More income, less crime
Evidence from Italy

Domenico Depalo *

Abstract

I study whether minimum income affects property crime and consumption, exploiting the introduction and subsequent withdrawal of Reddito di Cittadinanza (RdC), the Italian minimum income scheme introduced in 2019. Using administrative data and spatial variation in the intensity of program participation across regions and provinces, I estimate the aggregate effect of RdC on crime and household expenditures. I find that RdC reduced thefts by about 20% (on an annual basis) between 2019 and 2022, while it had no effect in 2023, when eligibility conditions were tightened. Over the same period, household consumption increased by approximately 2.5%, driven entirely by non-food expenditures. The effects are stronger in Southern Italy, where there were more program beneficiaries, and are mitigated by local employment conditions. Violent crimes are unaffected. A welfare analysis based on the Marginal Value of Public Funds yields an MVPF of 1.3, indicating that the policy repaid its fiscal cost. These results provide evidence that minimum income can substitute for illegal income sources and generate net social benefits, even over a relatively short policy horizon.

JEL classification: I38; K42; C21; D12; H53

Keywords: Crime; Minimum income scheme; Policy Evaluation; Welfare analysis; Marginal Value of Public Funds (MVPF)

*I would like to thank Nicola Curci, Giovanni Mastrobuoni, Enrico Moretti, Franco Peracchi, Paolo Pinotti, Silvia Vannutelli... I am indebted to Salvatore Lattanzio. Replication files and additional results will be available at the webpage: http://sites.google.com/site/domdepalo/. The views expressed in this paper are those of the author and do not imply any responsibility of the Bank of Italy. Corresponding address: Domenico Depalo, Banca d'Italia, Economics and Statistics Department, Via Nazionale, 91 - 00184 Roma, Tel.: 39-06-4792 5989, e-mail: domenico.depalo@bancaditalia.it.

1 Introduction

Between 2019 and 2023, Italian households whose income was below a given threshold benefited from the minimum income scheme "Reddito di Cittadinanza" (RdC), introduced to reduce poverty, inequalities, and social exclusion. According to official statistics from the Italian Statistical Institute (Istat), between 2018 and 2019, the poverty rate decreased from 7.3% to 6.7%. It increased in 2020, because of the Covid-19 pandemic (to 7.8%), and between 2022 and 2023, because of the surge of inflation (to 8.4%), but much less than it would have done without RdC (Carta and De Philippis, 2021).

In this paper, I investigate the presence of an additional effect of RdC. Besides poverty, did the minimum income scheme serve as a deterrent to property crimes? If so, what were the motivations?

Economic theory predicts that when legal income falls below the level necessary to afford a basket of goods, individuals may resort to crime to fill the gap (Becker, 1968; Ehrlich, 1973). In this paper, I exploit the quasi-experimental setting offered from RdC to test the relevance of substitutability among sources of income. To this aim, I test whether property crimes respond to minimum income, which represents a direct test of substitutability. Then, I investigate what happens to consumption. If earnings from property crimes filled the income gap completely, then expenditures would remain flat; otherwise, they would increase. The latter is a sufficient condition to name consumption as a possible motivation. The main challenge in identifying the causal effects of RdC stems from the simultaneous introduction of the scheme in the entire country. To overcome this limitation, I exploit the variation in the number of beneficiaries and applicants across narrowly defined geographical jurisdictions (Card, 1992). Several falsification tests support this approach. I leverage regional- and provincial-level administrative data between 2006 and 2023. Three features are central to the analysis. First, I focus on the entire Italian population. Therefore, I consider the entire target population of the policy and not only a fraction of it. Second, I distinguish between the introduction and the abolition of the scheme, which provides a comprehensive test of substitutability and more generally of the underlying motivations. Third, I perform a welfare analysis that informs about the effectiveness of the scheme.

I find that RdC explains the fluctuations in thefts. The scheme implied a reduction of thefts by about 20% (on annual base) between 2019 and 2022, but had no effect in 2023, when eligibility conditions were substantially tightened. Non-food consumption increased by 2.5% thanks to RdC, consistent with a relaxation of the consumption constraint among beneficiary households. For both outcomes, the national patterns hold within each macroregion, but the effects are stronger in Southern than in Center and Northern Italy, where the impacts of RdC are rather similar. This is consistent with a larger share of beneficiaries of the minimum income in the first area than in the others, where the shares are similar. Interestingly, a higher employment rate mitigates the effect of RdC on thefts and consumption, because the associated wage represents a legal solution to close the income gap. On the other hand, immigration has no effect, reflecting the design of the policy that excluded immigrants. The characterization of households who enjoy the benefit suggests that the most disadvantaged in terms of human capital endowments and labour market outcomes benefited the most from RdC. Finally, although RdC repaid itself, its benefit was much smaller than similar policies, due to the short implementation period of the scheme (5 years) and the absence of habit formation. The marginal value of public funds (MVPF; Hendren, 2016) of RdC is 1.3. As a reference, the return of the nutritional aid disbursement (SNAP) in the US was above 62 (Bailey et al., 2024), thanks to the long-term benefit for children. Net of this component, the MVPF for SNAP would be identical to MVPF for RdC.

This paper is related to the literatures on income and crime and on RdC. Other papers studied the relationship between income-related policies and various crimes. The study most closely related to this is Carr and Packham (2019), who use aggregated data to analyse the *causal* effect of 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 offences. Among these papers, de Blasio et al. (2016) 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 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 offences. 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. Fougère 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. Individual-level data are seldom available, and never for the entire population. For Finland, Aaltonen et al. (2025) exploit a randomized controlled trial on 2,000 unemployed individuals who received an unconditional transfer instead of the benefit associated with the existing social safety net. They find no effect on crime. Other papers focusing on unemployment conditions in the overall population reach conclusions similar to those of 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.

A small literature focused on RdC. Carta and Colonna (2025) consider the complementarities between partners' employment decisions in a setting of minimum income. They find that the scheme reduces the employment rate of married men of households in which only the husband works, but not the others. According to their results, overall RdC would reduce the employment rate by a negligible 0.5 percentage point (out of an employment rate of 65-70% during the policy years). Dachille et al. (2025) focused on fertility and show that RdC increased childbirth probability by about 20% in Southern Italy. I am not aware of any paper investigating the effect of RdC on crime.

This paper makes four contributions. It is the first that estimates the relationship between the minimum income scheme towards the overall population, rather than towards the unemployed only, 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. Second, I analyze the entire life-cycle of the RdC, whereby the existing literature (apart from Britto et al., 2022) considers only one specific moment, typically the introduction of the benefit. This is a key feature because there might be habit formation that inhibits crimes once the required skills depreciate. Third, 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 (Watson et al., 2020; Foley, 2011). Fourth, and clearly related to the previous point, I run the first welfare analysis of this literature.

The rest of 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).[1] In

[1] A comparison between ReI and RdC is in Curci et al. (2020). ReI provided 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

April 2019, the new scheme *Reddito di Cittadinanza* (Citizens' income, RdC) replaced ReI. In 2023, the Government revised the rules of RdC, which was then replaced by the *Assegno di Inclusione* in 2024.

RdC was an active labour policy designed to guarantee the right to work and to reduce poverty and inequality. It provided economic support to households at risk of social and economic marginalisation. A similar measure existed for retired households, but, for the present paper, the distinction is irrelevant. To access RdC, households had to satisfy some socio-demographic and economic conditions. Among the former, citizenship was the most important, because immigrants were excluded. Among the latter, the indicator of the (equivalized) economic situation (*Indicatore della Situazione Economica Equivalente*; ISEE) needed to be below a given threshold. The economic benefit of RdC consisted of two components: the integration to 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 was no higher than 780 euros per month. Curci et al. (2020) note that this amount is close to the relative poverty threshold, and it is a high level by international standards. RdC could extend 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 received the benefit on the same days of the month. The benefit was credited to an electronic payment card, named "RdC card". Therefore, there was no movement of cash.

According to INPS (2023), between 2019 and 2023, about 5.5% of Italian households had access to RdC. Curci et al. (2020) estimate that the Gini index of the equivalized 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 the

limited. Therefore, I proceed as if the Government never implemented ReI. 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.

latter 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.

In 2023, the Government relaxed some rules of RdC, namely that regarding citizenship, but made access conditions tighter, reduced the amount of the benefit, and shortened the period of the benefit. In particular, only households with an individual disabled, or younger than 18, or older than 59 years were eligible under the new regime, rents no longer increased the benefit, and renewals lasted for 7 months instead of 18. According to Bovini et al. (2023), the new requirements reduced the number of potential beneficiaries by 40%.

3 Data

I merge various administrative datasets. I investigate how RdC affected crime and consumption. The data regarding RdC rely on the official statistics from the Italian Social Security Institute (INPS). They cover the whole population of beneficiaries and applicants in Italy. Because households are responsible for the request of RdC, some of them may enjoy the benefit without being eligible. 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 the number of beneficiaries and applicants at the regional level, and only the number of applicants at the provincial level.

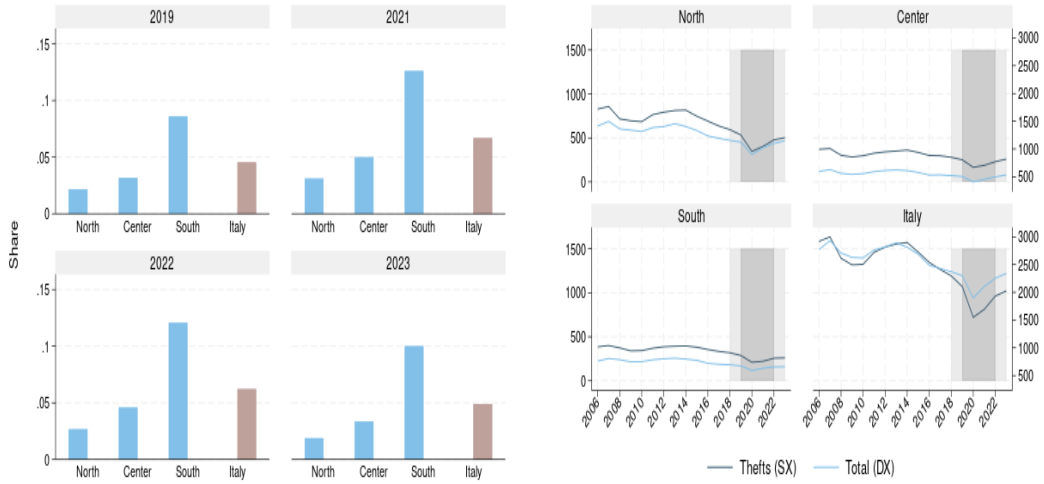
In Figure 1-a), I show the share of the beneficiaries of RdC over time by macroregion.[3] In 2019,

[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

in Italy, 4.5% of the population accessed RdC. The percentage increased between 2019 and 2021 (to more than 6%), and dropped again in 2023 (less than 5%) reflecting the more stringent rules. The South is always the area with the highest share of beneficiaries (more than 10%) and the North the area with the lowest (less than 3%). The shares in North and Center Italy are much more similar than with Southern regions.

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



(a) RdC (b) Crimes (× 1,000)

Note: In panel (b), 2020-2022 are represented with darker areas because the conditions of the RdC were more generous; 2019 is lighter because RdC started in April and 2023 because the eligibility conditions became much tighter.

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. 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 for the period 2006-2023. The primary source of 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

Adige, Valle d'Aosta, Veneto; "Center": Lazio, Marche, Toscana, Umbria; "South": Abruzzo, Basilicata, Calabria, Campania, Molise, Puglia, Sardegna, Sicilia.

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.[5] Therefore, I consider the time when the crimes are committed. Istat provides the data with a breakdown for the type of crime, which includes thefts (see Table B1 for the complete list). The units of observation are the provinces and the regions.

In Figure 1-b), I plot the time series of thefts and total crimes in Italy and by macroregions. The two variables moved together before the introduction of the RdC. In 2019, after the introduction of the minimum income, both aggregates reduced sharply. In all areas, the reduction in thefts is stronger than in total crime. (One may also consider thefts as a share of total income to reach similar conclusions.) 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), between 2015 and 2023. HBS provides some details on expenditures, including the distinction between food and non-food consumption.[6] This feature allows me to investigate how households allocates their budget. All the expenditures refer to the time of purchase, regardless of the moment of actual consumption. This characteristics warrants a perfect correspondence of the definition of time 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.[7] The finest geographical

[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.

[5] The focus on the identity of the criminals may introduce a sample selection bias in the characteristics of criminals, if the least skilled among them are more likely to be captured, as seems reasonable.

[6] 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.

[7] Income decile is available from 2019 onwards. Therefore, the information covers only the policy period.

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

4 Empirical strategy

I am interested in the Average Treatment Effect on the Treated (ATT)

$$ATT = E[y^*_{i,d'} - y^*_{i,d}] \tag{1}$$

where, for each geographical unit i, $y^*_{i,d}$ is the potential outcome under treatment $d \in D$. Throughout the paper, the parameter of interest is the aggregate effect induced by the observed spatial distribution of RdC beneficiaries/applicants across regions/provinces. I leverage variation in the number of beneficiaries and applicants across geographical units. I define the treatment as a continuous variable ($d \in [0, \infty]$) and exploit a Difference-In-Differences-type (DID) design. An early example of a similar approach is in Card (1992), who estimates the effect of a minimum wage on wage and employment in the US states.

For identification, consider a simplified case with 2 units (index i) and 2 periods (index t), the first without and the second with RdC, and all the adjusting covariates purged through the Frisch and Waugh (1933) theorem. The reference equation for the outcome y is

$$E[y_{i,t}] = \alpha_i + \gamma\, t + \beta\, RdC_{i,t}.$$

Under the same assumptions of the standard DID, namely non-anticipation and parallel trend

(Baker et al., 2025), identification proceeds as follows:

	$i=1$	$i=2$
$t=1$	$E[y_{1,1}] = \alpha_1 + \gamma \cdot 1$	$E[y_{2,1}] = \alpha_2 + \gamma \cdot 1$
$t=2$	$E[y_{1,2}] = \alpha_1 + \gamma \cdot 2 + \beta\, RdC_{1,2}$	$E[y_{2,2}] = \alpha_2 + \gamma \cdot 2 + \beta\, RdC_{2,2}$

and taking the differences

$$\begin{aligned} E[y_{1,2}] - E[y_{1,1}] &= \gamma \cdot (2-1) + \beta RdC_{1,2} \\ E[y_{2,2}] - E[y_{1,2}] &= \gamma \cdot (2-1) + \beta RdC_{2,2} \end{aligned}$$

$$(E[y_{1,2}] - E[y_{1,1}]) - (E[y_{2,2}] - E[y_{1,2}]) = \beta\,(RdC_{2,2} - RdC_{1,2}).$$

Like in the standard DID with a binary treatment, taking the differences across individuals and over time identifies the effect of RdC. Unlike the binary treatment design, the correct interpretation of β requires the definition of the subpopulation of interest. For a similar conclusion, see Hirano and Imbens (2005). As the aim of the paper is to estimate the effect of RdC in the country, a natural population to consider is the actual distribution of RdC within each geographical unit. In the empirical application, I use this as the policy-relevant aggregate effects, unless otherwise stated.

5 Results

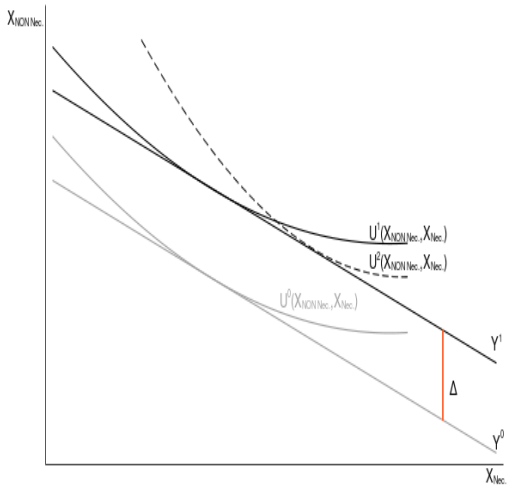
In this section, I provide a reference theoretical background that guides the empirical application (Section 5.1). To make results comparable across outcomes, I use the same model specification (described in Section 5.2) to estimate the effect of RdC on crimes (Section 5.3) and on households' consumption (Section 5.4). A welfare analysis concludes the study (Section 5.5).

5.1 Conceptual framework

Throughout the analysis, I use a simple theoretical background consistent with Becker (1968) and Ehrlich (1973). A generic household has a utility function that depends on two (bundles of)

goods, one necessary ($X_{Nec.}$), like food (Engel, 1857), and one non-necessary ($X_{NONNec.}$), i.e. $U(X_{Nec.}; X_{NONNec.})$. A representative household aims at achieving the utility function U^1, which requires a budget of Y^1, but has access to income $Y^0 < Y^1$, which entails a utility function U^0 (Figure 2). Income from crime may fill the gap $\Delta = Y^1 - Y^0 > 0$.

Figure 2: Theoretical framework



This simple framework provides insights into the empirical approach and delivers the most relevant predictions. As for the empirical approach, aspects that shape 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 ATT of eq. 1 should include the economic shock and the rapacity effect because they are a consequence of the RdC. 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. The substitutability between income sources should reduce crime. 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. In the empirical analysis, I proceed in two steps. First, I test the hypothesis that RdC caused a reduction in thefts (Section 5.3), thanks to the additional legal income. Second, I test the hypothesis that RdC caused an increase in consumption (Section 5.4). The latter effect is uncertain. If crime makes $\Delta = 0$ then there is no effect on consumption. If $\Delta > 0$, consumption increases. The latter represents a sufficient condition for consumption as a motivation.[8]

5.2 Model specification

To test whether RdC caused crime/consumption pattern, the outcome of interest is the absolute number of thefts/real expenditure. This represents an intermediate step of the analysis, because what I focus on is ultimately the percent evolution compared to the pre-treatment period. The percent evolution represents a natural normalization that allows me to compare different outcomes or geographical units. Not surprisingly, this is the typical approach in the literature (e.g. Carr and Packham, 2019). Two qualifications are in order. First, from the percent evolution I always drop 2020 because Italy implemented a strict stay-at-home policy in response to Covid-19. In all the following years, the legislative limitations did not meaningfully affect individual mobility.[9] Second, theft data are available at both the regional and provincial levels. The former level contains the number of beneficiaries and therefore it is more appropriate to test the causal effect of RdC. They may suffer from aggregation bias (apart from special cases, which include the homogeneity of the effect across *provinces*; for a discussion, see Blundell and Stoker, 2005). Provincial data contains only the number of applications. They may identify a biased effect if criminals move to maximize

[8] The condition is non-necessary because consumption could be a motivation, but crime before RdC already made $\Delta = 0$, so there is no need to revise expenditures.

[9] Italy initiated its vaccination campaign at the end of 2020. In 2021, the government implemented a tiered regulatory framework based on epidemiological risk levels. While some restrictions existed in 'red zones', they were highly localized and very short-lived, with most regions remaining in the 'orange zone' and 'white zone' (mild and no restrictions) for almost all the year. Apart from 2/3 weeks between March and April 2021, individuals were free to circulate. To ensure that these limited and temporary constraints do not bias the estimated RdC effect, I cross-validated institutional data with Google Mobility Reports. In 2021, visits to crowded venues such as shopping centers declined slightly relative to the pre-pandemic baseline (by about 10%), but this reduction was more than offset by increased visits to smaller retail stores and parks (which rose by almost 10% and 50%, respectively). Time spent in residential areas increased only marginally—by less than 5%—and remained comparable to levels observed in 2022. Crucially, I find no systematic association between regional variation in the share of RdC beneficiaries and mobility adjustments during this period. This evidence suggests that crime behaviour is not a mechanical byproduct of pandemic-related mobility patterns, although the pandemic may have influenced consumption behaviour.

the return to crime (Braakmann et al., 2023). Therefore, I run all the analyses at both geographical levels.[10]

Consistent with the identification properties discussed in Section 4, throughout the paper I consider the model

$$y_{i,t} = \gamma\, X_{i,t} + \beta RdC_{i,t} \;\; + \alpha_{1,i} + \alpha_{2,i}\, t_i + \varepsilon_{i,t}, \tag{2}$$

where $i = \{1, \ldots, I\}$ is the geographical unit, either regions or provinces, and t refers to years. X is a set of observable characteristics. The existing literature emphasizes the role of the economic conditions (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, the observable characteristics include employment rate,[11] immigration rate, and population size.[12] *RdC* is the policy indicator, either the number of beneficiaries (for regional data) or the number of applicants (for provincial data). To complete the specification, I control for time-invariant heterogeneity at the unit-level ($\alpha_{1,i}$) and a time trend (t_i). Fixed effects (FE) are important to take into account unobservable, but potentially relevant characteristics, like policy department resources (Mastrobuoni, 2020), or, most importantly, context (Ferraz et al., 2022). The linear time-trend is at the same level of the FE and takes into account the evolution of the outcome that would have been observed in the absence of the minimum income. Importantly, the results remain valid regardless of the imposed trend specification (see also Footnote 17). ε is a zero-mean random error. This specification is similar to the benchmark in the literature based on aggregated data (for example, see Carr and Packham,

[10] Also, I replicate all the results using provincial data, where I *impute* the number of beneficiaries. This requires assuming that the ratio between beneficiaries and applicants remains constant across provinces within a region, where the share is available. The differences in the results are negligible (results available upon request) - as it will be clear below.

[11] One might be concerned that RdC affects employment rate, which would then be a 'bad control' (Angrist and Pischke, 2008). By controlling for employment, I adopt a conservative approach that isolates the direct effect of RdC, netting out any indirect effect that operates through the labour market. Two alternative solutions to address the concern are: using the lagged value of employment rate, that returns results identical to those discussed here (results available upon request), consistent with the results in Carta and Colonna (2025); dropping employment rate from X, an alternative that I explicitly discuss below.

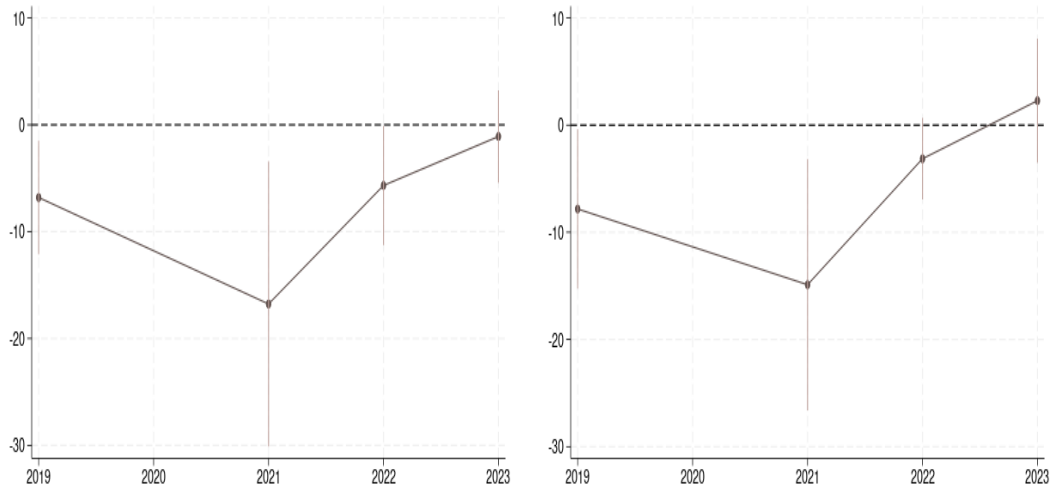
[12] At the provincial level, GDP is not available for 2023; therefore, I successfully checked the results using GDP until 2022. The same is for value added. These results are available upon request. Nevertheless, the employment rate is a more appropriate proxy for the *individual* economic condition than GDP, and therefore it is a standard indicator in the literature (Section 1).

2019; Palmer et al., 2019; Watson et al., 2020; Foley, 2011), although in fact it controls for a larger set of observable characteristics.

5.3 Effect of RdC on thefts

Consistent with the theoretical model of Section 5.1, RdC explains the fluctuation in thefts (Figure 3; details in Table B2).

Figure 3: RdC explains the fluctuation in thefts at the national level



(a) Regional data (b) Provincial data

Note: Details on the estimator are in Table B2. Vertical lines are 95% confidence intervals.

The quantitative effects are remarkably similar between regions and provinces, therefore mitigating the concern of a bias.[13] When households enjoyed RdC, between 2019 and 2022, their legal income increased. Without the need for an alternative, illegal source of income, property crimes decreased. In 2019, RdC started between April and May, and accordingly the magnitude of the effect is about half that in 2021 (7-8% vs 15-17%). These results align well with the study in Carr

[13]The aggregation bias in regional data is an attenuation bias (Blundell and Stoker, 2005), whereas the movement of criminals across provinces biases the results towards a stronger reduction of crimes (Kirchmaier et al., 2023). Here, not only is the effect similar between regions and provinces, but also for provinces is 1-2 percentage points lower than for regions, i.e. the opposite of what would be consistent with the theory.

and Packham (2019), who estimate a annual reduction in property crime by 20% for a targeted measure. In 2022, the direct effect of RdC is comparable to, albeit slightly lower than that of 2019 (3-6% and statistically significant at standard confidence levels for regions but only at 11% for provinces). Therefore, further mechanisms may have been at work. These other mechanisms may be related to the observable characteristics. By means of a Gelbach (2016) decomposition (Table 1), not controlling for employment rate would over-estimate the effectiveness of the RdC, whereas immigration rate plays no role. The former finding is consistent with substitutability between different sources of income to reach the level necessary to satisfy the utility function. In 2022 and 2023, the employment rate is 3-5 percentage points higher than in all previous years, namely in the pre-treatment period. The finding on immigration is consistent with immigrants not having access to the more generous version of RdC, i.e. until 2022 (Section 2). In 2023, with the restrictive eligibility rules regarding RdC, which in that year targeted a tiny fraction of the population, namely the youngest and the oldest individuals–that account for only 5-10% of thefts–, minimum income scheme has no explanatory role for thefts. Therefore, while confirming the relevance of the income motivation for property crimes, there is no habit formation in the sense that the response is immediate when RdC is introduced and when it is abolished. This is an important and novel result because the existing papers look only at a specific point in time, typically the introduction of the minimum scheme. It shall also be clear that this feature plays a major role in the welfare analysis (Section 5.5).

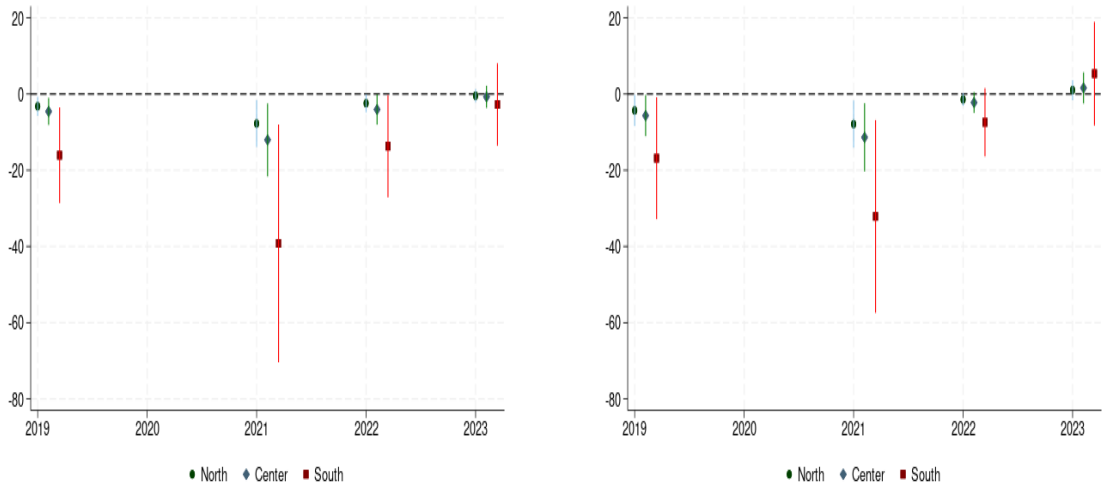
Table 1: Not controlling for employment would over-estimate the effectiveness of employment. Gelbach (2016) decomposition

	2019		2021		2022		2023	
Regional level analysis								
Empl.rate	-0.055	**	-0.018	*	-0.048	**	-0.085	**
	(0.027)		(0.010)		(0.020)		(0.036)	
Imm. rate	-0.035		-0.008		-0.029		-0.049	
	(0.033)		(0.019)		(0.029)		(0.040)	
Pop.	0.038		-0.006		0.029		0.068	
	(0.034)		(0.023)		(0.029)		(0.050)	
Total	-0.052	**	-0.033	***	-0.048	***	-0.065	**
	(0.023)		(0.012)		(0.018)		(0.028)	
Obs.	280		280		280		280	
Provincial level analysis								
Empl.rate	-0.010	**	-0.005		-0.013	**	-0.045	**
	(0.004)		(0.004)		(0.006)		(0.022)	
Imm. rate	-0.003		0.004		0.000		-0.004	
	(0.005)		(0.006)		(0.007)		(0.015)	
Pop.	-0.003		-0.031	***	-0.018	*	-0.021	
	(0.005)		(0.010)		(0.010)		(0.018)	
Total	-0.015	***	-0.032	***	-0.030	***	-0.069	**
	(0.005)		(0.007)		(0.011)		(0.030)	
Obs.	1455		1455		1455		1455	

Notes: Outcome: number of thefts at the regional (upper panel) or provincial (lower panel) level. Controls of the unrestricted model: population size, employment rate, immigration rate. Controls of the restricted and unrestricted models: time-trend and fixed effects as explained in Section 4. Period: 2006-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the same level of the thefts aggregate. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

To shed more light on previous results, I investigate heterogeneity across geographical areas (Figure 4). In all macroregions, the qualitative patterns are identical to the overall country. Moreover, there is monotonicity such that the higher the number of beneficiaries/applicants of RdC, the higher the reduction in property crimes. Accordingly, the percent evolution in Northern and Center Italy is smaller and much more similar between these two areas than with Southern Italy. This reflects the lower share of beneficiaries/applications in the North and Center, and the share similarity between these two macroregions. This regularity strengthens the credibility of the theoretical background to interpret the empirical findings.

Figure 4: RdC explains the fluctuation in thefts also at sub-national level. The pattern is consistent with the share of beneficiaries/applicants



(a) Regional data

(b) Provincial data

Note: Details on the estimator are in Table B2. Vertical lines are 95% confidence intervals.

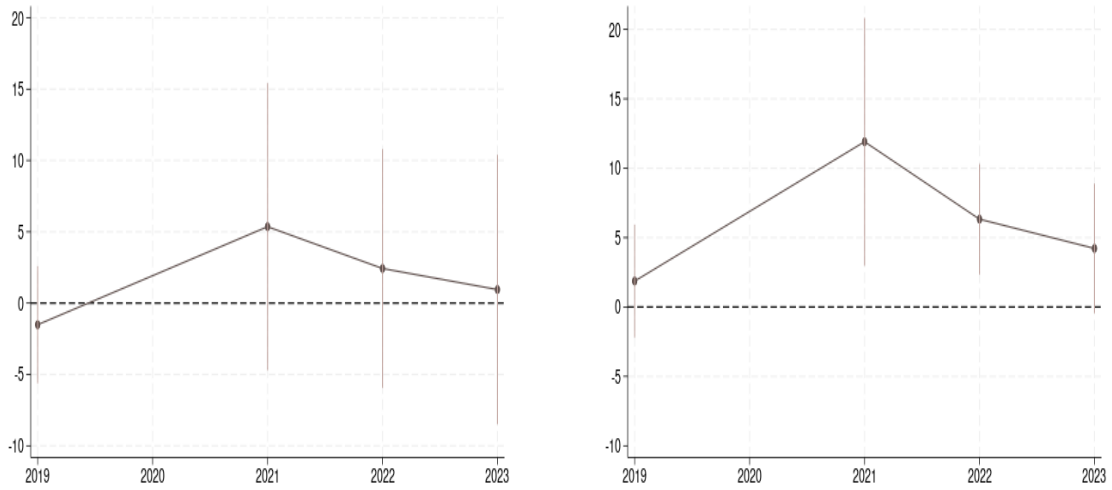
5.3.1 Robustness checks

To conclude this section, I produce evidence in favour of the identifying assumptions, using the pattern of violent crimes and a placebo test, and test the sensitivity of the conclusions to a different model specification based on the per-capita number of crimes.

As for the identifying assumptions, I estimate eq. 2 for violent crimes (Raphael and Winter-Ebmer, 2001), and find no evidence of a reduction after the introduction of RdC (Figure 5).[14] This pattern is suggestive that the estimated drop in thefts is not related to spurious effects, like cultural motivations. Also, it is consistent with the theoretical model of Section 5.1, because violent crimes do not directly depend on the balance constraint nor affect it (Becker, 1968; Ehrlich, 1973).

[14]Violent crimes include massacres, all homicides types, attacks, including sexual, all kinds of criminal associations.

Figure 5: RdC has no effect on violent crimes



(a) Regional data

(b) Provincial data

Note: See Table B3 for details on the estimator. Vertical lines are 95% confidence intervals.

In a second step, I address the main concern for identification, namely that regions/provinces with higher RdC penetration were on a different crime trajectory before the introduction of the scheme. To address this, I perform a placebo that mimics a dynamic DID framework. Specifically, I set the number of beneficiaries/applicants of each geographical unit to its 2019 level, and test whether RdC had some effect *before* its introduction (2015-2018). As shown in Table 2, the coefficients are close to zero and never statistically significant.[15,16] Therefore, when there is no policy there is no effect. This lack of pre-treatment effect is crucial. It confirms that high-RdC regions/provinces were not experiencing differential crime trends before the policy, validating the parallel trend assumption required for a causal interpretation of the post-2019 estimates.

Jointly, these tests provide suggestive evidence that identification conditions are not violated.

[15]The conclusion holds even if I use the average or the median number of beneficiaries/applicants over the period 2019-2023, instead of the 2019 level.

[16]For 2018, the non-significant effect confirms that ReI was not effective (De Angelis and Van Wolleghem, 2023). This justifies the approach of using that data-point as a pre-treatment period.

Table 2: RdC has no effect before it was introduced: placebo test

	2015	2016	2017	2018
Regional level analysis				
Beneficiaries	-0.050	-0.092	-0.112	-0.123
	(0.034)	(0.060)	(0.077)	(0.093)
Obs.	200	200	200	200
Provinciale level analysis				
Applicants	0.010	-0.045	-0.059	-0.067
	(0.020)	(0.061)	(0.062)	(0.082)
Obs.	1035	1035	1035	1035

Notes: Outcome: number of thefts at the regional (upper panel) or provincial (lower panel) level. Controls: population size, employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2006-2018. For each region, the number of beneficiaries in the placebo period (2015-2018) corresponds to its number of beneficiaries in 2019. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the same level of the thefts aggregate. "Beneficiaries": coefficient of the main equation at the regional level. "Applicants": coefficient of the main equation at the provincial level. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

As a different model, I investigate a definition of crimes over the resident population (Table 3).[17] However, it is important to bear in mind that the non-standardized number of thefts of the previous section is the most appropriate outcome to estimate the aggregate impact of RdC, because it considers directly the outcome of interest, and not a transformation of it.

[17] Strictly speaking, the results are conditional on the imposed time trend. Therefore, one may wonder what is the impact of the time trend on the results. I estimated three different model specifications. I imposed a 2- and 3-degree polynomial trend, as well as a different slope of the linear trend before and after the double-dip recession that affected Italy between 2008 and 2014. The results (available upon request) are similar to those presented here. Likewise, results are insensitive to the exact definition of employment rate, i.e. current vs lagged (results are available upon request).

Table 3: RdC explains the fluctuation in thefts even if the outcomes definition is per capita

	2019	2021	2022	2023
Regional level analysis				
Beneficiaries	-0.00120 **	-0.00227 **	-0.00024	0.00079
	(0.00049)	(0.00089)	(0.00035)	(0.00051)
Obs.	280	280	280	280
Provincial level analysis				
Applicants	-0.004 **	-0.011 **	-0.001	0.004
	(0.002)	(0.005)	(0.002)	(0.003)
Obs.	1455	1455	1455	1455

Placebo

	2015	2016	2017	2018
Regional level analysis				
Beneficiaries	-0.00036	-0.00052	-0.00039	-0.00018
	(0.00056)	(0.00087)	(0.00100)	(0.00110)
Obs.	200	200	200	200
Provincial level analysis				
Applicants	-0.00152	-0.00320	-0.00360	-0.00299
	(0.00155)	(0.00268)	(0.00305)	(0.00337)
Obs.	1035	1035	1035	1035

Notes: Outcome: number of thefts per capita at the regional (upper panel) or provincial (lower panel) level. Controls: employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2006-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the same level of the thefts aggregate. "Beneficiaries": coefficient of the main equation at the regional level. "Applicants": coefficient of the main equation at the provincial level. Beneficiaries and applicants are divided by 10000. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

Consistent with the previous analysis, there is a significant drop of thefts per person in 2019 (corresponding to about 3%, regardless I use regions or provinces) and in 2021 (about 10% for regions and 6% for provinces), as well as a small and imprecisely estimated reduction of 1% in 2022. Like in the main analysis, in 2023 RdC had no effect on thefts. An advantage of this different outcome definition is the possibility to run the same robustness checks conducted for the main, non-standardized outcome. In particular, it is possible to compare the size of the effect of regional- vs provincial-level data to shed light on the aggregation bias as opposed to criminals' movement

bias, and to run the placebo test to validate the identifying conditions. All the results of the main analysis are confirmed. The geographical units do not bias the estimator. If anything, the effects are larger with regional than with provincial data, which is inconsistent with the sources of possible bias. As for the placebo test, RdC has no effect on thefts per person before it was introduced (lower panel of Table 3).

To summarize, although the identifying assumptions are not directly testable, the falsification tests are suggestive that they hold in the analysis. Likewise, the stability of results for the absolute number of thefts and the per-capita definition reassures about the robustness of the conclusions discussed above.

5.4 Effect of RdC on household consumption

To explore the role of thefts as a way of financing consumption, I investigate how 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 5.4.1, I show that this proxy is appropriate. Consistent with the official publications from Istat (Section 3), low-income households are those in the first quintile of equivalent expenditure.

In Figure 6 (details in Table B4), I estimate eq. 2 using expenditures as the dependent variable. Panel a) is for total expenditures, Panel b) is for food, and Panel c) is for non-food consumption. When interpreting the results it is important to bear in mind that, in 2021, the fear of contagion affected consumption behaviour, even in the absence of restrictions to mobility (Banca d'Italia, 2022). I present the results for that year, but I do not discuss them in-depth.

Expenditures respond promptly and significantly to RdC. Total expenditures increased by 1.5-2% over the period 2019-2023 (apart from 2021). The basket of purchased goods delivers further insights. Food expenditures do not respond significantly to the minimum income scheme. In contrast, non-food expenditures increase by 2-2.5% over the sample period (again, except 2021). The different pattern is consistent with a sequential satisfaction of the utility function. Necessary

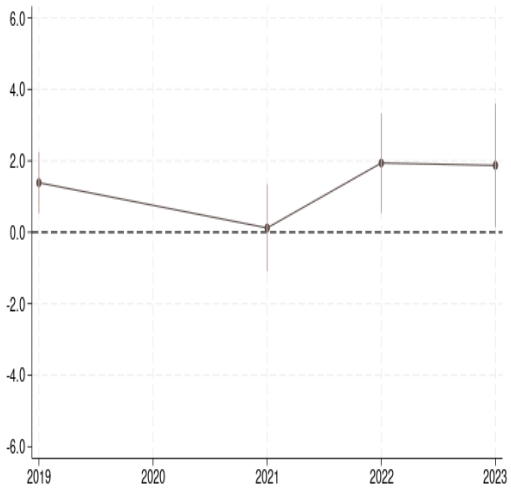
goods, like food, satisfy basic needs, therefore are the first purchase.[18] Then come non-necessary goods. Households allocate money accordingly. Therefore, no matter the source of income, money for food is always available (Engel, 1857; Chai and Moneta, 2010).

Interestingly, like for thefts, occupation supported total and non-food consumption, especially in 2022 and 2023, when employment reached historical peaks, but it had a negligible impact on the non-food aggregate (decomposition of Gelbach, 2016, not shown). Citizenship never played a significant role. These findings provide further support to the theoretical background of Section 5.1 as a reference to interpret the empirical findings, namely consumption is a possible motivation for thefts.

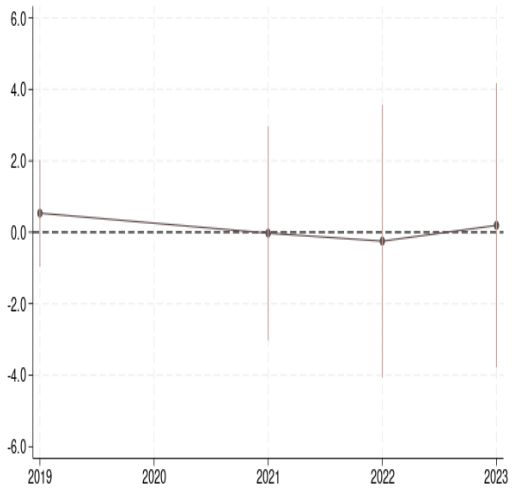
All these results hold within macroregions, with Southern regions responding more strongly than Center regions, and Center regions responding more strongly than Northern regions (Figure 7). The margins between the last two areas is remarkably narrower compared to Southern regions, reflecting the different distributions of beneficiaries, exactly like for criminality.

[18] Istat defines also a basket of necessary goods. The results with this basket of goods are qualitatively identical to those presented in the text (available upon request).

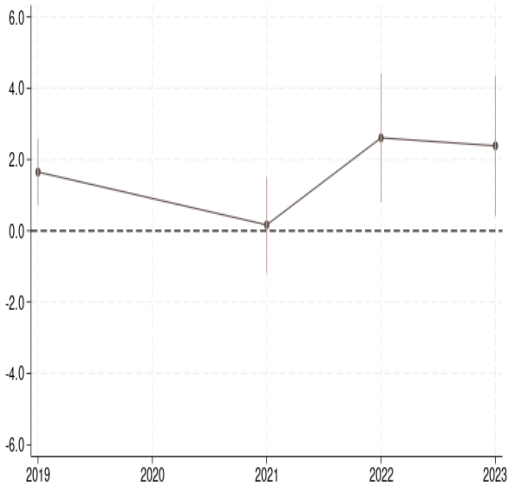
Figure 6: Income effect is a mechanism, in the entire country. RdC explains fluctuations in consumption.



(a) Total



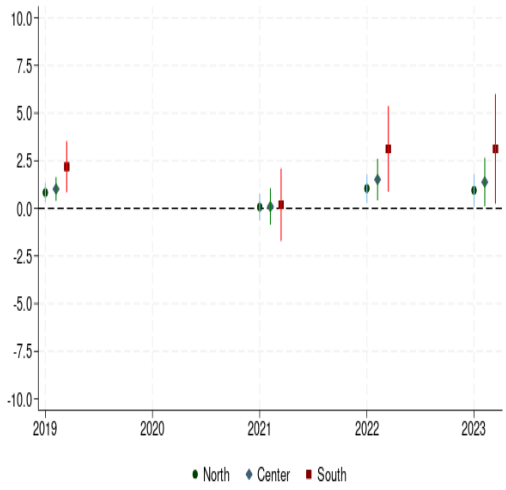
(b) Food



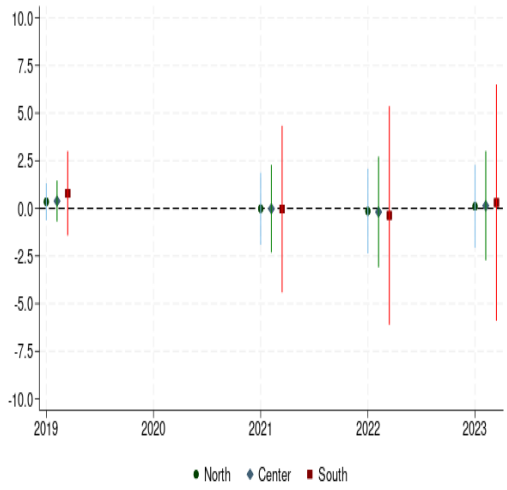
(c) Non-food

Note: Details on the estimator are in Table B4. Vertical lines are 95% confidence level.

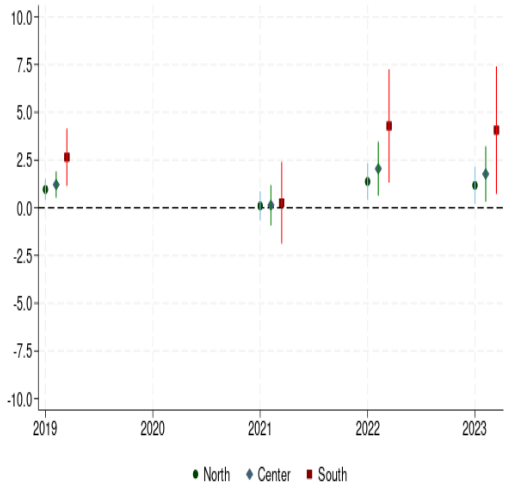
Figure 7: Income effect is a mechanism in the entire country. RdC explains fluctuations in consumption within macroregions.



(a) Total



(b) Food



(c) Non-food

Note: Details on the estimator are in Table B4. Vertical lines are 95% confidence intervals.

Jointly, the effects of RdC on thefts and consumption suggest that households have a desired utility function, which depends also on consumption. The satisfaction of that utility requires a minimum income amount, which may be reached legally or illegally. RdC was a legal substitute

for illegal income. This is why thefts dropped when minimum income was implemented. Also, as income from thefts does not completely close the gap to afford the desired consumption, expenditure nonetheless increased during RdC. There is possibly a sequential satisfaction of the utility function. Therefore, RdC affected only non-food consumption. This interpretation is consistent with standard non-homothetic preferences, where necessities are satisfied first and additional income is allocated to discretionary consumption.

5.4.1 Robusteness checks

As a robustness check, I support the identifying conditions and verify the results using a per-capita definition of consumption. Furthermore, I characterize the individuals who enjoy the benefit.

To support the identifying conditions, I implement the same placebo test of thefts. As HBS data are available for the years 2015-2023, I anticipated the introduction of RdC to 2017 and 2018, using 2015 and 2016 as a pre-treatment period. For each region, the number of beneficiaries is equal to its number of beneficiaries in 2019.[19] Table 4 shows that RdC played no role before its introduction on any outcome, as none of the coefficient is statistically significant at standard confidence levels. Based on this evidence, I conclude that the identifying conditions hold even for the case of consumption.

Table 4: RdC did not affect consumption before it was introduced: placebo test

	Total expenditures		Food expenditures		Non-food expenditures	
	2017	2018	2017	2018	2017	2018
Beneficiaries	0.075	0.309	-0.006	0.034	0.081	0.275
	(0.204)	(0.234)	(0.105)	(0.146)	(0.220)	(0.224)
Obs.	60	60	60	60	60	60

Notes: Outcome: monthly household expenditure for "Total expenditure", "Food expenditure", "Non-food expenditure". Controls: employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2015-2018. For each region, the number of beneficiaries in the placebo period (2017-2018) corresponds to its number of beneficiaries in 2019. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the regional level. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

I then estimate a different model, where the outcome of interest is per-capita expenditure (Table 5). Not only are all the qualitative conclusions confirmed, but also the quantitative results

[19] I experimented also with the mean and the median number over the period 2019-2023, obtaining the same results.

are remarkably stable for all expenditure definitions. Also, I exploit this outcome definition to check the identifying assumptions. Using the same placebo test as above, I do not detect any significant effect before the introduction of RdC (Table 6).

Table 5: RdC increased per-capita consumption

	2019	2021	2022	2023
		Total expenditure		
Beneficiaries	0.11272 **	0.02315	0.13469 ***	0.16823 **
	(0.04783)	(0.04143)	(0.04699)	(0.07024)
Obs.	100	100	100	100
		Food expenditure		
Beneficiaries	-0.00166	-0.01935	-0.02843	-0.03633
	(0.01627)	(0.02078)	(0.02713)	(0.03538)
Obs.	100	100	100	100
		Non-food expenditure		
Beneficiaries	0.11438 ***	0.04250	0.16313 ***	0.20457 ***
	(0.04281)	(0.03550)	(0.04801)	(0.06685)
Obs.	100	100	100	100

Notes: Outcome: per-capita monthly household expenditure for "Total expenditure", "Food expenditure", "Non-food expenditure". Controls: employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Beneficiaries are divided by 10000. Period: 2015-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the regional level. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

Table 6: RdC did not affect per-capita consumption before it was introduced: placebo test

	Total expenditures		Food expenditures		Non-food expenditures	
	2017	2018	2017	2018	2017	2018
Beneficiaries	-0.037	0.011	-0.026	-0.020	-0.011	0.030
	(0.095)	(0.100)	(0.039)	(0.050)	(0.094)	(0.081)
Obs.	60	60	60	60	60	60

Notes: Outcome: per-capita monthly household expenditure for "Total expenditure", "Food expenditure", "Non-food expenditure". Controls: employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Beneficiaries are divided by 10000. Period: 2015-2018. For each region, the number of beneficiaries in the placebo period (2017-2018) corresponds to its number of beneficiaries in 2019. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the regional level. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

As in the analysis on thefts, the two definitions, absolute and per-capita expenditure, that give identical qualitative and quantitative results reassure about the credibility of the results discussed above.

Characterization of beneficiaries

HBS allows me to describe the characteristics of the households in the first quintile of equivalized expenditure (characterization of beneficiaries; Table 7). I estimate a linear probability model where the outcome variable takes value 1 if the household belongs to the first quintile of expenditure, and covariates include region of residence, number of components, family types (e.g. married/non-longer married/never married couple), the distribution by gender, and age.[20] From Col. 1 of Table 7, households that spend the least are larger, live more frequently in Southern Italy, have lower educational attainments, are younger, 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 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 7, 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

[20] In a robustness check, I control for labour market outcomes (e.g. profession and contract type; Table B5). All the results are confirmed. I do not use this model specification as the benchmark to preserve the comparability with the Survey on Household Income and Wealth (SHIW), as I explain below.

of low-income households based on low-expenditure levels is a satisfactory second best. This also reassures that the analysis in Section 5.4, based on low-expenditure households, is legitimate.

Table 7: Characterization of households most in need

	HBS Consumption	SHIW RdC	
		Request	Access
Components	0.087 ***	-0.008	-0.013 *
	(0.005)	(0.008)	(0.007)
Women	0.008	-0.001	0.004
	(0.009)	(0.012)	(0.011)
Area:			
Center	0.021 ***	0.001	0.003
	(0.007)	(0.008)	(0.007)
South	0.162 ***	0.029 ***	0.022 ***
	(0.008)	(0.008)	(0.008)
Education:			
Primary	0.236 ***	0.080 ***	0.065 ***
	(0.009)	(0.013)	(0.012)
Secondary	0.074 ***	0.012	0.007
	(0.008)	(0.007)	(0.006)
Age:			
< 6	0.083 *	0.133 **	0.159 ***
	(0.044)	(0.058)	(0.054)
[6,14]	0.012	0.155 **	0.143 **
	(0.036)	(0.068)	(0.063)
[15,17]	-0.016	0.112	0.128 *
	(0.053)	(0.075)	(0.070)
[18,24]	0.181 ***	0.174 ***	0.133 ***
	(0.033)	(0.044)	(0.036)
[25,29]	0.225 ***	0.068 ***	0.065 ***
	(0.025)	(0.021)	(0.019)
[30,34]	0.185 ***	0.106 ***	0.096 ***
	(0.021)	(0.032)	(0.031)
[35,39]	0.161 ***	0.069 ***	0.051 ***
	(0.020)	(0.018)	(0.016)
[40,44]	0.166 ***	0.073 ***	0.062 ***
	(0.020)	(0.022)	(0.019)
[45,49]	0.098 ***	0.126 ***	0.113 ***
	(0.018)	(0.036)	(0.035)
[50,54]	0.100 ***	0.072 ***	0.064 ***
	(0.017)	(0.020)	(0.017)
[55,59]	0.072 ***	0.092 ***	0.072 ***
	(0.016)	(0.021)	(0.016)
[60,64]	0.024 *	0.080 ***	0.074 ***
	(0.014)	(0.018)	(0.016)
[65,69]	-0.043 ***	0.044 ***	0.046 ***
	(0.013)	(0.017)	(0.016)
[70,74]	-0.050 ***	0.035 ***	0.022 ***
	(0.013)	(0.012)	(0.007)
# ind. with income	-0.109 ***	-0.019 ***	-0.013 **
	(0.006)	(0.007)	(0.006)
Marital status:			
Non-married	-0.018 *	-0.014	-0.012
	(0.010)	(0.010)	(0.009)
No more married	0.028 **	0.013	-0.003
	(0.011)	(0.010)	(0.009)
Intercept	-0.174 ***	-0.038 **	-0.024 *
	(0.015)	(0.016)	(0.013)

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

5.5 Welfare analysis

A final policy-relevant issue concerns the effectiveness of RdC at improving social welfare. A major criticism to the scheme was that it was too expensive compared to the benefits. Accordingly, in 2023, the newly elected Government withdrew the scheme, following the electoral agenda. In this section, I conduct a welfare analysis of the RdC.

I derive the Marginal Value of Public Funds (MVPF; Hendren, 2016; Hendren and Sprung-Keyser, 2020). For a given policy, the MVPF is a benefit-cost ratio defined as the willingness to pay (WTP) of those affected by the policy over the net government expenditure (Bastani, 2025). The latter includes implementation costs and changes in revenues related to behavioural responses, in the short- and long-term. Results are in Table 8.

Table 8: Marginal Value of Public Funds (MVPF) associated with RdC

Item	Amount		Item
Reduction in theft costs	281	749	Gross cost of the transfer
Increase in consumption	350	-77	Additional VAT revenues (22%)
		-191	Savings in incarceration costs
WTP	631	481	Net government expenditure
MVPF = 1.31			

In this paper, the WTP stems from the reduction in thefts and the increase in expenditure power. The reduction in thefts associated to RdC amounts to 281 million (mio). Detotto and Vannini (2010) estimate the cost of a theft in Italy at about 2,600 euros between 2019 and 2023. This amount considers the inflation between 2006 (when the paper refers to) and the analysis period. The cost of a theft is the sum of the costs related to preventive measures to avoid it (1,100 euros), its consequences (1,250 euros), and response (250 euros). The reduction in the number of thefts implied by the estimates in Section 5.3 is 108,705 per year. Therefore, the average WTP over the period is 281mio ($= 108,705 \times 2,600$).[21] The second component of the WTP is related to

[21] I include the cost related to preventive measures because these usually are multi-years subscriptions. However, even if they can be entirely saved, i.e. if I exclude them, all the following qualitative results hold.

the expenditure power. According to the projections based on Section 5.4, expenditures increase by 350mio per year, reflecting the increase in consumption among beneficiaries (250 euros per 1,400,000 households). The overall WTP is then 631mio.

The net government expenditure includes the upfront costs of the project, which I estimate considering that the average benefit is 535 euros per household, therefore costing 749mio ($= 535 \times 1,400,000$), and the behavioural response. Behavioural response includes higher revenues from the value-added tax of 22%, due to the higher expenditure levels (77mio = 350mio $\times$ 22%), and saving of incarcerations. In Italy, a day of incarceration costs 137 euros per inmate. For thefts, the pain is up to 7 years (Italian law, art. 624 bis, Codice Penale) and the probability of being caught is less than 3%. Finally, each criminal makes between 5 and 10 thefts per year (Visher, 1986). The savings over the 7 years would then be 191mio.[22] The net government expenditure is 481mio ($= 749 - 77 - 191$). The MVPF is 1.3 ($= 630/481$). Therefore, the policy repays itself.

Differently from other policies, the MVPF of RdC does not include long-term response. This is because the scheme lasted for only 5 years and I find no habit formation. Bailey et al. (2024) estimate an MVPF of 62 for the Food Stamp Program in the US, which aims at the relief, cure, and prevention of poverty. Much of the gain over RdC stems from the long-term health benefits of treated children. Net of this component, the MVPF is 1.2, almost identical to that estimated for RdC. Although the health care organization is very different between Italy and the US, providing a long-term scheme would probably improve the welfare of Italian households in need.

6 Conclusions

In 2019, the Italian Government introduced the minimum income scheme "Reddito di Cittadinanza" to assist the households most in need. I exploit this policy to estimate how income affects thefts and the underlying motivations. I find that RdC explains the fluctuations in property crimes. The minimum income reduced thefts by about 20% (on annual base) between 2019 and 2022 (a

[22]The 7 years is probably too high, e.g. for first convictions. Using 3 years of incarceration, the qualitative results do not change. Also, I imposed a discount rate of 5% to derive multi-annual costs, following a standard approach in the literature. If I use 2%, consistent with the inflation target of the European Central Bank, the results change marginally.

result similar to Carr and Packham, 2019), but had no effect in 2023 when the newly elected Government tightened the eligibility conditions. In the same period, violent crimes did not decrease. Consumption is a possible motivation for thefts. During the implementation of RdC, total consumption increased by 1.5-2%, mainly driven by non-food consumption (2-2.5%). By macroregion, not only did the same qualitative effects hold, but also quantitatively they are related to the shares of beneficiaries/applicants. Namely, the effects are stronger in Southern Italy, where the minimum income is more widespread, than in Center or Northern Italy, where the shares of beneficiaries are similar and so are the effects on the relevant outcomes. Two further results are important. First, improving on the existing literature, I show that there is no habit formation, as the effect of RdC vanishes as soon as the Government withdrew the policy, in 2023. Second, employment mitigates the effect of RdC, because it provides a legal alternative to income from thefts. I interpret these results through the lens of a simple theoretical model (Becker, 1968; Ehrlich, 1973), whereby, in order to satisfy their utility function, households need a minimum income level that they can reach with legal or illegal means. The income support from RdC represented a substitute for the latter. Using the MVPF as a metric for welfare analysis (Hendren, 2016), RdC repaid itself, but its short life-period reduced its potential effectiveness, stemming from long-term benefits in similar policies (Bailey et al., 2024).

From a policy perspective, 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 further interventions in favour of households not covered by the new scheme are necessary to avoid that income from thefts fill the missing income (Buonanno et al., 2022; Ferraz et al., 2022). Avoiding criminal records would be important, because they may affect labour market trajectory in a way that is difficult to undo later (Agan et al., 2026). 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 crime as another relevant dimension.

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 large benefits from SNAP in the US stemming from the long-term horizon of the scheme (Bailey et al., 2024) suggests that a well designed, long-lasting support may be more effective than a one-shot intervention.

7 Compliance with Ethical Standards

I declare that I have no conflict of interest.

References

Aaltonen, M., M. Kaila, and E. Nix (2025, 12). The impacts of guaranteed basic income on crime perpetration and victimization. Technical report, National Bureau of Economic Research.

Agan, A., A. Garin, D. Koustas, A. Mas, and C. S. Yang (2026). Can you erase the mark of a criminal record? labor market impacts of criminal record remediation. *American Economic Journal: Economic Policy*, 1–79. Forthcoming.

Almeida, V., S. D. Poli, and A. Hernández (2022). The effectiveness of minimum income schemes in the eu. Technical report.

Angrist, J. D. and J.-S. Pischke (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Bailey, M. J., H. Hoynes, M. Rossin-Slater, and R. Walker (2024, 5). Is the social safety net a long-term investment? large-scale evidence from the food stamps program. *Review of Economic Studies 91*, 1291–1330.

Baker, A., B. Callaway, S. Cunningham, A. Goodman-Bacon, and P. H. C. Sant'Anna (2025). Difference-in-differences designs: A practitioner's guide. *Journal of Economic Literature*. Forthcoming.

Banca d'Italia (2022). *The economy of the Italian regions*. Banca d'Italia.

Bastani, S. (2025, 8). The marginal value of public funds: a brief guide and application to tax policy. *International Tax and Public Finance 32*, 919–956.

Beauchamp, A. and S. Chan (2014). The minimum wage and crime. *The B.E. Journal of Economic Analysis & Policy 14*(3), 1213–1235.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. In *The economic dimensions of crime*, Volume 76, pp. 13–68.

Blundell, R. and T. M. Stoker (2005, June). Heterogeneity and aggregation. *Journal of Economic Literature 43*(2), 347–391.

Bovini, G., E. Dicarlo, and A. Tomasi (2023, December). The revision of the anti-poverty measures in Italy. Questioni di Economia e Finanza (Occassional Papers) 820, Bank of Italy, Economic Research and International Relations Area.

Braakmann, N., A. Chevalier, and T. Wilson (2023). Expected Returns to Crime and Crime Location. *American Economic Journal: Applied Economics*. Forthcoming.

Britto, D. G. C., P. Pinotti, and B. Sampaio (2022). The Effect of Job Loss and Unemployment Insurance on Crime in Brazil. *Econometrica 90*(4), 1393–1423.

Buonanno, P., P. Vanin, and J. Vargas (2022). *A Modern Guide to the Economics of Crime*. Cheltenham, UK: Edward Elgar Publishing.

Card, D. (1992). Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage. *Industrial and Labor Relations Review 46*(1), 22.

Carr, J. B. and A. Packham (2019). SNAP benefits and crime: Evidence from changing disbursement schedules. *Review of Economics and Statistics 101*(2), 310–325.

Carta, F. and F. Colonna (2025, 9). Minimum income and household labour supply. *International Tax and Public Finance*.

Carta, F. and M. De Philippis (2021, February). The impact of the COVID-19 shock on labour income inequality: evidence from Italy. Questioni di Economia e Finanza (Occasional Papers) 606, Bank of Italy, Economic Research and International Relations Area.

Chai, A. and A. Moneta (2010, March). Retrospectives: Engel curves. *Journal of Economic Perspectives 24*(1), 225–40.

Coady, D., S. Jahan, B. Shang, and R. Matsumoto (2021, July). Guaranteed minimum income schemes in europe: Landscape and design. Working Paper 2021/179, IMF.

Curci, N., G. Grasso, P. Recchia, and M. Savegnago (2020, September). Anti-poverty measures in Italy: a microsimulation analysis. Temi di discussione (Economic working papers) 1298, Bank of Italy, Economic Research and International Relations Area.

Dachille, G., M. De Paola, and R. Nisticò (2025). Guaranteed minimum income and fertility. IZA Discussion Papers 18105, Institute of Labor Economics (IZA).

De Angelis, M. and P. G. Van Wolleghem (2023). Do the Most Vulnerable Know About Income Support Policies? The Case of the Italian Reddito d'Inclusione (ReI). *Italian Economic Journal 9*(2), 425–444.

de Blasio, G., G. Maggio, and C. Menon (2016). Down and out in Italian towns: Measuring the impact of economic downturns on crime. *Economics Letters 146*, 99–102.

Detotto, C. and M. Vannini (2010, November). Counting the cost of crime in italy. *Global Crime 11*(4), 421–435.

Ehrlich, I. (1973, may). Participation in Illegitimate Activities: A Theoretical and Empirical Investigation. *Journal of Political Economy 81*(3), 521–565.

Engel, E. (1857). Die productions- und consumtionsverhältnisse des königreichs sachsen. *Zeitschrift des Statistischen Bureaus des Königlichen Sächsischen Ministeriums des Innern 8*, 1–54.

Eurostat (2013). *European system of accounts - ESA 2010*. European Commission.

Ferraz, E., R. Soares, and J. Vargas (2022). *Chapter 8: Unbundling the relationship between economic shocks and crime*, pp. 184 – 204. Cheltenham, UK: Edward Elgar Publishing.

Foley, C. F. (2011). Welfare payments and crime. *Review of Economics and Statistics 93*(1), 97–112.

Fone, Z. S., J. J. Sabia, and R. Cesur (2023, 3). The unintended effects of minimum wage increases on crime. *Journal of Public Economics 219*, 104780.

Fougère, D., F. Kramarz, and J. Pouget (2009, September). Youth Unemployment and Crime in France. *Journal of the European Economic Association 7*(5), 909–938.

Frazer, H. and E. Marlier (2016, January). Minimum income schemes in europe - a study of national policies. Working paper, European Commission - Directorate-General for Employment, Social Affairs and Inclusion.

Frisch, R. and F. V. Waugh (1933, 10). Partial time regressions as compared with individual trends. *Econometrica 1*, 387.

Gelbach, J. B. (2016). When Do Covariates Matter? And Which Ones, and How Much? *Journal of Labor Economics 34*(2), 509–543.

Guardia di Finanza (2022). *Impegno "A Tutto Campo" Contro La Criminalita' a Tutela di Cittadini E Imprese*. Guardia di Finanza. In Italian.

Hendren, N. (2016). The policy elasticity. *Tax Policy and the Economy 30*, 51–89.

Hendren, N. and B. Sprung-Keyser (2020, 8). A unified welfare analysis of government policies. *Quarterly Journal of Economics 135*, 1209–1318.

Hirano, K. and G. Imbens (2005, 7). The propensity score with continuous treatments. *Applied Bayesian Modeling and Causal Inference from Incomplete-Data Perspectives: An Essential Journey with Donald Rubin's Statistical Family*, 73–84.

INPS (2023). *Reddito/Pensione di Cittadinanza - Report Gennaio 2023*. INPS. In Italian.

INPS (2024). Dichiarazioni Sostitutive Uniche (DSU) e valore ISEE. Technical report.

Istat (2015). *La Nuova Indagine Sulle Spese per Consumi in Italia*. Istat. In Italian.

Istat (2019). *Reati Contro la Persona e Contro la Proprietà: Vittime Ed Eventi*. Istat. In Italian.

Kirchmaier, T., M. Langella, and A. Manning (2023, 10). Commuting for crime. *The Economic Journal*, 1–26.

Mastrobuoni, G. (2020, nov). Crime is Terribly Revealing: Information Technology and Police Productivity. *The Review of Economic Studies 87*(6), 2727–2753.

Oster, A. and J. Agell (2007, June). Crime and Unemployment in Turbulent Times. *Journal of the European Economic Association 5*(4), 752–775.

Palmer, C., D. C. Phillips, and J. X. Sullivan (2019). Does emergency financial assistance reduce crime? *Journal of Public Economics 169*, 34–51.

Raphael, S. and R. Winter-Ebmer (2001, April). Identifying the Effect of Unemployment on Crime. *Journal of Law and Economics 44*(1), 259–283.

Rege, M., T. Skardhamar, K. Telle, and M. Votruba (2019). Job displacement and crime: Evidence from Norwegian register data. *Labour Economics 61*(C).

Visher, C. A. (1986). The rand inmate survey: A reanalysis. In A. Blumstein, J. Cohen, J. A. Roth, and C. A. Visher (Eds.), *Criminal Careers and "Career Criminals," Volume II*, pp. 161–211. Washington, DC: National Academies Press.

Watson, B., M. Guettabi, and M. Reimer (2020, oct). Universal Cash and Crime. *The Review of Economics and Statistics 102*(4), 678–689.

Wright, R., E. Tekin, V. Topalli, C. McClellan, T. Dickinson, and R. Rosenfeld (2017). Less cash, less crime: Evidence from the electronic benefit transfer program. *The Journal of Law & Economics 60*(2), pp. 361–383.

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 causes the pattern of thefts

	2019		2021		2022		2023
Regional level analysis							
β	-0.088	**	-0.136	**	-0.048	*	-0.012
	(0.035)		(0.055)		(0.024)		(0.023)
Obs.	280		280		280		280
% change:							
Italy	-6.811	**	-16.770	**	-5.680	**	-1.099
	(2.714)		(6.812)		(2.844)		(2.207)
North	-3.217	**	-7.734	**	-2.422	**	-0.439
	(1.282)		(3.141)		(1.213)		(0.881)
Center	-4.564	**	-12.035	**	-4.055	**	-0.742
	(1.818)		(4.888)		(2.030)		(1.490)
South	-16.049	**	-39.191	**	-13.690	**	-2.747
	(6.395)		(15.918)		(6.855)		(5.516)
Provincial level analysis							
β	-0.070	**	-0.187	**	-0.033		0.051
	(0.034)		(0.075)		(0.020)		(0.066)
Obs.	1455		1455		1455		1455
% change:							
Italy	-7.828	**	-14.890	**	-3.136		2.272
	(3.798)		(5.987)		(1.938)		(2.951)
North	-4.312	**	-7.877	**	-1.413		1.039
	(2.092)		(3.167)		(0.873)		(1.349)
Center	-5.658	**	-11.365	**	-2.230		1.603
	(2.745)		(4.569)		(1.378)		(2.082)
South	-16.820	**	-32.114	**	-7.403		5.345
	(8.161)		(12.911)		(4.576)		(6.942)

Notes: Outcome: number of thefts at the regional (upper panel) or provincial (lower panel) level. Controls: population size, employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2006-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the same level of the thefts aggregate. "β": coefficient of the main equation. "% change": is the percent change with respect to the pre-policy period. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

Table B3: RdC has no impact on violent crimes

	2019	2021	2022	2023
Regional level analysis				
β	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Obs.	280	280	280	280
% change:				
Italy	-1.512	5.359	2.430	0.951
	(2.095)	(5.135)	(4.268)	(4.816)
North	-0.956	3.309	1.387	0.508
	(1.325)	(3.170)	(2.436)	(2.575)
Center	-1.203	4.566	2.060	0.762
	(1.667)	(4.375)	(3.618)	(3.860)
South	-2.160	7.591	3.550	1.441
	(2.993)	(7.272)	(6.235)	(7.296)
Provincial level analysis				
β	0.000	0.001 **	0.000 ***	0.000 *
	(0.000)	(0.000)	(0.000)	(0.000)
Obs.	1455	1455	1455	1455
% change:				
Italy	1.866	11.906 ***	6.324 ***	4.218 *
	(2.074)	(4.554)	(2.032)	(2.381)
North	1.381	8.462 ***	3.828 ***	2.590 *
	(1.535)	(3.237)	(1.230)	(1.463)
Center	1.603	10.798 ***	5.343 ***	3.536 *
	(1.782)	(4.130)	(1.717)	(1.997)
South	2.423	15.518 ***	9.022 ***	5.996 *
	(2.694)	(5.936)	(2.900)	(3.386)

Notes: Outcome: number of violent crimes at the regional (upper panel) or provincial (lower panel) level. Controls: population size, employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2006-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the same level of the crime aggregate. "β": coefficient of the main equation. "% change": is the percent change with respect to the pre-policy period. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

Table B4: RdC increased expenditures

	2019		2021	2022		2023	
Total expenditure							
β	0.303	***	0.017	0.277	***	0.331	**
	(0.095)		(0.085)	(0.102)		(0.156)	
Obs.	100		100	100		100	
% change:							
Italy	1.388	***	0.122	1.939	***	1.872	**
	(0.435)		(0.623)	(0.713)		(0.880)	
North	0.829	***	0.071	1.045	***	0.945	**
	(0.260)		(0.363)	(0.384)		(0.444)	
Center	1.016	***	0.096	1.512	***	1.381	**
	(0.319)		(0.489)	(0.556)		(0.649)	
South	2.184	***	0.191	3.122	***	3.125	**
	(0.685)		(0.973)	(1.148)		(1.469)	
Food expenditure							
β	0.027		-0.001	-0.008		0.008	
	(0.039)		(0.049)	(0.065)		(0.084)	
Obs.	100		100	100		100	
% change:							
Italy	0.535		-0.025	-0.248		0.193	
	(0.767)		(1.529)	(1.945)		(2.029)	
North	0.346		-0.016	-0.145		0.106	
	(0.496)		(0.966)	(1.136)		(1.110)	
Center	0.383		-0.019	-0.189		0.139	
	(0.549)		(1.172)	(1.483)		(1.463)	
South	0.788		-0.036	-0.373		0.301	
	(1.129)		(2.231)	(2.927)		(3.167)	
Non-food expenditure							
β	0.276	***	0.017	0.285	***	0.323	**
	(0.080)		(0.072)	(0.101)		(0.135)	
Obs.	100		100	100		100	
% change:							
Italy	1.648	***	0.167	2.606	***	2.384	**
	(0.478)		(0.687)	(0.921)		(0.999)	
North	0.962	***	0.095	1.373	***	1.176	**
	(0.279)		(0.391)	(0.485)		(0.493)	
Center	1.215	***	0.132	2.047	***	1.771	**
	(0.352)		(0.542)	(0.723)		(0.742)	
South	2.651	***	0.267	4.288	***	4.068	**
	(0.768)		(1.095)	(1.515)		(1.705)	

Notes: Outcome: monthly household expenditure for "Total expenditure", "Food expenditure", "Non-food expenditure". Controls: employment rate, immigration rate; time-trend and fixed effects as explained in Section 4. Period: 2015-2023. Standard errors (parentheses) are robust to heteroskedasticity and clustered at the regional level. "β": coefficient of the main equation. "% change": is the percent change with respect to the pre-policy period. "Obs.": number of observations; ***[**](*) denotes significance at the 1[5](10)% confidence level.

Table B5: 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	-0.043***	-0.013*	-0.046***
	(0.005)	(0.006)	(0.008)	(0.012)	(0.007)	(0.010)
Women	0.008	0.000	-0.001	0.006	0.004	0.010
	(0.009)	(0.009)	(0.012)	(0.013)	(0.011)	(0.011)
Area:						
Center	0.021***	0.013*	0.001	-0.005	0.003	-0.002
	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)
South	0.162***	0.145***	0.029***	0.010	0.022***	0.004
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Education:						
Primary	0.236***	0.176***	0.080***	0.047***	0.065***	0.039***
	(0.009)	(0.011)	(0.013)	(0.013)	(0.012)	(0.012)
Secondary	0.074***	0.052***	0.012	-0.003	0.007	-0.005
	(0.008)	(0.009)	(0.007)	(0.009)	(0.006)	(0.008)
Age:						
< 6	0.083*	-0.033	0.133**	0.065	0.159***	0.097*
	(0.044)	(0.045)	(0.058)	(0.056)	(0.054)	(0.052)
[6,14]	0.012	-0.103***	0.155**	0.124*	0.143**	0.115*
	(0.036)	(0.038)	(0.068)	(0.066)	(0.063)	(0.061)
[15,17]	-0.016	-0.135**	0.112	0.060	0.128*	0.078
	(0.053)	(0.054)	(0.075)	(0.070)	(0.070)	(0.065)
[18,24]	0.181***	0.047	0.174***	0.130***	0.133***	0.095**
	(0.033)	(0.034)	(0.044)	(0.048)	(0.036)	(0.040)
[25,29]	0.225***	0.099***	0.068***	0.076***	0.065***	0.075***
	(0.025)	(0.027)	(0.021)	(0.026)	(0.019)	(0.024)
[30,34]	0.185***	0.067***	0.106***	0.111***	0.096***	0.104***
	(0.021)	(0.023)	(0.032)	(0.040)	(0.031)	(0.038)
[35,39]	0.161***	0.046**	0.069***	0.102***	0.051***	0.084***
	(0.020)	(0.023)	(0.018)	(0.024)	(0.016)	(0.022)
[40,44]	0.166***	0.053**	0.073***	0.106***	0.062***	0.095***
	(0.020)	(0.023)	(0.022)	(0.027)	(0.019)	(0.024)
[45,49]	0.098***	-0.014	0.126***	0.145***	0.113***	0.134***
	(0.018)	(0.021)	(0.036)	(0.036)	(0.035)	(0.034)
[50,54]	0.100***	-0.006	0.072***	0.084***	0.064***	0.077***
	(0.017)	(0.020)	(0.020)	(0.025)	(0.017)	(0.023)
[55,59]	0.072***	-0.025	0.092***	0.105***	0.072***	0.087***
	(0.016)	(0.019)	(0.021)	(0.026)	(0.016)	(0.022)
[60,64]	0.024*	-0.034**	0.080***	0.074***	0.074***	0.070***
	(0.014)	(0.015)	(0.018)	(0.017)	(0.016)	(0.016)
[65,69]	-0.043***	-0.057***	0.044***	0.036**	0.046***	0.039**
	(0.013)	(0.013)	(0.017)	(0.017)	(0.016)	(0.016)
[70,74]	-0.050***	-0.045***	0.035***	0.030**	0.022***	0.017**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.007)	(0.007)
# ind. with income	-0.109***	-0.120***	-0.019***	0.030***	-0.013**	0.033***
	(0.006)	(0.009)	(0.007)	(0.011)	(0.006)	(0.010)
Marital status:						
Non-married	-0.018*	-0.018*	-0.014	-0.000	-0.012	0.001
	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)
No more married	0.028**	0.026**	0.013	0.023**	-0.003	0.007
	(0.011)	(0.011)	(0.010)	(0.010)	(0.009)	(0.009)

Continues on next page

	HBS		SHIW			
	Consumption		Request		Access	
	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.	Demo.	Demo. & Lab.
Labour:						
Employed		0.023		-0.228***		-0.223***
		(0.022)		(0.045)		(0.042)
Self-employed		0.031		-0.253***		-0.231***
		(0.022)		(0.041)		(0.039)
Retired		-0.118***		-0.174***		-0.163***
		(0.016)		(0.033)		(0.031)
Blue collar		0.001		0.008		0.008
		(0.015)		(0.010)		(0.009)
White collar				-0.021		-0.008
				(0.015)		(0.013)
Manager				0.023		0.016
				(0.016)		(0.013)
White collar & Manager		-0.091***				
		(0.015)				
Intercept	-0.174***	0.017	-0.038**	0.132***	-0.024*	0.131***
	(0.015)	(0.025)	(0.016)	(0.036)	(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 equivalised expenditure. The dependent variable in columns SHIW takes value 1 if the household benefited from RdC. ***[**](*) denotes significance at the 1[5](10)% confidence level. Standard errors are robust to heteroschdasticity. Weighted data.