in Table 1 show no changes in slope neither with a linear (column 1) nor with a quadratic polynomial (column 2).

Importantly, results show the absence of a kink for the change in mobility during the first week of lockdown (the 10th week of 2020), providing an implicit placebo test since the policy was introduced at the beginning of the 13th week.

We now turn to our treatment effects. Figure 9 shows our main results using a linear polynomial and CCT optimal bandwidth. We show the treatment effects from the 11th week of 2020 (i.e., two weeks before the program's announcement) to the 18th week.

Figure 9: Program's impact on mobility - pre and post policy announcement

Note: The figure shows the change in the slopekink estimates and the associated standard errors using the CCT optimal bandwidth and a linear polynomial. Results are displayed for the weeks preceding the policy announcement (11 and 12) and the weeks after (13 to 18).

The two lockdown weeks before the program's announcement serve as a placebo test in the spirit of Card et al. (2015). The coefficients are virtually zero in magnitude and never statistically significant, suggesting that, before the actual distribution of food stamps, there was no relationship between mobility and the amount that municipalities received two weeks later.

In the first week following the program's announcement (March 30 - April 5), we observe that transfers cause a reduction in mobility of roughly three percentage points with respect to the baseline period. Given an average drop of 60 percentage points in the same week, we estimate that the increase in transfers determined by a -€1000 deviation from the cut-off – equal to €0.58 × Pop_m – causes a 5% reduction in mobility.

As expected, in the 14th week the effect shrinks and becomes not statistically distinguishable from the reduction in mobility caused by the closure of essential activities and the tightening of lockdown's enforcement for the Holy Week.

In the third week after the program's announcement, when authorities actually distributed most food stamps, and the aid scheme found the highest resonance in the media

Table 2: Kink estimates - Main

	Mobility Index				
	Linear models			*Quadratic models*	
	(1)	(2)		(3)	(4)
β_{13}	-0.0334*	-0.0280**		-0.0450*	-0.0343**
	(0.0184)	(0.0129)		(0.0271)	(0.0171)
CCT Bandwidth	1.199	1.220		2.347	2.654
N	1626	1622		2606	2736
Covariates	No	Yes		No	Yes
β_{15}	-0.0379**	-0.0352**		-0.0503	-0.0534**
	(0.0181)	(0.0149)		(0.0314)	(0.0259)
CCT Bandwidth	1.194	1.183		2.057	2.021
N	1622	1576		2421	2340
Covariates	No	Yes		No	Yes

Note: The table contains coefficients and standard errors showing the estimated effect in percentage points increase of weekly mobility at the vicinity of the threshold point. The coefficients β_{13} and β_{15} refer to 13th (March 30 - April 5) and the 15th week (April 13-19), respectively. The bandwidths are selected using the procedure by Calonico et al. (2019). Robust standard errors in parentheses. * p<.10 ** p<.05 *** p<.01.

(see Figure 12 in the discussion of results), municipality-level transfers cause a decrease in mobility of roughly 3.5 percentage points. The impact of the program then gradually stabilizes in four weeks until it becomes indistinguishable from the reduction in mobility observed in the municipalities lying below the cut-off.
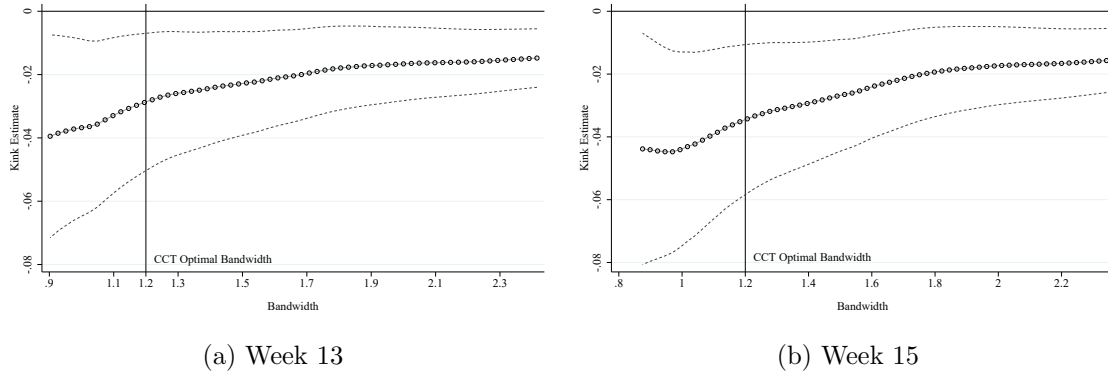
Table 2 shows the main results for the 13th and the 15th week and includes alternative specifications without covariates and allowing for quadratic polynomials. Figure A.3 in the Appendix replicates figure 9 using a quadratic polynomial. The full set of results for the other weeks is presented in the Appendix in Table A.1.

4.2 Robustness and placebo tests

Essential robustness checks in the RKD regard the sensitivity of results to the bandwidth choice and the polynomial order. While the main results rely on the CCT optimal bandwidth, Figure 10 illustrates how estimates change with varying bandwidths. We check the sensitivity of results for the 13th and the 15th week.

In Figure 10, the vertical lines represent CCT optimal bandwidths (roughly 1.2 in both
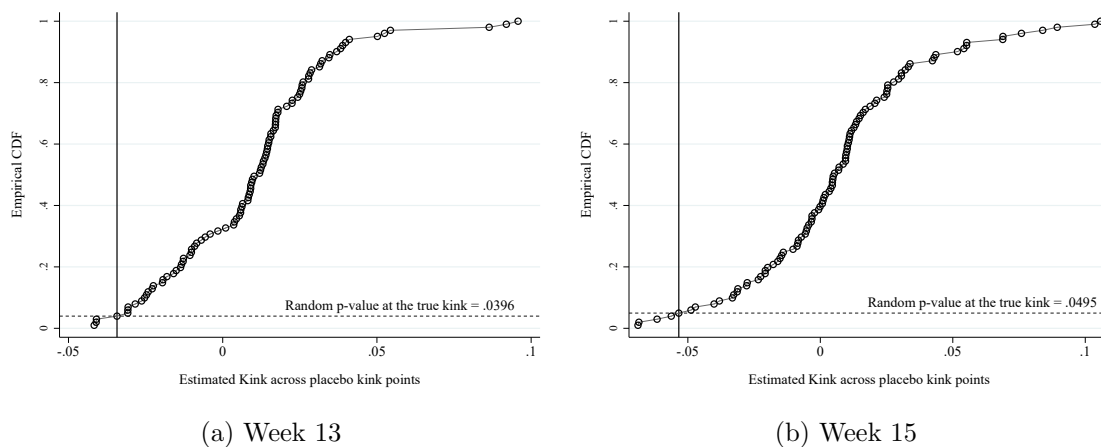
Figure 10: Kink estimates with varying bandwidths

(a) Week 13   (b) Week 15

Note: The figure shows the point estimates and 90 percent confidence intervals for the main effect (on the y-axis), using bandwidths between 0.9 and 2.4 for the week 13 and 0.8 and 2.3 for the week 15 (on the x-axis). The figure shows that the absolute value of the point estimate is robustly around 0.3 percentage points, regardless of the bandwidth. Narrowing the CCT bandwidth we obtain slightly lower values. The vertical line roughly at 1.2 represents CCT optimal bandwidth by recommended by Calonico et al. (2019).

cases). We consider a range of values between 0.9 and 2.3. Overall, the estimates' pattern corroborates our results, with stronger effects holding in the proximity of the cut-off and the coefficient stabilizing with distance from the threshold. In Figure A.4, we show similar patterns using a second-order polynomial. The effect is mostly stable along the spectrum of the considered bandwidths.

Although results using a quadratic polynomial are similar to our main estimates, we further test if nonlinearity in the relationship between mobility and our forcing variable is likely to induce spurious treatment effects. To this purpose, we follow Ganong and Jäger (2018) and compare the "true" kink estimates with a distribution of treatment effects obtained using "placebo cut-off points". To implement the test, we consider 100 evenly spaced cut-off points in the region between -2.5 and 2.5, i.e., roughly twice the size of the CCT optimal bandwidth. After estimating separate kink regressions at each kink point using CCT optimal bandwidths, we compute the position of our main treatment effects in the conditional density function (CDF) of placebo estimates. Results of this procedure are shown in Figure 11. The value of the CDF at the real cut-off corresponds to the value of the test statistics and is below 5% for both weeks.

Since the main specification considers a subsample of Italian provinces, we also show our main estimates for the 13th and the 15th week using the entire sample as a further

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Figure 11: Permutation test Ganong and Jäger (2018)

(a) Week 13   (b) Week 15

Note: The figures show the CDF of kink estimates using 100 evenly spaced placebo cut-off points in the interval [-2.5,2.5]. The vertical line corresponds to the value of the kink estimates at the true cut-off points. The horizontal dashed lines correspond to the values of the CDF at the true cut-off, i.e. the value of the permutation test.

robustness check. Table A.2 replicates the results in Table 2 with unchanged conclusions.
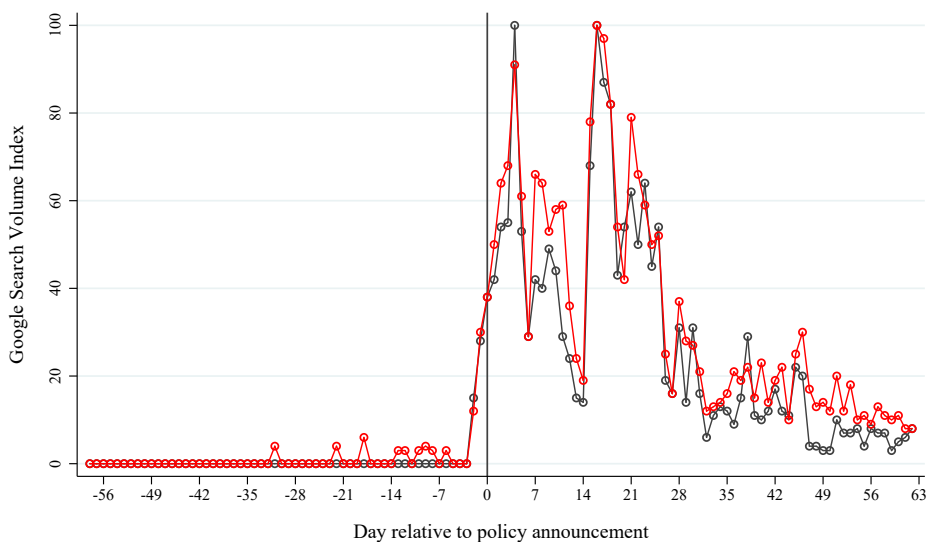
4.3 Discussion

More than one mechanism may have been at stake in channeling the impact of the aid program on compliance. The beneficiaries that received food at home certainly reduced their mobility needs. For those receiving the food stamps, the program may have reduced the marginal utility of non-compliance and raised the deterrence of penalties. However, the size of the effect suggests that behavioral spill-overs may also have occurred, affecting a larger population than the limited pool of the program's beneficiaries. At the end of March, Italy had enforced one of the strictest lockdowns in the Western world, causing unprecedented economic losses and remarkably increasing poverty and inequality (Brunori et al., 2020; Palomino et al., 2020). This policy response was particularly unfair to economically disadvantaged groups who could neither work from home nor afford the delivery of essential goods, to the point that social unrest rapidly mounted in lowest-income areas. The public economics literature suggests that when citizens perceive policies as inadequate or unfair, they tend to be less cooperative (Besley, 2020). In the coronavirus crisis, this negative attitude could result in lower compliance with confinement measures. Improving

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the fairness of the COVID-19 policy response may have strengthened lower-income agents' motives for staying at home by reinforcing the social contract between citizens and the state.

The pattern of the treatment effects we document in Figure 15 resembles the trend of the public's interest in the aid program we detect through online searches. Figure 12 illustrates the daily volume of Google searches for 'COVID-19 food stamps" (in red) and "COVID-19 voucher" (in black) from January 30 to May 30. We observe a spike in the week following the scheme's announcement. One week later, searches dropped with the Holy week and Easter holidays. In the third week, when authorities distributed most food stamps, online queries spiked again, suggesting that the food relief program was a particularly salient topic in the public's interest. Searches then plateaued slightly below the spike for the following three weeks before definitively decreasing. Previous research has documented that Google searches for a specific topic substantially reflect the media coverage of that topic (see, for example, Jetter, 2017). The resemblance in the patterns of the treatment effects (Figure

Figure 12: Online searches for food shopping vouchers

Note: Searches for *COVID-19 food stamps* (black line) and *COVID-19 voucher* (red line) are normalized for their relative popularity on a 0-100 scale over February 1 - May 31 (2 months before and after the program's announcement on March 30). The vertical line corresponds to the day of the announcement (0 on the x-axis).

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9) and the public's interest in COVID-19 food stamps' (Figure 12) is striking and suggests that the media coverage may have helped the government levying the aid program to nudge compliance.

Our finding that mobility decreased with the amount of the program's funds allocated across Italian municipalities is consistent with previous evidence that local stimulus shocks are associated with increased social distancing. Wright et al. (2020) show that transfers of the CARES Act, which mostly benefited more indigent individuals, significantly reduced movements across U.S. counties. The evidence that mobility is higher in poorer municipalities that we show in Figure 7 supports previous findings that poorer regions comply less with shelter-in-place policies, as low-income agents need not interrupt income-generating activities to escape poverty and hunger (Bargain and Aminjonov, 2020a; Dasgupta et al., 2020).

Overall, the evidence on the relationship between poverty and social distancing in the coronavirus crisis points out the importance of tuning relief programs also in light of their potential impact on compliance. While support to the hospitality sector through incentives to eat out may encourage mobility, thereby fostering the spreading of the viral disease (Fetzer, 2020), programs targeted at individuals with lower incentives to comply may play a significant role in containing contagion. On a more general note, our evidence is consistent with the public economics literature that investigates the moral and social dynamics of tax compliance (see Andreoni et al., 1998 for a review), suggesting that the perception of the political process as fair results in a stronger willingness to contribute to the welfare of the community (Feld and Frey, 2007; Besley, 2020).

5   Conclusion

Lockdowns are the most effective measures that governments implemented to flatten the contagion curve during the first wave of the novel coronavirus pandemic. However, the confinement of people at home causes tremendous economic losses, worsens poverty and inequality, and threatens the social contract between citizens and the state. The economic hardship and the suppression of civil liberties entailed by lockdowns can be unbearably unfair to impoverished individuals who cannot work from home and need to carry on income-generating activities.

In this paper, we exploited a sharp kink design in the allocation of an aid program's funds across Italian municipalities to study the impact of COVID-19 relief policies on com-

pliance with confinement measures. We provided robust evidence that, after the introduction of the program, mobility decreased with the resources allocated to each municipality. The effect is economically sizeable and resists typical and more recently developed RKD robustness checks, such as bandwidth changes, with stronger effects holding in the proximity of the cut-off and the coefficient stabilizing with distance from the threshold.

Our results put forward actionable insights for policymakers. The finding that mobility was higher in poorer municipalities suggests that low-income agents face the most challenging difficulties in coping with stay-at-home orders. However, alleviating the essential needs of economically disadvantaged groups has a substantial impact on social distancing. This result suggests that relief programs must also be designed in light of their potential impact on compliance with emergency measures. The COVID-19 policy response needs to complement stay-at-home orders with adequate compensation for the economic hardship they cause. Since low-income individuals have a higher incentive to infringe movement restrictions, targeting relief measures at economically disadvantaged groups could more effectively support compliance than indiscriminate fiscal stimuli.

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Appendix: Tables

Table A.1: Kink estimates - robustness

		Mobility Index			
	Linear models			*Quadratic models*	
	(1)	(2)		(3)	(4)
Panel A			Post policy period		
β_{14}	-0.0130	-0.00250		-0.0226	-0.00460
	(0.0169)	(0.0106)		(0.0242)	(0.0236)
CCT Bandwidth	1.329	1.493		2.698	2.131
N	1755	1888		2815	2417
Covariates	No	Yes		No	Yes
β_{16}	-0.0207	-0.0135		-0.0196	-0.0140
	(0.0162)	(0.012)		(0.0308)	(0.0226)
CCT Bandwidth	1.315	1.370		2.123	2.216
N	1742	1755		2466	2477
Covariates	No	Yes		No	Yes
β_{17}	-0.0166	-0.0044		-0.0227	-0.0123
	(0.0148)	(0.0095)		(0.0308)	(0.0244)
CCT Bandwidth	1.364	1.649		2.044	2.099
N	1785	2019		2411	2394
Covariates	No	Yes		No	Yes
β_{18}	-0.0022	-0.0006		-0.0007	0.0020
	(0.0173)	(0.0179)		(0.0362)	(0.0307)
CCT Bandwidth	1.522	1.338		2.284	2.369
N	1958	1732		2575	2558
Covariates	No	Yes		No	Yes
Panel B			Post policy period		
β_{11}	-0.0142	0.0001		-0.0058	-0.0031
	(0.0157)	(0.0074)		(0.0342)	(0.0207)
CCT Bandwidth	1.395	1.725		2.052	2.085
N	1827	2092		2418	2380
Covariates	No	Yes		No	Yes
β_{12}	-0.0067	-0.0002		-0.0075	-0.0073
	(0.0188)	(0.0122)		(0.038)	(0.024)
CCT Bandwidth	1.233	1.219		1.905	1.906
N	1671	1621		2301	2252
Covariates	No	Yes		No	Yes

Note: The table contains coefficients and standard errors showing the estimated effect in percentage points variation in weekly mobility at the vicinity of the threshold point. The coefficients β_{11} and β_{12} refer to the weeks prior to the policy announcement. The 11th and 12th weeks cover the periods March 16-22 and March 23-29, respectively. Post-policy coefficients are β_{14}, β_{16}, β_{17} and β_{18} covering the periods April 6-12 (Holy week), April 13-19, April 20-26, April 27-May 3, May 4-10, respectively. The full set of controls includes population, municipality size, a dummy for urban areas, firm density, the per capita share of non-essential workers, the density of nonprofit organizations and the change in mobility during the first week of the lockdown. The bandwidths are selected using the procedure by Calonico et al. (2019).
Robust standard errors in parenthesis. * p<.10 ** p<.05 *** p<.01.

Table A.2: Kink estimates - Post policy period with no selected provinces

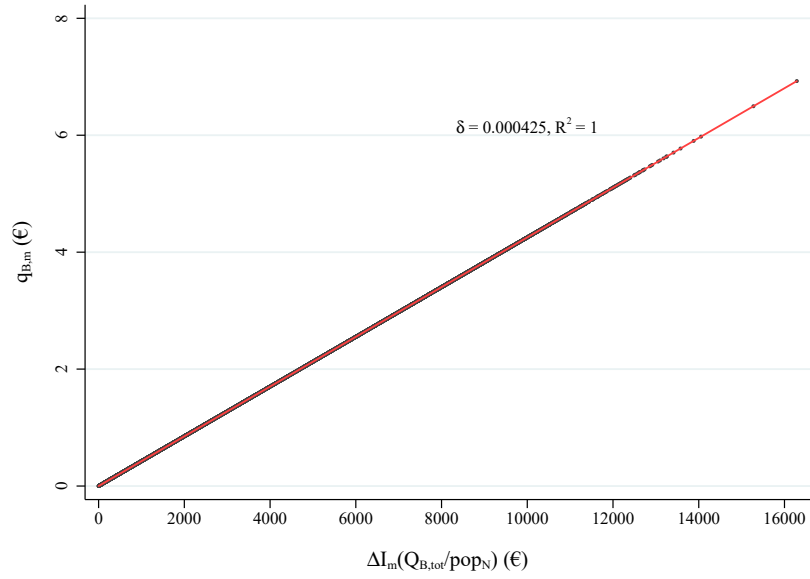| | Mobility Index | | | | |
| | Linear models | | | Quadratic models | |
	(1)	(2)		(3)	(4)
β_{13}	-0.0344**	-0.0243**		-0.0466*	-0.0327**
	(0.0170)	(0.0110)		(0.0258)	(0.0148)
CCT Bandwidth	1.199	1.220		2.347	2.654
N	1721	1793		2751	3019
Covariates	No	Yes		No	Yes
β_{15}	-0.0400**	-0.0246**		-0.0490*	-0.0351*
	(0.0159)	(0.0099)		(0.0259)	(0.0185)
CCT Bandwidth	1.304	1.601		2.379	2.634
N	1752	2015		2708	2831
Covariates	No	Yes		No	Yes

Note: The table contains coefficients and standard errors showing the estimated effect in percentage points increase of weekly mobility at the vicinity of the threshold point. The coefficients β_{13} and β_{15} refer to the 13th week (March 30 - April 5) and the 15th week (April 13-19), respectively. The full set of controls includes population, municipality size, a dummy for urban areas, firm density, the per capita share of non-essential workers, the density of nonprofit organizations and the change in mobility during the first week of the lockdown. The bandwidths are selected using the procedure by Calonico et al. (2019). Provinces having all municipalities either above or below the cut-off point are excluded from the sample.
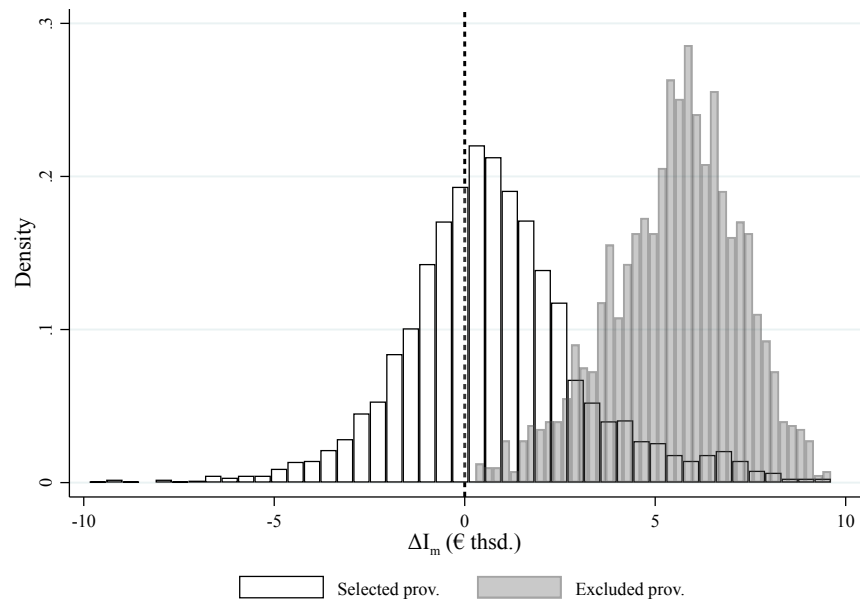Robust standard errors in parenthesis. * p<.10 ** p<.05 *** p<.01.

Appendix: Figures

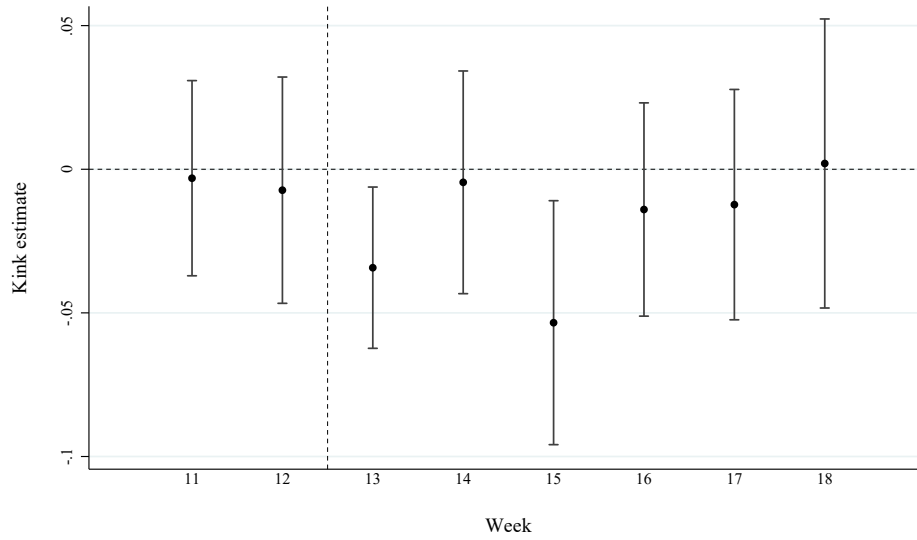Figure A.1: Testing the δ parameter

Note: The graph shows the estimated counterpart of the equation 5.

Figure A.2: Selection of provinces below and above the threshold
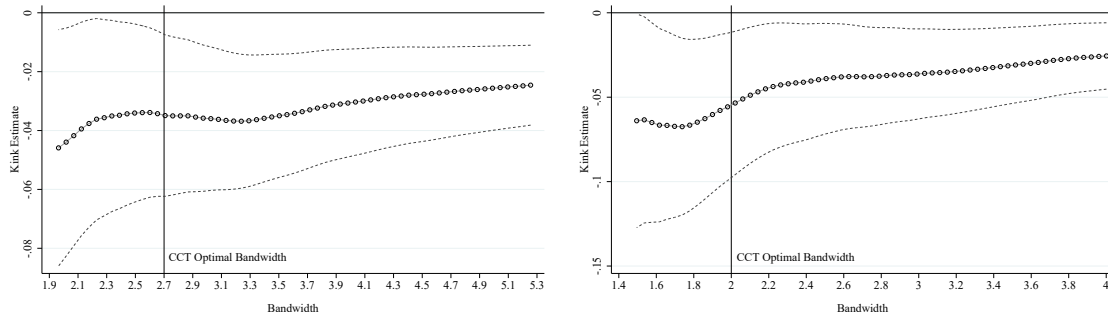

Note: The graph shows the density of municipalities' distribution with respect to the cut-off. The empty white bars define our main sample where we select provinces having municipalities both below and above the threshold point. The gray bars define provinces whose municipalities are either all above or all below the threshold point.

Figure A.3: Program's impact on mobility - robustness

Note: The figure shows the kink estimates and the associated standard errors using the CCT optimal band-width and a quadratic polynomial. Results are diplayed for the weeks preceding the policy announcement (11 and 12) and the weeks after (13 to 18).

Figure A.4: Kink estimates with varying bandwidths - robustness

(a) Week 13

(b) Week 15

Note: The figure shows the point estimates and 90 percent confidence intervals for the main effect (on the y-axis), using bandwidths between 1.9 and 5.3 for the week 13, and 1.4 and 4 for the week 15 (on the x-axis). The figure shows that the absolute value of the point estimate is robustly around 0.45 percentage points, regardless of the bandwidth. Narrowing the CCT bandwidth we obtain slightly lower values. The vertical lines roughly at 2.3 and 2.6 represents CCT optimal bandwidths recommended by Calonico et al. (2019).

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