THE POLITICAL ECONOMY OF STIMULUS TRANSFERS

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ABSTRACT

Stimulus transfers are widely used during economic downturns, yet they are often poorly targeted from an economic perspective. I show that political incentives might help explain this discrepancy. I study one of the largest stimulus tax credits in Italy which excluded the poorest individuals and targeted middle-income earners. Leveraging quasi-random geographic variation in recipient shares and a difference-in-differences design, I find that the transfer raised the incumbent party's vote share by 0.18 percentage points per 1 pp rise in recipients. These gains persist for at least five years. Political returns are stronger in areas with relatively richer beneficiaries, despite weaker consumption responses, and electoral punishment for exclusion is similarly asymmetric: higher-income excluded individuals reduce support for the incumbent, while poorer excluded individuals do not. Voters also punish incumbents when transfers are revoked, helping explain why temporary programs are rarely repealed. A counterfactual transfer targeting poorer households would have increased the consumption response by 30\% but reduced electoral returns by at least 15\%. These findings highlight a key political-economy trade-off in stimulus design, where electoral incentives skew transfers toward politically responsive recipients, as opposed to consumption responsive recipients.

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1   Introduction

Stimulus payments are a frequent tool used by governments during economic down-turns. Macroeconomic theory identifies three key principles for effective stimulus payments: they should be timely, targeted, and temporary (Summers 2008).[1] However, decisions about the adoption (and repeal) of stimulus transfers are usually made by elected officials. Political economy considerations are thus likely to influence and potentially distort transfers' design and lead to suboptimal policy choices.[2] If politicians maximize their expected electoral benefits, stimulus payments might be targeted towards the median voter, or the more *electorally-responsive* individuals, which might not necessarily be the most *consumption-responsive* ones. Furthermore, concerns about the political costs of repealing transfers may result in supposedly temporary measures becoming permanent features of the tax system. Despite their potential importance, political considerations have received little attention so far by researchers studying fiscal stimulus.

I fill this gap by providing the first causal evidence on the electoral effects of stimulus transfers. Estimating the electoral consequences of stimulus transfers presents two empirical challenges. First, previous work has been limited by the lack of detailed data on both transfer recipients and voting behavior. Second, the empirical estimation requires an identification strategy that addresses the potential endogenous targeting of transfers. I overcome these challenges by exploiting a unique natural experiment arising from the adoption of the so-called "80 Euro Bonus", the largest stimulus transfer ever adopted in Italy. This policy was enacted in April 2014 by the Democratic Party government, led by newly appointed Prime Minister Matteo Renzi, as one of its very first policy measures. The official goal was to stimulate consumption and support the lower-middle class in the aftermath of the prolonged recession that began in 2011. While initially adopted as a temporary measure only for 2014, the Bonus became per-

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[1]First, payments must be delivered quickly to provide immediate economic support when needed. Second, they should be targeted at low-income, liquidity-constrained households, as these recipients are more likely to spend rather than save the funds, creating the desired stimulus effect (Kaplan and Violante 2022, Boehm, Fize and Jaravel 2025). Finally, stimulus measures should be temporary to avoid persistent budget deficits that could undermine long-term fiscal stability and credibility (Barro 1974).

[2]For instance, for the first time in U.S. history, President Donald J. Trump's name appears on the CARES Act stimulus checks. Many observers criticized the move as being a clear electoral tactic, aiming at claiming credit with voters for delivering benefits (Rappeport 2020, Gittleson 2021).

manent in 2015 and since then has never been abolished by any of the five different governments that took office since its adoption.

The program entailed a monthly transfer of €80 (approximately $100 at the time) for all payroll employees having a gross annual income between €8,145 and €26,000. At the time, individual median (average) income was around €16,000 (€19,000), so the transfer targeted mostly middle-income individuals (see Figure 1).[3] Employers were required to assess employees' eligibility based on their annual payroll income and to credit the Bonus in their paycheck. Thus, the transfer receipt was automatic and salient: every month, any eligible individual receiving the bonus could clearly see it in the paycheck listed as a distinct entry. Over 10 million payroll employees, corresponding to 20% of the voting-eligible population received the Bonus. Eligible beneficiaries started receiving the money at the end of May 2014, the week of the national-level elections for the European Parliament. This unique timing—with benefits arriving just before a major election—creates an ideal setting to isolate the electoral effect of the transfer from other policies that might influence voters' assessments of the incumbent and voting behavior.

To evaluate the electoral effects of the policy, I combine detailed administrative data on the number of bonus beneficiaries, the income and employment distribution, as well as electoral records for the universe of Italian municipalities. I then exploit the quasi-random geographic variation in program intensity at the municipality level, induced by pre-determined variation in the income and payroll employment distribution, and apply a difference-in-differences design comparing the change over time in the Democratic Party's electoral performance in municipalities with more vs. fewer beneficiaries among the voting-eligible population.

The key threat to this design is the possibility that time-varying municipal-specific shocks are correlated with program intensity.[4] I overcome this threat in five ways. First, I include a wide range of predetermined municipal characteristics interacted with time effects that allow me to flexibly control for potential differential trends or time-varying shocks affecting municipalities with different characteristics. Most importantly,

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[3]For comparison, household median (average) income in 2014 was around €25,000 (€30,500).

[4]Unlike majoritarian systems where politicians may target swing or core constituencies, Italy's proportional electoral system means that national politicians are only concerned with maximizing their party's national vote share. This eliminates incentives for geographically-targeted benefits, simplifying the empirical analysis by reducing concerns about endogenous allocation of the stimulus across municipalities.

I control non-parametrically for the two eligibility criteria of the bonus, i.e. the overall share of payroll employees as well as the share of total taxpayers in each income bin, so that my identification only relies on the residual variation in the distribution of payroll employees within income bins. Thus, any other potential confounding shock should differentially affect municipalities that have the same income and same payroll distribution over time, but different relative share of payroll employees in any specific bin. Second, pre-event placebo tests show that the estimated effects are not driven by pre-existing trends across municipalities with different treatment intensities. Third, I show that the results are robust to a series of alternative specifications, such as the inclusion of region-by-year fixed effects and the use of different weighting schemes. Fourth, I implement an alternative identification strategy that combines the difference-in-differences design with an instrumental variable approach. Specifically, I instrument the actual share of recipients with a predicted share based solely on exogenous eligibility rules and obtain very similar results. Fifth, I exploit the granularity of the data to identify the effects in a manner akin to a regression discontinuity design, by relying only on variation in the share of marginal beneficiaries around the eligibility thresholds in otherwise identical municipalities.

I start by documenting evidence of large short-run electoral rewards: a 1 percentage point increase in the share of recipients leads to a 0.18 percentage points increase in the incumbent party's vote share in 2014 relative to 2013, from a pre-treatment mean of 27%. For the average municipality (20.15% beneficiaries), the introduction of the program increased the incumbent vote share by around 4 percentage points. This corresponds to 35% of the overall change in the Democratic Party's performance between 2013 elections ((where it earned 27% of the votes) and 2014 elections (where it earned 40.8% of the vote). This corresponds to 35

I then show that the positive electoral rewards persist over time, up to the elections of 2019, 5 years from the policy adoption, when the Democratic Party still exhibits a 0.09 percentage point higher vote share in places with a higher share of recipients.[5] This is a striking result, considering that the prime minister who adopted the policy, Matteo Renzi, resigned in 2016 and the Democratic Party was voted out of government in 2018.

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[5]I provide suggestive evidence that the salience of the Bonus and its strong association with the incumbent likely played an important role in explaining persistence, a result that is consistent with recent evidence on the role of visibility in explaining the electoral response of voters in the context of public works (Huet-Vaughn 2019, Marx 2018).

The persistence of electoral rewards in favor of the Democratic Party thus suggests that voters are rationally attributing credit to the party that directly benefited them, a result in contrast with some previous evidence from other countries documenting errors and irrationality in voters' credit attribution (Bagues and Esteve-Volart 2016, Wolfers 2007, Pop-Eleches and Pop-Eleches 2012).[6]

Overall, the persistence of the electoral rewards helps to understand why what was supposed to be a temporary stimulus policy became a permanent tax cut, and provides important evidence of the presence of dynamic inconsistency problems in fiscal spending decisions. Further supporting this interpretation, I show that voters react not only to the receipt of transfers but also to their unexpected withdrawal. I exploit a quasi-random negative income shock affecting 1.5 million individuals who were initially—but mistakenly—credited the Bonus and later had to repay it when filing taxes. These individuals initially show mild positive electoral responses, consistent with a surprise windfall. However, once the repayment is enforced, support for the incumbent drops significantly. The timing and direction of these effects align with retrospective voting: voters reward incumbents for tangible economic gains and punish them once those gains are reversed. These results highlight the political risks associated with repealing transfers and help explain why temporary programs often become politically irreversible.

While delivering substantial electoral gains, the stimulus largely failed in inducing the desired fiscal multiplier effects: beneficiaries spent around 20% (30%) of the bonus on non-durables (durables) and saved the rest (Neri, Rondinelli and Scoccianti 2017). The limited consumption responses are possibly due to the poor targeting of the policy, as more than 25% of the payroll employees earned less than €10,000 in 2014 and were therefore mainly excluded from the policy. Indeed, Andini et al. (2018) show that consumption responses could have been significantly higher had the lowest-income workers been targeted, a result that is in line with the findings of Nygaard, Sorensen and Wang (2020) and Chetty, Friedman and Stepner (2024) for the US Covid-19 fiscal stimulus.

Why did politicians choose not to target the most *consumption-responsive* individuals? I provide two pieces of evidence suggesting that electoral incentives may

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[6]On the other hand, these results are consistent with the evidence provided by Drago, Galbiati and Sobbrio (2020), who document that Italian voters correctly attribute responsibility and electorally punish the incumbent party for the negative effects of criminal justice policies.

lead politicians to shift stimulus allocations away from poorer voters—who are the most consumption-responsive—and toward middle-income voters, who appear to be the most electorally responsive. First, I exploit heterogeneity in the relative distribution of beneficiaries within the eligibility window, conditional on the share of overall beneficiaries, to investigate heterogeneous responses by recipients' income. Here, I find that electoral effects are weaker in municipalities with relatively poorer beneficiaries, and significantly stronger in those where recipients are relatively closer to the middle of the income distribution.

Second, I exploit variation in the share of excluded individuals - those falling either below the minimum or above the maximum income thresholds for eligibility. The results show that the incumbent faces little electoral penalty from the presence of low-income excluded individuals, but experiences significantly stronger backlash when higher-income voters are excluded.

Taken together, these findings indicate that the marginal propensity to vote out of transfers increases with income: voters closer to the median income respond more strongly at the ballot box. When combined with existing evidence that the marginal propensity to consume out of the bonus instead decreases with income, this creates a fundamental tension between political and economic efficiency. While targeting poorer voters would maximize the stimulative impact of transfers, electoral incentives push politicians to favor middle-income groups who are more likely to reward them at the polls. This tension helps explain why the design of stimulus policies often departs from economic optimality.

While I find that areas benefiting most from the policy show greater support for the Democratic Party, these effects may reflect different mechanisms, each with distinct implications for electoral accountability and policy design. Voters may reward personal gains (*pocketbook voting*, e.g., Healy, Persson and Snowberg 2017, Finan and Schechter 2012) or broader improvements in the economy (*sociotropic voting* or *political multiplier*, e.g., Ansolabehere, Meredith and Snowberg 2014, Lewis-Beck and Stegmaier 2018).[7] These mechanisms lead to different predictions: if only direct beneficiaries respond, politicians may target electorally responsive voters; if voters care about aggregate conditions, targeting should favor consumption responsiveness.

I provide several pieces of evidence to distinguish between these explanations. First,

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[7]See Healy and Malhotra (2013) for a review of retrospective voting theories.

the policy's immediate electoral impact in May 2014 predates any plausible macroeconomic effects, making a political multiplier mechanism unlikely. The long-run persistent effects are also inconsistent with a *political multiplier* response, as the theory suggests that voters would reward the current incumbent for the improvement in economic conditions, but in 2019 the Democratic Party was no longer part of the government. Moreover, expectations about future national conditions should be uniform across regions, and thus unrelated to local recipient density. While voters might respond to local economic gains, I show that controlling for local economic trends does not weaken—and may even strengthen—the estimated electoral effects, suggesting that local economic improvements are not the main driver.

In the last part of the paper, I supplement my aggregate-level results with rich individual-level survey data on a panel of voters interviewed right before and after the bonus receipt. I find evidence of a substantial increase in the probability of switching to support the Democratic Party among voters who are likely bonus recipients. This further suggests that the stimulus played a key role in shifting attitudes toward the incumbent. Overall, the evidence suggests that electoral rewards are more likely driven by the direct response of recipients rather than by any indirect effects of economic stimulus resulting from the bonus. These findings further help explain why electorally motivated politicians may choose to direct transfers away from poorer voters, who have a high marginal propensity to consume, and instead toward middle-income individuals, who are more likely to reward them with votes.

To quantify the tradeoff between economic political incentives, I conduct a simple counterfactual analysis of a policy alternative that: (a) preserves the total short-run cost by keeping the number of recipients fixed, (b) maintains policy feasibility by retaining the same transfer structure, and (c) shifts benefits toward the poorest potential recipients. Specifically, I analyze the effects of expanding eligibility to those earning between €0 and €20,000, rather than the original €8,000 to €26,000 range. Using heterogeneity estimates to linearly project the electoral response of poorer recipients, I find that this economically superior policy would reduce electoral returns by at least 15%. Importantly, estimates from the Bank of Italy indicate this alternative targeting would increase the overall consumption stimulus by roughly 30%. This implies a marginal rate of substitution of 2:1 between economic and political incentives—for each percentage point of electoral advantage politicians sacrifice, they could achieve a two

percentage point gain in economic stimulus effectiveness.

Contribution to the literature: This paper contributes to our understanding of how political considerations influence fiscal policy design and implementation, shedding light on the complex interplay between economic and political incentives in the context of stimulus transfers. My findings thus provide new insights to three strands of the literature. First, this paper contributes to the large literature on the effects of stimulus payments (e.g. Johnson, Parker and Souleles (2006), Parker et al. (2013), Kaplan and Violante (2014), Parker (2017), Coibion, Gorodnichenko and Weber (2020), Baker et al. (2020), Chetty et al. (2020), Kaplan, Moll and Violante (2020), Parker et al. (2022)). This literature has mostly focused on estimating the economic and consumption responses to payments, but has never looked at either the political determinants or consequences of transfers. A stable finding of this research is that consumption responses are larger among low-income and liquidity-constrained individuals, while higher-income individuals are more likely to save funds or use them to repay debt. Taking into account political economy considerations can help understand the design of fiscal stimulus programs and why sometimes adopted programs diverge from what would be predicted to be the most effective program from a stimulus point of view, as well as heterogeneity in the types of programs adopted across countries.

Second, I contribute to the literature investigating the political economy of government spending, by providing the first causal estimates of the electoral effects of direct transfer receipt in the context of a mature democracy. Previous studies finding pro-incumbent rewards for distributive allocations mostly focused on small conditional cash transfers or clientelistic promises in young democracies (Wantchekon 2003, Stokes 2005, Pop-Eleches and Pop-Eleches 2012, Finan and Schechter 2012, Labonne 2013, Galiani et al. 2019, Caprettini, Casaburi and Venturini 2019) or, for the US, on allocations of funds to locations, rather than individuals (Healy and Malhotra 2009, Cascio and Washington 2014, Huet-Vaughn 2019, Slattery 2022).[8] In the former, observed responses may reflect clientelistic pressures, where voters might fear losing future benefits unless they support the incumbent (Bobonis et al. 2022, Duarte et al. 2019). In the latter, it is harder to isolate mechanisms or trace heterogeneous responses, as transfers are not individually targeted and attribution is less clear.

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[8]Relatedly, in Italy, Albanese, de Blasio and Incoronato (2024) show electoral gains for parties promoting greater state intervention in localities that benefit from firm subsidies and infrastructure spending in the past.

While electoral rewards are more likely to be seen as a clientelistic exchanges in contexts with weak institutions or discretionary allocations, it is not clear whether we should expect the same type of reciprocal exchange in contexts with stronger institutions or when benefits' receipt is based on clear and transparent rules, and benefit receipt does not require any political intermediation.[9] By leveraging a nationally uniform, transparent, and individually targeted policy in an institutionalized setting, my design thus allows for a cleaner test of how voters respond to observable and attributable benefits—without political intermediation. I show that even in mature democracies, and in the absence of clientelism, voters reward incumbents for direct transfers, consistent with retrospective voting and reciprocal motivations. Furthermore, my unique design allows me to document for the first time heterogeneous voting responses by recipients' income — an essential dimension given that most redistributive policies are designed around income thresholds. I also distinguish between the effects of receiving a transfer and being excluded from one, showing that electoral rewards and punishments are asymmetrically distributed across the income distribution. This distinction is key to understanding how different groups of voters react to policy assignments and how electoral incentives shape the design of transfer programs.

Third, and more generally, this paper relates to the broad literature on economic voting, documenting a robust relationship between economic conditions and incumbent electoral performance. Some recent papers have highlighted the presence of voters' attribution errors when rewards are present for improvements in economic conditions that are unrelated to the incumbent's actions (Wolfers 2007, Healy and Malhotra 2009, Ansolabehere, Meredith and Snowberg 2014, Bagues and Esteve-Volart 2016, Healy, Persson and Snowberg 2017). My results provide evidence against the hypothesis of attribution errors, and suggest that voters are fairly sophisticated and respond to policies benefitting them in a way that is consistent with models of retrospective voting and electoral accountability.

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[9]Indeed, recent work by Rendleman and Yoder (2024) find limited and short-lived effects of State EITC expansions on voting for implementing governors.

2 Institutional Background

2.1 The 80 Euro Bonus

Among European countries, Italy was one of the most severely affected by the 2008 global financial crisis and the subsequent Eurozone sovereign debt crisis. From 2007 to 2013, Italian GDP experienced a contraction of 8.7%, a decline four times as large as the average experienced by the euro area. Total consumption decreased by over 6% and the recession was prolonged by stagnation in internal demand (Andini et al. 2018). Given the economic context, in 2014 one of the first announcements of the new Democratic Party's Prime Minister, Matteo Renzi, was the creation of a large stimulus transfer to redistribute income towards poorer workers and foster the recovery by boosting consumption. The policy was first announced as the "80 euro bonus" on 12 March 2014 and formally adopted on 24 April (Law Decree 66/2014).[10]

The Bonus is a monthly tax credit of €80 for *payroll employees* with a total gross annual income between €8,145 and €26,000[11] The transfer represents an increase of 6 to 12% in the monthly wage bill. For comparison, the average wage growth in 2014 was around 1.4%. Transfer enrollment is automatic: The employer is required to assess eligibility based on the individual's earnings and reduce payroll tax withholdings accordingly, thus increasing take-home pay by €80 each month. Importantly, the Bonus appears as a separate line in the paystub, making it particularly visible and tangible to recipients. Appendix Figure A1 provides an example of a standard paycheck highlighting the distinct Bonus credit.[12] Eligibility is determined based on the individual's total annual gross income, but is assessed by employers based on an incomplete information set, as workers might receive taxable income from other sources. As a consequence, some individuals were initially misclassified as eligible, and thus had to reimburse the Bonus received when they filed taxes the next year. Overall, 9.7 million individuals were granted the Bonus in 2014, but about 1.5 million people had to return it in 2015.[13]

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[10]The opening statement of the Law Decree explicitly mentions that the objective of the policy was to stimulate consumption.

[11]There is a small phase-out region for earnings between €24,000 and €26,000, where the amount of the Bonus is reduced linearly with income.

[12]Indeed, Matteo Renzi frequently underscored the Bonus's visibility in paychecks—mentioning it in public addresses and, in May 2014, tweeting a photo of public sector workers' first pay slips displaying the €80 credit.

[13]Conversely, approximately 500,000 individuals who were eligible did not receive the Bonus in 2014

While initially funded only for 2014, the Bonus became permanent with the Budget Law for 2015. By automatically setting aside the financial resources needed to pay for the Bonus in future years, this choice imposes constraints also on future governments, making it difficult and politically costly to abolish the policy in the future. Notably, the policy is still in place to date.[14]

Administrative and economic factors help explain the structure of the policy and eligibility criteria. The choice of giving the Bonus only to payroll employees was motivated by the objective to make the implementation fast and automatic, with the Bonus directly credited by the employer as a reduction in the automatic deductions. The income eligibility, and in particular the lower threshold, is related to the fact that the policy was designed as a tax credit, and the government decided to make it non-refundable, that is to grant the bonus only to those having earnings greater than the tax deduction amount, €8,145.

While officially framed as an economic stimulus, the timing and communication surrounding the policy suggest a clear electoral intent: in the press conference announcing the Bonus, Prime Minister Renzi emphasized the government's effort to ensure payments would reach paychecks before the European elections. This perception was reinforced by strong and coordinated opposition backlash. Silvio Berlusconi, former Prime Minister and leader of the center-right Forza Italia party, dismissed the Bonus as an "electoral tip"; Beppe Grillo, founder and political leader of the anti-establishment Five Star Movement, called it a "miserable bribe"; and Giorgia Meloni, then leader of the national-conservative party Fratelli d'Italia and now Prime Minister, claimed the measure was useful only to "gather consensus" rather than support the real economy (see Appendix Figure A2).

2.1.1 The economic impacts of the Bonus

A series of studies finds that, despite its salience and magnitude, the €80 Bonus generated only modest real-economy effects. Government estimates indicate that the policy amounted to a fiscal transfer of 7 billion euros in 2014—roughly 0.4% of GDP. However, the Italian Parliamentary Budget Office (UPB)—Italy's counterpart to the U.S.

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and were thus able to claim the Bonus during tax filing in 2015.

[14]Beginning January 2018, the maximum income threshold was slightly raised from €26,000 to €26,600. In January 2020, the amount was raised to €100 per month.

Congressional Budget Office—estimated that its impact on GDP was only about 0.2% in 2015.

In a study conducted by the Bank of Italy, Neri, Rondinelli and Scoccianti 2017 exploit the panel component of the Survey of Household Income and Wealth and estimate a marginal propensity to consume out of the Bonus of around 0.5 in terms of overall consumption, and of 0.25 when focusing only on non-durables. They also estimate that the consumption response was twice as large among lower-income, liquidity constrained households.

Using the same data, the Italian Congressional Budget Office analyzed the consumption and redistributive impacts of the policy (UPB 2015). The main results are reproduced in Appendix Figure A3 . Beneficiary incidence peaks at 43–47% in income deciles 2–8 but falls to 39% in the poorest decile, counter to the policy's redistributive intent. Simultaneously, estimated MPCs among recipients decline from about 55% in the lowest decile to roughly 30% in the highest, indicating that a substantial share of the Bonus was received by households with lower marginal spending responses.

Andini et al. (2018) find similar estimates of the marginal propensity to consume and show that 30% of the total stimulus spending was allocated to recipients who are not consumption constrained. Using machine learning techniques, they show that the effectiveness of the program could have been substantially increased if the allocation had been decided using a simple machine learning algorithm trained to identify individuals with the highest propensity to consume based on observable characteristics available in the politician's information set.

Exploiting rich matched employer-employee data, Zurla (2021) shows that firms captured up to 30% of the transfer, by lowering earnings of eligible workers by around 2% after the bonus introduction. The limited pass-through of the transfer likely contributed to attenuating the fiscal stimulus resulting from the policy.

Collectively, these patterns imply that the Bonus failed both its redistributive objective—by excluding many low-income households—and its consumption-stimulus goal—by channeling resources toward recipients with lower MPC. Coupled with wage offsetting by firms, the overall macroeconomic impact appears limited.

2.2 Elections

Italy is a parliamentary republic with a multi-party system. Both national parliamentary elections and European elections are held every five years using a proportional representation system with at-large districts. This institutional feature—unlike the majoritarian or district-based systems used in countries like the United States—reduces the incentive for politicians to target specific geographic constituencies (such as swing districts or counties) and implies that political parties aim to maximize national vote shares rather than focus on local pork-barrel strategies.

European elections are particularly important in Italy because they occur one year after national elections and serve as a key opportunity for voters to express approval or discontent with the sitting government. Importantly, these elections do not affect the survival of the national government, allowing voters to signal their preferences without the risk of causing political instability. This creates a unique empirical setting to study how voters respond to stimulus transfers: any observed change in incumbent support is more likely to reflect genuine sentiment about policy performance, rather than strategic behavior motivated by concerns over future access to benefits.

The 2014 European elections were especially significant for the newly formed government. Following the 2013 parliamentary elections, Italy faced a fragmented political landscape, with no coalition commanding a clear majority. The center-left Democratic Party (PD), the center-right coalition led by Silvio Berlusconi's PDL, and the anti-establishment 5-Star Movement each captured substantial shares of the vote. A coalition government led by Enrico Letta of the PD was eventually formed. However, in late 2013, Matteo Renzi won the PD's leadership primary and, by February 2014, replaced Letta as Prime Minister. This made the upcoming European elections in May 2014 a critical test of Renzi's national mandate.

The introduction of the 80 Euro Bonus shortly before the election drew criticism from the opposition, particularly the 5-Star Movement, which denounced it as a transparent electoral maneuver aimed at boosting short-term support for the incumbent party.

Despite this, the 2014 European elections marked a historic victory for the Democratic Party. While populist and euroskeptic parties made gains across much of Europe, Italian voters delivered a decisive endorsement of the pro-European PD under Renzi's leadership. The PD won 40.8% of the national vote—the highest share ever achieved

by a single party in a national election since 1958.

Subsequently, in December 2016, the government held a national referendum on a package of constitutional reforms. Renzi had personally championed the reforms and vowed to resign if they were rejected, effectively turning the referendum into a vote of confidence on his leadership. The 'No' vote won decisively, with 60% of the vote, leading to Renzi's resignation. Nevertheless, the PD remained the dominant party in government.

New parliamentary elections were held in March 2018 and resulted in the formation of a coalition government between the 5-Star Movement and the right-wing Northern League. The next round of European elections took place in 2019. A timeline of all major political events relevant to this analysis is presented in Appendix Figure A4 .

3   Data and Empirical Strategy

3.1   Data

To conduct my analysis, I combine municipal-level data from several sources. First, I use data on the annual income distribution of the universe of Italian municipalities, based on individual Tax Returns, provided by the Ministry of Treasury, for the years from 2008 to 2019. The data report the total amount of income declared and the total number of individuals declaring income in each of the following seven income groups: 0-10,000; 10,000-15,000; 15,000-26000; 26000-55,000; 55,000-75,000; 75,000-120,000; and above 120,000. Since 2014, the data also contains information about the number of 80 Euro Bonus recipients and the average amount per capita.[15] Second, I use data on national and European elections from 2008 to 2019 for each municipality, provided by the Italian Ministry of the Interior. Third, I use data on municipal characteristics from the the Italian Statistical Office (ISTAT) on factors that might influence voting behavior to be used as controls. These include the educational level of the municipal population, the share of foreign-born individuals[16], the share of women, and the

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[15]For 2014 only, I have access to more detailed information about both bonus recipients as well as overall number of payroll employess for 1000-euro sized income bins.

[16]Foreign-born individuals are eligible to receive the bonus if they are payroll employees, but they are not eligible to vote in national elections, unless they become citizens. Among them, registered residents who are citizens of EU member states are eligible to vote in European elections.

local unemployment rate. Fourth, I complement the aggregate-level information with individual-level data from the Italian National Elections Study Survey (ITANES), for the years 2013-2014. The data contains information on individuals' demographics and political orientation, voting behavior, subjective evaluation of the economic situation as well as overall evaluation of government actions. Additional details on the survey design and on the specific questions used are available in Appendix B.

Figure 1 shows the taxable income distribution of Italy in 2013 using data from official tax records, for both the universe of taxpayers (green) - almost 41 million individuals - and the sub-sample of payroll employees (orange), which represent 50% of total taxpayers. The figure delivers two main messages. First, the transfer has a very wide coverage: 44% (46%) of Italian taxpayers (payroll employees) reported annual tax earnings between €10,000 and €26,000 in 2013,and thus could be eligible for the Bonus. Second, the transfer targets exactly the middle of the income distribution: 32% (25%) of taxpayers (payroll employees) earn less than €10,000, and thus were mostly excluded from the Bonus. Moving from individual to household data, according to the Bank of Italy SHIW data, 21.9% of Italian households receive the Bonus. Given the absence of family means-testing, the share of recipients increase among households with more than one income recipients: 28.2% of households with 2 income recipients benefited from the Bonus. Looking at equalized disposable income, 13% (17%) of recipients belong to the bottom (top) 5th of the distribution (Neri, Rondinelli and Scoccianti 2017).

To analyze the electoral response to the 80 Euro Bonus, I construct a measure of treatment intensity at the municipality level, measured by the number of Bonus recipients in 2014 over the voting eligible population:

$$Recipients_{m,2014} = \frac{N.Recipients_{m,2014}}{VotingEligiblePopulation_{m,2014}}$$

Descriptive statistics for all outcomes and relevant controls are displayed in A1 . Control variables are measured in 2013, prior to the introduction of the policy.

3.2 Empirical Strategy

My empirical strategy exploits cross-sectional variation across municipalities in the share of recipients, which is driven by pre-determined variation in the income and occupation distribution that determine bonus eligibility, and thus local exposure to

the policy, combined with variation over time in a difference-in-differences design.

My baseline specification estimates the short-run effect of the introduction of the Bonus on the incumbent party vote-share, as follows:

$$\Delta Y_{m,2013-2014} = \beta Recipients_{m,2014} + X'_m \zeta + \delta_t + \epsilon_{mt} \tag{3.1}$$

where $\Delta Y_{2013-2014}$ denotes the change in the number of votes for the Democratic Party over total voters, in municipality m, between the elections in year 2014 and 2013, $Recipients_{m,2014}$ is my main treatment intensity variable, measuring the number of Bonus recipients in 2014 over the voting eligible population[17], X_m is a matrix of controls measured at baseline, including % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high-school, % with college degree, % of foreign-born, % of women, and the local unemployment rate, and δ_t is a year fixed-effect, allowing me to control for year-specific shocks commonly affecting all municipalities. The coefficient of interest is β, capturing the differential impact of the 80 Euro Bonus' treatment intensity on incumbent's vote share. Standard errors are clustered at the municipality level.

Since the dependent variable is the change in vote share across two consecutive elections, this specification is analogous to estimating a two-period panel with municipality fixed effects, thus differencing out any time-invariant unobserved characteristics that might affect electoral outcomes across municipalities.

As it is standard in difference-in-differences estimation, the identification of my coefficient of interest relies on two assumptions. The first is the absence of contemporary shocks that differentially affect municipalities with more vs. fewer recipients. After a careful review, I was not able to identify any other policies targeting payroll employees in the eligible income range that occurred before, concurrently with the 80 Euro Bonus or afterwards. Furthermore, this was the very first policy announced and adopted by the newly formed government of Matteo Renzi, in a sudden and unexpected way. These unique features allows me to isolate the effect of the specific policy on voting behavior. The second assumption is the presence of counter-factual parallel trends between high and low intensity municipalities. This assumption is fundamentally untestable, but could be proxied by looking at pre-event trends, which should be informative of post-

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[17]In Italy, as in many other European countries, there is no voter registration requirement.

event trends.[18]

To estimate pre-event trends, as well as to inspect long-run persistence of the treatment effect, I extend the horizon of analysis and run the following econometric specification to estimate a standard dynamic difference-in-differences model:

$$Y_{m,t} = \sum_{k \neq 2013} \beta_k \left(\text{Year}_{t=k} \times \text{Recipients}_{m,2014} \right) + \sum_k \gamma_k \left(\text{Year}_{t=k} \times X_{m,2013} \right) + \mu_m + \epsilon_{m,t}$$

(3.2)

Where $Y_{m,t}$ denotes the vote share for the Democratic Party in year k, with k=2008, 2009, 2014, 2018, 2019, and year 2013 is omitted as reference year. So, for example, when k=2008, I estimate the placebo effect of the 80 Euro Bonus intensity on the electoral performance of the Democratic Party between 2008 and 2013. Controls are measured at baseline in 2013 and interacted with year fixed-effects.

A natural concern with this strategy is that municipalities with a high share of recipients may also differ in other important ways that independently influence the evolution of their electoral outcomes over time. For example, one might be concerned that places with a large number of recipients might be relatively poorer, and be more likely to experience a shift towards the left between 2013 and 2014 for reasons unrelated to the policy. I address this concern by exploiting the particular eligibility requirements of the policy. In order to be eligible, a worker needs to: a) be a payroll employee and b) earn between €8,145 and €26,000.

Because the policy was implemented automatically by employers using tax data, and because individuals did not have to apply or opt in, the share of Bonus recipients in each municipality is thus largely predetermined by local income and employment dis-

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[18]This notion that pre-treatment trends across different levels of treatment intensity can proxy for post-treatment counterfactual trends is analogous to the "strong parallel trends" assumption discussed by Callaway, Goodman-Bacon and Sant'Anna (2024). As they highlight, in the presence of continuous or dose treatments, identification of causal effects require that "the path of outcomes for lower-dose units must reflect how higher-dose units' outcomes would have changed had they instead experienced the lower dose". This requires assuming the absence of treatment effect heterogeneity by dose levels or, more lightly, that units cannot select-on-gains into a particular treatment intensity, an assumption that is untestable but likely to be satisfied by design in my context given that units in my context cannot manipulate treatment intensity. Furthermore, the evidence in the left panel of Figure 3 trasparently illustrates in a non-parametric way the relationship between treatment intensity and changes in the share of PD votes across all values of treatments. The figure displays a clear positive and close to linear relationship between the two variables. This linearity suggests that changes in outcome variables are relatively constant across levels of exposure, implying limited scope for treatment effect heterogeneity.

tributions prior to the policy's announcement and plausibly exogenous to local political preferences.

In particular, I can control separately for the two requirements, namely the share of payroll employees and the share of workers whose income falls in the eligibility range, while still having variation in the actual share of recipients, which stems from the fact that different municipalities might have different shares of payroll employees *within* the eligibility threshold. This allows me to compare municipalities that had differential exposure to the treatment because of the intersection between the two relevant dimensions for eligibility (income and payroll employment), but that were otherwise similar along other dimensions.

The key identifying assumption hence is that, once I control for income distribution and payroll employment separately, the residual variation in treatment exposure is assumed to be orthogonal to unobserved, time-varying shocks to electoral outcomes. There are several reasons why this assumption is not only plausible, but in fact quite conservative. First, the Bonus was introduced abruptly, announced and implemented within the span of a few months by a new government, with no prior consultation or gradual rollout that could have allowed for strategic behavior at the local level. Individuals could not self-select into the policy, and municipalities could neither apply for additional funds nor shape implementation.

Second, the specific income band used to define eligibility—€8,145 to €26,000—is itself arbitrary in the sense that it does not align with other major policy thresholds. It does not mark the boundary of any welfare program, tax bracket, or social contribution schedule. Nor does it correspond to a socially cohesive or politically salient group. It does not isolate "the poor" (many of whom fall below the lower threshold), nor does it pick out a middle-class constituency. As such, there is little reason to believe that the interaction of this band with payroll employment identifies a group that would have experienced unique political dynamics in the absence of the Bonus.

A graphical intuition of the variation exploited for identification can be grasped by looking at Figure 2. On the left, I plot the raw variation in the share of Bonus recipients at the municipality level, while in the right panel I plot the residual variation, after controlling for the income distribution and the share of payroll employees. While the left panel displays a strong geographic pattern in the share of recipients, this disappears on the right, suggesting the importance of including income and payroll distribution

controls.

To further validate that my treatment variable captures variation driven by institutional eligibility rules—and not by unobserved confounders, I examine how much of the variation in Bonus recipient shares across municipalities can be explained by predetermined eligibility factors. Appendix Table A2 reports a sequence of regressions where the dependent variable is the share of Bonus recipients in 2014, and each column adds a richer set of predictors. Payroll and income shares alone explain 78.4% of variation in recipient share. Province fixed effects and detailed income brackets raise the to 86.6%, and a predicted interaction term increases it further to 87.1%. Attribution error and demographic controls add little additional explanatory power. Thus, nearly all of the variation in treatment intensity is driven by mechanical, pre-determined features of the policy. This provides reassurance that the identifying variation used in the main analysis is closely aligned with exogenous eligibility rules.

A remaining concern is that the residual variation not explained by income and occupation distribution could still be correlated with omitted variables that correlate with changes in the electoral behavior over time, leading to biased estimates of electoral effects. To test this, I regress the residuals from Column (4) on a comprehensive set of pre-treatment municipal characteristics, including political behavior (2013 PD vote share, turnout, change in PD vote share from 2008 to 2013), labor market conditions (unemployment rate, median income, income inequality), demographic structure (age, gender, education, foreign-born population), and urbanization indicators. Each regression is run separately, and all independent variables are standardized to facilitate comparison of effect sizes. The results are presented in Table A3 .

Column 1 of Table A3 shows that many of these variables are strongly associated with the raw Bonus recipient share. For instance, the share of immigrants, the unemployment rate, and education levels are all positively correlated with Bonus receipt, reflecting the underlying structure of eligibility. However, Column 2, which examines residual variation, reveals that these associations largely disappear after accounting for the mechanical components of the policy. Coefficients are small in magnitude, statistically insignificant, and lack a consistent directional pattern. Notably, the residual variation is not positively correlated with pre-policy support for the Democratic Party, turnout levels, or changes in political alignment between 2008 and 2013. This weak correlation with prior vote shares and turnout mitigates concerns that residual expo-

sure to the Bonus reflects local strategic targeting or endogenous political alignment. In interpreting these results, it is useful to focus on the political variables. The raw share of bonus recipients is negatively associated with support for right-wing parties and positively associated with the share of foreign-born residents. However, these relationships are plausibly driven by labor market composition, not political manipulation. Importantly, the Bonus share is uncorrelated with changes in PD support, turnout or support for other parties between 2008 and 2013, which are critical for assessing pre-trends. These findings suggest that the Bonus was not disproportionately directed to municipalities where the Democratic Party had been gaining or losing support in the years prior to the policy.

Residual variation is also uncorrelated with levels or changes in voter turnout, suggesting that the treatment variation does not reflect anticipated political mobilization effects. While one might worry that the Bonus could have been targeted at politically valuable municipalities—such as provincial capitals or areas with high past turnout—the analysis finds no robust evidence of such patterns. The few statistically significant residual correlations (e.g., with migrant share or capital status) are modest in size and do not align with electoral incentives.

Taken together, the predictive and residual diagnostic exercises provide strong support for the validity of the identification strategy. The vast majority of cross-sectional variation in treatment exposure is accounted for by institutional rules and implementation frictions, and the remaining variation is not systematically correlated with factors that plausibly affect voting behavior.

4   Results

4.1   Short-run Effects

The first key results of the paper are summarized in the two graphs of Figure 3. The left graph provides a visual analysis of the short-run relationship between treatment intensity and the change in the share of votes for the incumbent party (the PD) between 2013 and 2014. The binned scatterplot displays a clear positive and linear relationship. Table 1 formalizes the analysis and presents results of the difference-in-differences estimation highlighted in equation 3.1 and allows me to assess stability across different

specifications. The effect is positive and significant. A 1 percentage point increase in the share of Bonus recipients raises the vote share of the Democratic Party by 0.15 percentage points. The magnitude of the effect increases to 0.18 after including controls in column (2). The estimates indicate that, in a city with average Bonus exposure (21.5% of recipients), the reward for the incumbent party is approximately 4 percentage points, an increase of 17% relative to 2013. Results are robust to the clustering of standard errors at the province level in column (3), allowing for spatial correlation (4). and become even larger when weighting the regression by the size of the electorate in 2013, as shown in column (5), suggesting that effects are stronger in larger municipalities.Results are also robust to the inclusion of region-times-year fixed effects in column (6), which allow me to flexibly account for any region-specific time-varying shocks. As a further robustness check, in column (7), I inspect the change in the vote share relative to the past European election in 2009, and obtain very similar results. This alleviates concerns related to the fact that European elections might not be comparable with national ones. Results of the pre-event placebo regressions, displayed in Panel B of Figure 3, confirm the robustness of the estimates, as I cannot detect any significant effect of the policy intensity on the pre-policy trends in votes of the Democratic Party.

As an additional robustness exercise, I implement an alternative instrumental variables (IV) strategy where I predict the share of Bonus recipients at the municipal level based solely on pre-determined, structural features of the income and employment distribution. Specifically, I construct a predicted share of recipients by interacting the total share of payroll employees in the municipality with the share of all taxpayers—both payroll and non-payroll—earning between €8,000 and €26,000 euros. This composite measure captures exogenous variation in potential eligibility driven by the local income structure and employment composition, abstracting from endogenous political or economic shocks. Therefore, this approach also helps address concerns that the main results may be confounded by endogenous municipal characteristics influencing both Bonus uptake and political preferences. By leveraging only the predicted, mechanical component of treatment intensity, this IV strategy isolates plausibly exogenous variation and offers a complementary and credible identification of the policy's electoral effects. Indeed, column (4) of Table A2 confirms that this predicted eligibility-based instrument is a strong and consistent determinant of actual treatment intensity, even after controlling for a rich set of demographic and socioeconomic characteristics, as well

as province fixed effects. The strength of the first stage is illustrated in Figure A6 , which plots the relationship between the predicted and actual share of Bonus recipients across municipalities. The figure reveals a strong linear relationship, with municipalities tightly clustered around the fitted line. The corresponding first-stage estimates in Table A4 show high F-statistics and first-stage coefficient around 0.8, validating the strength and relevance of the instrument.

The resulting 2SLS estimates, reported in Table A4 , show positive and statistically significant effects of treatment intensity on the change in Democratic Party vote share between 2013 and 2014. The magnitude of the effect closely aligns with baseline OLS estimates, reinforcing the interpretation that the observed electoral response is driven by Bonus receipt rather than other omitted variables or selection bias.

4.2 Drivers of Electoral Responses: Persuasion vs. Mobilization

A natural question to ask is whether the gains in Democratic Party's performance are primarily driven by changes in the extensive margin, namely voter turnout (*mobilization*), or by shifts in vote choices among existing voters (*persuasion*). Although disentangling these two effects precisely with aggregate-level data poses challenges, I provide suggestive evidence by separately examining the impacts on votes received by other parties and on overall turnout. Results are reported in Table 2.

The empirical evidence indicates minimal to no negative impact on votes garnered by competing parties, as shown by small and mostly statistically insignificant coefficients in columns (1)–(3). Instead, the primary channel through which the Bonus policy influenced electoral outcomes appears to be significantly increased voter turnout. Specifically, column (4) shows that municipalities with higher shares of Bonus recipients experienced a significantly higher relative turnout. This is particularly relevant given that European elections typically exhibit substantially lower turnout than national contests. Indeed, the average dependent variable in this specification is –14 percentage points, confirming a general turnout decline between 2013 and 2014. Against this backdrop, the observed positive coefficient should be interpreted not as a net increase in participation, but rather as a relative retention effect: the Bonus policy appears to have mitigated the natural turnout drop between national and European elections, success-

fully keeping voters engaged who would have otherwise dropped out into abstention. Consistent with this evidence, I find even larger effects when using the share of votes for the Democratic Party divided by the share of the voting eligible population.

4.3   Magnitudes and Cost per Vote

I perform two exercises to assess the magnitudes of the estimated effects and their policy implications. First, I calculate the persuasion rate, following standard methods from previous literature (DellaVigna and Gentzkow 2010, Enikolopov, Petrova and Zhuravskaya 2011). This measure allows me to compare the effectiveness of the policy intervention to previous estimates in the literature focusing on other forms of political persuasion, such as media campaigns or advertising. A persuasion rate quantifies the fraction of exposed individuals who were convinced to change their behavior as a result of the exposure. In my context, the "exposure" is receiving the Bonus, and the "behavior change" could be voting for the Democratic Party (PD) among those who otherwise would not have. By assumption, the voters who could have been swayed are either those who would have voted for other parties or who would have not voted at all. Following Enikolopov, Petrova and Zhuravskaya (2011), I denote the number of people who would have voted for the Democratic Party even absent the policy by y_0. Then, the exposure to the Bonus increases this nuber by $(1 - y_0) * e * f$, where f is the persuasion rate and e is exposure to the Bonus. The persuasion rate is then calculated as follows:

$$f = \frac{1}{1 - \nu_0 t_0}(t\frac{d\nu}{de} + \nu\frac{dt}{de}) = \frac{1}{1 - \nu_0 * t_0}(t_0 * \beta_{PD} + \nu_0 * \beta_{turnout}) \tag{4.1}$$

where ν reflects the vote share for the Democratic Party and t is turnout.

To compute ν_0 and t_0, I use the fitted values from equation (3.1), setting Bonus exposure to zero. This yields an average predicted PD vote share $\nu_0 = 0.32$ and a predicted turnout rate $t_0 = 0.51$. Using estimated coefficients from my main regressions, I set $\beta_{\text{PD}} = 0.20$ and $\beta_{\text{turnout}} = 0.57$. Plugging into the formula:

$$f = \frac{1}{1 - 0.51 \cdot 0.32} (0.32 \cdot 0.57 + 0.51 \cdot 0.20)$$
$$= \frac{1}{0.837} (0.1824 + 0.102)$$
$$= \frac{0.2844}{0.837} \approx 0.34.$$

This implies that roughly one in three persuadable recipients voted for the PD as a result of receiving the Bonus. This magnitude is substantially larger than the estimated persuasive impact of Fox News on Republican vote share (approximately 10%; DellaVigna and Kaplan 2007), but is comparable to the effects of populist radio on Roosevelt's vote share in the 1930s (Wang 2021), and is about half the size of the persuasion rate estimated by Caprettini, Casaburi and Venturini (2019) in response to land reform in post-war Italy.

Second, I calculate the average cost of a vote. Based on the estimated electoral effects, a 1 percentage point increase in the recipients' share brings about 2 percentage points increase in incumbent vote share. Given the average share of recipients of 20.15%, this amounts to 4 percentage points increase on average. In 2014, there were about 28 million voters, so this in total amounts to around 1.12 million additional votes. Given the total annual cost of the Bonus or roughly €640 (€80 for 8 months, until December 2014) for the first year, and the total number of beneficiaries of 9.7 million, the average short-run cost of a vote equals €5,540 ($6000). However, if one takes into account the persistence of the electoral effects, as well as the fact that the Bonus remained in place, the estimates change. By summing up the percentage point increase in Democratic Party vote share across the elections, one gets a cumulative effect of 0.47 percentage point increase for a 1 pp increase in Bonus recipients. Given the national average of 20.15% recipients' share, this converts to a cumulative effect of 9.6 percentage points. Given the average total number of voters between 2014 and 2018 of 30.3 million, this roughly amounts to 2,9 million votes gained. The cumulative total cost of the Bonus per capita, from 2014 to 2019, is around 5,400, and given the 10 million total beneficiaries, this amounts to an average long-run cost of a vote of about 19,000 €. This number is close in magnitude to existing estimates of vote costs for aggregate local spending in the

U.S[19], but is orders of magnitudes larger than estimates from the campaign spending literature (Bombardini and Trebbi 2011, Bekkouche, Cagé and Dewitte 2022).

4.4   Electoral Responses among Marginal Beneficiaries

Up to now, results display a significant positive impact of the 80 Euro Policy on the change in the Democratic Party vote share between 2013 and 2014. The identification of these effects relies on variation in the share of payroll relative to self-employed employees with earnings between 8,000 and 26,000 euro. Results could be biased in presence of unobserved time-varying factors that differentially affect municipalities with high vs. low share of eligible employees and are also correlated with voting behavior. Therefore, I exploit detailed data on the income distribution for the year 2014, to achieve a more local identification. The data are analogous to the ones used in the main analysis, giving information on the income distribution in the universe of Italian municipalities, based on individual tax records, but they are disaggregated in bins of 1,000 euro size. This allows me to focus the attention on the "marginal beneficiaries", namely those who are close to the eligibility thresholds. The thought experiment is as follows: I compare places that have the same share of payroll employees in a bandwidth around eligibility threshold, but different relative share above vs. below it, thus giving rise to quasi-exogenous differences in local treatment intensity. Figure A7 provides a visual example of the empirical strategy. I repeat the same experiment for both the lower and upper threhsold, keeping constant the size of the bandwidth around the threholds. The empirical specifications are the following:

$$\Delta Y_{m,2013,2014} = \beta_1 AroundThreshold_{m,2014} + \beta_2 RelAbove_{m,2014}$$
$$+ X'_m \zeta + \gamma_t + \delta_t + \epsilon_{mt} \tag{4.2}$$

$$\Delta Y_{m,2013,2014} = \beta_1 AroundThreshold_{m,2014} + \beta_2 RelBelow_{m,2014}$$
$$+ X'_m \zeta + \gamma_t + \delta_t + \epsilon_{mt} \tag{4.3}$$

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[19]For example, Healy and Malhotra 2009 found it costed exactly $27,000 to buy an additional vote through disaster relief, Levitt and Snyder 1997 estimates a cost of $14,000 per vote through pork-barrel federal dollars, while Slattery (2022) estimates a total cost per vote from subsidy giving to firms of about 7,000$.

If the Bonus is really responsible for producing electoral effects, then we would expect β_2 to be positive.

Appendix Table A5 presents the results, where I use three different bandwidths of, respectively, €2,000, 4,000 and 8,000 for both threhsolds. For example, for the lower threshold and the bandwidth of 4,000, *RelAbove* is the share of payroll employees earning between 8,000 and 10,000 among those earning between 6,000 and 10,000 euro. Similarly, for the upper threshold, *RelBelow* is the share of payroll employees earning between 24,000 and 26,000 among those earning between 24,000 and 28,000 euro . The key take-away is that, consistently across different bandwidths, the coefficients for the variables representing the *RelAbove* and *RelBelow*, corresponding to β_2, are positive and of broadly similar magnitude across specifications.[20]

As a further robustness check, I implement an instrumental variables (IV) strategy using local income distribution around the eligibility thresholds to isolate quasi-exogenous variation in treatment intensity. Specifically, in Panel A of Table A6 , I instrument the overall share of Bonus recipients with the share of payroll employees just above the lower eligibility threshold, while in Panel B I use the share just below the upper threshold as an instrument. As before, each column represents the results for a slightly larger bandwidth around the threshold. The intuition is that, conditional on income distribution and other covariates, variation in the relative density of taxpayers near the thresholds shifts local treatment intensity independently of unobserved political preferences or demand-side factors. Across both panels, the IV estimates of the effect of recipient share on Democratic Party vote share are positive and statistically significant, and comparable in magnitude to the baseline OLS estimates. This helps strengthen causal identification and further supports the interpretation that the electoral response is indeed driven by the Bonus policy itself, and not by other unobserved local shocks or endogenous sorting around eligibility.

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[20]While the coefficients near the upper threshold are somewhat smaller and not statistically significant, this attenuation is plausibly due to the design of the policy: the upper end of the eligibility range overlaps with the phase-out region of the transfer, where the marginal benefit of the Bonus diminishes.

4.5 Long-run Persistence

So far, I have documented a strong and significant short-run effect of the 80 Euro Bonus on the Democratic Party's electoral performance in 2014. But how persistent are these electoral rewards over time?

From a theoretical standpoint, the persistence of voting responses depends on how voters process past government actions. In models of rational retrospective voting, persistent rewards would be expected so long as the policy remains active and continues to shape voters' material conditions. However, several cognitive mechanisms—such as myopia, reference dependence, or the "peak-end rule"—could cause voters to heavily discount past benefits, especially if these are no longer salient at the time of the election.

Empirically, little is known about the long-run political returns to distributive policies. Most existing studies have focused on short-run effects, largely due to data limitations and identification challenges. A notable exception is Bechtel and Hainmueller (2011), who finds that around 25% of the electoral reward from flood-related disaster relief in Germany persists into the next election, but the effect vanishes thereafter.

To investigate the long-run effects of the Bonus, I estimate equation (3.2) on a series of subsequent elections held after the Bonus was introduced. Specifically, I focus on three key elections: (i) the 2016 constitutional referendum, in which then-Prime Minister Matteo Renzi proposed a sweeping set of institutional reforms; (ii) the 2018 national parliamentary elections, which marked a major political shift away from the Democratic Party and toward populist formations; and (iii) the 2019 European Parliament elections, held after the PD had entered the opposition. The corresponding coefficients for Bonus exposure in these three contests are plotted in the right panel of Figure 3. For the 2016 referendum, I use as outcome the municipality-level share of "Yes" votes, i.e., the share of voters supporting Renzi's proposed reforms. This choice reflects the fact that Renzi explicitly framed the referendum as a vote of confidence in his government and pledged to resign if it failed—which he did following its defeat. In this context, support for the "Yes" option can be interpreted as direct support for Renzi's leadership and platform.

The results indicate a notable degree of persistence. While the estimated effects are attenuated relative to 2014, they remain positive and statistically significant up to five years later. This is especially striking given the substantial political and economic changes that occurred in the intervening period, and particularly given the fact that

Matteo Renzi, the Prime minister of the Democratic Party who adopted the policy, resigned at the end of 2016 as Prime Minister after losing a constitutional referendum, and in the 2018 National elections two populist parties, the 5 Star Movement and the Northern League, won the elections and formed a coalition government. The persistence of the effect in 2019 is therefore even more notable, given that the Democratic Party is no longer in the governing majority, but has switched to the opposition since losing elections in 2018.

Two factors might help explain this pattern. First, the policy itself remained in place well beyond its original temporary nature, as it was made permanent with the budget law for 2015. Second, and more importantly, the policy continued to be strongly associated with Matteo Renzi. Even after stepping down as Prime Minister, Renzi maintained high visibility in the media and political discourse, repeatedly claiming credit for the Bonus. A simple Google search for "Bonus 80 Euro" confirms this: 8 of the top 10 results mention Renzi. Appendix Figure A8 further shows that while media coverage declined after 2014, mentions of the Bonus and Renzi remained stable, indicating a persistent narrative linking the policy to its political sponsor.

The 2016 referendum results are particularly informative in this regard. Since Renzi explicitly tied his political fate to the outcome of the vote, support for the referendum can be interpreted as a vote of confidence in his leadership. That Bonus exposure positively predicts support for his position in this context confirms that the policy had durable personal electoral returns.[21] The fact that similar patterns emerge in both the 2018 and 2019 elections—despite the PD's loss of power and the rise of populist alternatives—underscores the long-term impact of the Bonus on voting behavior.

Additional evidence of persistent political effects comes from the analysis of bonus restitutions. As described in Section 3.2, approximately 1.5 million recipients were later required to return the Bonus due to administrative misclassification, typically stemming from employers' incorrect assessments of workers' eligibility. Initially, these mistaken recipients appeared to reward the incumbent, likely due to the short-run surprise gain from receiving the unexpected transfer. However, once the repayment was triggered

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[21]Although Renzi ultimately lost the 2016 constitutional referendum and resigned, the "Yes" vote share was over 40%, matching the Democratic Party's vote share in the 2014 European elections—the highest ever obtained by the party. The referendum outcome should thus not be interpreted as a collapse in the Democratic Party support base, but rather as a byproduct of the institutional format of the vote: unlike regular proportional elections in which opposition parties run separately, the binary nature of the referendum allowed all opposition parties to coordinate against the Prime Minister.

during the 2015 tax filing season, this positive effect vanished and turned negative. In the longer run, as shown in Table 3, the coefficient on the share of recipients who had to return the Bonus becomes negative and statistically significant in both the 2016 referendum and 2018 elections. This suggests a clear pattern of retrospective punishment following the realization of a negative income shock. Notably, when the variable is rescaled by its standard deviation, the magnitude of the punishment effect is similar to the original pro-incumbent effect from correctly attributed Bonus receipt. These findings underscore the political risks of transfer repeal, even when the initial misclassification lies with employers rather than policymakers.

Importantly, the punishment effect fades over time: by 2019, the coefficient is halved in size and no longer statistically significant. Nonetheless, its presence in 2016 and 2018 confirms that transfer clawbacks can generate lasting political costs, reinforcing the idea that voters respond more strongly to realized losses than to foregone gains.

Together, these findings suggest that when a redistributive policy is not only sustained over time but also remains strongly associated with its initiator, it can generate significant and persistent electoral dividends. At the same time, they highlight the asymmetric political risks of policy withdrawal: voters are more likely to punish perceived losses than to reward unexpected gains. This dynamic may help explain why temporary transfers, once implemented, often become politically entrenched and difficult to reverse.

4.6 Heterogeneity in Electoral Rewards by Recipients' Income

Up to now, I focused on the average electoral response to the Bonus, exploiting variation in the overall share of beneficiaries. However, conditional on the overall share of recipients, municipalities vary in the distribution of recipients within the eligibility thresholds. Here, there are two potential forces that might play in opposite directions. On the one hand, the transfer represents a more sizeable income shock for relatively poorer voters. Thus, we might expect a stronger response in places with relatively poorer beneficiaries. On the other hand, these poorer voters may be less likely to turn out or, if they are already strong partisans (infra-marginal), less sensitive at the margin. If this is the case, then we should expect to observe stronger effects in places

with relatively richer beneficiaries, which are closer to the median income and thus more likely to be marginal voters. In Table 4, I therefore investigate whether the electoral response varies depending on the type of beneficiaries. I am going to compare municipalities that have the same share of overall beneficiaries, but that vary in the relative distribution of beneficiaries within the eligibility window. To do so, I construct two additional variables that reflect the share of recipients closer to the two eligibility thresholds $\%Recipients8-12k$ and $\%Recipients20-24k$.[22] Given that I am going to look at interaction effects among continuous variables and to improve comparability, all explanatory variables are standardized. For reference, in column (1), I reproduce the analysis of Table 1, but using the standardized share of bonus recipients, $\%Recipients(STD)$. In columns (2) and (3), I look at the differential effect of having one standard deviation higher relative share of, respectively, $Recipients8-12k$ and $Recipients20-24k$. Here, two opposite patterns emerge. On the one hand, conditional on the overall share of recipients, having a relatively higher share of $Recipients8-12k$ attenuates the electoral response by about 10%. On the other hand, having a relatively higher share of $Recipients20-24k$ amplifies the electoral response by about 27%. Finally, in column (4), I repeat the same exercise but include both variables at the same time, obtaining very similar results.

To explore this heterogeneity in finer detail, I then estimate separate regressions interacting the overall (standardized) share of recipients with the standardized share of recipients in each 2,000 euro income bin between 8000 and 26000, as per the following specification:

$$\Delta Y_{m,2014-2013} = \beta\,\text{Recipients}_m^{\text{std}}+\theta_b\left(\text{Recipients}_m^{\text{std}}\times\text{ShareBin}_{m,b}^{\text{std}}\right)+X_m'\zeta+\delta_t+\varepsilon_{mt}\,, \quad (4.4)$$

I display these interaction coefficients and their 95 percent confidence intervals in Figure 4. The interaction terms are small or negative in the poorest bins, indicating that a higher concentration of low-income beneficiaries slightly dampens the baseline electoral effect. The pattern of the coefficients then increases steadily, reaching its maximum in the €18,000–20,000 and €20,000–22,000 bins, right around the middle of the

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[22]For the upper threshold, I focus on the share of recipients between 20,000 and 24,000, as opposed to those between 22,000 and 26,000, so as to avoid the potentially confounding role played by the fact that recipients between 24,000 and 26,000 receive progressively decreasing amounts due to the transfer phase-out region.

income distribution. The interaction effect goes back to be indistinguishable from zero for the topmost bin, that corresponds to the phaseout region of the transfer. Notably, however, municipalities with relatively more recipients in this phase-out region—despite receiving lower amounts—still exhibit larger electoral rewards than those with a larger share of the poorest beneficiaries. This granular pattern confirms that, conditional on overall penetration, municipalities whose recipients cluster around the median income deliver the largest electoral returns whereas those with more lower-income individuals are less electorally responsive.

Several mechanisms could explain the higher electoral responsiveness of middle-income individuals:

1. *Political participation*: Middle-income voters typically have on average higher voting rates, and might be easier to mobilize, making each benefit receipt more likely to translate into actual votes.

2. *Distribution of Preferences*: If median income voters are more likely to be marginal voters for the Democratic Party, as the median voter hypothesis would predict, then they might be more likely to switch their votes in response to benefit receipt.

3. *Information and salience*: Higher-income recipients may be more politically informed, and thus more likely to both notice the existence of the transfer and attribute credit for it to the incumbent party.

4.7 Punishment among Excluded Individuals

While delivering substantial benefits to the incumbent among recipients, stimulus transfers might create bitterness among excluded individuals. Exclusion from a widely publicized benefit program may generate resentment, but the extent to which this translates into electoral punishment is theoretically ambiguous and empirically important. Understanding who punishes the government for being left out—and under what conditions—is central to assessing the political incentives that shape policy design. For instance, lower-income individuals may have stronger reactions to exclusion because the forgone benefit represents a larger share of their income. Yet they may also be less likely to express dissatisfaction through electoral channels due to lower turnout rates, weaker political engagement, or higher baseline support for the Democratic Party. In contrast, relatively higher-income voters—who are more electorally active and politi-

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cally responsive—may be more inclined to punish exclusion even when the monetary loss is relatively smaller. Ultimately, how different groups respond to exclusion is an empirical question—one that has received surprisingly little attention in the existing literature on the political effects of economic policies.

In what follows, I repeat the main analysis, considering both the positive impact on recipients and the potential negative impact on those excluded from the program. Results are presented in Table 5. Columns (1) and (2), respectively, introduce variables to account for groups marginally excluded from the bonus due to the income thresholds separately. *ExcludedBelow* represents the share of individuals in the voting eligible population that are excluded because their income was below the minimum threshold, while *ExcludedAbove* represents those marginally excluded because their income was above the maximum threshold.[23] In Column (3), I include both variables simultaneously. Interestingly, we observe asymmetric effects of exclusion. While both coefficients are negative, indicating electoral punishment among excluded individuals, only the effect of *Excluded Above* is consistently statistically significant and substantially larger—more than three times the magnitude of the effect for *Excluded Below*, , suggesting that the electoral penalty is concentrated among relatively higher-income non-recipients. To explore heterogeneity among the excluded in finer detail, I then estimate a series of regressions in which, for each €2,000 income bin $b \in \{0\text{–}2, 2\text{–}4, \ldots, 32\text{–}34\}$, I add the standardized share of eligible voters in bin b. Specifically, I estimate:

$$\Delta Y_{m,2014-2013} = \beta\, \text{Recipients}_m^{\text{std}} + \gamma_b\, \text{ExcludedBin}_{m,b}^{\text{std}} + X_m'\zeta + \delta_t + \varepsilon_{mt}, \qquad (4.5)$$

Consistent with the previous analysis, results in Figure 5 show that the poorest excluded bins (0–2 k and 2–4 k) have small, insignificant γ_b, whereas bins just above the upper threshold (26–28 k through 32–34 k) exhibit large, significant negative γ_b (around –0.5 STD units).[24] This asymmetry in the exclusion effects is particularly noteworthy. It suggests that higher-income voters who were excluded from the bonus program reacted more negatively in their voting behavior compared to lower-income excluded

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[23]For consistency, I adopt a symmetric window and consider the individuals earning between 26,000 and 34,000€as those excluded from above.

[24]To isolate the effect of exclusion from that of ex-post restitutions, all specifications in this section control for the share of recipients who had to return the Bonus due to misclassification. This ensures that the estimated asymmetry in electoral punishment is not conflated with the separate effects of benefit revocation.

voters. This finding aligns with theories of political behavior that posit greater political engagement and responsiveness among higher-income individuals (Solt 2008, Elsässer and Schäfer 2023). Finally, the analysis also highlights that, despite the negative effects from excluded groups, the overall impact of the bonus program on the Democratic Party's vote share remains positive and remarkably stable across specifications.

What explains the muted response among lower-income recipients and the absence of electoral punishment among lower-income excluded individuals? At first glance, this pattern seems counterintuitive: lower-income voters experience larger relative income gains from transfers and arguably have higher marginal utility of income. Two explanations may account for this finding.

First, lower-income individuals may be less likely to vote. A large literature documents a strong positive correlation between income and turnout (Solt 2008, Bonica et al. 2013, Leighley and Nagler 2013, Elsässer and Schäfer 2023). Alternatively, it may be that lower-income individuals—whether recipients or excluded—are more likely to be core supporters of the Democratic Party, and thus less responsive at the margin.

Several pieces of evidence suggest that the turnout explanation is more plausible. First, I find no indication that municipalities with a higher share of lower-income recipients or excluded individuals were more supportive of the Democratic Party before the policy was introduced. Figure A9 (top panel) presents binned scatterplots showing no systematic relationship between the share of i) lower-income recipients (those earning €8,000–12,000), and ii) lower-income excluded individuals (those earning below €8,000), and the Democratic Party's vote share in 2013. This casts doubt on the idea that these groups were stronghold voters and hence non-marginal.

By contrast, Table 2 and the bottom panel of Figure A9 show that municipalities with more lower-income recipients and excluded individuals also exhibited significantly lower turnout in 2013. These findings are consistent with the evidence from Schafer et al. (2022), who document substantial turnout inequality by income in Italy. In 2013, turnout was approximately 88% among voters in the top income quintile but dropped to 68% among those in the lowest quintile. Moreover, the decline in turnout from 2008 to 2013 was more than twice as large for poorer voters compared to wealthier ones, further widening the participation gap. They also show that income changes have larger effects on turnout among middle- and upper-income individuals: only individuals above the fourth income decile respond meaningfully to changes in income along the turnout

margin.[25] These patterns suggest that the lower responsiveness of poorer excluded voters may stem less from political alignment and more from disengagement from the electoral process.

On the other hand, in Table 2 I show that indeed turnout plays an important role in explaining the effects. Futhermore, in the bottom panel of Figure A9 I show that municipalities with a higher share of low-income recipients and lower-income non-recipients display lower turnout in 2013. This evidence is consistent with the results documented by Schafer et al. (2022), who provide evidence of substantial turnout inequality by income in Italy: while average turnout among voters in the top income quintile is around 88%, it drops monotonically along the income distribution, with turnout among the lowest income quintile around 68% in the 2013. They further document a drop in turnout from 2008 to 2013 that was more twice as large among the poorer voters than among the richest ones, futher exacerbating the income participation gap. Finally, Schafer et al. (2022) also show that changes in individual income have heterogeneous effects on turnout depending on baseline income: they are significantly larger among individuals with incomes from the fourth decile onwards, implying that middle-income individuals are significantly more responsive along the turnout margin to changes in income, relative to lower-income individuals.[26]

4.8 Why Voters Reward Politicians for Transfers?

4.8.1 Rewards for Personal vs. Aggregate Improvements in Economic Conditions

Existing research suggests two potential mechanisms that may explain the observed electoral results. First, beneficiaries might directly reward the incumbent government for the receipt of the Bonus and the improvement in their personal economic conditions, a phenomenon known as *pocketbook voting*. Indeed, previous papers have provided evidence on how improvements in personal economic conditions benefit incumbents (Manacorda, Miguel and Vigorito 2011, Ansolabehere, Meredith and Snowberg 2014, Bagues and Esteve-Volart 2016), and instead how experiences of economic hardship shifts individuals away from parties they deem responsible (Fetzer 2019, Margalit 2019,

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[25]See Figure 2 of Schafer et al. (2022).
[26]See Figure 2 of Schafer et al. (2022).

Choi et al. 2024, Arteaga and Barone 2025). Alternatively, the observed electoral gains might reflect improvements in local economic conditions, as the 80 Euro Bonus was designed as a consumption stimulus measure, a phenomenon commonly referred to as *economic voting* or *political multiplier effect*. Existing evidence on economic voting and the political multiplier effect is highly mixed. While many studies indeed find that voters reward incumbents for improvements in national economic conditions, even when these improvements are exogenous and unrelated to incumbents' direct actions (Wolfers 2007, de Benedictis-Kessner and Warshaw 2020, Graham et al. 2023). In contrast, other research fails to detect any significant role of changes in economic conditions in shaping electoral outcomes (Anderson 2007, Wright 2012, Hansford and Gomez 2015, Huet-Vaughn 2019). Moreover, interpretations of economic performance are further complicated by partisan biases, which can endogenously influence how voters perceive and attribute credit or blame for economic conditions (Rudolph 2003, Malhotra and Kuo 2008, Healy and Malhotra 2013).

In what follows, I discuss several reasons why my findings lend limited support to a political multiplier effect and instead align more closely with pocketbook voting.

First, the electoral effects appear immediately, coinciding closely with the initial distribution of Bonus payments. Given that broader economic stimulus impacts take time to materialize, the immediacy of the observed results is inconsistent with a stimulus-driven explanation.

Second, the political reward associated with improved economic conditions typically extends broadly to incumbents regardless of the level of government or party affiliation. However, my analysis consistently attributes electoral gains specifically to the Democratic Party, even after the center-left coalition was no longer the incumbent government. This partisan specificity undermines the hypothesis that electoral gains resulted solely from generalized improvements in economic conditions.

Third, if voters respond to improvements in national economic conditions, then we would expect to observe similar voting responses for both recipients and non-recipients, as they both should be happy about the bonus stimulus effect of the economy, independently on bonus receipt. Put it differently, we shouldn't expect to observe a correlation between local treatment intensity and voting behavior. An alternative reason could be that voters reward the incumbent for improvements in local, as opposed to national, economic conditions, a theory that has received relatively limited support up to

now, with the exception of Brunner, Ross and Washington (2011) and Healy and Lenz (2017). If that's the case, then the elasticity of voting to local recipients' share could reflect the fact that places with a larger share of beneficiaries experienced stronger economic stimulus.

To explicitly test for the role of improvements in local economic conditions in explaining the effects, I include time-varying controls capturing changes in local economic conditions. Specifically, I include the full set of variables capturing the distribution of individuals across the income and occupation distribution but allow them to vary over time. I also include a control for the overall share of taxpayers and local average taxable income. Alltogether, changes in these variables should capture changes in local economic conditions and thus they would account directly for economic dynamics that might be induced by the bonus. Results from this analysis, detailed in Appendix Table A8 , demonstrate that the inclusion of these economic controls does not diminish—and, in fact, often slightly increases—the magnitude and significance of the estimated electoral effects. This empirical robustness strongly argues against the hypothesis that electoral outcomes primarily reflect local economic fluctuations stemming from stimulus-induced improvements.

Overall, these findings suggest electoral rewards from the Bonus policy reflect direct beneficiary responses rather than broader improvements in local economic conditions resulting from the stimulus.

4.8.2 Motivations Behind Pocketbook Voting: Retrospective Voting, Prospective Voting, and Reciprocity

Pocketbook voting, wherein voters reward incumbents based on personal economic benefits received, can be driven by several distinct theoretical mechanisms. These motivations broadly include retrospective voting, prospective voting, and reciprocity.

Retrospective voting refers to voters evaluating incumbents based on past economic performance. According to this theory, voters assess government effectiveness by looking backward and determining their vote based on incumbents' prior actions and the concrete benefits previously realized. Retrospective voting requires voters only to reflect on outcomes already achieved, thereby demanding relatively modest levels of voter sophistication and information processing. Empirical literature strongly supports retrospective voting, demonstrating that voters frequently evaluate politicians based on

tangible personal economic gains achieved during the incumbents' term.

In contrast, *prospective voting* involves voters assessing candidates based on expected future policies and anticipated continued benefits. This forward-looking approach assumes that voters carefully evaluate policy proposals and campaign promises, rewarding incumbents or challengers who present credible plans likely to enhance voters' future economic welfare. Thus, prospective voting entails higher cognitive demands, requiring voters to forecast and compare complex future policy scenarios.

Finally, *reciprocity* emphasizes voters' psychological or emotional inclination to reward incumbents out of gratitude or perceived obligation following the receipt of economic transfers. Reciprocity differs subtly but importantly from retrospective voting, as it explicitly highlights emotional and normative dimensions rather than purely rational economic evaluations. Voters driven by reciprocity motives feel socially or morally compelled to reward incumbents, reflecting behavioral economics theories on reciprocal altruism and social preferences (Finan and Schechter 2012).

My empirical findings align more closely with retrospective voting and reciprocity rather than prospective voting, supported by several critical observations. Firstly, the electoral gains associated with the 80 Euro Bonus manifest immediately upon payment distribution, indicating voters responded directly to tangible benefits rather than anticipated future policies or ongoing economic performance. Secondly, the context of the elections studied reinforces the retrospective and reciprocity explanations. Specifically, the observed electoral reactions occur during European parliamentary elections, widely recognized as inconsequential for determining national policy outcomes. Thus, voters have minimal incentives to strategically retain incumbents based on prospective expectations. Instead, the observed voting behavior likely stems from retrospective evaluations or emotional gratitude triggered by immediate economic benefits.

Furthermore, my results regarding the punishment of incumbents following bonus restitution episodes provide additional support for retrospective voting theory. Specifically, electoral penalties for the incumbent party emerged only after beneficiaries experienced the negative economic shock of having to return Bonus payments, demonstrating that voters retrospectively punished incumbents based on personal economic outcomes already experienced rather than anticipated future conditions or abstract evaluations.

Collectively, the timing, context, and explicit evidence of voter punishment following realized negative outcomes in my study provide robust support for retrospective and

reciprocity-based motivations underlying observed pocketbook voting behavior.

4.9 Survey Evidence

To corroborate the strength of my aggregate findings and better understand the mechanisms driving the electoral impact of the 80 Euro Bonus, I complement the municipality-level analysis with individual-level survey data. Specifically, I draw on the Itanes-University of Milan electoral cycle online panel, which offers a unique four-wave panel around the 2013 General Election and the 2014 European Election. This dataset includes two pre-election and two post-election waves, allowing me to observe the same individuals both before and after each electoral cycle. Crucially, the 2014 pre-electoral survey was conducted between May 5th and May 19th, after the Bonus was publicly announced but before payments were distributed. The post-election survey was administered between June 10th and June 18th, when the Bonus had been received by all eligible recipients.

This survey setup enables me to isolate the effect of Bonus receipt from other confounding factors—such as the general popularity of the policy or the appeal of the newly appointed Prime Minister Matteo Renzi—by comparing pre- and post-election attitudes among otherwise similar individuals. It thus provides a valuable test of the Bonus's direct political effects, helping to rule out the possibility that observed support was driven simply by the announcement of the policy or ideological affinity with Renzi.

Before turning to causal estimates, I first document descriptive differences in political preferences and turnout behavior across income and employment categories. These patterns help illustrate which groups were most politically engaged and electorally responsive before the Bonus rollout, providing further insight into the strategic logic of the policy's design and targeting.

To that end, I classify individuals along two dimensions: *income class* and *employment status*. Because the survey does not collect income data directly, I construct a proxy using a combination of self-reported social class and occupational status. I define three groups: Below (low-income), Eligible (middle-income, likely within the Bonus threshold), and Above (high-income). Employment status is divided into six mutually exclusive categories: Payroll employees, Self-employed, Unemployed, Housewives, Students, and Pensioners.

Table A9   presents average political outcomes across these categories. Middle-

income individuals and payroll employees—those most likely to receive the Bonus—are systematically more supportive of the Democratic Party (PD) and of Matteo Renzi prior to the 2014 elections, and also report higher past turnout and lower intended abstention. These groups thus represent a politically salient and electorally responsive constituency. Interestingly, many payroll-employed and low-to-middle-income individuals also supported the 5 Star Movement (M5S) in 2013, consistent with the idea that Renzi may have been able to win back previously disaffected protest voters.

I then exploit the panel structure of the survey to estimate the causal effect of Bonus receipt on changes in voting and attitudes. I define likely Bonus recipients as those who are payroll employees in 2013 and fall within eligible occupational income brackets.[27] For each individual, I compare changes in key outcomes between the pre- and post-electoral surveys in 2014:

$$\Delta Y_{Pre-Post,2014} = \beta_1 Payroll_{2013} + \beta_2 EligibleIncome_{2013} + \beta_3 80EuroRecipient_{2013} + X_i'\zeta + \delta_p + \epsilon_i$$
(4.6)

where $\Delta Y_{Pre-Post,2014}$ denotes the change in an outcome for individual i between the two waves. All regressions include demographic controls and province fixed effects.

Column (1) of Table 6 considers a composite measure of partisan switch: it equals 1 for individuals who began supporting the PD after the Bonus, -1 for those who stopped supporting it, and 0 otherwise. Bonus recipients are 11 percentage points more likely to switch to support the PD—an effect similar in magnitude to the one estimated by Manacorda, Miguel and Vigorito (2011) for Uruguay's CCT program and closely aligned with the average effects found in aggregate data.[28] As a placebo test, Column (2) replicates the analysis for the 2013 election cycle and finds no comparable effect, further supporting the causal interpretation.

To explore mechanisms, I then turn to respondents' economic perceptions. Specifically, I examine whether the Bonus influenced individuals' evaluations of the national and personal economic situation, both retrospectively and prospectively. Columns (3)–(6) report results from regressions where the dependent variable equals 1 if economic as-

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[27]These include artisans, shopkeepers, sales representatives, teachers, blue-collar and white-collar employees, and agricultural workers.

[28]Manacorda, Miguel and Vigorito (2011) evaluate the impact of conditional cash transfers on support for the incumbent in Uruguay. They use subjective assessments of government performance rather than vote choice.

sessment improved, -1 if worsened, and 0 otherwise.

The results point to a clear pattern: Bonus recipients significantly revise retrospective assessments of the national economy in a more favorable direction, but show no change in expectations about the future, nor in the evaluation of their personal financial situation. This asymmetry is hard to reconcile with a purely rational, forward-looking interpretation and is instead consistent with endogenous belief formation driven by partisanship. That is, receiving the Bonus may have strengthened identification with the incumbent party, leading individuals to rationalize the recent economic past in a way that aligns with their new political orientation—even if their material circumstances remain unchanged.

To have a sense of how much of the shift in voting is due to the shift in views of the national economy, in column (7), I repeat the same specification of column (1), but now including also two additional dummies, $EconomicAssessmentWorsened$ and $EconomicAssessmentImproved$, capturing the change in views on the national economy before and after the elections in 2013. The coefficient on β_3 remains very similar in magnitude, but slightly loses significance, while the direct effect of the improved economic assessment is about half the size of the effect of the Bonus.

Overall, these survey findings closely mirror the results obtained from the aggregate data: individuals most likely to receive the Bonus are also the most likely to shift support toward the incumbent. By leveraging the panel structure and rich individual heterogeneity in the survey, this analysis sharpens identification and provides further evidence that the Bonus had a direct impact in changing individuals' voting behavior.

5 Discussion and Conclusion: Electoral Returns, Economic Efficiency, and the Tradeoff in Stimulus Design

This paper provides the first causal evidence on the electoral impact of automatic, broad-based stimulus tax transfers. Leveraging detailed administrative and electoral data from the implementation of the 2014 "80 Euro Bonus" in Italy, I show that municipalities with a higher share of Bonus recipients experienced a sizable and persistent increase in support for the incumbent Democratic Party. These effects are robust across

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electoral cycles and persist beyond the immediate post-transfer period, suggesting that the political rewards from this kind of stimulus are not merely short-lived or transitory.

Crucially, the evidence points to a direct political channel: the electoral effect is not mediated through improvements in local economic conditions or employment growth, but rather stems from the individual experience of receiving a visible, automatic transfer. The design of the 80 Euro Bonus—delivered as a monthly wage subsidy to middle-income payroll employees—made the transfer both salient and easy to attribute to the incumbent, likely enhancing its effectiveness as a political tool.

Yet, this success in generating political support raises important questions about the economic rationale behind the policy design. From the standpoint of economic theory, stimulus payments should be targeted toward low-income, liquidity-constrained households, who are more likely to spend additional income, thereby amplifying the consumption stimulus and boosting aggregate demand. Indeed, prior empirical studies—such as Neri, Rondinelli and Scoccianti (2017) and Andini et al. (2018)—have shown that in the Italian context, lower-income households have significantly higher marginal propensities to consume.

To explore the tension between economic and political returns, I construct a simple counterfactual that: (a) preserves the total short-run cost of the policy by keeping the total number of recipients fixed, (b) maintains policy feasibility by retaining the same transfer structure, but (c) shifts benefits toward the poorest potential recipients.

Specifically, I analyze the effects of a left-ward shift in the eligibility window to include individuals with annual incomes between €0 and €20,000. This alternative design maintains the same total number of beneficiaries but redirects benefits toward those with potentially higher marginal propensities to consume. By comparing the electoral consequences of this alternative policy design to the actual implementation, I can quantify the political costs of economically efficient targeting and estimate the implicit marginal rate of substitution between economic and political returns from the politician's perspective.

To estimate the counterfactual electoral effect, I leverage the coefficients from my analysis in Table 4 that captures heterogeneous impacts of cash transfers across income brackets. Specifically, the interaction terms indicate that, conditional on the same total share of recipients, places with a higher concentration of €8,000-12,000 recipients experience a dampened electoral effect (negative interaction), while areas with a higher

concentration of €20,000-24,000 recipients experience an amplified effect (positive interaction).

The proposed counterfactual policy would shift approximately 30% of total recipients from the €20,000-26,000 bracket to the €0-8,000 bracket. To calculate the net electoral impact of this redistribution:

$$\text{Lost Effect}_{20-24k} = 0.30 \times 0.224 = 0.067 \tag{5.1}$$

$$\text{Gained Effect}_{0-8k} = 0.30 \times 0.138 = 0.041 \tag{5.2}$$

Yielding a net change in electoral effect:

$$\Delta\text{Effect} = 0.041 - 0.067 = -0.026 \tag{5.3}$$

This result indicates that redirecting benefits from middle-income recipients to the poorest potential recipients would reduce the electoral return by approximately 0.026 percentage points in vote share. Relative to the original policy's effect of approximately 0.18 percentage points per percentage point of recipients, this represents a reduction of about 15% in electoral effectiveness.

Importantly, research from the Bank of Italy (Andini et al. 2018) indicates that consumption responses would have been approximately 30% higher had the policy targeted the lowest-income workers. This aligns with broader findings on stimulus payments that consistently show higher marginal propensities to consume among lower-income households (Nygaard, Sorensen and Wang 2020, Kaplan and Violante 2022).

This implies an implicit marginal rate of substitution (MRS) of 2:1 between economic and political returns from the perspective of the policymaker: for every one percentage point decrease in electoral support, a policymaker could have gained two percentage points in economic efficiency. In other words, the observed policy design appears to reflect a political calculus that prioritized electoral gains over maximizing the consumption stimulus—a tradeoff that may be central to understanding how governments respond to economic downturns in democratic settings.

These findings contribute to a growing literature on the political economy of redistribution and fiscal policy. While previous research has emphasized the role of distributive politics and the importance of targeting pivotal voters, this paper provides a precise,

quantitative assessment of how political considerations can distort the targeting of economically optimal policies. It helps explain why, in practice, fiscal stimulus programs often prioritize the "broad middle" of the income distribution rather than focusing narrowly on the poor—even when doing so would deliver larger aggregate economic benefits.

The analysis also highlights broader implications for policy design. If policymakers wish to maximize the effectiveness of stimulus transfers while maintaining political feasibility, they may need to consider hybrid strategies that combine broad eligibility criteria with progressive benefit structures. For example, programs could be framed as universal or nearly universal but deliver larger amounts to those most in need. Alternatively, in contexts where poor households are harder to reach or mobilize electorally, complementary interventions—such as voter registration drives, informational campaigns, or improved payment mechanisms—may help amplify both the economic and political returns of more targeted policies.

Moreover, these findings underscore the importance of policy visibility and attribution. The 80 Euro Bonus was delivered automatically through payroll systems and clearly associated with the incumbent government, enhancing its political salience. In contrast, less visible or more complex transfer mechanisms—such as tax refunds or in-kind benefits—may generate weaker political feedback, even if they are more progressive in intent. This suggests that the form of delivery can be as politically consequential as the level or targeting of benefits.

Looking ahead, this study opens several avenues for further research. First, while the paper provides a point estimate of the electoral cost of targeting efficiency, more work is needed to understand how voters perceive the fairness, salience, and intent of different policy designs. Experimental or survey-based studies could help unpack whether voters respond differently to universal versus targeted benefits, and whether they reward policies that are framed as promoting equity versus those that appear self-serving. Second, future work could explore how the trade-off between economic and political returns varies across institutional settings. In systems with stronger party discipline, proportional representation, or automatic stabilizers, the political incentives to distort policy targeting may differ substantially from the Italian case. Similarly, policies implemented at different levels of government—municipal, regional, or national—may face different visibility and accountability dynamics. Third, integrating these findings

into formal models of policymaking under electoral constraints could help generate testable predictions about the conditions under which governments prioritize political versus economic objectives. Such models could also explore how voter behavior endogenously shapes policy design over time, especially when voters are forward-looking or exhibit partisan biases in their evaluation of economic outcomes.

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Figure 1

Taxable Income Distribution 2013 (Official Tax Records)
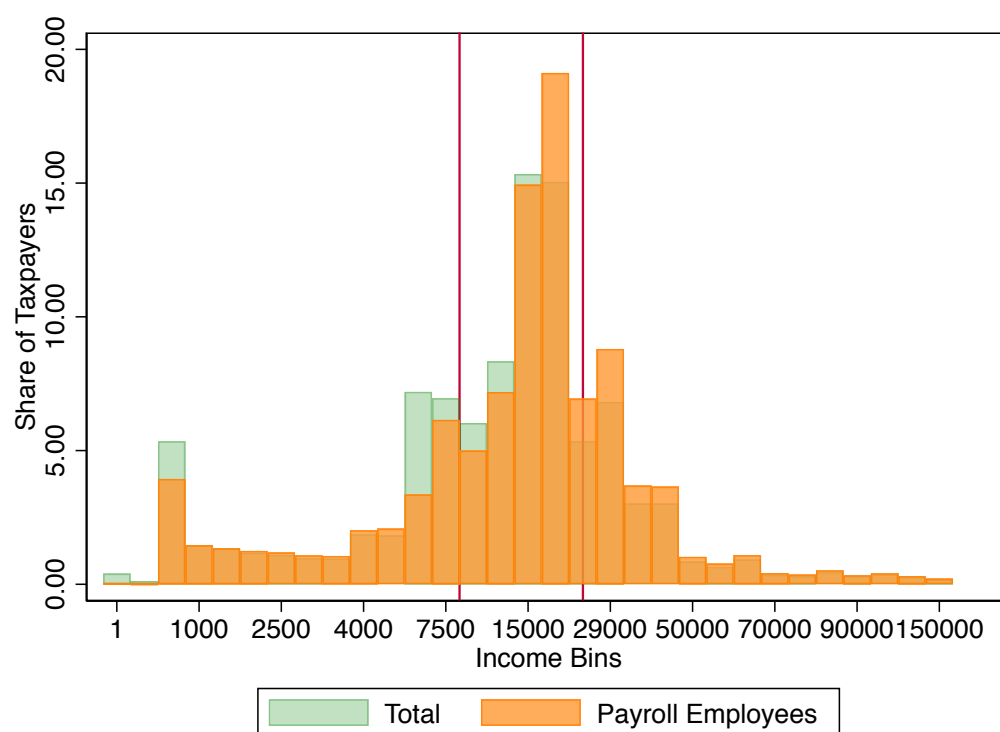
Notes: The figure plots the distribution of taxable income in 2013 of the universe of taxpayers (green) and of payroll employees only (orange). The red vertical lines indicate, respectively, the lower and upper threshold of eligibility for the €80 Bonus.

Figure 2

The Short-Run and Long-Run Effects of the 80 Euro Bonus on Democratic Party Electoral Performance
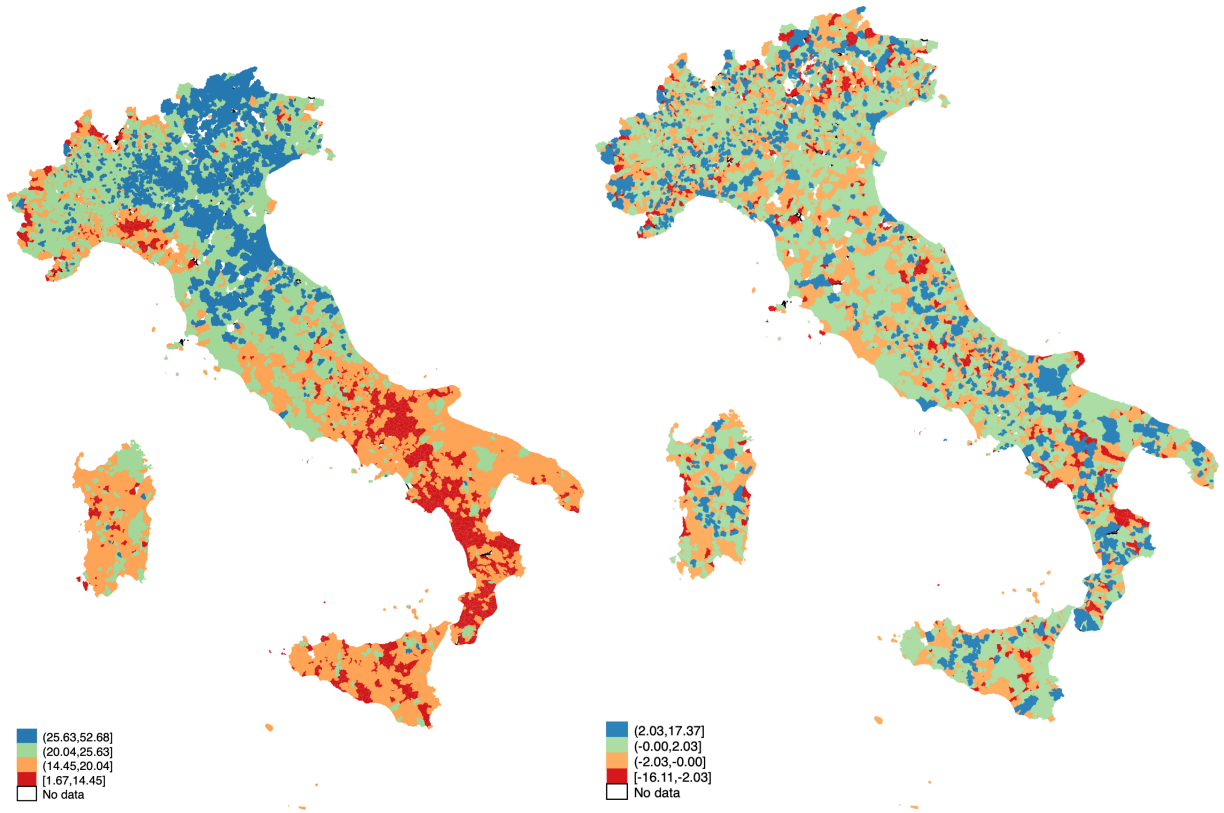
Notes: The figure shows two maps of the share of recipients across Italian municipalities, providing intuitions for the variation used in the identification. In particular, the left map shows the map of the actual share of recipients, while the right map shows the residuals from the following regression: $Recipients_{m,2014} = \%Payroll_{m,2013} + \%8 - 26k_{m,2013} + \delta_p + \epsilon_m$ where $\%Payroll_{m,2013}$ is the share of payroll employees in the municipality in 2014, $\%8 - 26k_{m,2013}$ is the share of individuals declaring income between 8 and 26,000 euro and δ_p are 110 province fixed-effects.

Figure 3
Incumbent's Electoral Performance and Policy Intensity
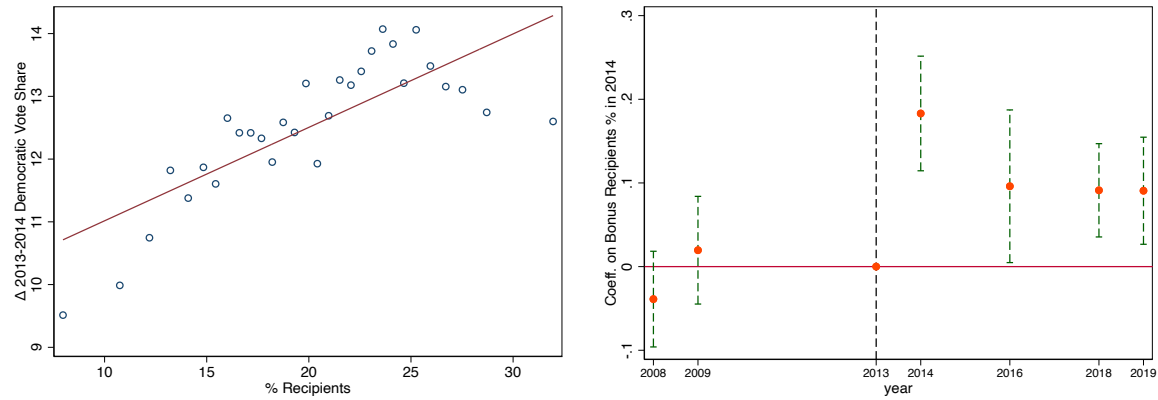
Notes: The left panel shows the unconditional binscatter displaying the relationship between the change in the vote share of the Democratic party between 2013 and 2014 (Y axis) and the share of Bonus recipients (X axis). From the full sample of 7712 municipalities, 20 equal-sized bins are created, and the points represent the X and Y averages within each bin, while the line represents the slope of the OLS bivariate regression. The right panel plots the coefficients of the regression as highlighted in equation 3.2.

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Figure 4
Heterogeneity by Income in Electoral Rewards
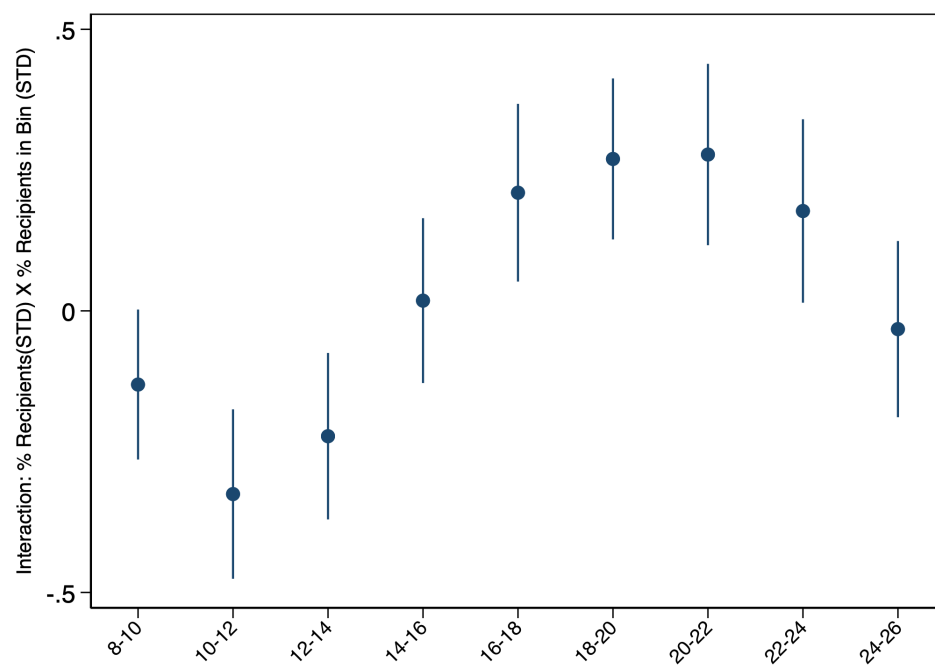
Notes: The figure plots the coefficients with 95 percent confidence intervals of separate regressions as highlighted in equation 4.4.

Figure 5
Heterogeneity by Income in Electoral Punishment from Excluded Individuals
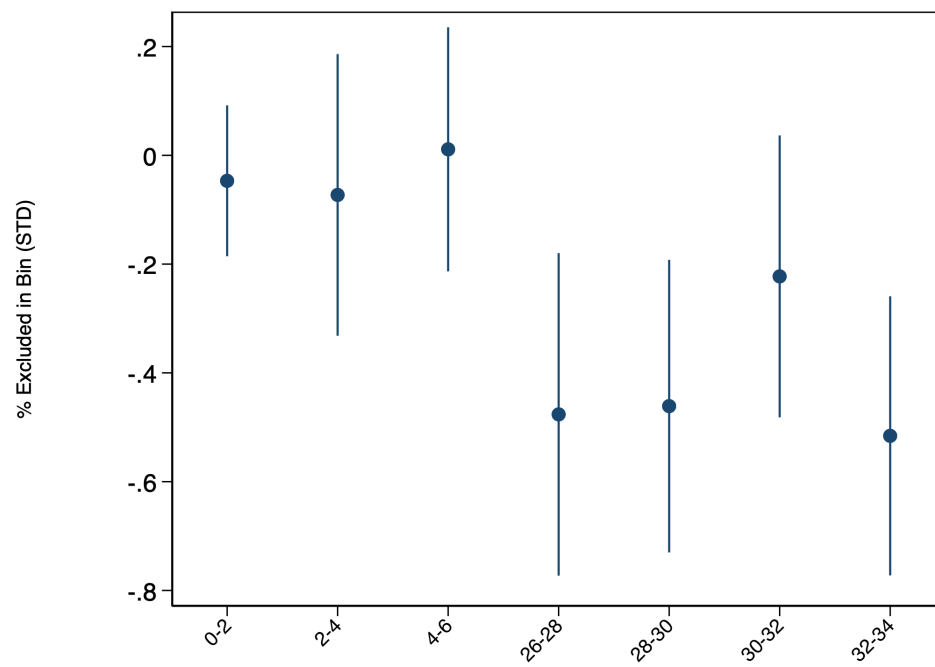
Notes: The figure plots the coefficients with 95 percent confidence intervals of separate regressions as highlighted in equation 4.5.

Table 1

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance

	$\Delta 2013 - 2014$ Share PD						$\Delta 2009 - 2014$ Share PD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
% Recipients	0.149***	0.178***	0.178***	0.178***	0.280***	0.0821**	0.138***
	[0.0139]	[0.0353]	[0.0594]	[0.0635]	[0.0532]	[0.0363]	[0.0407]
Pre-T Mean	22.90	22.90	22.90	22.90	22.90	22.90	21.40
Observations	7712	7712	7712	7712	7712	7712	7697
R2	0.814	0.822	0.822	0.0617	0.922	0.839	0.822
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Prov. S.E.	No	No	Yes	No	No	No	No
Spatial HAC S.E.	No	No	No	Yes	No	No	No
Weighted	No	No	No	No	Yes	No	No
RegXYear FE	No	No	No	No	No	Yes	No

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$. The dependent variable in Columns (1)-(4) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, while in column (5) it is the change in the vote share of the Democratic party between the 2009 and the 2014 elections, and in column(6) is the change in total votes for the Democratic party between 2013 and 2014. The 2013 election was a national parliamentary election, while 2009 and 2014 elections were European parliamentary elections. %*Recipients* is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. $NRecipients$ is the absolute number of Bonus recipients. All specifications include the following controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high-school, % with college degree, % of foreign-born, % of women, and the local unemployment rate, all measured at baseline.

Table 2

The Effect of the 80 Euro Bonus on Turnout and Votes to Other Parties

	$\Delta 2013 - 2014$ Share 5Stars	$\Delta 2013 - 2014$ Share Right	$\Delta 2013 - 2014$ Share Extreme Right	$\Delta 2013 - 2014$ Turnout	$\Delta 2013 - 2014$ Share PD (VEP)
	(1)	(2)	(3)	(4)	(5)
% Recipients	0.000854	-0.0176	-0.0350*	0.570***	0.356***
	[0.0247]	[0.0321]	[0.0185]	[0.0789]	[0.0282]
Pre-T Mean	22.68	21.79	5.777	74.92	17.17
R2	0.601	0.528	0.425	0.603	0.609

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. The dependent variable in columns (1) is the change in the vote share of the 5 Stars Movement between the 2013 and 2014 elections, in column (2) is the change in the vote share for the Center-Right coalition led by the party of Berlusconi, Forza Italia, between the 2013 and 2014 elections; in column (3) it is the change in the vote share for the extreme right parties, includi Giorgia Meloni's "Brothers of Italy" and Matteo Salvini's Northern League, between the 2013 and the 2014 elections, in column (4) it is the change in turnout between the 2013 and the 2014 elections and in column (5) it is the change in the number of votes to the Democratic Party divided by the voting eliible population between the 2013 and the 2014 elections. *%Recipients* is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. All specifications include the following controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high-school, % with college degree, % of foreign-born, % of women, and the local unemployment rate, all measured at baseline.

Table 3

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Punishment from Transfer Repeals

	$\Delta 2013-2014$ Share PD	$\Delta 2013-2016$ Share PD	$\Delta 2013-2018$ Share PD	$\Delta 2013-2019$ Share PD
	(1)	(2)	(3)	(4)
Bonus Recipients (Share)	0.161***	0.140***	0.0720**	0.0639*
	[0.0370]	[0.0497]	[0.0304]	[0.0330]
% Recipients, Give Back	0.0267	-0.0430*	-0.0461***	-0.0229
	[0.0199]	[0.0259]	[0.0161]	[0.0177]
Dep. Var. Mean	22.90	22.90	22.90	22.90
% Restitions Mean	15.03	15.03	15.03	15.04
Observations	162.4	162.4	162.4	162.4
R2	7061	7058	7060	6997
r2	0.0801	0.348	0.121	0.114

Notes: Standard errors clustered at the municipality level in brackets. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. The dependent variable in columns (1) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, in column (2) is the change between the vote share fo rthe Democratic Party in 2013 and the share of Yes votes in the 2016 Referendum, in columns (3) it is the change in the vote share of the Democratic party between the 2013 and the 2018 elections, and in columns (4) it is the change in the vote share of the Democratic party between the 2013 and the 2019 elections. The 2013 and 2018 elections were national parliamentary elections, while 2014 and 2019 elections were European parliamentary elections. $\%Recipients$ is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. *% Recipients, Give Back* is the share of Bonus recipients that had to return the Bonus in 2015 because they turned out to be ineligible. All columns include the following controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high-school, % with college degree, % of foreign-born, % of women, and the local unemployment rate, all measured at baseline, as well as the average Bonus amount per recipient and the average amount that individuals had to give back.

Table 4

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Heterogeneity by Recipients' Income

	$\Delta 2013 - 2014$ Share PD			
	(1)	(2)	(3)	(4)
% Recipients (std)	0.984***	1.041***	1.138***	1.130***
	[0.198]	[0.198]	[0.201]	[0.200]
% Recipients Below 12k (std)		0.0589		0.135
		[0.106]		[0.115]
% Recipients (std) × % Recipients Below 12k (std)		-0.212***		-0.116
		[0.0747]		[0.0858]
% Recipients 20-24k (std)			0.127	0.162
			[0.122]	[0.133]
% Recipients (std) × % Recipients 20-24k (std)			0.279***	0.219**
			[0.0816]	[0.0952]
Dep. Var. Mean	12.53	12.53	12.53	12.53
SD Bonus	5.590	5.590	5.590	5.590
SD Recipients 8-12k	4.215	4.215	4.215	4.215
Recipients 20-24k	7.138	7.138	7.138	7.138
N	7684	7684	7684	7684
r2	0.0607	0.0628	0.0629	0.0638

Notes: Standard errors clustered at the municipality level in brackets. The dependent variable is the change in the vote share of the Democratic party between the 2013 and 2014 elections. *%Recipients* is the number of Bonus recipients in the municipality in 2014 over the voting-eligible population, standardized. *Recipients 20-24k* is the share of recipients earning between 8,000 and 12,000 euros, standardized. *Recipients 20-24k* is the share of recipients earning between 20,000 and 24,000 euros, standardized. All columns include the following controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high school, % with college degrees, % of foreign-born, % of women, and the local unemployment rate, all measured at baseline. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

Table 5
The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Punishment Among Excluded Individuals

	$\Delta 2013 - 2014$ Share PD		
	(1)	(2)	(3)
% Recipients (STD)	1.093***	1.001***	1.047***
	[0.202]	[0.202]	[0.203]
Excluded Below (STD)	-0.168		-0.287**
	[0.113]		[0.118]
Excluded Above (STD)		-0.788***	-0.904***
		[0.187]	[0.192]
Dep. Var Mean	12.51	12.51	12.51
SD Bonus	5.59	5.59	5.59
SD Excl.Below	2.96		2.96
SD Excl.Above		2.03	2.03
Observations	7697	7697	7697
R2	0.06	0.06	0.06

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. The dependent variable is the change in the vote share of the Democratic party between the 2013 and 2014 elections. %*Recipients* is the number of Bonus recipients in the municipality in 2014 over the voting-eligible population, standardized. *Excluded Below* is the share of individuals ineligible because they earn less than 8,000 euros, standardized. *Excluded Above* is the share of individuals ineligible because they earn more than 26,000 euros, standardized. All columns include the following controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high school, % with college degrees, % of foreign-born, % of women, and the local unemployment rate, all measured at baseline, as well as the share of Bonus recipients that had to return the Bonus in 2015.

Table 6

The Effect of the 80 Euro Bonus on Democratic Electoral Performance, Survey Evidence

	Switch Support to PD		Improved Assessment				Switch Support to PD
	(1) 2014	(2) 2013	(3) Past Econ.	(4) Own Past Econ.	(5) Future Econ.	(6) Own Future Econ.	(7) 2014
Payroll	-0.0815 [0.0500]	-0.00165 [0.0527]	-0.221*** [0.0784]	-0.0433 [0.0740]	-0.0385 [0.0718]	0.0226 [0.0698]	-0.0733 [0.0498]
Eligible Income	-0.0491 [0.0312]	0.0441 [0.0424]	-0.0915 [0.0682]	-0.00966 [0.0565]	0.0194 [0.0592]	0.0347 [0.0487]	-0.0440 [0.0311]
80Euro Recipient	0.110** [0.0548]	-0.0337 [0.0571]	0.166** [0.0828]	-0.0270 [0.0756]	0.0545 [0.0768]	-0.0952 [0.0719]	0.101* [0.0542]
Econ. Assessment Worsened							0.0321 [0.0278]
Econ. Assessment Improved							0.0569** [0.0263]
Observations	3026	3026	3026	3026	3026	3026	3026
R-sq	0.0756	0.0830	0.0774	0.0711	0.0711	0.0695	0.0784

Notes: Robust standard errors in parenthesis. $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. Data are individual-level observations from the Itanes-University of Milan electoral cycle online panel survey. The dataset includes four waves: 2 pre-electoral surveys and 2 post-electoral surveys, both for the 2013 and the 2014 elections. All dependent variables denote the variation in the respondent view between the Pre-electoral and the Post-electoral survey. In particular, the dependent variable is equal to 1 (-1) if the evaluation increased (decreased), and equal to 0 if the evaluation stayed the same or the information is missing. All regressions includes individual-level demographic controls (gender, age, marital status), as well as province fixed effects. Columns (1) and (2) inspect changes in support for the Democratic Party, respectively for the pre-post 2014 and 2013 elections. Columns (3) and (5) look at the assessment of the national economic situation, both in the past and in the future year, while columns (4) and (6) look at individuals' assessment of their personal economic situation, both in the past and in the future respectively. Finally, column (7), repeats the same specification of column (1), but including also two dummies, *EconomicAssessmentWorsened* and *EconomicAssessmentImproved*, capturing the change in respondents' retrospective assessment on the past national economic conditions.

Appendix A: Additional Tables and Figures

Figure A1
The introduction of the 80 Euro Bonus: Example of a Paycheck

PAGA BASE		CONTINGENZA		SCATTI D'ANZIANITA	AD PERSONAM		PREMIO PRODUZIONE	EDR-VARIE 1		VARIE 2	VARIE 3
											TOTALE
											1.500,00
CODICE			DESCRIZIONE			QUANTITA'		VALORE BASE		COMPETENZE	TRATTENUTE
300		Retribuzione mensile								1.500,00	
819		Rata acconto add. comunale									24.81
826		Rata addiz. comunale									6,14
828		Rata addiz. Regionale									3,66
900		Bonus D.L. n. 66/2014								80,00	
IMP PREVIDENZIALE		CTR PREVIDENZ		IMP IVS AGGIUNT	CTR AGG IVS		IMP ALTRI	CTR ALTRI			TOT CTR PREVID
1.500,00		142,35									142,35
IMP PREVINDAI		CTR PREVINDAI						CTR FASI			TOT CONTRIBUTI
											142,35
IMP FISCALE		IRPEF LORDA		DETRAZ. LAVORO	DETRAZ. FAMIL.		IRPEF NETTA			RESTITUZIONE IRPEF	IRPEF NETTA
M	1.357,65	316,57		121,37	227,56		0				0
A											IRPEF TASS SEP
GG	DETRAZ. LAV.	ULTER. DETRAZ.		DETRAZ. CONIUGE	DET. FIGLI	DET.FIGLI < 3 AA	DET. ALTRI FAM.			TOTALE COMPETENZE	TOTALE TRATTENUTE
M 31	121,37			57,50	170,06						
A										1.580,00	176,96
IMP TFR 1.1.2001		IMP ALTRE		IMPOSTA 1.1.2001	IMP TFR 31.12.2000		IMPOSTA 31.12.2000	%		ARROT	NETTO A PAGARE
	HH/GG	IMPONIBILE		ACCANT	CTR 0,5%	TFR AI FONDI P.	ACCANT NETTO		IMPONIBILE INAIL		1.403,04
TFR											
ABI CAB											
NOTE						FIRMA			PERIODO PAGA		MAGGIO 2014

Notes: This figure shows an example of the paycheck of an Italian payroll employee eligible to receive the Bonus. I highlight in yellow the relevant line for the 80 euro bonus amount.

Figure A2
Opposition Parties' reactions to the introduction of the 80 Euro Bonus

Berlusconi: "The 80 euros are an electoral bribe"   *Grillo: "Renzi's 80 euros are a miserable handout"*   *Meloni: "Only useful to hoard votes"*

Notes: This figure shows three newspaper articles reflecting the main opposition parties' reactions to the 80 Euro Bonus policy.

Figure A3
The Economic Effects of the Bonus

Share of Households with Bonus Recipients
Marginal Propensity to Consume
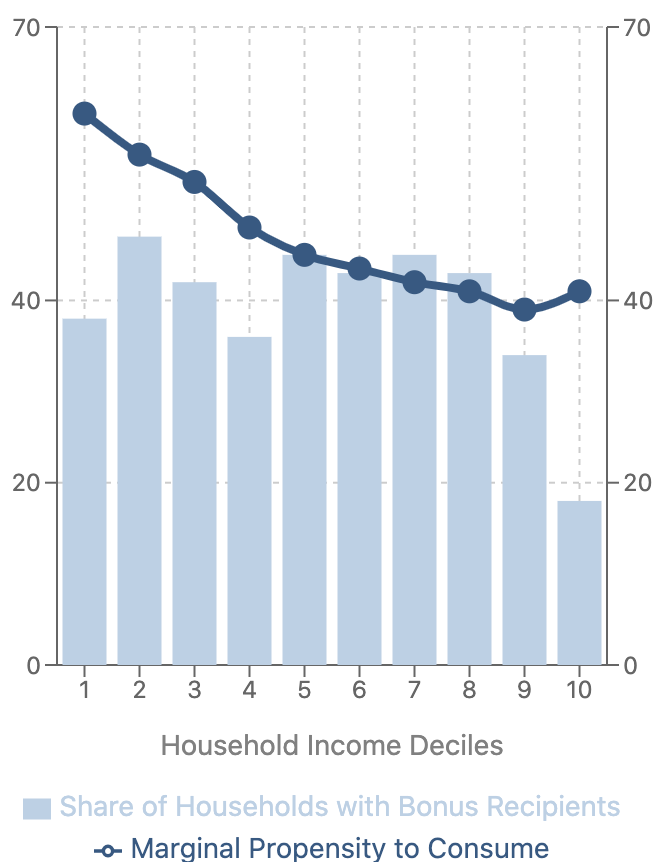
Notes: The Figure displays the incidence of beneficiaries and their marginal propensity to consume (MPC) by quintiles of household income. Receipt peaks at roughly 43–47 percent in deciles 2–8 but is only 39 percent in the poorest decile. Meanwhile, estimated overall MPC among recipients declines steadily—from about 55 percent in the lowest decile to around 30 percent in the highest. Calculations from the Italian Congressional Budget Office (UPB 2015) based on estimates of the Bank of Italy (Neri, Rondinelli and Scoccianti 2017).

Figure A4
Timing of the relevant events
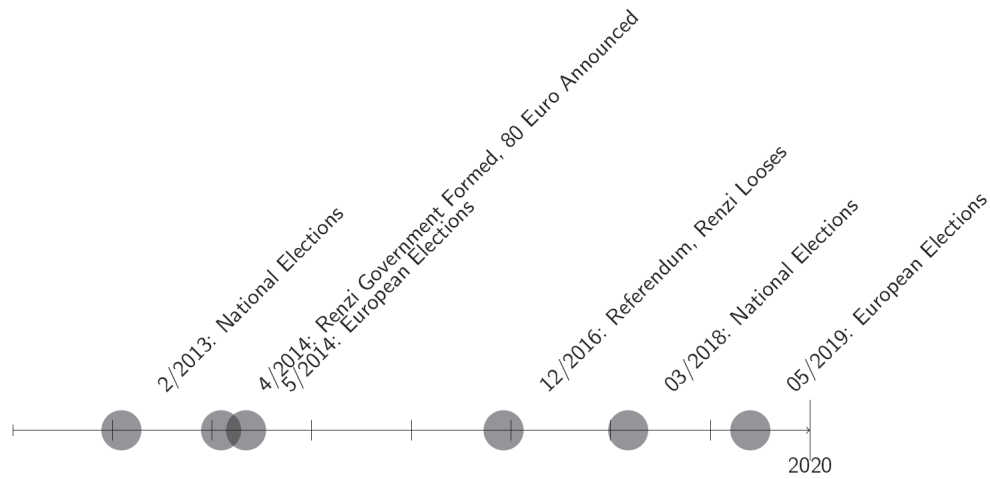
Notes: The figures shows a timeline illustrating key electoral events and policy implementation dates relevant for the analysis.

Figure A5
Evolution over Time of Outcomes in Municipalities with High vs. Low-Treatment Intensity
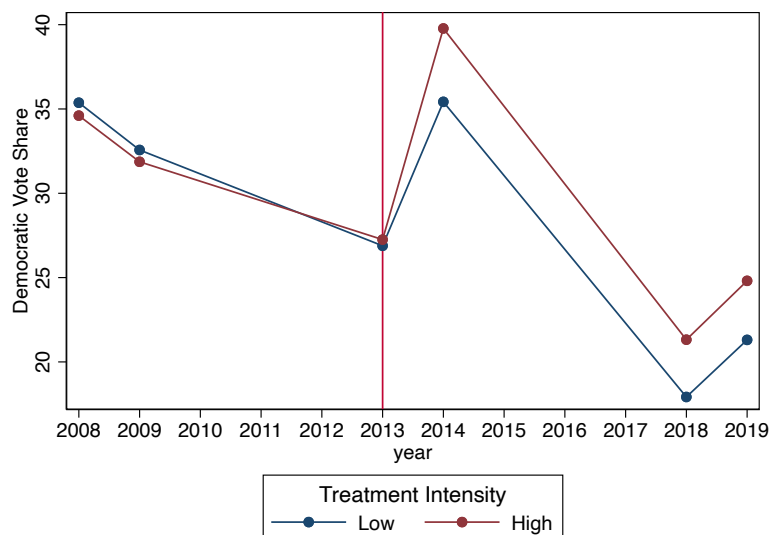
Notes: The figure shows the evolution of Democratic Party vote share over time from 2008 to 2019 for municipalities classified as high vs. low treatment intensity (above/below median Bonus recipients share).

Figure A6

First Stage Relationship between Predicted and Actual Share of Recipients
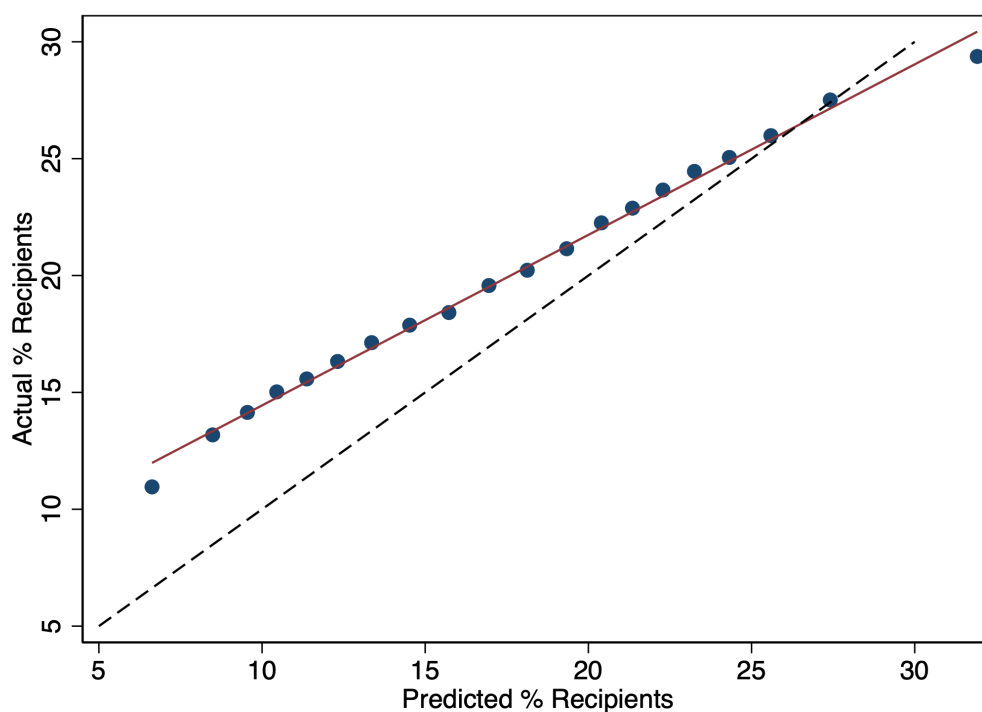
Notes: The figure plots the actual share of Bonus recipients in 2014 against the predicted share instrument, constructed as the interaction between the share of payroll employees and the share of all taxpayers earning between €8,000 and 26,000. Each dot represents a municipality. The fitted line corresponds to the bivariate regression line from the first stage. The dashed line represents the 45-degree line.

Figure A7

Local Identification Strategy around Eligibility Thresholds: Example Illustration
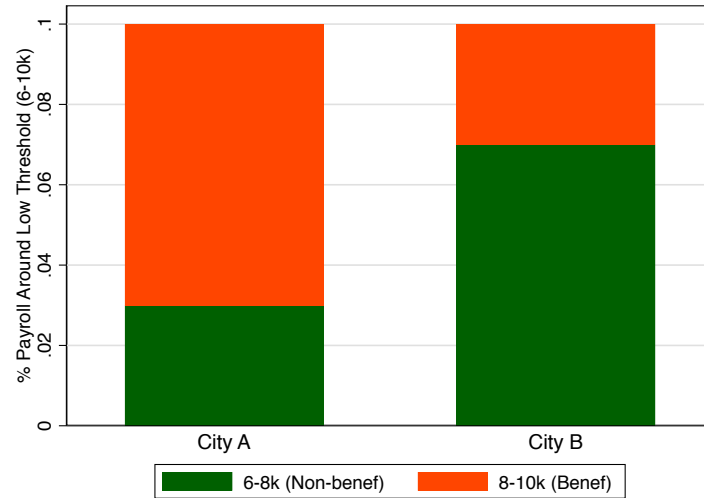
Notes: The figure provides an illustration of the local identification strategy comparing municipalities with an equal share of individuals around the lower eligibility threshold, but different distribution of individuals above (orange) vs. below (green) the threshold.

Figure A8

News containing "80 Euro" and "Renzi"
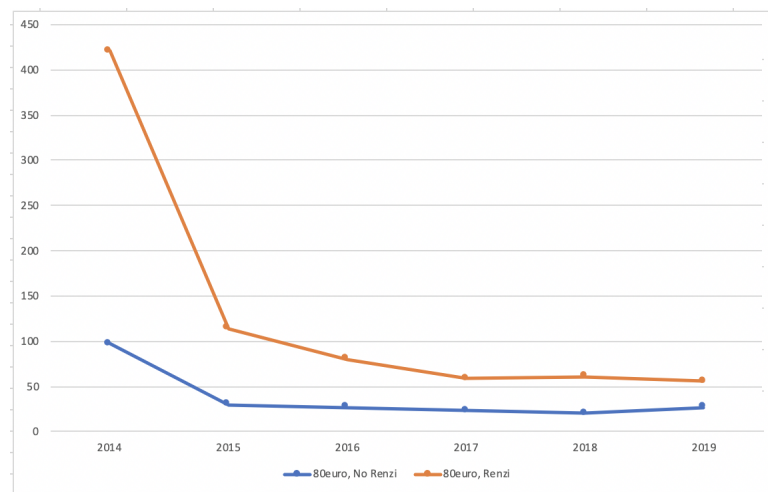
Notes: The figure shows the evolution over time of the number of articles containing the words "80 Euro" and "Renzi" from a Factiva search of the main Italian newspaper, Corriere della Sera, between January 2014 and December 2019. The blue line represents the volume of news titles including only the words "80 Euro", while the orange line represents the volume of news titles including both the words "80 Euro" and "Renzi".

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Figure A9
Understanding Limited Responses among Low-Income Individuals
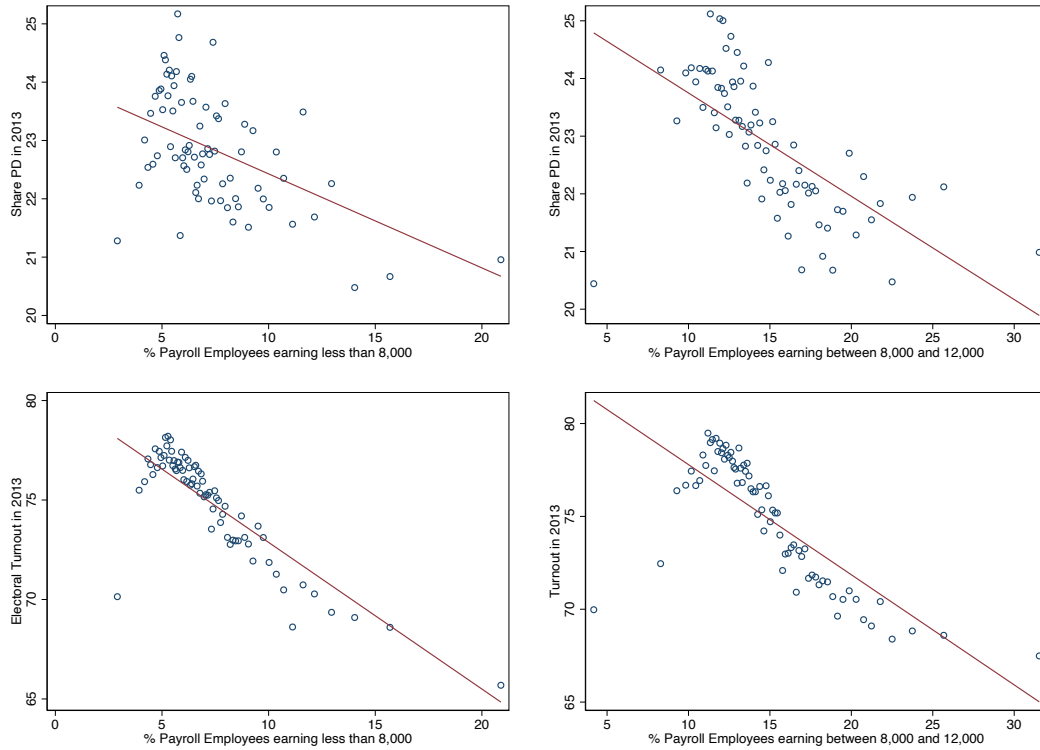
Notes: The figure show binned scatterplots of the relationship between a) the share of payroll employees who earn less than 8,000 euro per year and the share of votes for the Democratic Party in 2013 (top-left), b) the share of payroll employees who earn between 8,000 and 12,000 euro per year and the share of votes for the Democratic Party in 2013 (top-right) c) the share of payroll employees who earn less than 8,000 euro per year and electoral participation in 2013 (bottom-left), d) the share of payroll employees who earn between 8,000 and 12,000 euro per year and electoral participation in 2013 (bottom-right).

Table A1
Descriptive Statistics

Panel A: Political Outcomes

	Mean	Median	S.D.	Min.	Max.	N
PD Share 2013	22.86	22.05	7.67	0.90	56.53	7,712
Share 5 Stars 2013	22.72	22.70	6.84	0.20	56.46	7,712
Share Right 2013	16.73	15.92	6.60	0.00	71.85	7,712
Share Extreme Right 2013	7.65	5.26	7.79	0.00	50.93	7,712
Turnout 2013	61.30	64.03	15.62	13.67	97.25	7,712
Share PD over VEP 2013	17.12	16.47	6.15	0.78	49.17	7,712

Panel B: Treatment and Income Variables

	Mean	Median	S.D.	Min.	Max.	N
Bonus Recipients (Share)	20.04	20.14	5.59	1.67	52.68	7,712
Predicted % Recipients	18.29	18.38	6.15	0.56	38.97	7,712
% Recipients, Give Back	2.93	2.93	1.44	0.00	16.01	7,712
% Payroll	41.42	40.79	9.59	6.67	93.42	7,712
% Income ≤ 0	0.34	0.36	0.28	0.00	2.19	7,712
% Income 0-10k	29.45	27.92	8.45	11.50	95.83	7,712
% Income 26-55k	13.99	14.14	5.70	0.00	32.92	7,712
% Income 55-75k	1.06	0.95	0.88	0.00	21.26	7,712
% Income 75-120k	0.69	0.58	0.67	0.00	11.25	7,712
% Income Above 120k	0.25	0.08	0.37	0.00	6.61	7,712

Panel C: Controls

	Mean	Median	S.D.	Min.	Max.	N
% Females	50.91	51.08	1.74	32.73	61.80	7,712
High-School graduates	30.94	31.29	5.22	9.59	54.83	7,712
Unemp. Rate	10.33	7.87	6.33	0.00	42.18	7,712
Immigrants	6.54	5.77	4.28	0.00	41.25	7,712
University graduates	8.09	7.68	3.05	0.00	32.50	7,712
Province Capital	0.03	0.00	0.18	0.00	1.00	7,712

Notes: This table reports summary statistics for municipal-level political outcomes, treatment and income variables, as well as demographic and socioeconomic controls, measured in 2013.

Table A2

Determinants of Treatment Intensity

	% Recipients					
	(1)	(2)	(3)	(4)	(5)	(6)
% Eligible Income	0.251*** [0.00615]	0.272*** [0.0111]	0.148*** [0.0113]	-0.0179 [0.0202]	-0.0265 [0.0203]	-0.0368* [0.0203]
% Payroll	0.341*** [0.00678]	0.336*** [0.00868]	0.435*** [0.00888]	0.156*** [0.0371]	0.138*** [0.0372]	0.125*** [0.0359]
% ≤ 0			-0.121 [0.127]	-0.0189 [0.127]	-0.0361 [0.125]	-0.104 [0.126]
% 0-10,000			-0.207*** [0.0120]	-0.0933*** [0.0157]	-0.0926*** [0.0159]	-0.106*** [0.0164]
% 26-55k			-0.269*** [0.0206]	-0.171*** [0.0207]	-0.166*** [0.0208]	-0.191*** [0.0222]
% 55-75k			-0.188* [0.103]	-0.0386 [0.100]	-0.0270 [0.0977]	-0.0653 [0.101]
% 75-120k			-0.320*** [0.0967]	-0.159 [0.0979]	-0.156* [0.0946]	-0.168* [0.0973]
% Above 120k			-0.283* [0.168]	-0.140 [0.164]	-0.145 [0.157]	-0.188 [0.152]
Predicted % Recipients				0.629*** [0.0785]	0.644*** [0.0789]	0.642*** [0.0769]
% Recipients, Give Back					0.218*** [0.0281]	0.229*** [0.0285]
Females						-0.0130 [0.0214]
High-School graduates						0.0330*** [0.0112]
Unemp. Rate						-0.00922 [0.00896]
Immigrants						0.0824*** [0.0102]
University graduates						-0.0275 [0.0184]
Province Capital						1.613*** [0.122]
Observations	7712	7712	7712	7712	7712	7712
R2-Adj	0.784	0.834	0.866	0.871	0.873	0.877
Province FE	No	Yes	Yes	Yes	Yes	Yes

Notes: This table reports coefficients for a series of regressions analyzing the determinants of treatment intensity. Independent variables progressively include eligibility criteria, implementation frictions (bonus restitution rates), province fixed effects, and municipal demographic and socioeconomic characteristics. Significance levels: $^*p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

Table A3

Correlates of Bonus Recipients and Residual Variation

	% Recipients (STD)	Residuals (STD)
Political Variables		
PD Share 2013	-0.028	-0.049***
Delta PD Share 2008-2013	-0.074	-0.019
Vote Share 5Stars 2013	0.055	-0.020
Vote Share Right 2013	-0.381***	0.078***
$\Delta 2013 - 2008$ Vote Share Right	-0.205***	-0.021
Turnout 2013	0.616***	0.019
$\Delta 2013 - 2008$ Turnout	-0.278***	-0.001
Main Controls		
% Women	-0.091*	-0.000
Unemp. Rate	-0.537***	0.005
% Migrants	0.467***	0.082***
High-School graduates	0.464***	0.031
University graduates	-0.013	0.002
Province Capital	-0.027	0.066***
Additional City-Level Observables		
% taxpayers	0.739***	-0.001
Average income (log)	0.626***	0.009
Average income growth (log)	-0.044*	0.009
Gini index	-0.259***	0.011
% Employees with fixed-term contracts	-0.300***	-0.004
Share pop. outside of urban centers	-0.082*	-0.002
Share of population aged 75 and over	-0.428***	-0.022
Share of foreign minors	0.383***	0.000
Share of mixed couples	0.252***	0.031
Foreign employment rate	0.247***	0.013
Italian/foreign employment ratio	0.238***	0.019
Italian/foreign unemployment ratio	-0.355***	-0.016
Foreign residential mobility index	-0.123***	-0.011
Foreign school attendance index	0.131***	-0.023
Average family size	0.229***	0.026
Share of young people living alone	-0.172***	-0.023
Share of single-parent young families	-0.002	0.018
Share of young couples without children	0.516***	0.023
Share of elderly living alone	-0.337***	-0.026
Adults in lifelong learning	-0.118**	-0.002
Adults with secondary/higher vs lower ed	0.021	-0.007
Share of illiterate adults	-0.502***	0.000
Share of adults with higher education	0.126**	-0.003
Education level of 15–19 year olds	0.039	0.008
Share of adults with lower secondary edu	0.026	0.021
NEET rate (15–29)	-0.488***	0.010
Ratio of active to inactive young people	0.245***	0.033
Male unemployment rate	-0.514***	-0.004
Female unemployment rate	-0.476***	0.014
Employment rate (15–29)	0.560***	0.006
Share employed in agriculture	-0.345***	-0.016
Share employed in manufacturing	0.476***	-0.015
Share employed in services	-0.272***	-0.002
Share employed in high/med-skilled occup	0.068**	-0.026
Share employed in manual occupations	0.298***	0.016
Share employed in low-skilled occupation	-0.300***	-0.018
Daily mobility for work or study	0.708***	0.030
Out-of-town mobility for work or study	0.423***	-0.004
Index of social vulnerability	-0.475***	0.017
Families in housing hardship	-0.034	-0.004
Share of large families	0.182***	0.022
% families in economic hardship	-0.426***	0.000
Share living in overcrowded conditions	-0.123***	0.025
Share of young people excluded from work	-0.426***	-0.006
Share of families with potential care ha	-0.359***	-0.001

Notes: This table reports standardized coefficients from separate bivariate regressions of (i) the raw share of Bonus recipients (column 1) and (ii) the residualized share of Bonus recipients (column 2), on each covariate listed. All variables are measured prior to the policy (2013 or 2011) and standardized to have mean zero and unit variance. Residuals in the right panel are obtained from a regression of Bonus recipient share on policy eligibility criteria (payroll and income distribution) in 2013. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, IV
Estimates

	$\Delta 2013 - 2014$ Share PD	
	(1)	(2)
Bonus Recipients (Share)	0.128***	0.174***
	[0.0155]	[0.0217]
Pre-T Mean	22.90	22.90
Observations	7712	7712
R2	0.0186	0.0389
First Stage Coef	0.730	0.770
First Stage F-stat	18553.4	7254.9
Year FE	Yes	Yes
Controls	No	Yes

Notes: Standard errors clustered at the municipality level in brackets. Significance levels $^*p <$ $0.1,^{**}p < 0.05,^{***}p < 0.01$. %Recipients denotes the actual share of Bonus recipients in the voting-eligible population, instrumented by the predicted share of recipients. The instrument is constructed by interacting the municipality-level share of payroll employees with the share of all taxpayers (payroll and non-payroll) earning between €8,000 and €26,000, based on 2013 income tax records. This predicted value captures exogenous variation in treatment intensity arising from pre-policy income and employment structures. The first-stage F-statistics are reported at the bottom of each column.

Table A5

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Local Difference-in-Differences

	Dependent Variable: $\Delta 2013 - 2014$ Share PD			
	(1)	(2)	(3)	(4)
	PANEL A: Lower Threshold			
% Pop 7-9k	-0.720***			
	[0.204]			
Rel. Share Above Min. Threshold	0.0119*			
	[0.00629]			
% Pop 6-10k		-0.488***		
		[0.129]		
Rel. Share Above Min. Threshold		0.0154*		
		[0.00905]		
% Pop 5-11k			-0.360***	
			[0.100]	
Rel. Share Above Min. Threshold			0.0339***	
			[0.0110]	
% Pop 4-12k				-0.288***
				[0.0828]
Rel. Share Above Min. Threshold				0.0322***
				[0.0117]
Mean Dep. Var.	12.55	12.54	12.53	12.53
Mean Upper Density	1.578	3.126	4.667	6.188
Mean Rel. Density Below	53.55	53.66	54.10	53.52
R2	7024	7073	7085	7092
Controls	0.0384	0.0382	0.0401	0.0396
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
MunicipalityFE	Yes	Yes	Yes	Yes
	(1)	(2)	(3)	(4)
	PANEL B: Upper Threshold			
% Pop 23-29k	-0.108			
	[0.0773]			
Rel. Share Below Max. Threshold	0.0105			
	[0.0106]			
% Pop 22-30k		-0.0575		
		[0.0631]		
Rel. Share Below Max. Threshold		0.0181		
		[0.0119]		
% Pop 21-31k			-0.0483	
			[0.0542]	
Rel. Share Below Max. Threshold			0.0186	
			[0.0140]	
% Pop 20-32k				-0.0356
				[0.0486]
Rel. Share Below Max. Threshold				0.0202
				[0.0159]
Mean Dep. Var.	12.52	12.52	12.52	12.52
Mean Bonus80	20.06	20.06	20.06	20.06
Mean Upper Density	5.756	7.755	9.661	11.57
Mean Rel. Density Below	55.73	58.57	60.70	62.92
N	7000	7049	7062	7069
R2	0.149	0.147	0.148	0.147

Notes: Standard errors clustered at the municipality level in brackets. Significance levels $^*p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$. All columns include the full set of controls. $\%Recipients$ is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. Each of the different variables $\%Pop.$ represent the share of taxpayers with incomes within the chosen bandwidth, while the variables $\%Rel.ShareAboveMin.Threshold$ and $\%Rel.ShareBelowMax.Threshold$ represent, respectively, the relative share of taxpayers within the bandwidth with income just above or just below the threshold for Bonus eligibility.

Table A6

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Local Difference-in-Differences, IV Estimates

	Dependent Variable: $\Delta 2013 - 2014$ Share PD			
	(1) PANEL A: Lower Threshold	(2)	(3)	(4)
% Recipients	0.646* [0.347]	0.332* [0.198]	0.445*** [0.145]	0.326*** [0.118]
% Pop 7-9k	-0.0215 [0.422]			
% Pop 6-10k		-0.261 [0.189]		
% Pop 5-11k			-0.144 [0.126]	
% Pop 4-12k				-0.175* [0.0954]
Mean Dep. Var.	12.55	12.54	12.53	12.53
R2	7014	7063	7075	7082
Controls	0.0284	0.0540	0.0490	0.0547
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
MunicipalityFE	Yes	Yes	Yes	Yes
	(1) PANEL B: Upper Threshold	(2)	(3)	(4)
% Recipients	0.702 [0.447]	0.525* [0.286]	0.436** [0.179]	0.507*** [0.154]
% Pop 23-29k	-0.738 [0.467]			
% Pop 22-30k		-0.388 [0.271]		
% Pop 21-31k			-0.161 [0.145]	
% Pop 20-32k				-0.139 [0.117]
Mean Dep. Var.	12.54	12.53	12.52	12.52
R2	50.49	53.90	55.72	58.56
Controls	7034	7072	7087	7087
Year FE	0.0241	0.0449	0.0497	0.0441
Controls	Yes	Yes	Yes	Yes
YearFE	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the municipality level in brackets. Significance levels $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. All columns include the full set of controls. $\%Recipients$ is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. In each column, this variable is instrumented using the relative share of taxpayers just above the Bonus eligibility threshold, i.e. $\%Rel.ShareAboveMin.Threshold$ (Panel A) or just below the Bonus eligibility threshold, i.e. $\%Rel.ShareBelowMax.Threshold$ (Panel B), within a specified bandwidth around the threshold. Each of the different variables $\%Pop.$ represent the share of taxpayers with incomes within the chosen bandwidth.

Table A7

The Effect of the 80 Euro Bonus on Democratic Party Performance and Turnout, Levels Specification

	$\Delta 2013 - 2014$ Votes PD			$\Delta 2009 - 2014$ Votes PD	$\Delta 2013 - 2014$ Voters	$\Delta 2009 - 2014$ Voters
	(1)	(2)	(3)	(4)	(5)	(6)
N. Recipients	0.429***	0.429***	0.522***	0.552***	0.579***	0.360*
	[0.0742]	[0.0741]	[0.124]	[0.141]	[0.130]	[0.218]
Pre-T Mean	1081.6	1081.6	1081.6	1081.6	1081.6	1081.6
Observations	7712	7712	7712	7697	7712	7697
R2	0.874	0.874	0.963	0.877	0.960	0.710
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluter Prov S.E.		Yes				
Weighted	No	No	Yes	No	No	No

Notes: Standard errors clustered at the municipality level in brackets, unless otherwise noted. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$. All columns include the full set of controls. $\%Recipients$ is the number of Bonus recipients in the municipality in 2014 over the voting eligible population.

Table A8

The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Controlling for Local Changes in Economic Conditions

	$\Delta 2013 - 2014$ Share PD	$\Delta 2013 - 2016$ Share PD	$\Delta 2013 - 2018$ Share PD	$\Delta 2013 - 2019$ Share PD
	(1)	(2)	(3)	(4)
Bonus Recipients (Share)	0.189***	0.136***	0.0947***	0.0863***
	[0.0454]	[0.0479]	[0.0294]	[0.0334]
Dep. Var. Mean	22.90	22.90	22.90	22.90
Observations	7709	7703	7484	7472
R2	0.0575	0.306	0.108	0.122
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Weighted				

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,$ $^{**}p < 0.05,$ $^{***}p < 0.01.$ The dependent variable in columns (1) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, in column (2) is the change between the vote share fo rthe Democratic Party in 2013 and the share of Yes votes in the 2016 Referendum, in columns (3) it is the change in the vote share of the Democratic party between the 2013 and the 2018 elections, and in columns (4) it is the change in the vote share of the Democratic party between the 2013 and the 2019 elections. The 2013 and 2018 elections were national parliamentary elections, while 2014 and 2019 elections were European parliamentary elections. %*Recipients* is the number of Bonus recipients in the municipality in 2014 over the voting eligible population. All columns include the following time-varying controls: % of payroll employees in the municipality and the % of individuals in each of the 7 income groups, % who attained high-school, % with college degree, % of foreign-born, % of women, average income, % of taxpayers and the local unemployment rate.

Table A9

Voting and Participation Patterns by Income and Employment Status

	Income Group			Employment Status					
	Low Income	Middle Income	High Income	Payroll	Self-Employed	Unemployed	Housewife	Student	Pensioner
	mean	mean	mean	mean	mean	mean	mean	mean	mean
Panel A: Voting									
Left-Right Self-Placement	5.08	5.24	5.30	5.31	5.60	5.32	5.58	4.77	4.94
Voted PD 2008	0.18	0.23	0.28	0.22	0.23	0.15	0.22	0.16	0.33
Voted PD 2013	0.24	0.27	0.30	0.23	0.23	0.20	0.30	0.16	0.39
Voted M5S 2013	0.38	0.27	0.22	0.36	0.22	0.38	0.30	0.27	0.16
Voted Berlusconi 2013	0.14	0.21	0.24	0.16	0.22	0.15	0.21	0.19	0.28
Voted Extreme Right 2013	0.06	0.07	0.08	0.07	0.09	0.08	0.05	0.07	0.06
Voted Center 2013	0.09	0.12	0.12	0.12	0.13	0.07	0.10	0.15	0.10
Last-minute Decision	0.36	0.37	0.28	0.35	0.35	0.39	0.41	0.39	0.26
Renzi Evaluation	6.13	6.88	6.75	6.68	6.63	5.76	6.60	6.47	7.10
Panel B: Turnout									
Voted in 2008	0.69	0.76	0.85	0.78	0.84	0.68	0.71	0.63	0.87
Voted in 2013	0.73	0.76	0.83	0.78	0.80	0.69	0.67	0.75	0.86
Abstained for Protest	0.58	0.43	0.44	0.51	0.53	0.47	0.52	0.35	0.57
Likely to Abstain in Future	5.05	4.41	3.64	4.43	4.02	5.38	5.18	4.19	3.45
Observations	736	1528	762	1158	302	266	300	298	612

Notes: The table shows descriptive statistics from ITANES survey data illustrating voting patterns and electoral participation differentiated by respondents' income groups and employment status.

Appendix B: Survey Details

The Italian National Elections Study Survey (ITANES)- University of Milan electoral cycle panel is a large representative survey including four waves: a pre-post data collection for the 2013 national election and a pre-post data collection for the 2014 European election. Data collection is based on on-line interviews of a sample of respondents from an opt-in on-line community of a private research company (SWG). The pre-electoral wave for the 2013 national election is a rolling cross section conducted between January and February 2013 with daily samples of 200 respondents, for a total of over 8000 interviews. Out of these respondents, 3026 have been selected for the second wave. In my analysis, I focus only on the reduced sample of 3026 respondents. Further details can be found here http://www.itanes.org/en/. Below is a translated list of questions used in the analysis

- If the national elections were to be held tomorrow, which party would you vote for?

- Which party did you vote for in the last (national/European) elections?

- In any case, which of the following parties do you sympathize with more, or which of the following parties do you consider closer to your own ideas?

- In general, how would you describe the overall action taken by the incumbent government, in a scale from 0 (very bad) to 10 (very good)?

- In general, what is your judgement of Matteo Renzi on a scale from 0 (very bad) to 10 (very good)?

- Regarding Italy's overall economic situation in the past year, how would you describe it: particularly improved, improved, unchanged, worsened, particularly worsened?

- Regarding Italy's overall economic situation in the future year, how would you describe it: will be much better, will be better, will remain unchanged, will be worse, will be much worse?

- Regarding your personal overall economic situation in the past year, how would you describe it: particularly improved, improved, unchanged, worsened, particularly worsened?

- Regarding your personal economic situation in the future year, how would you describe it: will be much better, will be better, will remain unchanged, will be worse, will be much worse?