More income, less crime
The case of Italy

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Abstract

I use administrative data to estimate the causal effect on crime of Reddito di Cittadinanza (RdC), the Italian minimum income scheme introduced in 2019. Whilst thefts decreased by 3.5 percentage points (pp), the other crimes remained constant. This result suggests that the income effect is a possible mechanism. To support this hypothesis, I estimate the effect of RdC on consumption, which increased by 3.5 pp. Therefore, an increase in consumption by 1 pp reduced crimes by the same percentage, especially for the households most in need. To identify the causal effects, I address identification issues using partial identification that delivers bounds.

JEL classification: I38; K42; C21
Keywords: Crime; Minimum income scheme; partial identification; bounds

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1 Introduction

In 2019, the Italian Government introduced the minimum income scheme "Reddito di Cittadinanza" (RdC) to reduce poverty, inequalities, and social exclusion. The benefit consisted of economic support to households below a given income level. According to official statistics from the Italian Statistical Institute (Istat), the poverty rate between 2018 and 2019 decreased from 7.0% to 6.4%. This rate increased in 2020 and 2021 because of the Covid-19 pandemic (7.5% in 2021), but much less than it would have done without RdC (Carta and De Philippis, 2021). Also, there is evidence that economic-related offenses are correlated with poverty rate and economic conditions (see for example de Blasio et al., 2016, for Italy).

In this paper, I investigate the presence of an additional effect of RdC. Besides poverty, did the policy reduce crime? If so, what were the mechanisms at work? I conduct the analysis using administrative data aggregated at the regional level. A distinctive feature of this study is the focus on the entire Italian population, without suffering from typical issues that may affect individual-level analyses, namely the sample selection bias, which may arise if the least able criminals are more likely to be captured.

The motivation behind the research questions is that individuals need a minimum amount of money to afford a basket of goods. If the individuals do not collect enough money, income from crime may fill the gap. In this scenario, RdC *causes* a reduction in crime because minimum income is a substitute for criminal income. The main challenge in identifying the causal effect in this analysis is that the Italian Government introduced RdC simultaneously in the entire country. Therefore, there is no natural counterfactual that researchers may use to disentangle association from causation. I address identification issues using partial identification methods (Manski, 1990). Partial identification allows me to relax some key assumptions necessary to build a counterfactual. Here, I exploit the "bounded variation" approach of Manski and Pepper (2013, 2018). Intuitively, I start as if the assumption of a specific design, e.g. the Regression Discontinuity (RD) in time, exactly holds. Then I add a measure of uncertainty. In this way, even though I do not know the true counterfactual, I can bound it within a range. The comparison between the actual outcome

after the policy and the range of the counterfactual delivers a set of admissible parameters for the treatment effects, going from a lower to an upper bound.

I find that RdC reduced thefts -i.e. crimes that provide money fast, like pickpocketing and burglary- by 3.5 percentage points (pp) in 2019. The impact is significant even after taking into account uncertainty through bounds. The effect was higher in Southern Italy (4.6 pp), lower in Center Italy (3.9 pp), and even lower in Northern Italy (2.7 pp). This rank is consistent with that of the share of the households that benefited from minimum income, which was higher in the South, followed by the Center and then Northern Italy. In 2021, as the share of beneficiaries increased, thefts declined by 20 pp compared to the pre-treatment period. Consistently with the idea that income effect is a possible mechanism, the other crimes remained constant. When I consider the causal effect of RdC on consumption in 2019, households' expenditure, for total and for necessary goods, increased by about 3.5 pp. With these results at hand, two further results follow. Firstly, increasing consumption by 1 pp reduced crime by the same percentage. This is a Local Average Treatment Effect (LATE; Angrist et al., 1996), which refers to households that consumed more thanks to RdC but not otherwise, i.e. compliers. Secondly, when I describe the households who enjoyed the benefit, they were the most economically disadvantaged in human capital endowments and labour market outcomes.

Some existing papers have studied the relationship between income-related policies and various crimes. The study most closely related to mine is Carr and Packham (2019), who use aggregated data to analyse the *causal* effect of the nutritional aid disbursement (SNAP) on crime in the US. Their identification strategy relies on staggered payments, a feature different from the Italian. They find that the policy reduced crimes and thefts at grocery stores by about 20%. Other papers use aggregated data to estimate the relationship between crimes and business cycle, showing the relevance of the economic motivation behind offenses. Among these papers, the most important for my analysis is de Blasio et al. (2016) who estimate the causal impact of the economic downturn on crime in Italy. They find that moving an area from the 75th to the 25th percentile of the distribution of the variation in economic conditions increased thefts by more than 5% in 2009. Differently from de Blasio et al. (2016), the largest majority of this branch of the literature exploits

unemployment variations to show a negative *correlation* between economic growth and crime. For example, Raphael and Winter-Ebmer (2001) analyze the relationship between unemployment and crime using US state-level data on seven felony offenses. They find a positive effect on property crime rates, but not on violent crimes. Oster and Agell (2007) study the impact of unemployment in Sweden during the 1990s. They find a significant effect on burglary and theft of cars; the unstable family life conditions represent a mechanism. Fougre et al. (2009) focus on France between 1990 and 2000, using regional-level data. They estimate a positive association between crime and unemployment. They demonstrate a causal effect for burglaries and thefts. When available, individual-level data reach conclusions similar to aggregated data. For example, for Brazil, Britto et al. (2022) find that unemployment benefits decrease the probability of committing a crime by more than 20% for men, but the effect vanishes after the benefit expires. Rege et al. (2019) estimate that job displacement increases criminal activity by 20% for young adult Norwegian men. The effect is particularly high for property-related offenses, coherent with the idea that displaced workers substitute work with crime to replace lost earnings. Still related to the substitution between different sources of earnings, Fone et al. (2023) and Beauchamp and Chan (2014) consider the effect of an increase in the minimum wage on crime. Both papers find an increase in criminal activity, motivated by the smaller job opportunities for workers more at disadvantage. Finally, there is evidence that means-tested measures reduce crime more than universal cash transfers (Watson et al., 2020; Foley, 2011).

This paper makes empirical and technical contributions to the existing literature. Concerning the former contribution, this is the first paper that estimates the relationship between minimum income scheme and crime in a European country, considering Italy, the third largest economy in the European Union. Like in many other countries, in Italy, the minimum income scheme is one of the two pillars of income support to protect households from income shock and poverty, together with unemployment benefits. Differently from unemployment benefits, RdC is not linked to contributions and lasts longer (Coady et al., 2021). Therefore, the population coverage is larger. For this reason, the policy implications of the minimum income scheme and the unemployment benefits may differ. On the technical side, I investigate a policy introduced at the same time in the entire country. I use

aggregated data, which pose different challenges from the individual-level data. I exploit partial identification to identify a causal effect whereby the existing literature estimates correlations. As, most of the time, the data are available only aggregated, using bounds to solve identification issues in this circumstance may be of more general interest. Second, aggregated data allow me to bridge criminality to consumption, whereas all the existing literature can only produce suggestive evidence of the relationship. Finally, I describe the characteristics of the beneficiary households. These characteristics are of policy relevance because they may suggest the most appropriate design to target the desired population. They may also be informative on the differences between means-tested policies and universal cash transfers.

The paper is organized as follows. In Section 2, I present the policy under scrutiny, the Italian minimum income scheme "Reddito di Cittadinanza". In Section 3, I describe the administrative data. In Section 4, I introduce the empirical methods that allow me to identify the parameters of interest for the empirical application of Section 5. Finally, Section 6 offers some concluding remarks.

2 The Italian Minimum Income

Minimum income schemes are means-tested cash transfers to individuals or households. Until 2018, in the European Union, only Greece and Italy had no such scheme (Frazer and Marlier, 2016). In 2018, Italy introduced a minimum income named *Reddito di Inclusione* (Inclusion income; ReI). In April 2019, the new scheme *Reddito di Cittadinanza* (Citizens' income, RdC) replaced ReI.[1]

RdC is an active labour policy designed to guarantee the right to work and to reduce poverty and inequality. It offers economic support to households at risk of social and job marginalization. A similar measure exists for retired households, but, for the present paper, the distinction

[1] A comparison between ReI and RdC is in Curci et al. (2020). ReI provides a limited amount of money (up to 250 euros), for a short period (6 months). De Angelis and Van Wolleghem (2023) show that the take-up of ReI was limited, and conclude that the policy did not work well. Therefore, I proceed as if the Government never implemented ReI. In a robustness check, I show that this assumption is not restrictive because ReI had no effect on crime. If anything, imposing that ReI had no impact would make the estimated effect of RdC lower than the true effect. To see this, suppose that ReI reduced crime. By ignoring it, I would attribute a lower number of crimes to the pre-treatment period. In this case, I would mechanically reduce the difference between post- and pre-treatment criminal activity and therefore the estimated effect of RdC.

is irrelevant. To access the RdC, households must satisfy some socio-demographic and economic conditions. Among the former, citizenship is the most relevant. Among the latter, the most important is the Equivalent Economic Situation Indicator (*Indicatore della Situazione Economica Equivalente*; ISEE). The economic benefit of the RdC consists of two components: the integration of the household income, up to 6,000 euros per year and, if needed, the payment of rent, up to 3,360 euros per year. Therefore, the total benefit is no higher than 780 euros per month. The RdC extends for 18 consecutive months, with the possibility of renewal following an interruption of one month. In contrast to similar measures in other countries, like the US (Carr and Packham, 2019; Foley, 2011), all beneficiaries receive the benefit on the same days of the month. The benefit is credited to an electronic payment card, named "RdC card". Therefore, there is no movement of cash.

According to INPS (2023), between 2019 and 2021, about 5% of Italian households had access to RdC. There is a large consensus that the measure did not work well as an active labour policy (and I provide further evidence of this), but much better to reduce poverty. Curci et al. (2020) estimate that the Gini index of the equivalised disposable income decreased by more than one percentage point after the introduction of the scheme (from 35.3 to 34.2%).

To limit the impact of the Covid-19 pandemic, between March and September 2021, the Government provided Emergency Income (*Reddito di Emergenza*; REm), a temporary monetary support to households not covered by RdC. RdC and REm were similar, although the requirements of REm were slightly weaker than those of the former. According to official statistics, REm reached about 2% of the households, with the same geographical distribution as RdC. Given their similarity, I do not distinguish the two measures unless needed. Importantly for the rest of the paper, no other reform of the welfare system (or justice) occurred in the analysis period.

3 Data

I use different administrative datasets. I investigate how RdC affected crime and the possible mechanisms. The data regarding RdC rely on the official statistics from the Italian Social Security

Institute (INPS). They cover the whole population of beneficiaries in Italy. Because individuals are responsible for the request of RdC, some ineligible individuals may enjoy the benefit. There is no possibility to adjust for this. However, according to official documentation from Guardia di Finanza (2022), i.e. the police responsible for detecting these frauds, the share of undue beneficiaries is negligible (about 1%).[2] The potential consequences for the identified effects are correspondingly negligible. Also, the average ISEE, i.e. the most important economic condition to enjoy the benefit (Section 2), remained broadly constant or improved after the introduction of RdC (INPS, 2024). This feature mitigates the concern for manipulation around the threshold. Indeed, for cheating to be a mechanism, economic conditions would have had to worsen. INPS releases data for the entire year t on January of year $t+1$, starting in 2019. The units of observation are the 20 Italian regions, although some information is available also by provinces.

In Figure 1-a), I show the share of individuals who benefited from the RdC over time by macroregion.[3] Two facts emerge. Firstly, in 2019 in Italy, 4.5% of the population accessed RdC. The percentage increased between 2019 and 2021 (to more than 6%). Secondly, the heterogeneity in the shares across macroregions is substantial. The South is always the area with the highest share of beneficiaries (more than 10%) and the North with the lowest (about 3%). Also across regions, the heterogeneity is substantial, with Campania, Sicilia, and Calabria recording the largest share of beneficiaries over the population (INPS, 2023).

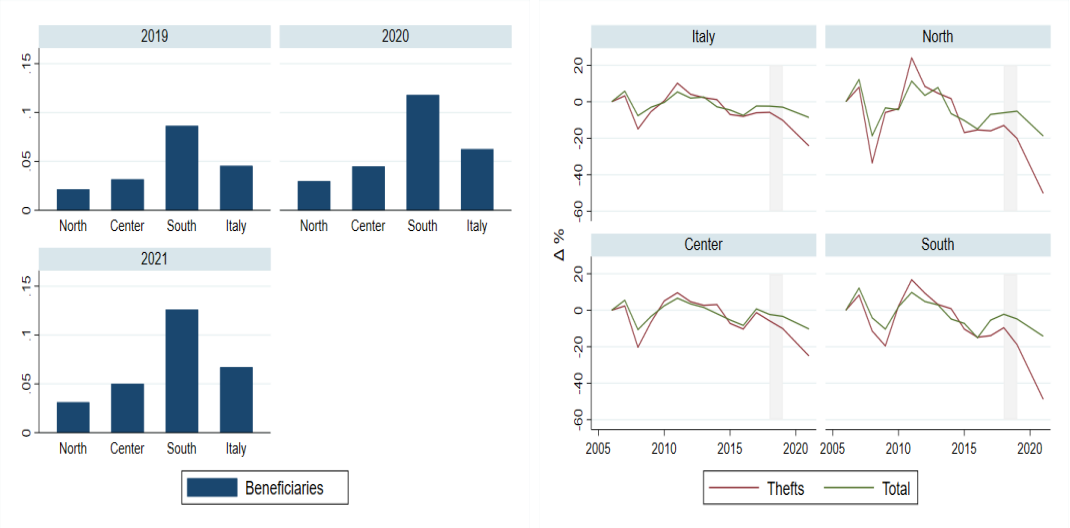
The second dataset that I use is on crimes committed in Italy. According to Italian law, the lawsuit, i.e. the declaration with which the victim expresses the willingness to proceed against the offender, must be filed within three months after one becomes aware of the event (12 months for sexual harassment). Then, there may be a sentence. One may consider various steps of the process between when the crime is committed and when the sentence is issued. Therefore, three aspects are worth discussing: the timing when the crime is observed, the available details, and the units of observation.

The data cover the entire Italian population and are available through the National Statistical

[2] The share is most likely upward biased because the investigations of Guardia di Finanza are not at random.

[3] Macroregions are: "North": Emilia-Romagna, Friuli-Venezia Giulia, Liguria, Lombardia, Piemonte, Trentino Alto Adige, Valle d'Aosta, Veneto; "Center": Lazio, Marche, Toscana, Umbria; "South": Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sardegna, Sicilia.

Figure 1: Evolution of beneficiaries of RdC and crimes, by macroregion and year



(a) RdC

(b) Crimes

Institute (Istat), with a lag of 2 years. The primary source of the information is the judicial authority, which cooperates with the police (i.e. the State Police, Arma dei Carabinieri, and Guardia di Finanza). The administrative nature of the data warrants that I consider all the crimes without measurement errors or item-/unit-non-response.[4] I observe all the steps of the judicial process. However, to investigate the impact of the RdC on crime, what matters is that a crime is committed, not the identity of those who committed it. Therefore, I consider the time when the crimes are committed. Istat releases a detailed list of crimes, which includes thefts (see Table B1 for the complete list). For the latter, a further breakdown is available: bag-snatching, pickpocketing, burglary, shoplifting, theft from vehicles, theft of art objects, theft of cargo trucks carrying freights, moped theft, motorcycle theft, and car theft. Finally, the units of observation are the provinces and the regions between 2006 and 2021. For crimes, with the data at hand, considering provinces in this analysis would likely be wrong. Criminals move to maximize the return to crime (Braakmann et al., 2023). However, Kirchmaier et al. (2023) find that the cost of distance is very high, especially

[4] In principle, the under-reporting of crime is possible. However, Istat (2019) shows that the under-reporting behaviour remains constant over time, and, for thefts, is not an issue. Therefore, the consequences for the empirical analysis are negligible. Given the empirical strategy that I use (Section 4), if under-reporting remains constant or even proportional to the underlying true number of crimes, then there is no consequence on the estimated effects.

for crimes with low financial gain. Therefore, province-level data would wrongly assign (part of) the variation in crime committed by individuals of province i to other provinces $\neq i$, because the distance between different provinces is typically small. With regions, this risk is greatly mitigated as it applies only to boundaries, which, in Italy, often coincide with rivers, mountains, and seas.

In Figure 1-b), I plot the time series of percentage change in thefts and total crimes in Italy and by macro-regions. Three results stand out. Firstly, the two variables moved together before the introduction of the RdC. Secondly, in 2019, after the introduction of the minimum income, both aggregates reduced sharply. Thirdly, in all the areas, the reduction in thefts in 2019 is higher than in total crime. (One may also consider thefts as a share of total income to reach similar conclusions; see Figure B2.) Therefore, there is descriptive evidence that crimes, namely thefts, decreased after the introduction of RdC.

The final data that I use are from the Household Budget Survey (HBS). The HBS is a survey harmonized at the European Union level that aims to measure and analyze consumption expenditure behaviours of households residing in Italy (Istat, 2015). HBS provides many details on expenditures.[5] Therefore, it is possible to investigate how households allocate their budget. Following Istat (2009), I identify a basket of necessary goods, required to live a worthy life. The basket includes food, clothes, housing, health, and education expenditures. All the expenditures refer to the time of purchase, regardless of the moment of actual consumption. This feature warrants a perfect correspondence between HBS and data on crimes. Being a survey, HBS provides some personal information at the household and individual levels. Among the former is the number of components; among the latter are demographic characteristics (gender, age, marital status, and citizenship), educational attainments, and occupational information (e.g. sectors, occupation, or type of contract). The income level is not available.[6] The finest geographical detail of HBS is at the regional level. According to official documentation, even though these data are from a survey, they are considered so reliable that they provide the weights for the goods that belong to the

[5] The finest detail includes: Food and non-alcoholic beverages; Alcoholic beverages and tobacco; Clothing and footwear; Housing, water, electricity, gas and other fuels; Furnishings, household equipment and routine household maintenance; Health; Transport; Communication; Recreation and culture; Education; Restaurants and hotels; Miscellaneous goods and services.

[6] The decile of equivalent income (Carbonaro, 1985) is available from 2019 onwards. Therefore, the information covers only the policy period.

basket of the consumer price index (CPI) and the households' final consumption for the National Accounts System (Eurostat, 2013), both of which are scopes of administrative level. Finally, in the official documentation, Istat provides a breakdown of the population by quintiles of equivalent expenditure. I follow their approach to split the population.

From these datasets, I always dropped 2020 because Italy implemented a strict stay-at-home policy in response to Covid-19. During 2021, the legislative limitations did not affect individual mobility - at least within the same region. For this reason and because the finest common geographical detail is the region, this is the unit of observation in the empirical analysis.

4 Empirical strategy

Identifying the causal effect of the RdC is difficult with aggregated data because the treatment started for all the regions at the same time. Therefore, it is unclear what is the appropriate counterfactual, i.e. units identical to those treated apart from the treatment status. The quantity of interest is:

$$ATE = E[y^*_{i,1} - y^*_{i,0} | d = 1], \tag{1}$$

where, for each region i, $y^*_{i,d}$ is the potential outcome under treatment $d \in \{0, 1\}$, with $d = 1$ for treated and $d = 0$ for control periods. Define $y_{i,d}$ the actual outcome. The inferential problem is that the analyst observes only $y^*_{i,1} = y_{i,1}$ for treated and $y^*_{i,0} = y_{i,0}$ for control periods. If I had access to individual data, I could invoke useful assumptions, e.g. the parallel trend (Roth et al., 2023), and compare the criminal activity of those who benefited from RdC vs those who did not. Given the characteristics of the data and the policy, after the introduction of RdC I observe $y^*_{i,1} = y_{i,1}$ for all the regions, at the same time. Therefore, there is no natural counterfactual. To this aim, one might consider using specific (group of) crimes, e.g. total crimes net of thefts. Unfortunately, with this strategy, the estimated parameters would be biased in an unpredictable way. Estimates of ATE would be *downward* biased if different crimes are linked through unobservable characteristics, for example, if thefts are a "leading crime", which Granger causes the others. ATE can be *upward* biased because the police supply is fixed. If thefts decline, police can focus more on other crimes

(Mastrobuoni, 2020), reducing the probability of success for criminals. Standard economic models predict that crimes decrease in these circumstances (Becker, 1968; Ehrlich, 1973). If minimum income affects other crimes (Ferraz et al., 2022), the bias of ATE may be upward or downward.

For these reasons, the best solution is to impose the "time invariance" assumption that, in the absence of RdC, the evolution of crime would have been unchanged, and compare a region after the reform with itself before it, like in a Regression Discontinuity Design (RDD) using time as running variable. However, with yearly data, the continuity at the cut-off date (Hausman and Rapson, 2018), which is central to RDD, is questionable because crimes far from the threshold contribute to the estimate. To address the concerns on continuity and time invariance, and more generally those regarding the identifying assumptions, I exploit partial identification as in Manski and Pepper (2013, 2018).

Partial identification is a novel approach to investigating minimum/universal income and crime. Examples in crime are Manski and Pepper (2018), Dix-Carneiro et al. (2018), and Possebom (2023). Manski and Pepper (2018) do not assume that all the required assumptions of an estimator are perfectly satisfied but that they are violated only to *some extent*. After defining the size of this violation, one may derive bounds. To be more precise, Manski and Pepper (2013) introduce the "bounded time variation", which relies on an "uncertainty parameter" (δ).[7] The uncertainty parameter is a positive constant specified by the researcher that tells by how much, at most, the assumptions are violated, i.e. $|y^*_{i,0} - y_{i,0}| \leq \delta$. Then, the counterfactual $y^*_{i,0}$ is bounded by

$$y^*_{i,0} = y_{i,0} \pm \delta. \tag{2}$$

By substitution of eq. 2 in eq. 1, it follows that the bounds of ATE are

$$ATE \equiv y^*_{i,1} - y^*_{i,0} = y_{i,1} - (y_{i,0} \pm \delta) = (y_{i,1} - y_{i,0}) \mp \delta. \tag{3}$$

A great advantage of this estimator over standard approaches is that the reasons why $\delta \neq$

[7] Manski and Pepper do not introduce a name for δ. Also, they consider the case where *all* the individuals in a region are treated or not. In Appendix D, I show that their method works even if only a portion of the population is treated, like in my setup.

0 (i.e. exact invariance does not hold) need not be known. Namely, exact invariance may be violated because continuity or time invariance do not hold or the conditional expectation function is misspecified. Furthermore, for exposition purposes, in eq. 2, I used the same δ for the upper and lower bounds. One may use asymmetric δs for the two - as I do in the empirical application. A potential disadvantage of the estimator is that one must set a priori the size of the uncertainty. However, the larger the δ, the more credible the identified bounds (Manski, 2011). All the existing literature searches over a grid where δ is free to vary, using the pre-treatment evidence as a guide for the limits of the grid (I provide more details on this in Section 5).

As in Manski and Pepper (2018), I do not perform statistical inference because I observe the entire population of interest.[8] Examples of empirical applications with these bounds include Manski and Pepper (2018), Depalo (2019), Mechoulan (2020), Sadayuki and Arimura (2021), and Wang et al. (2022).

In Appendix C, I provide more details on the estimator and the setting of δ. Figure C1 offers a graphical representation.

5 Results

In this section, I provide a reference theoretical background that guides the empirical application (Section 5.1). Then I apply the bounds of Manski and Pepper (2013) on crimes (Section 5.2) and households' consumption (Section 5.3). Finally, I bring together the two pieces of the analysis to estimate the impact of consumption on crime (Section 5.4).

5.1 Reference theoretical background

Throughout the analysis, I use a simple theoretical framework consistent with Becker (1968) and Ehrlich (1973). A generic household has a utility function that depends on two (bundles of) goods, one necessary ($X_{Nec.}$) and one non-necessary ($X_{NONNec.}$), i.e. $U(X_{Nec.}; X_{NONNec.})$. To satisfy the

[8] Manski and Pepper (2018, p. 234) explain that "a fundamental reason for not performing statistical inference is that measurement of statistical precision requires specification of a sampling process that generates the data. Yet we are unsure what type of sampling process would be reasonable to assume in this application. One would have to view the existing [Italian regions ...] as the sampling realization of a random process defined on a super-population of alternative [regions ... The literature] has not engaged the question."

utility function, the household needs a budget of at least Y^1 but has access to $Y^0 < Y^1$ (Figure B1). Income from crime may fill the discrepancy $\Delta = Y^1 - Y^0 > 0$.

Although the model does not distinguish among different types of crime, those filling Δ should be 'easy', i.e. with a high probability of success, and provide money fast. For criminals, thefts have these characteristics.

This simple framework provides insights into the empirical approach and delivers the most relevant predictions. As for the empirical approach, aspects that affect the utility function are the source of the economic shock, the rapacity effect (Ferraz et al., 2022) -i.e. the increase in overall income increases the probability that the return to criminality is positive, which makes crimes more palatable from an economic point of view-, and the context. The ATE of eq. 1 should include the economic shock and the rapacity effect because they are a consequence of the RdC. Instead, an appropriate model specification should control for the context, like cultural factors that may increase or reduce the attitude towards criminality.

The theoretical background delivers relevant predictions. RdC provided income to close the income gap Δ for poor households. Hence, the income effect is a potential channel through which RdC reduces crime. However, the rapacity effect works in the opposite direction. Therefore, even though the rapacity effect is unlikely, because the RdC targeted the poorest households and provided the benefit through an electronic payment card without affecting the amount of cash in the streets (Wright et al., 2017), the consequences of the minimum income ultimately are an empirical issue.

To summarize, I test the hypothesis that RdC caused a reduction in crime and, specifically, in thefts, thanks to the income effect.

5.2 The effect on crime

To test whether RdC caused a reduction in crime, the outcome of interest is the percent evolution in the number of thefts (y). The percent evolution represents a natural normalization that allows me to compare different areas.[9] I use the first difference of the dependent variable to control

[9] Whilst the results are fully confirmed without this normalization (available upon request), the comparison of the change in levels across different areas would be problematic because the absolute numbers of crimes are extremely different across macroregions. In the North, the average number of thefts is about 700,000 per year compared to

for the region-specific fixed effects (α). Fixed effects capture heterogeneity in the unobservable characteristics, like population structure, policy department resources, and context (Ferraz et al., 2022). Also, as these characteristics remain relatively stable over the period of this analysis, fixed effects greatly reduce the possibility that the uncertainty parameter (δ in eq. 2) captures model misspecification. Therefore, δ mainly handles departures from exact invariance and, more generally, from the ideal identifying assumptions. The reference model is:

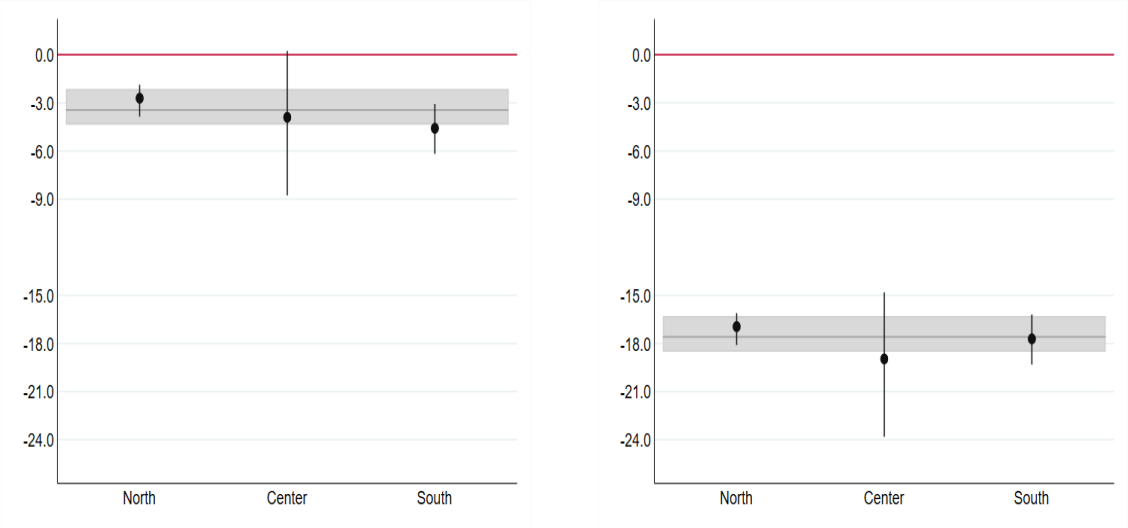
$$E[\frac{\Delta y_{i,t}}{y_{i,t-1}}] = 1(RdC_{i,t})\,\beta + \alpha_i, \tag{4}$$

indexed over region i in year t; $1(\cdot)$ is an indicator function taking value 1 if the event in parentheses is verified. Being the benchmark in the literature based on aggregated data (for example, see Carr and Packham, 2019; Palmer et al., 2019; Watson et al., 2020; Foley, 2011), this is my preferred specification. Yet, the existing literature emphasizes the role of the business cycle (for Italy, see de Blasio et al., 2016). Moreover, a straightforward extension of the theoretical model in Becker (1968) suggests that the larger the population size, the larger the opportunity for crimes, e.g. rapacity effect. Therefore, I checked the results also controlling for the GDP and the population size (the results are remarkably stable when I add this information; see Appendix B). Finally, the linear representation of eq. 4 is useful, although more restrictive than what I do. I am interested in ATE = $\mathrm{E}[y_1^* - y_0^* | d = 1]$. Under the linear representation, which imposes the exact invariance assumption, $y_0^* = y_0$, and $\mathrm{E}[y_1 - y_0 | d = 1] = \beta$. Under bounded variation, $y_0^* = y_0 \pm \delta$, and $\mathrm{E}[y_1 - y_0 | d = 1] = \beta \mp \delta$.

The results that exploit bounded variation are in Figure 2. The units of observation are the regions (Section 3). The figure presents the bounds aggregated by macroregion. For each macroregion, the vertical line represents the bounds with their own δ; the boldface point is the treatment effect imposing exact invariance, i.e. $\delta = 0$. The shaded area reports the bounds for Italy; the boldface line imposes exact invariance.

300,000 in the Center, and 350,000 in the South. Below, I take into account the population size.

Figure 2: RdC reduced thefts in 2019 and in 2021, in the entire country



(a) 2019 (b) 2021

Note: The figure plots the ATE of number of thefts as the dependent variable, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2018. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

Two main results follow. Firstly, soon after the introduction of the RdC in Italy in 2019, the number of thefts decreased by 3.5 percentage points (boldface line). These results remain negative even for large δs, i.e. if the uncertainty in the definition of the counterfactual is sizable (shaded area). To increase the credibility of the bounds, I present results with asymmetric δ, equal to the largest and the smallest per cent variation of thefts between 2015 and 2018.[10] For a comparison, Manski and Pepper (2018) use δ based on the 75th percentile. However, some researchers may prefer a larger size of the "uncertainty parameter" associated with the potential outcome. To this aim, in Figure B5, I plot the bounds for $\delta \in [0, 3]$ percentage points for both the lower (left panels) and the upper (right panels) bounds. The lower bounds are always remarkably negative. By construction,

[10] I start in 2015 because that year was not affected by the recessions that severely hit Italy between 2008 and 2014, a period also known as double-dip recessions (Accetturo et al., 2022). The results do not depend on the time definition. In a robustness check, I also added a time trend, obtaining a stronger effect: for Italy, the estimated reduction of thefts would be 4.8% in 2019 and 20.1% in 2021. For this reason, and because, between 2015 and 2018, the time series of thefts is flat (Figure 1a), I present the results without time trends.

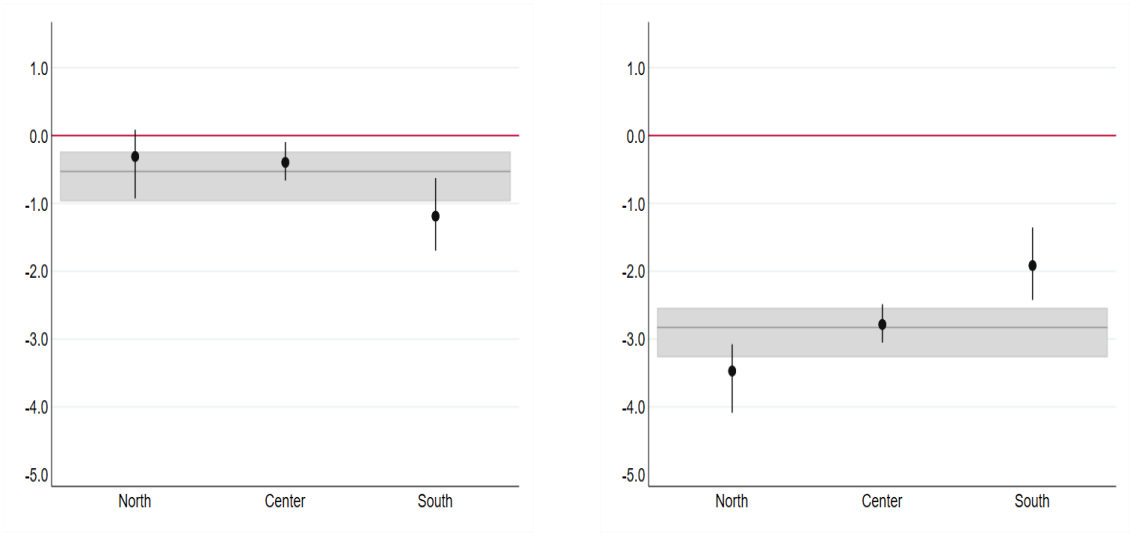
the upper bounds move towards zero as δ increases. However, even with $\delta_{Upper} = 3$ pp, the upper bounds remain negative. Only in Northern Italy, the ATE would be basically 0 (Table B2), but the uncertainty $\delta_{Upper} = 3$ pp is more than twice the largest per cent change observed before the implementation of the RdC. Although possible, this uncertainty seems too large and, therefore, unlikely (Manski and Pepper, 2018).

The second important finding is that, as the share of beneficiaries increased between 2019 and 2021, thefts reduced more markedly in 2021, by approximately 20 percentage points. The magnitude of the effect is similar to that of other targeted measures (Carr and Packham, 2019) and higher than those estimated for universal transfers (Watson et al., 2020; Foley, 2011), suggesting that targeting plays a central role. The effect of 2021 may also reflect different individual preferences, possibly induced by the Covid-19 pandemic - including the introduction of REm. Addressing this concern with the bounds of Manski and Pepper (2018) is extremely simple because it only requires an increase in the size of the "uncertainty parameter" δ. Specifically, for the true effect to be zero, δ must be 20 pp, i.e. ten times larger than that observed during the pre-treatment period. This size is very unlikely.

Based on the above results, I conclude that RdC caused a sizable reduction in thefts.

Two possible threats to identification of the previous findings are that crimes may decline thanks to better labour market conditions rather than the RdC and that I identify the compounded effect of ReI and RdC (Section 2). To address the former concern, I repeated the same analysis using the employment rate as the dependent variable (Figure 3). I do not find evidence of confounding effects from employment, which remained stable or even worsened. Incidentally, this result confirms that RdC did not work well as an active labour policy (Curci et al., 2020). As for the ReI, I run a placebo test that anticipates the treatment in 2018. The test confirms that there was no reduction in crime before RdC (Figure B4). Therefore, in this analysis, imposing that ReI was not effective is not problematic (as argued in De Angelis and Van Wolleghem, 2023).

Figure 3: The reduction in thefts does not depend on better labour market conditions (higher employment rate), in any macroregion



(a) 2019 (b) 2021

Note: The figure plots the ATE on the employment rate as the dependent variable, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2018. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

To gain some preliminary insights into the mechanisms, I investigate heterogeneity along two dimensions: by geographical area and by type of theft. In 2019, the effect of the RdC was higher in the Southern regions, followed by the Center, and then the Northern regions. In 2021, the effect for the Southern and Center regions was almost identical. By regions, I find a negative rank correlation between the share of the population who access the RdC and the reduction in crime (Figure B6). In the regions where a higher fraction of the population benefited from RdC, the number of thefts decreased more. The rank correlation is -0.218 in the whole sample, also reflecting the cases of Sicilia, where the local criminal organization, namely the mafia, might have contrasted "unauthorized" thefts (Daniele and Le Moglie, 2020), and Valle d'Aosta, where the number of crimes is usually so low that even small changes make huge differences. If I exclude these regions, the rank correlation goes to -0.986. The negative rank correlation is coherent with the fact that the smaller the share of households in need, the smaller the criminal activity (de Blasio et al., 2016).

The second dimension of heterogeneity that I explore is by type of theft (Figure B7). In both years, but especially in 2021, crimes that provide fast money, i.e. that do not require a re-sell of the stolen goods, decreased more. These are pickpocketing, theft of goods from vehicles, shoplifting, and burglary.[11]

Finally, I estimate eq. 4 for all the crimes net of thefts and find that zero is always included in the bounds (Figure B8).

Overall, this analysis is coherent with money, namely the income effect, as a driver of crime reduction. In the next section, I provide more formal evidence in favour of this interpretation.

5.3 The effect on households' consumption

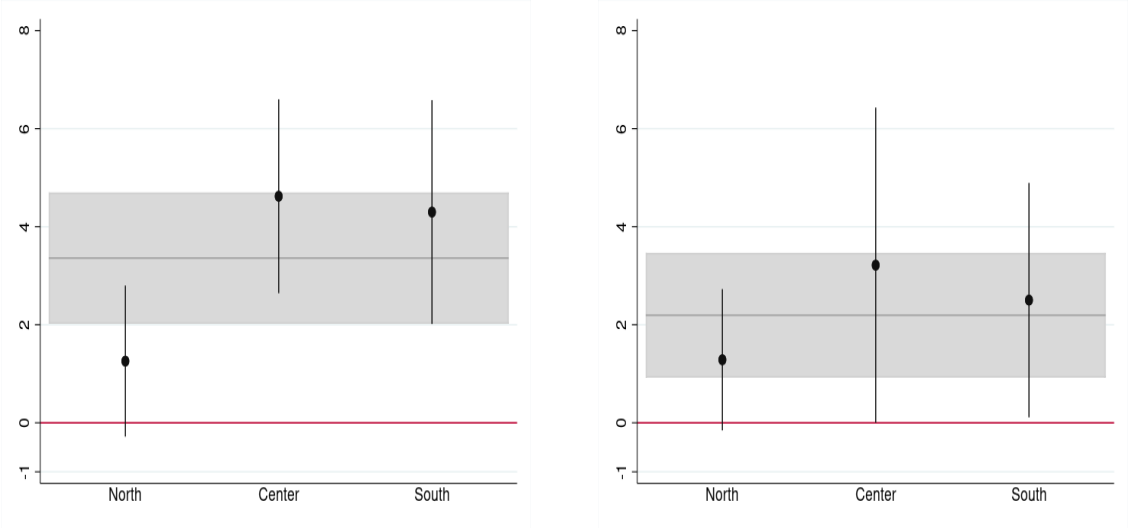
To formally support the hypothesis that the income effect is a possible motivation behind the reduction in crime, I investigate how the expenditure behaviour changed after the introduction of RdC. I use the HBS data on expenditures. Only low-income households had access to the minimum income (Section 2). Therefore, it is natural to focus on them. As HBS provides information on income starting in 2019, i.e. only for the treatment period, I consider "low-expenditure" instead of "low-income" households. In Section 5.4, I show that the conclusions based on this proxy are appropriate. Moreover, I evaluate the treatment effect only for 2019. Banca d'Italia (2022) warns that, in 2021, consumption decreased because of the fear of contagion, even in the absence of restrictions to mobility.

Like in the official publications from Istat (Section 3), low-income households are those in the first quintile of equivalent expenditure. In Figure 4, I estimate eq. 4 using the percent evolution of their expenditures as the dependent variable. Panel a) is for total expenditures, and Panel b) is for necessary goods.[12]

[11] The number of thefts of trucks is so small that even a small change makes a lot of difference, so that this exception is not concerning. Notice that, in the figure, I set $\delta = 0$ for easier readability, without loss of information (results with $\delta \neq 0$ are fully consistent with those presented here and they are available upon request).

[12] The bounds with the complete grid of the "uncertainty parameter" δ are in Figure B10.

Figure 4: Income effect is a mechanism, in the entire country. RdC increased consumption in 2019.



(a) Total

(b) Necessary

Note: The figure plots the ATE on expenditure by type consumption as the dependent variable, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2018. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

Both expenditures increased thanks to RdC. In Italy, total expenditure was 3.4 pp higher than before RdC; the increase in necessary expenditure was 2.2 pp. In both cases, the increase in the Center and Southern Italy was similar (4.5 pp for total expenditure and 3 pp for necessary goods) and remarkably larger than in Northern Italy (about 1 pp for both expenditures); for all the macroregions, bounds that account for the uncertainty in the definition of the counterfactual shift up or down preserving this regularity.

The results on consumption expenditure are consistent with the heterogeneity analysis of Section 5.2, and both confirm that the income effect associated with the RdC is a possible mechanism. Also, this analysis unveils that households allocated part of their additional expenditure to (what Istat defines as) non-necessary goods.[13] This finding is of policy relevance to quantify how much money

[13] The results based on a finer definition of low-expenditure households, e.g. the first vigintile -which is coherent with the share of households having access to RdC (Section 3)- rather than on quintile, are more striking (Figure B9).

households need to match basic needs, i.e. to fill the gap Δ in Section 5.1. This information would help allocate public resources more efficiently, especially in the current debate where the Italian Government replaced RdC with AdI while considering further support to households not covered by the new scheme.[14]

5.4 The reaction of thefts to consumption

Previous sections show that RdC caused a reduction in thefts and an increase in consumption, which is consistent with the income effect as a mechanism. In this section, I put together the two analyses to answer the following question of policy relevance: how high was the reaction of thefts to consumption? A regression of the latter on the former may provide a biased estimate because unobservable confounding factors, like attitude or preferences, may jointly influence the two variables. For this reason, it is convenient to consider RdC as an instrumental variable. This policy is externally set and pre-determined. Sorting is still possible, but only as cheating, which is negligible (Guardia di Finanza, 2022). Consistently, official statistics on the economic conditions, namely the ISEE, which slightly improved over time, are in stark contrast to the cheating channel (Section 3). These features and the absence of other welfare system or justice reforms during the analysis period imply that RdC is a valid exclusion restriction (Angrist et al., 1996).

Define the introduction of RdC as Z, thefts as Y, and consumption as C. From Sections 5.2–5.3, I obtain $E[Y|Z=1] - [Y|Z=0]$ and $E[C|Z=1] - [C|Z=0]$. With these quantities, under the assumptions of Angrist et al. (1996), I estimate a Local Average Treatment Effect (LATE).[15] This LATE captures the effect on thefts for households who change their consumption expenditure because they enjoy RdC but not otherwise. Following the standard terminology, I call these households "compliers". For compliers, an increase in consumption causes a reduction in thefts

[14] In 2024, a new minimum income scheme called "Assegno di Inclusione" (AdI) replaced RdC. Although the Government relaxed some conditions, e.g. citizenship, others became tighter. Firstly, only households with an individual disabled, or younger than 18, or older than 59 years are eligible. Secondly, some economic requirements became more restrictive, e.g. rents no longer increase the benefit and renewals is of 12 months instead of 18. According to Bovini et al. (2023), the new requirements reduced the number of potential beneficiaries by 40%.

[15] Besides exogeneity, the other relevant assumption is "monotonicity". The assumption requires that none of the households that enjoy the RdC reduces consumption because of the benefit. With the data at hand, this assumption is untestable. However, the only possible threat is related to cheating. Ineligible households might decide to save the benefit, in the case that the police detects the fraud. These cases amount to 1% of the cases (Guardia di Finanza, 2022), therefore the issue is negligible. For a more formal argument in this direction, see de Chaisemartin (2017).

in all the macroregions. At the national level, the reaction of thefts to total expenditure is -1.0 (=-3.4/3.4), and to the necessary expenditure is -1.5 (=-3.4/2.2). Therefore, a policy that increased expenditures, especially for necessary goods, was also effective in reducing thefts. Two aspects are worth stressing. Firstly, the elasticity of crime to earning -instead of consumption- may be smaller (Britto et al., 2022; Campaniello and Gavrilova, 2018). This is a direct consequence of the fact that households may use income for other reasons beyond consumption, including saving. Secondly, I identify the effect for compliers. The response of these households is likely stronger than in the rest of the population (just like the return to education is higher for marginal individuals; Card, 1999). Therefore, it would be interesting to describe the characteristics of the complier households. This description is impossible with the data at hand, but I can describe the characteristics of the households most in need, i.e. eligible for RdC.

In Table 1, I describe the characteristics of the households in the first quintile of equivalized expenditure. The linear probability model controls for several households' characteristics, namely the region of residence, number of components, family types (e.g. married/non-longer married/never married couple), the distribution by gender, and age.[16] From Col. 1 of Table 1, households that spend the least are larger, live more frequently in Southern Italy, have lower educational attainments, are younger, non-married, and their labour market outcomes, measured as the number of individuals with a labour income, are worse. These are the households most in need to increase their consumption. They represent the natural target for policies that aim at reducing poverty and social exclusion (Almeida et al., 2022).

The above description relies on households' expenditures, not on their access to RdC. However, the Survey on Household Income and Wealth (SHIW; Appendix A) allows me to show that the qualitative conclusions from the two indicators are identical. In 2020, SHIW asked whether the household requested and had access to RdC. The survey provides several individual characteristics that overlap with the HBS. Unfortunately, the labour market outcomes (e.g. profession and contract type) refer to the moment of the interview and not to the time of the benefit. Because of this time

[16] I run two robustness checks. One controls for labour market outcomes (e.g. profession and contract type; see also Table B3). One controls for additional characteristics of the reference individual in the household (the one with the highest earning; available upon request). All the results are confirmed.

inconsistency, it would be wrong to control for them to explain RdC. I run the same probability model as above, where the outcome variable is the request of (Col. 2) or the access to (Col. 3) RdC. By comparison of cols. 1 vs 2–3 in Table 1, the patterns of the coefficients are identical. Since, for means-tested benefits, no paper can match the access to the benefit with consumption at the household level, which represents the ideal information, this exercise shows that the proxy of low-income households based on low-expenditure levels is a satisfactory second best. This also reassures that the analysis in Section 5.3, based on low-expenditure households, is legitimate.

Table 1: Characterization of households most in need

	HBS Consumption	SHIW RdC	
		Request	Access
Components	0.087 ***	-0.008	-0.013 *
	(0.005)	(0.009)	(0.008)
Women	0.008	0.014	0.009
	(0.009)	(0.015)	(0.014)
Area:			
Center	0.021 ***	-0.002	-0.001
	(0.007)	(0.008)	(0.006)
South	0.162 ***	0.036 ***	0.029 ***
	(0.008)	(0.010)	(0.009)
Education:			
Primary	0.236 ***	0.097 ***	0.077 ***
	(0.009)	(0.015)	(0.014)
Secondary	0.074 ***	0.027 **	0.020 **
	(0.008)	(0.011)	(0.010)
Age:			
< 6	0.083 *	0.120 **	0.135 **
	(0.044)	(0.061)	(0.057)
[6,14]	0.012	0.152 **	0.108 **
	(0.036)	(0.065)	(0.054)
[15,17]	-0.016	0.186 *	0.146
	(0.053)	(0.096)	(0.090)
[18,24]	0.181 ***	0.142 ***	0.106 ***
	(0.033)	(0.041)	(0.029)
[25,29]	0.225 ***	0.089 ***	0.066 ***
	(0.025)	(0.023)	(0.020)
[30,34]	0.185 ***	0.157 ***	0.139 ***
	(0.021)	(0.048)	(0.048)
[35,39]	0.161 ***	0.074 ***	0.046 ***
	(0.020)	(0.019)	(0.017)
[40,44]	0.166 ***	0.087 ***	0.081 ***
	(0.020)	(0.027)	(0.026)
[45,49]	0.098 ***	0.117 ***	0.097 ***
	(0.018)	(0.033)	(0.031)
[50,54]	0.100 ***	0.101 ***	0.058 ***
	(0.017)	(0.027)	(0.019)
[55,59]	0.072 ***	0.087 ***	0.064 ***
	(0.016)	(0.018)	(0.015)
[60,64]	0.024 *	0.098 ***	0.090 ***
	(0.014)	(0.026)	(0.025)
[65,69]	-0.043 ***	0.030 ***	0.025 **
	(0.013)	(0.011)	(0.010)
[70,74]	-0.050 ***	0.032 ***	0.016 ***
	(0.013)	(0.011)	(0.006)
# ind. with income	-0.109 ***	-0.022 ***	-0.013 **
	(0.006)	(0.008)	(0.006)
Marital status:			
Married	-0.018 *	-0.001	0.004
	(0.010)	(0.011)	(0.009)
No more married	0.028 **	0.015	0.001
	(0.011)	(0.009)	(0.008)
Intercept	-0.174 ***	-0.068 ***	-0.045 ***
	(0.015)	(0.015)	(0.013)

Notes: Linear probability models. The dependent variable in columns HBS takes value 1 if the household is in the lowest quintile of equivalised expenditure. The dependent variable in columns SHIW takes value 1 if the household benefited from RdC. ***[**][*] denote significance at the 1[5][10]% level. Standard errors are clustered at the municipality. Weighted

6 Conclusions

In 2019, the Italian Government introduced the minimum income scheme "Reddito di Cittadinanza" to assist the households most in need. This paper considers the effect of the benefit on crime. I test whether RdC caused a decline in crimes, and, more specifically, in thefts. I find that thefts decreased significantly (3.5 pp in 2019 and 20 pp in 2021), while the other crimes remained constant. The income effect is a mechanism behind the crime reduction. According to HBS, in 2019, total expenditure and expenditures for necessary goods increased for the poorest households by 3.5 and 2.2 pp. Therefore, an increase in consumption by 1 pp reduced crimes by the same percentage, for the households most in need. These households have lower human capital endowments and worse labour market outcomes, and RdC reached them. This is a strength of the Italian scheme because targeting improves the effectiveness of the measure. Yet, there is room for a more efficient allocation of public resources. These conclusions are robust to the uncertainty in the definition of the counterfactual, which I took into account using partial identification that delivers bounds.

This paper provides evidence to foster the debate on minimum income schemes, which is increasingly relevant in Italy and the European Union. In Italy, with the 2024 Budget Law, the Government replaced the RdC with AdI. The latter scheme is one of the most restrictive in Europe (Frazer and Marlier, 2016; Coady et al., 2021), and it excludes a large share of the RdC beneficiaries (Bovini et al., 2023). The present paper suggests that an increase in criminality, especially in thefts, is possible to compensate for the missing income (Buonanno et al., 2022; Ferraz et al., 2022). Not surprisingly, the Government promised further interventions in favour of households not covered by the new scheme. The composition of the additional expenditure after the introduction of RdC may provide information to design AdI without leaving households behind. The results of the present paper may be of more general policy relevance because the European Parliament asked the Member States to strengthen minimum income schemes with a formal Resolution (on 15 March 2023). The

motivations behind the Resolution focused on the relationship between minimum income and the labour market. This paper introduces another dimension to consider.

Whilst most of the existing literature about the causal effect of minimum income on crime considers the US, this is the first paper regarding a European country. Although the differences between Italy and the US are huge, the similarity in the results suggests that policymakers may learn lessons from other experiences. For example, the findings of Carr and Packham (2019) may suggest to the Italian Government to adopt staggered payments to RdC beneficiaries instead of specific dates common to all of them.

The present paper applies relatively recent techniques to identify a causal effect when the source of identification is not obvious, e.g. because data are aggregated and treatment starts at the same time for all the units. This is a common challenge. The partial identification approach of Manski and Pepper (2013) is a solution to evaluate the policy and inform the policymaker, even in this situation. Also, individual-level data may fall short exactly on this goal. They are fully informative *for the sample at hand*, but not necessarily for the population at large, even when the observable characteristics of the two are similar (Heckman et al., 2006). Individual data may also be contaminated by substantial measurement errors and systematic sample selection, e.g. if the police identifies more easily the least skilled criminals. Aggregated data on the entire population do not suffer from these potential drawbacks. Given the pros and cons of the two approaches, it is clear that the ideal data that jointly address all these limitations are currently unavailable. However, the similarity of the results between the present and the existing papers based on individual-level data is particularly reassuring for the credibility of both. Future research should combine both sources to obtain all the required information to inform the policymaker, produce cost-benefit analyses, and suggest the best possible design of similar policies.

7 Compliance with Ethical Standards

I declare that I have no conflict of interest.

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A The Survey on Household Income and Wealth (SHIW)

The Survey on Household Income and Wealth (SHIW) is conducted every two years by the Bank of Italy on a representative sample of the Italian population. The data contain information about a wide range of personal (age, gender, marital status, educational level, region of residency) and occupational (employment status, contract, and professional level) characteristics, for each component of the interviewed households. A sample of weights makes the sample representative of the target population and they are used in all the analyses. The data and more information are accessible from the dedicated website.

B Additional tables & figures

Table B1: List of crimes

Main group	Detail
mass murder	
Information intentional homicides	
	homicides for theft or robbery
	homicides of mafia
	terrorist homicides
attempted homicides	
infanticides	
manslaughter	
Information unintentional homicides	
	homicides from road accident
blows	
culpable injuries	
menaces	
kidnappings	
offences	
sexual violence	
sexual activity with a minor	
corruption of a minor	
exploitation and abetting prostitution	
child pornography and possession of paedo-pornographic materials	
Information thefts	
	bag-snatching
	pickpocketing
	burglary
	shoplifting
	theft from vehicle
	theft of art objets
	theft of cargo trucks carrying freights
	moped theft
	motorcycle theft
	car theft
	Information robberies
	house robbery
	bank robbery
	post office robbery
	shop robbery
	street robbery

Continues on next page

Main group	Detail
extortions	
swindles and cyber frauds	
cybercrime	
counteractions of goods and industrial products	
intellectual property violations	
receiving stolen goods	
money laundering	
usury	
damages	
Information arson	
	forest arson
damage followed by arson	
trafficking and drugs possession	
attacks	
criminal association	
mafia criminal association	
smuggling	
other crimes	
total	

Table B2: RdC reduced thefts, in the entire country - detailed bounds for 2019

δ_{Lower}	Lower bound	Upper bound δ_{Upper} 0.000	0.500	1.000	1.500	2.000	2.500	3.000
				Italy				
0.000	-3.444	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
0.500	-3.944	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
1.000	-4.444	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
1.500	-4.944	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
2.000	-5.444	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
2.500	-5.944	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
3.000	-6.444	-3.444	-2.944	-2.444	-1.944	-1.444	-0.944	-0.444
				North				
0.000	-2.709	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
0.500	-3.209	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
1.000	-3.709	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
1.500	-4.209	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
2.000	-4.709	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
2.500	-5.209	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
3.000	-5.709	-2.709	-2.209	-1.709	-1.209	-0.709	-0.209	**0.291**
				Center				
0.000	-3.903	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
0.500	-4.403	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
1.000	-4.903	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
1.500	-5.403	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
2.000	-5.903	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
2.500	-6.403	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
3.000	-6.903	-3.903	-3.403	-2.903	-2.403	-1.903	-1.403	-0.903
				South				
0.000	-4.585	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
0.500	-5.085	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
1.000	-5.585	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
1.500	-6.085	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
2.000	-6.585	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
2.500	-7.085	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585
3.000	-7.585	-4.585	-4.085	-3.585	-3.085	-2.585	-2.085	-1.585

Notes: Bounds based on Manski and Pepper (2018). The δ_i, with i={Lower, Upper} are the uncertainty parameters for the bounded time variation. The entries of the table are the lower and the upper bound. The combination that imposes exact time invariance $(\delta_{Lower}, \delta_{Upper}) = (0, 0)$ is highlighted. The **boldface** emphasizes the combinations where the sign of ATE switches between lower and upper bound. Standard errors are not reported for the reasons in Manski and Pepper (2018, p. 234).

Figure B1: Theoretical framework

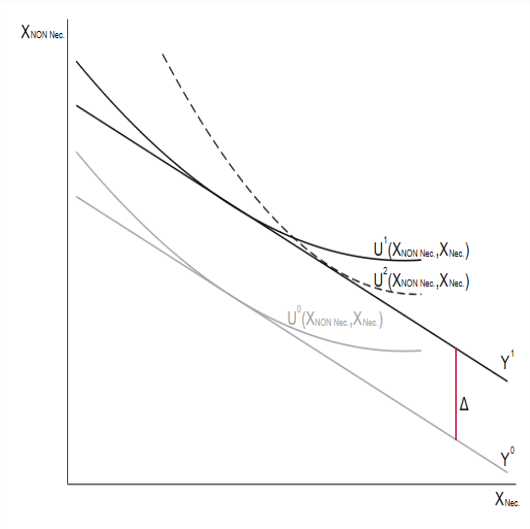
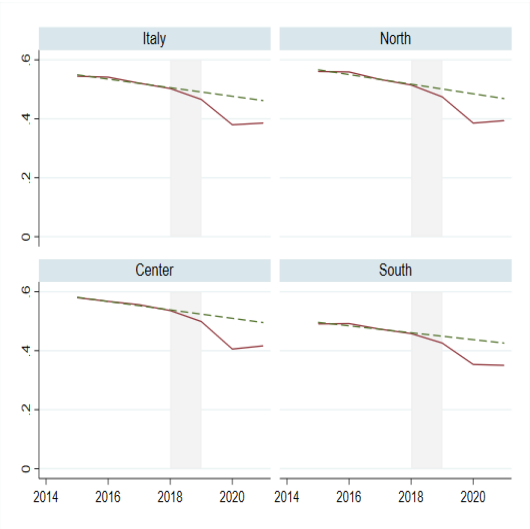
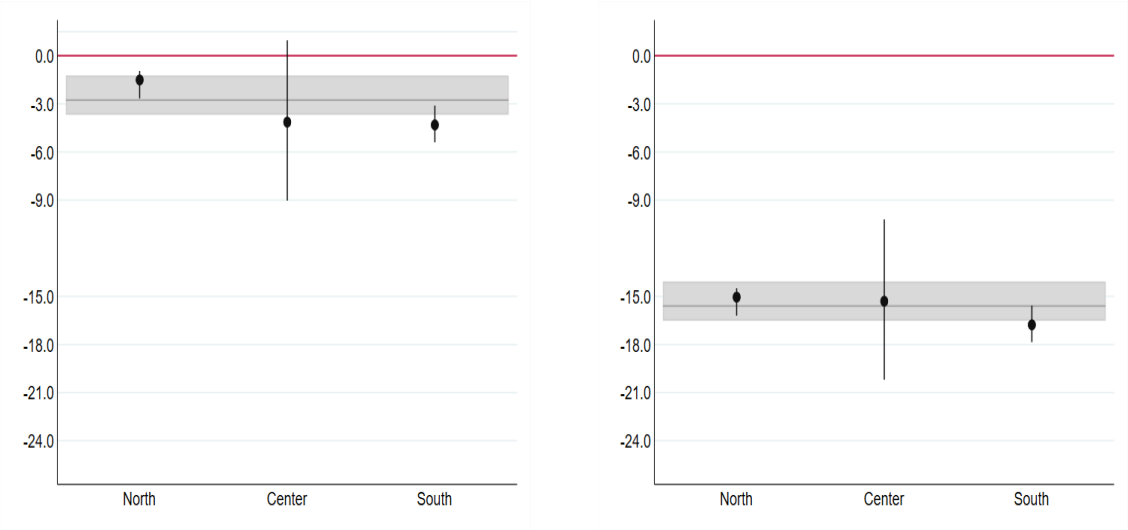


Figure B2: Thefts as a share of total crimes, by macroregion and year



Note: Dotted lines are for linear trend.

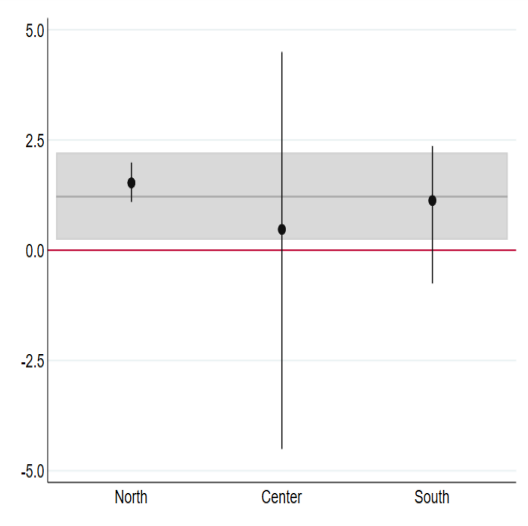
Figure B3: RdC reduced thefts in 2019 and in 2021, in the entire country. Controlling for GDP and population



(a) 2019 (b) 2021

Note: The figure plots the ATE on the number of thefts as the dependent variable, using the first difference to control for area fixed effects, in addition to GDP and population. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2018. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

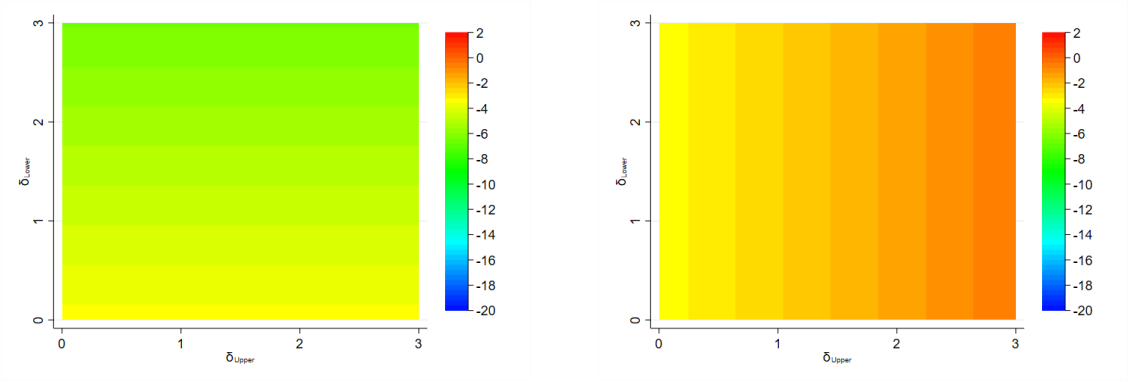
Figure B4: RdC did NOT reduce thefts before its introduction, in any macroregion. Placebo test imposing treatment starts in 2018 instead of 2019. This confirms that ReI had no effect on crime (see also De Angelis and Van Wolleghem, 2023)



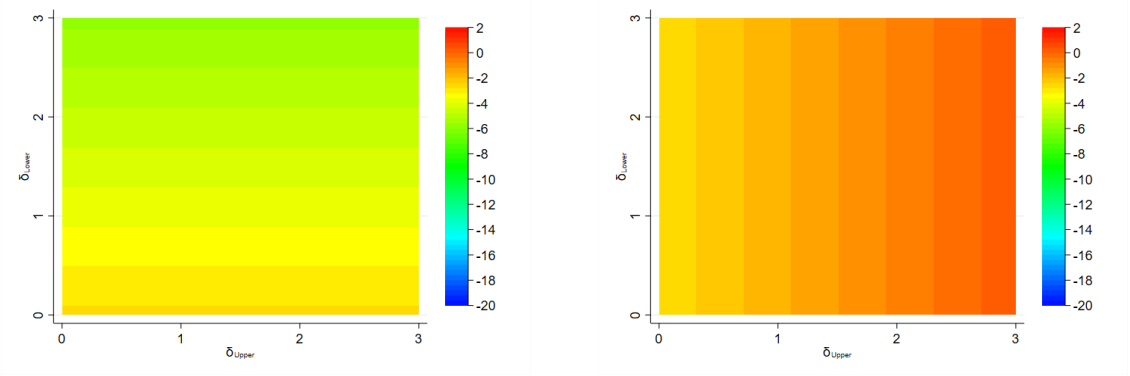
Note: The figure plots the ATE on thefts as the dependent variable, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2017. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

Figure B5: RdC reduced thefts, in the entire country - detailed bounds for 2019

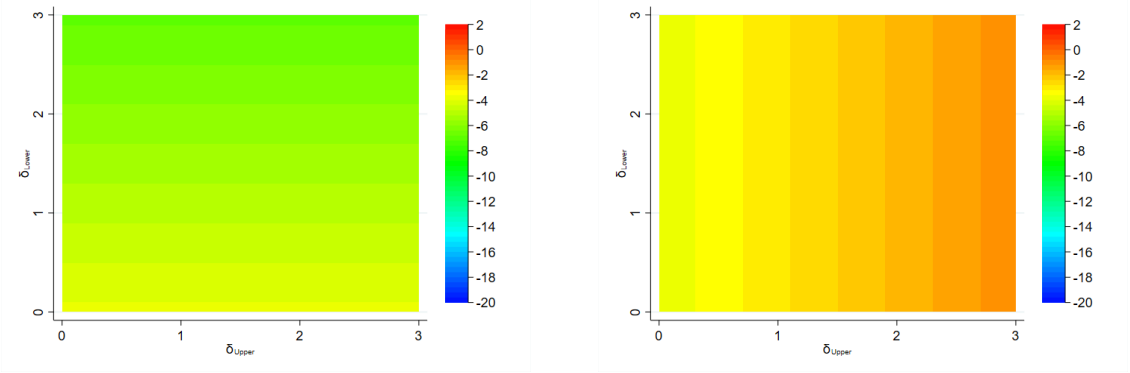
Lower Upper



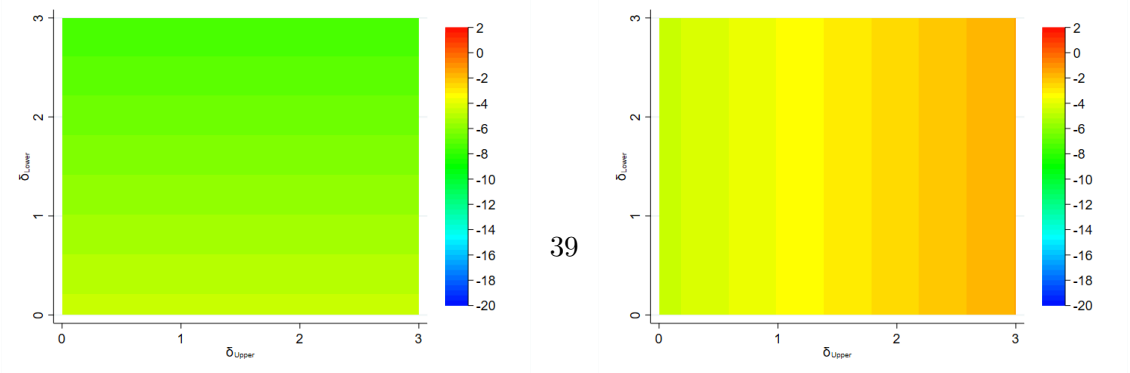
(a) Italy



(b) North



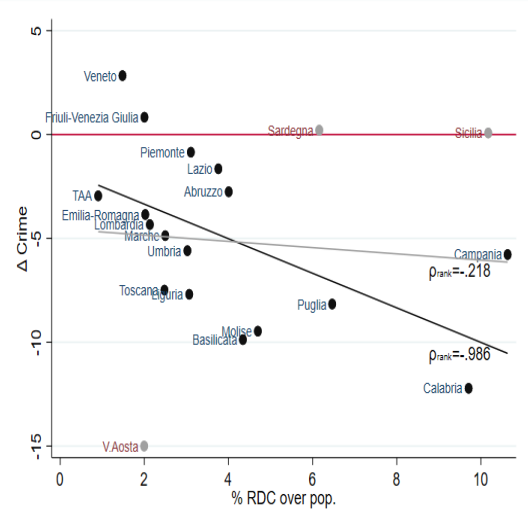
(c) Center



(d) South

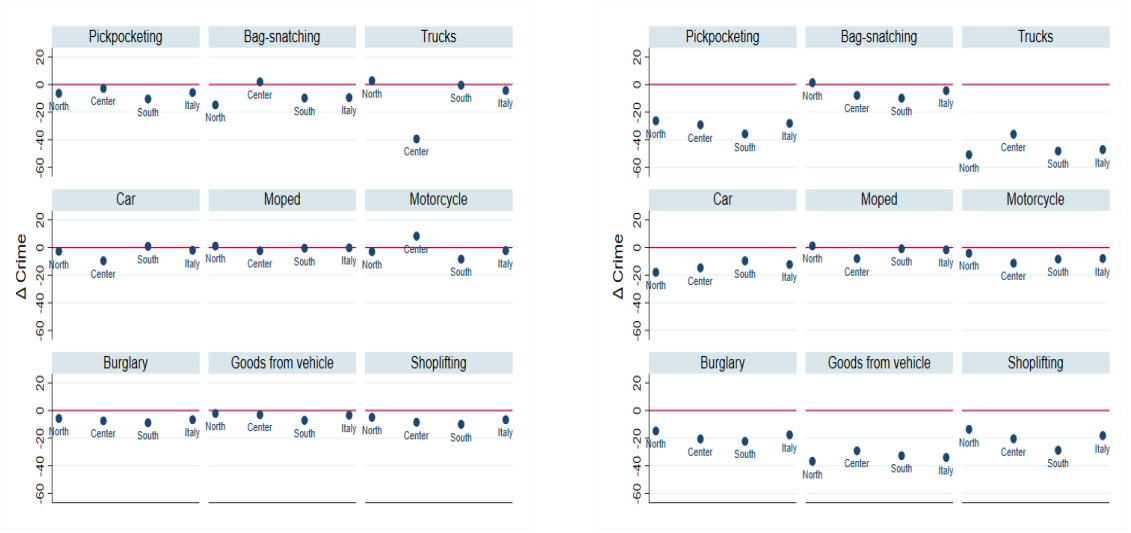
Note: The figure plots the ATE on thefts as the dependent variable, using the first difference to control for area fixed effects.

Figure B6: Rank correlation between treatment effect and share of population who enjoy RdC in 2019, by region.



Note: The figure plots the ATE on thefts as the dependent variable, using the first difference to control for area fixed effects. I report only bounds of Manski and Pepper (2018), with $(\delta_{Lower}, \delta_{Upper}) = (0, 0)$*. The figure does not weight by population size. The results are robust to weights.*

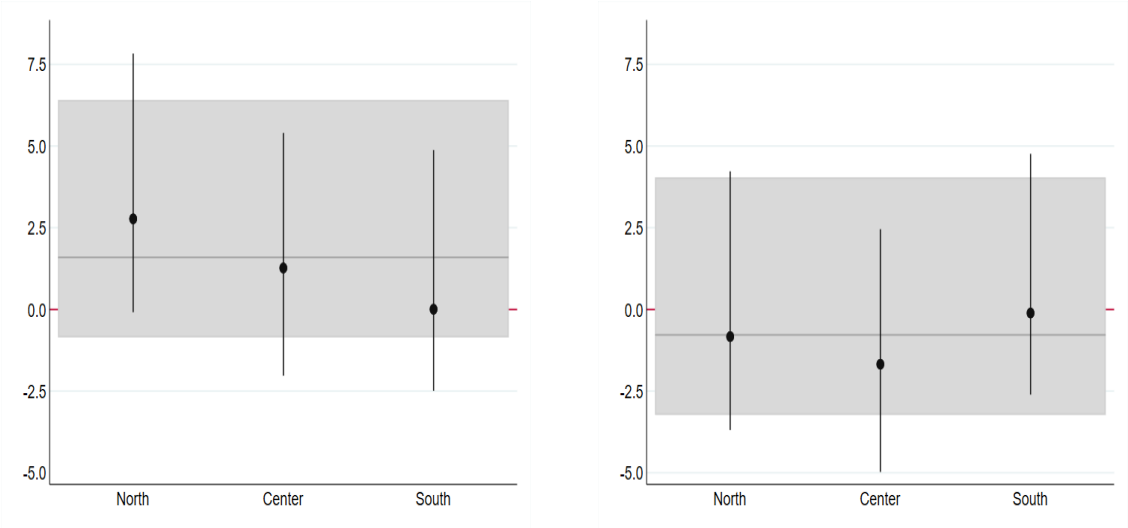
Figure B7: RdC reduced more thefts who provide fast money, in 2019 and in 2021, in the entire country.



(a) 2019 (b) 2021

Note: The figure plots the ATE on thefts as the dependent variable, using the first difference to control for area fixed effects. I report only bounds of Manski and Pepper (2018), with $(\delta_{Lower}, \delta_{Upper}) = (0, 0)$*.*

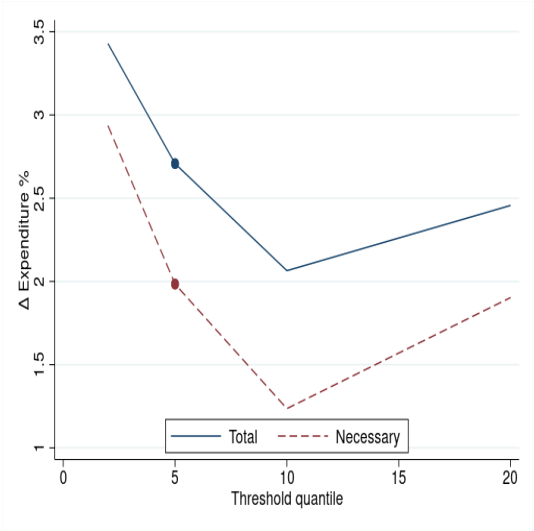
Figure B8: RdC had no impact on crimes different from thefts, in 2019 and in 2021, in the entire country.



(a) 2019 (b) 2021

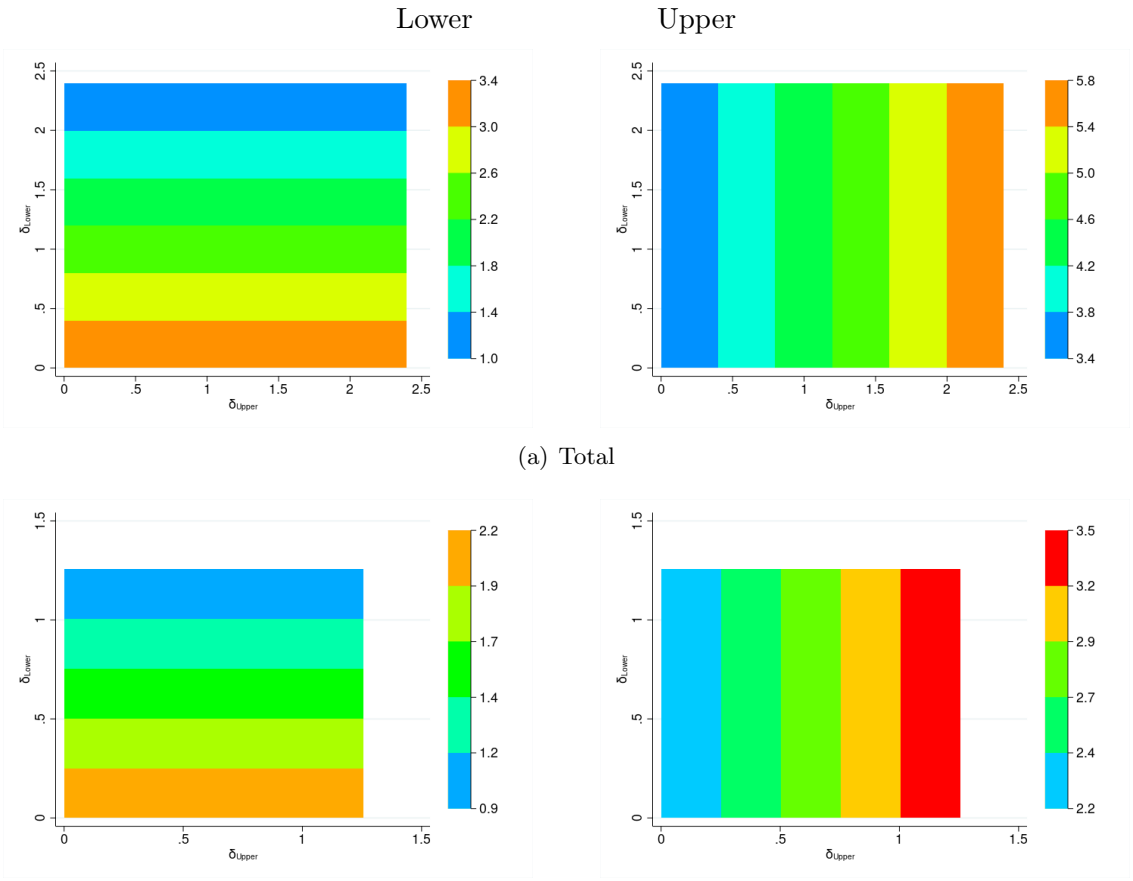
Note: The figure plots the ATE on crimes net of thefts as the dependent variable, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with δ equal to the largest absolute first difference observed between 2015 and 2018. The boldface point is the ATE with $\delta = 0$. Shadow area is for Italy.

Figure B9: The results based on other definitions of low-expenditure households are more striking



Note: The figure plots the ATE on expenditure by type of consumption as defined in the figure, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018), with $(\delta_{Lower}, \delta_{Upper}) = (0, 0)$. The points superimposed on the two lines are at the threshold of equivalized expenditure equal to 5%, which is coherent with the share of households having access to RdC.

Figure B10: Bounds on consumption expenditure for 2019 in Italy, by type of consumption



(a) Total

(b) Necessary

Note: The figure plots the ATE on expenditure by type of consumption as defined in the figure, using the first difference to control for area fixed effects. Identification relies on bounds of Manski and Pepper (2018) δ_i, with i={Lower,Upper}, as defined in the figure.

Table B3: Characterization of complier households

	HBS		SHIW			
	Consumption		Request		Access	
	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.
Components	0.087***	0.090***	-0.008	-0.050***	-0.013*	-0.052***
	(0.005)	(0.006)	(0.009)	(0.012)	(0.008)	(0.011)
Women	0.008	0.000	0.014	0.022	0.009	0.017
	(0.009)	(0.009)	(0.015)	(0.014)	(0.014)	(0.013)
Area:						
Center	0.021***	0.013*	-0.002	-0.007	-0.001	-0.006
	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.007)
South	0.162***	0.145***	0.036***	0.016*	0.029***	0.010
	(0.008)	(0.008)	(0.010)	(0.009)	(0.009)	(0.008)
Education:						
Primary	0.236***	0.176***	0.097***	0.044***	0.077***	0.036***
	(0.009)	(0.011)	(0.015)	(0.012)	(0.014)	(0.011)
Secondary	0.074***	0.052***	0.027**	0.000	0.020**	0.000
	(0.008)	(0.009)	(0.011)	(0.010)	(0.010)	(0.009)
Age:						
< 6	0.083*	-0.033	0.120**	0.073	0.135**	0.089
	(0.044)	(0.045)	(0.061)	(0.058)	(0.057)	(0.055)
[6,14]	0.012	-0.103***	0.152**	0.129**	0.108**	0.082
	(0.036)	(0.038)	(0.065)	(0.063)	(0.054)	(0.053)
[15,17]	-0.016	-0.135**	0.186*	0.147*	0.146	0.105
	(0.053)	(0.054)	(0.096)	(0.084)	(0.090)	(0.078)
[18,24]	0.181***	0.047	0.142***	0.091**	0.106***	0.057*
	(0.033)	(0.034)	(0.041)	(0.045)	(0.029)	(0.033)
[25,29]	0.225***	0.099***	0.089***	0.106***	0.066***	0.079***
	(0.025)	(0.027)	(0.023)	(0.029)	(0.020)	(0.027)
[30,34]	0.185***	0.067***	0.157***	0.178***	0.139***	0.155***
	(0.021)	(0.023)	(0.048)	(0.051)	(0.048)	(0.050)
[35,39]	0.161***	0.046**	0.074***	0.110***	0.046***	0.077***
	(0.020)	(0.023)	(0.019)	(0.030)	(0.017)	(0.027)
[40,44]	0.166***	0.053**	0.087***	0.126***	0.081***	0.115***
	(0.020)	(0.023)	(0.027)	(0.033)	(0.026)	(0.030)
[45,49]	0.098***	-0.014	0.117***	0.152***	0.097***	0.126***
	(0.018)	(0.021)	(0.033)	(0.035)	(0.031)	(0.033)
[50,54]	0.100***	-0.006	0.101***	0.127***	0.058***	0.079***
	(0.017)	(0.020)	(0.027)	(0.033)	(0.019)	(0.026)
[55,59]	0.072***	-0.025	0.087***	0.117***	0.064***	0.090***
	(0.016)	(0.019)	(0.018)	(0.026)	(0.015)	(0.023)
[60,64]	0.024*	-0.034**	0.098***	0.091***	0.090***	0.081***
	(0.014)	(0.015)	(0.026)	(0.021)	(0.025)	(0.020)
[65,69]	-0.043***	-0.057***	0.030***	0.021	0.025**	0.016
	(0.013)	(0.013)	(0.011)	(0.013)	(0.010)	(0.012)
[70,74]	-0.050***	-0.045***	0.032***	0.022*	0.016***	0.008
	(0.013)	(0.013)	(0.011)	(0.012)	(0.006)	(0.007)
# ind. with income	-0.109***	-0.120***	-0.022***	0.035***	-0.013**	0.040***
	(0.006)	(0.009)	(0.008)	(0.012)	(0.006)	(0.011)
Marital status:						
Married	-0.018*	-0.018*	-0.001	0.009	0.004	0.013
	(0.010)	(0.010)	(0.011)	(0.011)	(0.009)	(0.010)
No more married	0.028**	0.026**	0.015	0.023**	0.001	0.009
	(0.011)	(0.011)	(0.009)	(0.010)	(0.008)	(0.008)

Continues on next page

	HBS		SHIW			
	Consumption		Request		Access	
	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.
Labour:						
Employed		0.023		-0.253 ***		-0.248 ***
		(0.022)		(0.050)		(0.048)
Self-employed		0.031		-0.302 ***		-0.270 ***
		(0.022)		(0.046)		(0.044)
Retired		-0.118 ***		-0.192 ***		-0.183 ***
		(0.016)		(0.036)		(0.035)
Blue collar		0.001		0.001		0.002
		(0.015)		(0.010)		(0.009)
White collar				-0.039 *		-0.017
				(0.021)		(0.018)
Manager				0.020		0.016
				(0.017)		(0.015)
White collar & Manager		-0.091 ***				
		(0.015)				
Intercept	-0.174 ***	0.017	-0.068 ***	0.140 ***	-0.045 ***	0.143 ***
	(0.015)	(0.025)	(0.015)	(0.034)	(0.013)	(0.032)
Obs.	18718	18718	6155	6155	6155	6155

Notes: Linear probability models. The dependent variable in columns HBS takes value 1 if the household is in the lowest quintile of equivalis expenditure. The dependent variable in columns SHIW takes value 1 if the household benefited from RdC. ***[**](*) denotes significance at t 1[5](10)% confidence level. Standard errors are robust to heteroschdasticity. Weighted data.

C Intuition of the bounds

In this appendix, I provide an intuition of what the bounds of Manski and Pepper (2013, 2018) do. The goal of the approach is to recover an appropriate counterfactual. To this aim, one may impose several assumptions. The most natural for the present paper would be the "time invariance" assumption, such that the outcome in one region would have remained unchanged *over time*, in the absence of the policy.

Under "*exact* time invariance", $y_0^* = y_0$ (where the subscript i for regions is implicit). If one imposes this restriction, even minimal departures from "exactness" would make the estimated treatment effect biased. For this reason, Manski and Pepper (2013) relax the exact invariance hypothesis in favour of bounded variation.

Bounded variation restricts the absolute difference between the true, unknown, counterfactual y_0^* and the counterfactual which is built under the time invariance assumption not to exceed a certain amount, that I label as "uncertainty parameter" (δ). Bounded variation identifies a *set* of admissible effects of the treatment (or 'bounds'; Manski, 1990).

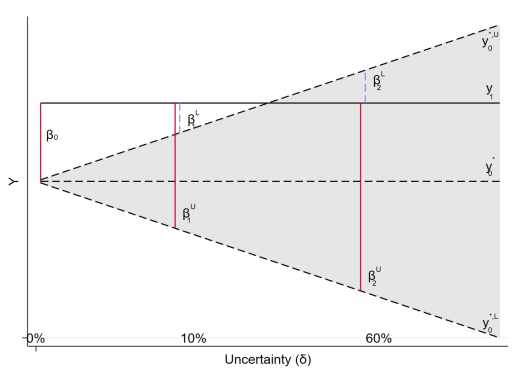
An intuition of the method is as follows. Start as if the exact time invariance holds and derive y_0^*. If I am uncertain by a certain degree about the true counterfactual that is built with this assumption, the appropriate counterfactual against which to compare y_1 is *at most* equal to $y_0 + uncertainty = y_0 + \delta \equiv y_0^{*,U}$, which is coherent with a treatment effect that is equal to $y^1 - y_0^{*,U} = \beta^L$; similarly, the appropriate counterfactual is *at least* equal to $y^0 - uncertainty = y_0 - \delta \equiv y_0^{*,L}$, coherent with a treatment effect that is equal to $y^1 - y_0^{*,L} = \beta^U$. Therefore, the true effect of RdC is somewhere in between β^L and β^U.

For a graphical representation, consider Figure C1.[17] I observe the potential outcome under treatment, which amounts to $y_1^* = y_1$. To identify the treatment effect, I need to recover the potential outcome under non-treatment y_0^*. Under exact invariance, there is no uncertainty surrounding the counterfactual, i.e. $\delta = 0$. With bounded variation, the larger the size of uncertainty the larger the identified bounds. If the uncertainty amounts to 10%, then $y^1 - y_0^{*,U} = \beta_1^L$ and $y^1 - y_0^{*,L} = \beta_1^U$, so that $ATE \in [\beta_1^L, \beta_1^U]$; if the uncertainty amounts to 60%, then $y^1 - y_0^{*,U} = \beta_2^L$ and $y^1 - y_0^{*,L} = \beta_2^U$, so that $ATE \in [\beta_2^L, \beta_2^U]$.

From this figure, it is clear that the larger the size of the uncertainty, the larger the credibility of the results because it relaxes the exact invariance assumption (Manski, 2011). However, in general this makes the widths of the bounds, i.e. the distance between the upper and the lower bounds, larger. Then, it is crucial how one fixes the "uncertainty parameter" δ. Unfortunately, there is no theory that suggests "the" correct value of δ, which eventually depends on the judgment of the researcher as well as on the problem at hand. Manski and Pepper (2013) suggest to set δ according to the pre-treatment period and to set a grid for it. The pre-treatment guidance is based on the per cent difference in the outcome of interest. I set δ equal to the largest and smallest differences; Manski and Pepper (2013) suggest to use the more conservative 75th percentile.

[17] The same figure is in (Depalo, 2019).

Figure C1: Graphical representation of the Manski and Pepper (2013) bounds



Note: The figure plots the ATE identified with the bounds of Manski and Pepper (2013). Under exact invariance, $\delta = 0 \Rightarrow ATE = \beta_0$; if uncertainty amounts to $\delta = 10\%$, then $y_1 - y_0^{*,U} = \beta_1^L$ (dashed line) and $y_1 - y_0^{*,L} = \beta_1^U$ (solid line), so that $ATE \in [\beta_1^L, \beta_1^U]$; if uncertainty amounts to $\delta = 60\%$, then $y_1 - y_0^{*,U} = \beta_2^L$ (dashed line) and $y_1 - y_0^{*,L} = \beta_2^U$ (solid line), so that $ATE \in [\beta_2^L, \beta_2^U]$.

D An equivalence result between time variation with aggregated data and Difference-In-Differences with individual-level data

In the bounds of Manski and Pepper (2013), all the individuals in a region (or state, in their application) are treated or not. In my case, some individuals benefit from RdC whereas others not. In this appendix, I show that the bounds in Manski and Pepper (2013) identify the same treatment effect that I would identify with individual-level data.

Define $j = \{1, \ldots, J\}$ a generic household in region i, and $d \in D$ the treatment indicator, which takes value 1 if the household enjoys the benefit and 0 otherwise. For each year t, I observe the outcome y aggregated at the regional level:

$$\begin{aligned} y_{it} &= \sum_J y_{ijtd} \\ &= \sum_J \left[y_{ijt0} \left(1 - D_{ij}\right) + y_{ijt1} \, D_{ij} \right]. \end{aligned} \tag{5}$$

In the framework of the present paper, there are two periods. In $t = 0$, there is no policy; in $t = 1$, the households most in need enjoy an additional income that has an effect ρ_{RdC} on the outcome

variable. This implies that in region i

$$\begin{aligned} t=0 &\equiv \sum_J \left[y_{ij00}\left(1-D_{ij}\right)+y_{ij01}\,D_{ij}\right] \\ t=1 &\equiv \sum_J \left[y_{ij10}\left(1-D_{ij}\right)+\left(y_{ij11}+\rho_{RdC}\right)D_{ij}\right]. \end{aligned} \tag{6}$$

For simplicity, assume that the identifying assumptions holds perfectly. (Relaxing this assumption is straightforward after the equivalence is shown.) If I had individual-level data, I would exploit a Difference-In-Differences (DID) design. The core assumption of the DID design is the parallel trend (Roth et al., 2023), i.e. $(y_{ij10}-y_{ij00})=(y_{ij11}-y_{ij01})$. Imposing parallel trend, the time difference in eq. 6 is equal to

$$\text{DID}=\sum_J \rho_{RdC}\,D_{ij}.$$

Exploiting the time design assumption that the outcome in one region would have remained unchanged *over time*, in the absence of the policy, the outcome difference in region j amounts to

$$\begin{aligned} t=1-t=0 &= \sum_J \left[y_{ij10}\left(1-D_{ij}\right)+\left(y_{ij11}+\rho_{RdC}\right)D_{ij}\right] \\ &- \sum_J \left[y_{ij00}\left(1-D_{ij}\right)+y_{ij01}\,D_{ij}\right] \\ &= y_{i,t=1}-y_{i,t=0} \\ &= y_{i,t=0}+\sum_J \rho_{RdC}\,D_{ij}-y_{i,t=0} \\ \text{time variation} &= \sum_J \rho_{RdC}\,D_{ij}, \end{aligned} \tag{7}$$

where the third equality follows from the identifying assumption. Then, using the time design with aggregated data is equivalent to using a DID design with individual-level data over the entire population.