The Political Economy of Stimulus Transfers

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Abstract

Stimulus payments are one of the most common policy tools during economic downturns. To maximize effectiveness, transfers should target liquidity-constrained individuals, yet they often end up benefiting the middle class. I argue that political incentives might explain this puzzle. I study one of the largest stimulus tax credits in history, that targeted median earners and excluded the lowest-income groups. The transfer modestly boosted consumption but significantly increased the incumbent's vote share by 0.18 pp per 1 pp rise in recipients. Electoral rewards persist up to five years post-policy introduction. I then document stronger responses in localities with relatively richer beneficiaries, suggesting that electoral incentives may prompt politicians to prioritize middle-income, electorally responsive groups over the poorer, more consumption-responsive ones. Finally, I document significant punishment of the incumbent among individuals who lose access to the transfer, which help explain politicians' reluctance to repeal stimulus tax cuts, despite their substantial costs. Overall, these findings demonstrate the importance of considering political incentives to understand the design of major taxes and transfers. JEL: D72, H23, H53, I38, O15.

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1 Introduction

Stimulus payments are a frequent tool used by governments during economic downturns. 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, as opposed to the most *consumption-responsive* ones, as suggested by economic theory. 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 economy 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. The Democratic Party government, led by the newly-appointed prime minister Matteo Renzi, adopted the policy as one of its first measures in April 2014, to stimulate consumption after the long-lasting recession that started in 2011.While initially adopted as a temporary measure only for 2014, the Bonus became permanent in 2015 and since then has never been abolished by any of the five different governments

[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).

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] Over 10 million payroll employees, corresponding to 20% of the voting-eligible population received the Bonus. Employers were required to assess employees' eligibility based on their annual payroll income and to credit the Bonus in their paycheck. 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.

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.[4]

The key threat to this design is the possibility that time-varying municipal-specific shocks are correlated with program intensity. I overcome this threat in four 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, 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

[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."

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 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.

Results show 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 national change in the Democratic Party's performance between 2013 (pre) and 2014 (post) elections: from 27% to 40.8%. I further document that the positive electoral rewards persist over time, up to the elections of 2019, 5 years from the policy adoption.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. 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).[5]

While delivering substantial electoral gains, the stimulus failed at inducing the desired fiscal multiplier effects: beneficiaries spent around 20% (30%) of the bonus on non-durables (durables) and saved the rest (Neri and Rondinelli 2017). The limited consumption responses are potentially due to the poor targeting of the policy, as over 25% of payroll employees earned less than €10,000 in 2014 and were thus mostly 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

[5]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.

result that is in line with the findings of Nygaard, Sorensen and Wang (2020) for the US Covid-19 stimulus checks.

Why did politicians choose not to target the most *consumption-responsive* individuals? I provide two pieces of evidence that suggest that electoral incentives distorted the allocation away from poor voters, who would be the most *consumption responsive*, and in favor of the middle-income voters, who seem to be the most *electorally responsive* ones. First, I exploit variation across localities in the share of excluded recipients, either because they fall below the minimum or above the maximum threshold. Here, I find evidence that the incumbent experiences only mildly lower rewards in the presence of low-income excluded recipients, but much stronger reductions in the presence of more high-income excluded ones. Second, I explore heterogeneity in the relative distribution of beneficiaries within the eligibility window, conditional on the share of overall beneficiaries. Here I find that the electoral effects are attenuated in places that tend to have relatively poorer beneficiaries, and significantly magnified in localities with relatively richer ones. These analyses confirm that beneficiaries closer to the median income are significantly more responsive from an electoral perspective, a result which is consistent with the median voter hypothesis. Overall, these results have important implications for understanding the political economy of stimulus transfers and why their design often deviates from economic optimality. They suggest that electoral incentives may bias allocations towards middle-income, electorally responsive groups instead of poorer, more consumption-responsive ones.

While I find evidence that places that benefited most from the policy exhibit higher support for the Democratic Party, estimated effects are potentially consistent with alternative explanations, which have distinct implications for electoral accountability and politicians' behavior in policy design. On the one hand, it could be that individuals respond directly to the improvement in their own economic conditions, for either instrumental (Healy, Persson and Snowberg 2017) or reciprocal motives (Finan and Schechter 2012). On the other hand, if the policy achieved the intended economy-wide stimulus effects, voters might be rewarding the incumbent for the improvement in overall economic conditions, a phenomenon known as *sociotropic economic voting* (Ansolabehere, Meredith and Snowberg 2014, Lewis-Beck and Stegmaier 2018).[6] These

[6] A large political science literature has devoted attention to testing these different theories, finding different results depending on the context and the type of policy at stake. See Healy and Malhotra (2013) for a broad review of issues related to retrospective voting.

explanations have distinct implications for politicians' behavior in policy design. If voters only reward politicians that directly benefit them, then electoral concerns might bias politicians towards designing transfers targeting the most *electorally responsive* voters. On the other hand, if voters reward incumbents for overall improvements in economic conditions, then electoral and economic incentives are aligned and politicians should aim at maximizing the amount of economic stimulus when designing transfers by targeting the most *consumption-responsive* voters.

I provide several pieces of evidence to disentangle between these alternative explanations. First, I argue that the timing and persistence of effects allow me to partially learn about the potential mechanisms at play. Specifically, the sudden emergence of positive rewards in the very first election of May 2014 is unlikely to be explained by a *sociotropic* response, as the first bonus payments arrived in the days around the elections and the positive stimulus effects on the economy had yet to materialize. The long-run persistent effects are also inconsistent with a *sociotropic* 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.[7]

To further disentangle the rewards for personal versus aggregate improvements in economic conditions, I leverage two different sources of plausibly exogenous variation. First, I compare municipalities that have the same number of program beneficiaries but receive a different total amount per capita.[8] Intuitively, places with larger allocations should have experienced larger consumption stimulus responses and, therefore, larger electoral effects if voters reward the incumbent for overall improvements in economic conditions. Second, I exploit the occurrence of errors in bonus attribution by employers. Due to these mistakes, about 1.5 million people had to give back the Bonus when they filed tax returns in April 2015, giving rise to an ex-post local negative income shock. Here, I find evidence of significant ex-post electoral punishment among excluded recipients, but no significant difference among places where recipients received or had to give

[7]Additionally, while expectations about future economic improvements from the stimulus might influence voting behavior, such expectations would be uniform across the country following a national policy announcement. Therefore, any variation in expectations should be uncorrelated with the local density of beneficiaries.

[8]The heterogeneity in allocated amounts arise because of variation in a) the number of months worked, as the bonus is paid out monthly and b) the income of recipients, as recipients with income between €24,000 and €26,000 are awarded a lower transfer.

back larger average amounts, thus suggesting a limited role for overall improvements in economic conditions in explaining the electoral response observed.

Furthermore, I exploit 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.[9]

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, in the last part of the paper I conduct a counterfactual analysis of a simple 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 provide new insights to three strands of the literature.

[9] I also provide suggestive evidence that the salience of the Bonus and its strong association with the incumbent likely played an important role, 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).

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.[10]

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 conditional cash transfers or clientelistic promises in young democracies (Wantchekon 2003, Zucco 2013, Pop-Eleches and Pop-Eleches 2012, Labonne 2013, Galiani et al. 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). While electoral rewards are more likely to be seen as a clientelistic exchanges in contexts with weak institutions – where voters might be induced to think they would lose benefit eligibility or their locality would receive less funds in the future if they don't vote for the incumbent – 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.[11] I provide new evidence that similar dynamics emerge in mature democracies and for

[10]For example, Ding et al. (2024) find sizeable effects of Chinese government-issued digital coupons designed to encourage spending in certain categories during Covid-19. While highly effective in stimulating consumption in targeted sectors, the program induced welfare gains for consumers that were only 50 percent of the ones they would have gained from receiving cash. This casts doubts on the political feasibility of similar programs in countries with electorally-motivated officials.

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

allocations that can be seen as fully independent from voters' behavior.

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.

2 Institutional Background

2.1 The 80 Euro Bonus

Among European countries, Italy was one of the mostly 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 one experienced by the euro area. Total consumption dropped by over 6% and the recession was prolonged by internal demand stagnation (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 March 12th 2014 and formally adopted on April 24th (Law Decree 66/2014).[12]

The Bonus is a monthly tax-credit of €80 for *payroll employees* with a total gross annual income between €8,145 and €26,000.[13] The transfer represents an increase of 6 to 12% in the monthly wage bill. For comparison, average wage growth in 2014 was around 1.4%. Enrollment in the transfer is automatic: the employer is required to assess

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

[13]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.

eligibility based on the individual's earnings and reduce withheld tax contributions, thus increasing the monthly pay-check by €80. Eligibility is determined based on the individual's total annual income, and 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. While initially funded only for 2014, the Bonus became permanent in 2015 and is still in place.[14]

Administrative and economic factors help explain the structure of the policy and the 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 credited by the employer. The income eligibility, and in particular the lower threshold, is partially 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.[15]

The choice to exclude tax-exempt individuals had important implications in terms of policy targeting and effectiveness: the impacts on consumption were in fact below the government's expectations, and could have been higher had the lowest-income workers been included, as they were likely to be the ones with the highest propensity to consume. According to government estimates, the policy in 2014 induced a fiscal transfer of 7 billion, equivalent to 0.4% of GDP. The Parliamentary Budget Office (UPB) - the Italian analog to the Congressional Budget Office - estimated a positive impact of the policy on GDP of around 0.2% for 2015. Using data from the Bank of Italy's Survey of Household Income and Wealth, the Parliamentary Budget office estimated that only 39% of families in the first income decile benefited from the Bonus. These families had an estimated average propensity to consume of 60%, while the average propensity to consume of the beneficiaries was esitmated to be 46%, in line with the national average. On the other hand, over 45% of families in the 7th and 8th

[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.

[15]The threshold of €8,145 applies to individuals who worked the entire year and are thus entitled to the full tax deducion of €1,880. For individuals with with fixed-term contracts who have been employed for less than 365 days, the deduction is reduced but it can never be lower than €1,380, thus amounting to a minimum threshold to receive the monthly bonus of €6,000.

decile of the income distribution benefited from the Bonus, the share going down to 35% for the 9th and 20 % for the top decile (UPB 2015).

2.1.1 The economic impacts of the Bonus

In a study conducted by the Bank of Italy, Neri and Rondinelli 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, 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 that are not consumption constrained. Using machine learning tecniques, they show that the effectiveness of the program could have been substantially increased if the allocation had been decided using a simple machine learning agorithm 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.

2.2 Elections

Italy is a parliamentary Republic that operates with a multi-party system. both National parliamentary elections and European elections take place every 5 years using a proportional system, but they take place at different points in time. European elections are particularly significant because they are held a year after national elections and are considered a crucial moment to show approval or discontent with the incumbent government, without compromising its survival. This feature offers a unique analytical advantage to study voters' responses to stimulus transfers, as it rules out the possibility that voters might re-elect the incumbent out of concern for losing access to benefits should a new government take office. Instead, voters are able to express their opin-

ions on government policy without impacting the immediate stability of the national government.

The 2014 European Elections were especially critical for the incumbent government. In 2013, the Italian parliamentary elections had led to a challenging three-way split among the center-left coalition (led by the Democratic Party - PD), the center-right coalition (led by Silvio Berlusconi's PDL), and the 5-Star Movement. The PD and PDL eventually formed a coalition government led by Enrico Letta after complex negotiations. However, in late 2013, Matteo Renzi won the PD's primary election, forming a new government in February 2014. This made the 2014 European elections a crucial test of national support for the new prime minister. Given the pivotal nature of this election, the adoption of the 80 Euro Bonus shortly beforehand was criticized by the 5-Star Movement—Italy's main opposition party—as a mere electoral tactic to win votes.

Indeed, the European elections of 2014 turned out to be a success for the Democratic Party. While voters in other European countries expressed support for populist and euroskeptic parties, the Italian electorate gave a huge endorsement to the pro-European Democratic Party (PD) guided by Matteo Renzi. The PD took 40,8 % of the votes, the best result ever received by a single party in a national election since 1958. Subsequently, in December 2016, voters were called to vote in a Referendum to approve a large package of constitutional reforms promoted by the government. The significance of the reforms was emphasized by Renzi's promise to resign as Prime Minister if the referendum was rejected. Thus, the referendum became a vote on the incumbent's permanence in office, making it as salient as a national parliamentary election. The 'No' vote won with 60% of the votes, leading to Renzi's resignation as Prime Minister, but the Democratic Party remained the main party guiding the government. New parliamentary elections took place in March 2018, which led to the creation of a coalition government between the 5 Star Movement and the Northern League. Finally, new European elections were held in 2019. The timeline of relevant events is depicted in Appendix Figure A1 .

3 Data and Empirical Strategy

3.1 Data

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. For 2014, 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. 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 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.

[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.

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 and Rondinelli 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. This research design has the advantage of allowing me to control for unobservables which are fixed over time with the use of municipality fixed effects.

Municipalities with relatively lower share of recipients serve as a control group for the behavior of municipalities with relatively higher share of recipients. Under the identifying assumption that absent the policy municipalities with high vs. low intensity of treatment would have followed parallel trends in the period from 2014 onwards, I would be able to capture the effect of the policy on electoral outcomes. 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, 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 eligibility was based on administrative criteria and implemented automatically through employers, the policy design offers a unique opportunity to exploit plausibly exogenous variation in fiscal exposure. 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 interaction of the two. 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. This specification recovers causal estimates of the effect of variation in treatment intensity on electoral outcomes, provided that no other confounding shock or omitted variable that varies differentially by the share payroll employees earning between €8,000 and €26,000 in a municipality, conditional on the overall income distribution and the overall share of payroll employees.

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 pre-determined 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. Column (1) includes two variables corresponding directly to the policy's eligibility requirements: the share of payroll employees and the share of taxpayers earning between €8,000 and €26,000. These two margins alone explain nearly 78.4% of the variation in Bonus exposure. Column (2) includes province

fixed effects to absorb regional differences in labor markets and income structure; this improves the adjusted R-squared to 83.4%. In column (3), I replace the two margins with a single predicted eligibility share constructed as the product of the payroll and income shares, assuming independence. This predicted share alone explains 86.8% of the variation in actual Bonus receipt. Column (4) introduces a measure of attribution error—namely, the share of recipients who had to return the Bonus after filing taxes (ex post ineligibility). These variables account for observed implementation frictions and raise the explanatory power slightly, to 87.0%. Finally, column (5) adds a battery of municipal-level controls—including education levels, unemployment rate, gender composition, immigrant share—to check whether residual variation is systematically correlated with demographic or socioeconomic features that might also influence electoral outcomes. The adjusted R-squared increases only marginally to 87.3%, and the coefficients on most added controls are small in magnitude, with limited statistical significance.

Taken together, these results demonstrate that the vast majority of variation in Bonus treatment intensity across municipalities can be attributed to mechanical 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 exposure 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.

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.

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. The second 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-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 speci-

[17] In Italy, as in many other European countries, there is no voter registration requirement. The voting eligible population thus coincides with the population of registered voters.

[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, 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 PD vote share across all values of treatments. The figure displays a clear positive and 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.

fication to estimate a standard dynamic difference-in-differences model:

$$Y_{m,t} = \sum_{k \neq 2013} \beta_k \left(\text{Year}_{t=k} \times \text{Recipients}_{m,2014} \right) + \sum_{k} \gamma_k \left(\text{Year}_{t=k} \times X_{m,2013} \right) + \mu_m + \epsilon_{m,t} \tag{3.2}$$

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 and interacted with year fixed-effects.[19]

4 Results

4.1 Short-run Effects

The 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 including region-times-year fixed effects (5), and become even larger when weighting the regression by the size of the electorate in 2013, as shown in column (6), suggesting that

[19] Alternatively, I also run the following separate regressions, each for a different time horizon $\Delta Y_{m,2013,k} = \beta Recipients_{m,2014} + X'_m\zeta + \gamma_t + \delta_t + \epsilon_{mt}$. Results from the two specifications deliver results that are qualitatively and quantitatively very similar. Results from this alternative specification are displayed in Figure A6 .

effects are stronger in larger municipalities.[20] As a further robustness check, in column (6), 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.

A natural question to ask is whether the gains in Democratic Party's performance are due to changes in the extensive margin, i.e. related to who decides to turnout to vote, or in the vote choices of existing voters. While it is difficult to fully disentangle these effects looking at aggregate-level data, I provide some evidence of the two concurring mechanisms by looking at the impacts, respectively, on votes to other parties and turnout. Results are reported in Appendix Table A8 .Overall, I find little evidence that the Bonus affected negatively votes towards other parties, while most of the effects seem to operate via a significant increase in turnout. 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 (column 5 of the Table).

4.2 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. For the lower threhsold, these

[20] In fact, by splitting the sample between large and small municipalities, the coefficient is 0.47 (0.18) for municipalities above (below) 10,000 inhabitants.

individuals are the ones who benefit the most from the policy, as for them the size of the transfer relative to their annual income is largest. Therefore, they are also the ones from which we might expect a larger electoral response. 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. Figure A3 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:

$$\begin{aligned}\Delta Y_{m,2013,2014} = \beta_1 AroundThreshold_{m,2014} + \beta_2 RelAbove_{m,2014} \\ + X'_m\zeta + \gamma_t + \delta_t + \epsilon_{mt}\end{aligned} \tag{4.1}$$

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

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

Appendix Table A4 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 bewteen 8,000 and 10,000 among those earning between 6,000 and 10,000 euro (*AroundBelow*). 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 similar magnitude across specifications.

4.3 Long-run Persistence

So far, I have documented a strong, positive short-run effect of the 80 Euro Bonus on incumbent electoral performance. But how persistent are electoral rewards? Theoretically, models of rational, retrospective voting would predict long-lasting electoral rewards,

given that the policy remained in place in the long run. On the other hand, voters might not be perfectly rational. Cognitive biases like reference dependence, voters' myopia, or "peak-and-end" heuristic might lead to the fast dissipation of electoral rewards. The persistence of electoral rewards has been somewhat overlooked empirically, either for data limitation or identification challenges. Previous papers have mostly documented short-run beneficial electoral effects, with the exception of Bechtel and Hainmueller (2011), who show that 25% of the electoral rewards from flooding disaster relief persist three years later in Germany. The right panel of Figure 3 displays the results for the long-run effects of the policy, according to estimates from specification 2. The electoral rewards persist, but are attenuated in magnitude over time. Looking at the effect on 2018 national elections, the coefficient is 0.067, less than half the size of the short-run effect in 2014, while the one for the European elections in 2019 is even smaller, 0.05. It is somewhat surprising to observe persistence of the effects up to 5 years later, 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 electionstwo 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 the incumbent. There are two things to note to better understand the results. First, the 80 Euro Bonus is still currently in place, and the number of beneficiaries has been slightly increasing over time, as the upper threshold has been raised in 2018 and 2020. Second, and most importantly, even after stepping down as Prime Minister, Matteo Renzi remained a central character of the Italian political arena, and he constantly highlighted that he was the "father" of the 80 Euro Bonus. Therefore, the policy remained strongly associated with him: when searching on Google for "Bonus 80 Euro", 8 out of the 10 top search outcomes contain the name of "Renzi". This is further confirmed by looking at media articles: after an initial drop between 2014 and 2015, the number of articles mentioning the 80 Euro with and without the name of Renzi both remain constant over time, suggesting a persistent pattern association of the policy with his creator, as shown in Appendix Figure A4).

4.4 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 voters are more likely to be infra-marginal, meaning that they were already voting for the Democratic Party before. 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 3, I thus 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, which reflect the share of recipients closer to the two eligibility thresholds $\%Recipients8-12k$ and $\%Recipients20-24k$.[21] Given that I am going to look at interaction effects among continuous variables, and in order to enhance comparability across different variables, 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 (4), I reproduce these main results but introduce, respectively, $\%Recipients8-12k$ and $\%Recipients20-24k$ as additional explanatory variables. Results are unaffected and neither of the two variables has any significant impact per se. In columns (3) and (5), 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

[21] 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.

higher share of $Recipients20-24k$ amplifies the electoral response by about 27%. Finally, in column (6), I repeat the same exercise but include both variables at the same time, obtaining very similar results.

Overall, this analysis confirms that beneficiaries closer to the median income are significantly more responsive from an electoral perspective, a result which is consistent with the median voter hypothesis, and that might help explain why politicians tend to target stimulus transfers towards the middle, as opposed to the bottom of the income distribution.

4.5 Punishment among Excluded Individuals

While delivering substantial benefits to the incumbent among recipients, stimulus transfers are likely to create bitterness among excluded individuals. The degree of punishment among different groups of the population is likely to matter for politicians' strategic design choices. In particular, if relative poorer, consumption-responsive individuals are less likely than relatively richer voters to punish the incumbent for being excluded from bonus receipt, this might furher contribute to explain why observed transfers are frequently shifted towards the middle of the income distribution.

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 2. For reference, In column (1), I reproduce the analysis of Table 1, but using the standardized share of bonus recipients. Columns (2) and (3), respectively, introduce variables to account for groups excluded from the bonus due to income thresholds. *ExcludedBelow* represents the share of individuals in the voting eligible population that are excluded because their income was below the minimum threshold of 8,000€, while *ExcludedAbove* represents those marginally excluded because their income was above the maximum threshold. For consistency, I adopt a symmetric window and consider the individuals earning between 26,000 and 34,000€as those excluded from above. Interestingly, we observe asymmetric effects of exclusion. While both coefficients are negative and significant, suggesting the presence of punishment among excluded recipients, the effect of *ExcludedAbove* is over three times larger in magnitude relative to the one on *ExcludedBelow*. 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 be-

havior compared to lower-income excluded 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 lower response in areas with more low-income recipients and the lack of punishment among lower-income excluded non-recipients? This pattern seems counterintuitive since low-income recipients should have higher marginal utility from transfers and receive proportionally larger income increases from the bonus. Two reasons could potentially explain this result. First, lower-income beneficiaries might be less likely to vote. Indeed, a large body of work has shown that voter turnout is positively correlated with income (Solt 2008, Elsässer and Schäfer 2023). Alternatively, it can be that lower income recipients are more likely to be a left-wing supporters anyways, and thus they are less marginal for the Democratic Party. I provide several pieces of evidence that seem to suggest that the former explanation is more likely to drive the results. First, I do not find any evidence that municipalities with higher shares of lower-income recipients displayed higher propensity to vote for the Democratic Party before the stimulus was adopted, a result that seems to go against the idea that lower-income recipients are strong-hold supporters, and thus non-marginal voters for the Democratic Party. A similar resoning applies for lower-income excluded individuals. Specifically, in the top panel Figure A8 , I show two binned scatterplots of the relationship between, respectively, ii) the share of lower-income recipients (i.e. those earning between 8,000 and 12,000 euro) and the share of votes for the Democratic Party in 2013 (left) and ii) the share of lower-income excluded non-recipients (i.e. those earning below 8,000) individuals and the share of votes for the Democratic Party in 2013 (left).

On the other hand, in Table A7 I show that indeed turnout play an important role in explaining the effects. Futhermore, in the bottom panel of Figure A8 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.[22]

4.6 Disentangling Rewards for Personal vs. Aggregate Improvements in Economic Conditions

There are two potential alternative mechanisms that can explain the persistence result. On one hand, beneficiaries might be effectively rewarding the incumbent for the policy. On the other hand, if the 80 Euro Bonus achieved the intended consumption stimulatory effect, then places with a higher share of beneficiaries might have experienced a stronger improvement in economic conditions. This second effect cannot explain the very short-run response (as most beneficiaries received the first Bonus payment in the days around the elections), but it is more likely to play a role in the longer run. Previous empirical work shows that voters reward incumbents for general improvement in economic conditions ((Wolfers 2007, Bagues and Esteve-Volart 2016, Ansolabehere, Meredith and Snowberg 2014)). However, research has shown that the incumbent reward mechanism should extend to any type of incumbent. This story is inconsistent with the observed, positive result in 2019, when the center-left coalition was no longer the governing incumbent.

To disentangle the direct effect of the policy from the indirect effect of potential consumption stimulus, I leverage two different types of plausibly exogenous variations. First, I compare municipalities that had the same number of program beneficiaries but received different total amounts per capita. Specifically, I exploit the variation arising from the fact that a) the Bonus is awarded based on the number of "full-time" days worked, so workers who worked part-time or only for a few months received lower total amounts; b) workers who earn between €24,000 and €26,000 are subject to linear Bonus

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

phase-out. The thought experiments is as follows: consider two municipalities, in which 25% of the electorate receives the Bonus. In municipality A, most beneficiaries are full-time payroll employees who earn around €20,000, thus in 2014 they received the full Bonus amount of €640 per capita. In municipality B, half of the beneficiaries are richer, earning €25,000, and thus receiving only €40 per month, or a total of €320 in 2014. If the electoral rewards are driven by reciprocity motives/retrospective evaluations that do not depend on the level of income received, then, conditional on the total share of recipients, the per capita amount should have no additional explanatory power. On the other hand, if the electoral rewards in the long run are due to the observation of an economic boom, which might be partly due to the fiscal stimulus induced by the Bonus, then we should observe an additional positive effect of the per-capita amount of the Bonus, as municipalities where the average amount per recipient is higher were subject to a stronger fiscal stimulus. Results are displayed in the odd columns of Table 4. In column (1), I repeat the same regression of column (3) of Table 1, but now also include the variable $AmountPerCapita$, which is equal to the municipal average amount received per recipient.[23] The coefficient is very close to zero in magnitude and insignificant, consistent with the fact that, in 2014, the policy couldn't have displayed any fiscal stimulus effect, as most of the recipients hadn't received the first payment yet. In columns (3) and (5), I inspect the long-run effect of the policy, thus the dependent variables are the change in the vote share for the democratic party between 2013 and 2018, and between 2013 and 2019 respectively. Results show that, even in the long run, when the fiscal stimulus effect of the policy should have already taken place, there is no significant effect of having a higher $AmountPerCapita$ of the Bonus. Moreover, notably across all three columns, the coefficient on $\%Recipients$ remains unchanged.

As a further check, I exploit the Bonus misattribution by employers, giving rise to an ex-post negative income shock.[24] About 1.5 million people had to give back the Bonus when they filed tax returns in April 2015. This potentially induces two effects. First, individuals who had to give back the Bonus might resent the incumbent, and

[23] See Appendix Figure A5 for the distribution of the variable across municipalities.

[24] In particular, employers assessed the beneficiaries' eligibility based on the observed total annual income under the employee's contract, but Bonus eligibility is based on total payroll annual income, and this gave rise to incorrect Bonus assignment. For example, individuals who held multiple contracts were sometimes wrongly given the credit by multiple employers. Alternatively, individuals who work only a few months over the year were sometimes given the credit, even though their total annual income ended up being within the tax-exempt amount, so they were not entitled to the Bonus

thus vote against him, independently of how much money they had to return. Second, even conditional on the share of recipients who had to give back the money, there could be an additional negative effect due to the negative income shock caused by the sudden restitution. In the even columns of Table 4 I investigate the effect of Bonus restitutions. In particular, I introduce two additional explanatory variables to my main estimating equation: $\%RecipientsGiveBack$, which is simply the share of individuals who had to return the Bonus among Bonus recipients; and $\%AmountPerCapitaGiveBack$, which is the average amount that individuals had to give back.[25]

In column (1) of Table 4, I look at the short-run effect on Democratic Party performance in 2014 elections. Interestingly, conditional on the overall share of Bonus recipients, in municipalities where a higher share of people will turn out to be wrongly given the credit, the incumbent party received 0.04 percentage points more votes. This positive effect suggests that those who received the Bonus and then had to give it back are probably more likely to be "marginal voters", meaning that they are more likely to be on the fence of voting vs. not voting for the incumbent, or are more likely to be positively surprised by the unexpected "gift" received by the incumbent. This might be reasonable if one considers that those who are more likely to be wrongly given the Bonus are poorer individuals who do not have any positive taxable income at the end of the year, for whom the marginal value of the Bonus is higher relative to the rest of the population.

When looking at the long-run effect of the policy, the coefficient in column (4) on $\%RecipientsGiveBack$ remains essentially identical in magnitude, but flips sign. Notably, the election in 2018 is the first national election taking place after the recipients had to give back the money. The sign flip suggests that the recipients punish the incumbent for the mis-attribution committed by their employers. The size of the effect is smaller, but should be rescaled by the fact that the two variables have different variations in the sample (the S.D. for $\%Recipients$ is 5.6 while $\%Recipients, GiveBack$ is 3.6). If one rescales both coefficients by the standard deviations of the variables, the two coefficients are in fact very similar in magnitude (0.0138 vs 0.0121).[26] Notably, if one doesn't take into account the Bonus restitutions and their punishment effect, the coefficient on $\%Recipients$ is biased downward, as one can see comparing the coeffi-

[25] Appendix Figure A7 display the distribution of these two variables.

[26] In Appendix Table A9 , I repeat the same analysis but rescaling the number of recipients that had to give back the policy by the number of eligible voters and obtain very similar results.

cient estimate in column (4) with the one in column (3) and Figure 3. The punishment effect vanishes over time, as suggested by results in column (6), where the coefficient on $\%Recipients, GiveBack$ is not only insignificant, but also much closer to zero in magnitude. On the other hand, the amount per capita of Bonus that recipients have to give back does not have any significant effect across all the columns. Overall, results suggest that the electoral rewards are driven by the direct positive responses to transfer receipt and not by the indirect response to positive fiscal stimulus induced by the policy.

4.7 Survey Evidence

To corroborate the strength of my analyses and better understand the mechanisms driving the electoral impact of the 80 Euro Bonus, I complement the aggregate evidence with individual-level data. I exploit the availability of a unique survey data, the Itanes-University of Milan electoral cycle online panel. The dataset includes four waves: 2 pre-electoral surveys and 2 post-electoral surveys, for the 2013 General Election and the 2014 European election respectively. Therefore, for each individual, it is possible to observe her attitudes before and after the elections, for both elections of interest. Importantly, the pre-electoral survey for 2014 was conducted between May 5th and May 19th, when the 80 Euro Bonus policy had been already announced but not yet distributed; while the post-election survey was conducted between June 10th and June 18th, when the Bonus had been distributed to all recipients. Before turning to causal estimates, Before turning to causal estimates, I begin by documenting descriptive differences in political preferences and turnout behavior across income and employment categories. These patterns not only highlight which groups are most politically engaged and electorally responsive, but also provide insight into the potential political logic underlying the design and targeting of the policy.

To that end, I classify individuals along two dimensions: *income class* and *employment status*. Income is not reported directly in the survey, so I proxy it using a combination of self-reported social class and occupation-based indicators. I define three income classes: **Below** (low-income individuals), **Eligible** (middle-income, i.e., those likely to fall within the Bonus eligibility threshold), and **Above** (high-income individuals, likely above the threshold). In practice, individuals are coded as "Below" if they report low social class or report occupations such as unemployed or home-maker; as

"Eligible" if they report to belong to the middle social class plus matching occupational status; and as "Above" if they belong to the upper social class or are identified as high-income pensioners or professionals. Employment status is classified into six mutually exclusive categories based on current labor market position: **Payroll** employees (including those in temporary public wage-subsidy schemes), **Self-employed**, **Unemployed**, **Housewives**, **Students**, and **Pensioners**.[27]

Table A10 reports averages of key voting and participation indicators across three income groups and six employment categories. The data reveal marked heterogeneity in both political preferences and electoral participation. Middle-income individuals and payroll employees, who constitute the core target group of the Bonus, are more likely to support the Democratic Party (PD) in both 2008 and 2013, and evaluate Matteo Renzi (the PD leader at the time) more favorably than other groups. These same categories also exhibit relatively high turnout rates and lower intentions to abstain in the future, suggesting they are both politically aligned with the incumbent and suggesting greater potential for electoral responsiveness.

In contrast, support for the 5 Star Movement (M5S) in 2013 is concentrated among lower-income individuals, the unemployed, and students, groups that also appear less attached to the PD and more prone to abstention. Interestingly, some of these groups, particularly payroll-employed individuals and individuals the lower and middle-income brackets, report relatively high M5S support in 2013. This pattern is consistent with the idea that Renzi might have been able to win back previously disaffected voters, especially those who had turned to M5S in protest during the previous election.

Support for other parties also follows predictable patterns. Berlusconi's party performs better among higher-income and pensioner groups, while support for extreme-right parties remains low across all categories, rarely exceeding 0.09. Centrist parties receive modest support, peaking slightly among students and the self-employed. These additional patterns further underscore that Renzi's electoral base was likely concentrated among salaried, middle-income voters and suggest that the Bonus may have been electorally effective precisely because it targeted groups with moderate prior alignment, high political salience, and the potential to swing support back to the incumbent party.

I now turn to exploit the panel dimension of my survey data to isolate the effect

[27]Payroll includes individuals with payroll employment contracts as well as those receiving wage support through CIG/CIGS schemes (q180==6 or 7). Self-employed are identified via q178; unemployed, housewives, students, and pensioners follow standard q180 occupational codes.

of bonus receipt on individuals' voting behavior. As previously mentioned, the dataset includes four waves: two pre-electoral and two post-electoral surveys, for the 2013 General Election and the 2014 European Election. This structure allows me to track the same individuals across both elections and exploit the timing of the Bonus rollout. The pre-electoral survey for 2014 was conducted between May 5$^{\text{th}}$ and May 19$^{\text{th}}$, after the policy was announced but before payments were distributed; the post-electoral survey was conducted between June 10$^{\text{th}}$ and June 18$^{\text{th}}$, after all eligible recipients had received the transfer.

While the survey does not report Bonus receipt directly, I use information on employment status and occupation to proxy for eligibility. I define Bonus recipients as individuals who are both payroll employees and fall within occupational categories whose average earnings fall within the eligible income window.[28]

For each individual, I compare the change in the dependent variable between the pre and post 2014 survey. This is essentially equivalent to including an individual fixed effect. The regression is as follows:

$$\Delta Y_{Pre-Post,2014} = \beta_1 Payroll_{2013} + \beta_2 EligibleIncome_{2013} + \beta_3 80EuroRecipient_{2013} + X'_m\zeta + +\delta_p + \epsilon_{mt} \tag{4.3}$$

where $\Delta Y_{Pre-Post,2014}$ denotes the change in the outcome for individual i between the pre- and post-electoral waves in 2014. The coefficient β_3 captures the effect of being a likely Bonus recipient. All specifications include a number of individual-level demographic controls (gender, age, marital status), as well as province fixed effects.

There are two types of dependent variables of interest. The first involves self-reported support for the Democratic Party and voting behavior; the second involves self-reported assessment of the national and personal economic situation, both in the past year and in the future year. [29] In particular, I want to investigate whether individuals who receive the Bonus are more likely to vote for the incumbent, and if this is accompanied by a shift in the assessment of the national economic situation

[28]The following occupations are considered to have "eligible income": artisan, shopkeeper, self-employed agent or sales representative, teacher, blue-collar employee, white-collar employee, agricultural worker.

[29]See Appendix 6 for a list of the questions used.

(sociotropic voting) or of the personal economic situation (pocketbook voting).

In column (1) of Table 5, the dependent variable is a dummy equal to 1 if the respondent switched to support the Democratic Party in the post-election survey, equal to -1 for those that reported to support the Democratic Party in the pre-election survey, but not in the post-election survey, and it is equal to 0 for those that did not change.

[30] Results show that Bonus recipients are 11 percentage points more likely to shift towards supporting the Democratic Party. This is essentially the same magnitude found by Manacorda, Miguel and Vigorito (2011), and, once rescaled by the pre-treatment mean, is in line with the magnitudes found for the aggregate effects.[31] Importantly, I cannot detect any shift when looking at the same variable but for the change around 2013 elections, in column (2). This provides further evidence of the robustness of the main effect, and of the absence of pre-existing trends. To investigate the mechanisms behind this electoral shift, I examine whether the effect of Bonus receipt operates through changes in the respondents' perceptions of their own economic conditions, or of the economic environment. This allows me to test two types of mechanisms: pocketbook voting (personal economic improvements), and sociotropic voting (perceptions of general economic conditions). Across all specifications, the dependent variable is equal to 1 (-1) if the individual's assessment improved (worsened) and to 0 if it stayed the same or no answer was given. Columns (3) and (4) look at the assessment of the national economic situation, both in the past and in the future year, while columns (5) and (6) look at the respondent's own economic situation. Interestingly, Bonus recipients report significantly improved retrospective assessments of the national economy, but no changes in their personal financial outlook. This pattern points to a sociotropic retrospective mechanism, where voters reward incumbents based on broader economic perceptions rather than personal material gains. 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 $EconomicAssessmentImproved$, cap-

[30] In order to maintain a constant sample size, I set to 0 also the cases in which the information is missing. These represent a very limited number of instances, less than 9 % of the sample. Results are robust to the exclusion of these observations.

[31] Manacorda, Miguel and Vigorito (2011) focus on the consequences of conditional cash transfers on political support in Uruguay. They do not measure actual voting intentions/behavior, but rather overall assessment of the incumbent as follows "in relation to the previous government, do you believe that the current government is worse, the same, better?".

turing 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, survey data confirm that bonus recipients are more likely to switch to support the incumbent.

5 The Tradeoff between Economic and Political Returns

Economic theory suggests that stimulus payments should be targeted toward low-income, liquidity-constrained households to maximize consumption responses and economic stimulus effects. However, as demonstrated in the previous sections, politicians may face electoral incentives that pull in different directions, potentially leading to policy designs that prioritize political returns over economic efficiency. In this section, I use my estimates to quantify this tradeoff by performing some back-of-the-envelope calculations to estimate the counterfactual electoral effects of redirecting cash transfers from middle-income to lower-income recipients.

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, and (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, rather than the original €8,000 to €26,000 range. This alternative design maintains the same total number of beneficiaries but redirects benefits toward those with potentially higher marginal propensities to consume — precisely the type of targeting that economic theory and empirical estimates from Neri and Rondinelli (2017) and Andini et al. (2018) suggest would maximize consumption stimulus effects, given the budget and implementation constraints. 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 3 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). To facilitate meaningful comparisons, I standardize these coefficients using the observed standard deviations. A critical challenge is estimating the effects for the €0-€8,000 income bracket, which was excluded from the original policy. I address this through linear extrapolation based on the pattern observed across the measured income brackets. Using the calculated effects for the €8,000-€12,000 and €20,000-€24,000 groups, I compute the slope of the linear relationship between income and electoral effect, and then, using this slope (-0.00476), I extrapolate backward to estimate the effect for the €0-€8,000 group, which amounts to 0.138.

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 house-

holds (Nygaard, Sorensen and Wang 2020, Kaplan and Violante 2022).

Combining these estimates allows me to compute the implicit marginal rate of substitution between economic and political returns:

$$\text{MRS} = \frac{\text{Gain in Economic Effectiveness}}{\text{Loss in Electoral Effectiveness}} = \frac{30\%}{15\%} = 2:1 \tag{5.4}$$

This 2:1 ratio suggests that for each percentage point of electoral advantage politicians sacrifice, they could achieve a two percentage point gain in economic stimulus effectiveness. Put it differently, this quantified trade-off reveals the potential implicit political calculation that shapes policy design: politicians valued political returns at least twice as much as economic efficiency in this context.

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

1. *Political participation*: Middle-income voters typically have higher voting rates, making each benefit recipient more likely to translate into actual votes.

2. *Median voter targeting*: The €20,000-€24,000 income range is closer to the median voter, who receives disproportionate attention in electoral competition models.

3. *Information and salience*: Higher-income recipients may be more politically informed and thus more likely to attribute benefits correctly to the incumbent party.

6 Discussion and Conclusion

This study provides the first causal evidence of the effect of stimulus tax transfers on incumbent electoral performance. I find that municipalities with a larger share of beneficiaries among the voting eligible population experience a large and persistent increase in the share of votes for the party who adopted the policy. The effect is not driven by the indirect impact of the stimulus payment on the local economy. The results suggest that incumbent can reap significant electoral benefits through automatic payments benefitting a large share of the population and targeting middle-income voters. This might help explain why we frequently observe the adoption of stimulus spending programs targeted towards the median voter, as opposed to the lowest-income individuals, who would have a higher propensity to consume but are also harder to mobilize and less likely to be pivotal in the political process.

To have a better sense of the magnitude of my estimates and of their policy implications, a useful thing to do is to 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 0.32 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 6.4 percentage points. Given the average total number of voters between 2014 and 2018 of 30.3 million, this roughly amounts to 2 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 over 27,000 €. This number is close in magnitude to existing estimates of vote costs for aggregate local spending in the U.S[32], but is orders of magnitudes larger than estimates from the campaign spending literature (Bombardini and Trebbi 2011, Bekkouche, Cagé and Dewitte 2022). Had it been only a temporary measure, as it was announced to be, the 80 Euro Bonus would have been a pretty good deal in terms of vote cost. However, while short-run vote costs might be relatively low, they become significantly higher when the Bonus payments are made permanent. The persistence of the Bonus can also be explained by electoral tactics, given that the incumbent suffers significant electoral punishment when former recipients are excluded from the policy. This points towards the importance of taking into account dynamic inconsistency problems and the political difficulty to cut back spending.

[32]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.

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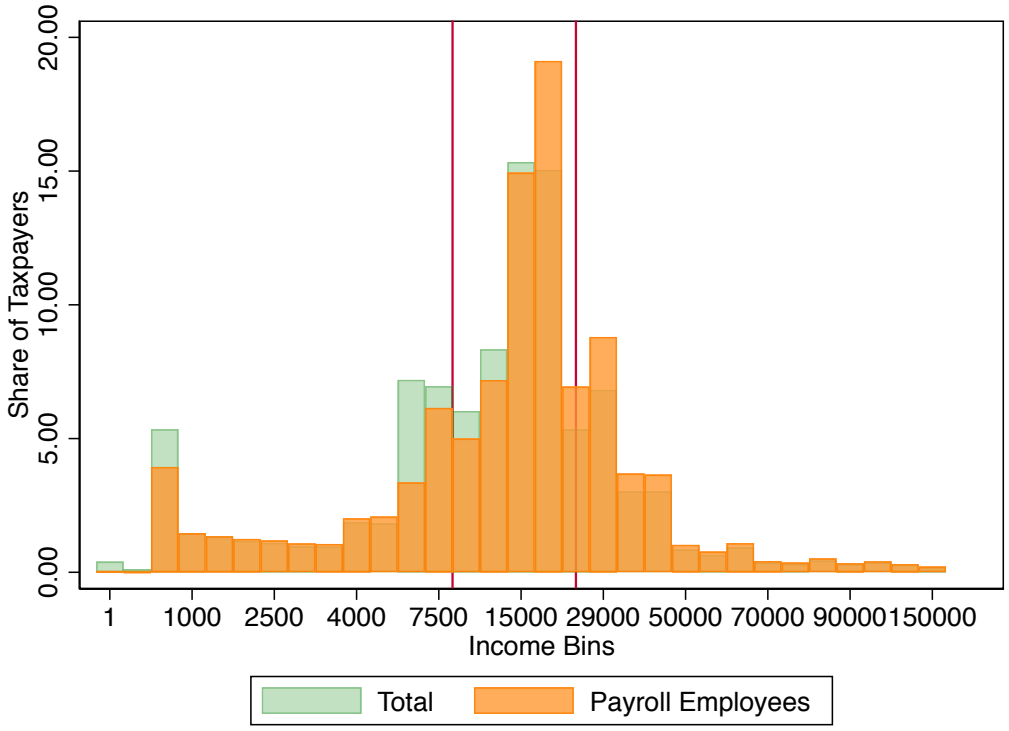
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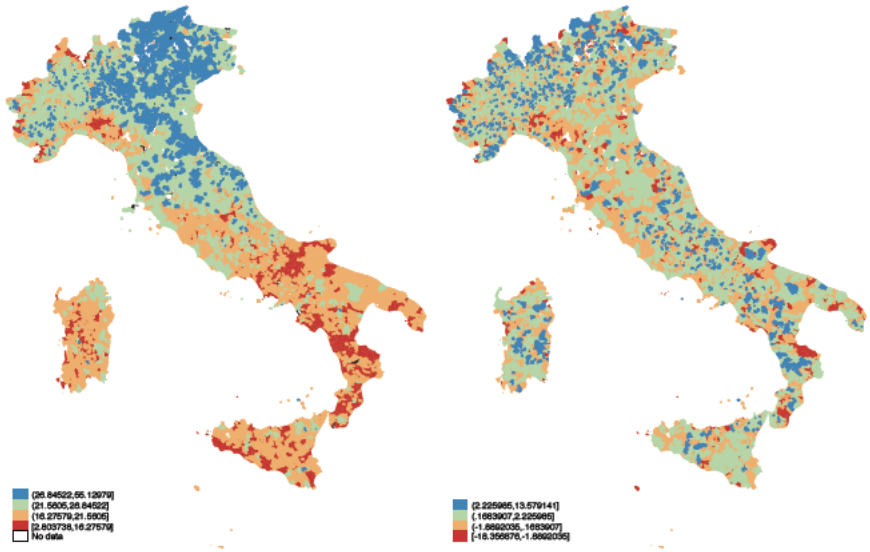
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Figure 1
Taxable Income Distribution 2013 (Official Tax Records)



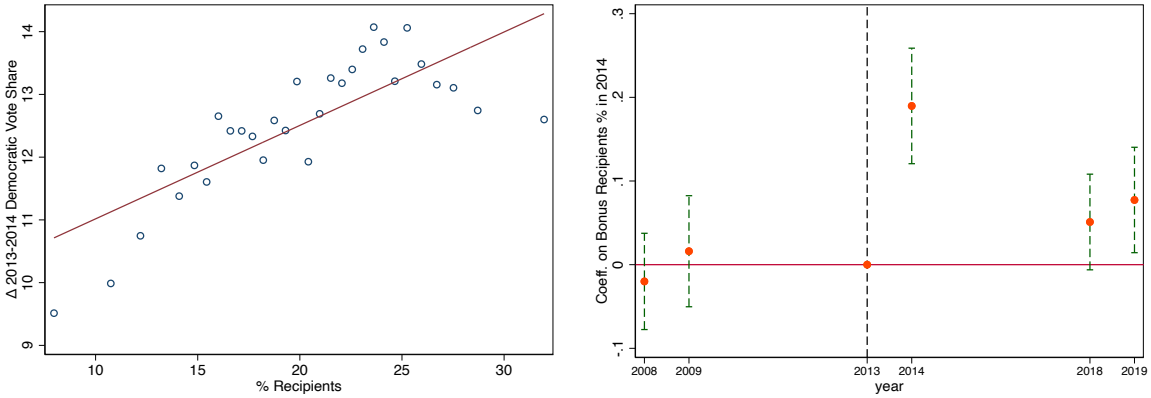
Notes: The figure plots the distribution of taxable income in 2013 of the universe of taxpayers (green) and of payroll employees only (orange). The red vertical lines indicate, respectively, the lower and upper threshold of eligibility for the 80 €Bonus.

Figure 2
Variation in Policy Intensity Across Municipalities



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 7697 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.

Table 1
The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Short Run

	Δ2013 − 2014 Share PD						Δ2009 − 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	No	No	No	Yes	No
Spatial HAC S.E.	No	No	Yes	Yes	No	No	No
RegXYear FE	No	No	No	Yes	No	No	No
Weighted	No	Yes	No	No	Yes	No	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. Columns (2) to (6) 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 Democratic Party Electoral Performance, Punishment Among Excluded Recipients

	$\Delta 2013 - 2014$ 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. *Excluded Below* is the share of individuals ineligible because earning less than 8,000 Euro. *Excluded Above* is the share of individuals ineligible because earning more than 26,000 Euro. 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.

Table 3
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***	1.047***	0.984***	1.031***
	[0.198]	[0.202]	[0.199]	[0.204]
% Recipients 8-12k (STD)		0.0458		0.0523
		[0.114]		[0.115]
% Recipients (STD) × % Recipients 8-12k (STD)		-0.0985**		-0.0821**
		[0.0405]		[0.0415]
% Recipients 20-24k (STD)			0.0586	0.0495
			[0.116]	[0.117]
% Recipients (STD) × % Recipients 20-24k (STD)			0.275***	0.256***
			[0.0852]	[0.0848]
Dep. Var Mean	12.51	12.51	12.51	12.51
Observations	7684	7684	7684	7684

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)-(2) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, in columns (3)-(4) it is the change in the vote share of the Democratic party between the 2013 and the 2018 elections, and in columns (5)-(6) 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.

Table 4
The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Mechanisms

	Δ2013 − 2014 Votes PD		Δ2013 − 2018 Votes PD		Δ2013 − 2019 Votes PD	
	(1)	(2)	(3)	(4)	(5)	(6)
% Recipients	0.189***	0.189***	0.0664**	0.0688**	0.0583*	0.0604*
	[0.0359]	[0.0361]	[0.0287]	[0.0289]	[0.0310]	[0.0310]
AmountPerCapita	-0.00358		0.000122		-0.00237	
	[0.00311]		[0.00238]		[0.00253]	
% Recipients, Give Back		0.0653***		-0.0489**		0.000998
		[0.0248]		[0.0194]		[0.0208]
AmountPerCapita, Give Back		-0.0172*		0.00577		-0.00819
		[0.00918]		[0.00741]		[0.00774]
Pre-T Mean	22.90	22.90	22.90	22.90	22.90	22.90
Observations	7697	7697	7696	7696	7630	7630
R2	0.0594	0.0604	0.112	0.113	0.112	0.112
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	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)-(2) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, in columns (3)-(4) it is the change in the vote share of the Democratic party between the 2013 and the 2018 elections, and in columns (5)-(6) 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. *Amount Per Capita* is the average Bonus amount per recipient, in a given municipality. *%AmountPerCapita,GiveBack* is the average amount that individuals had to give back. 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.

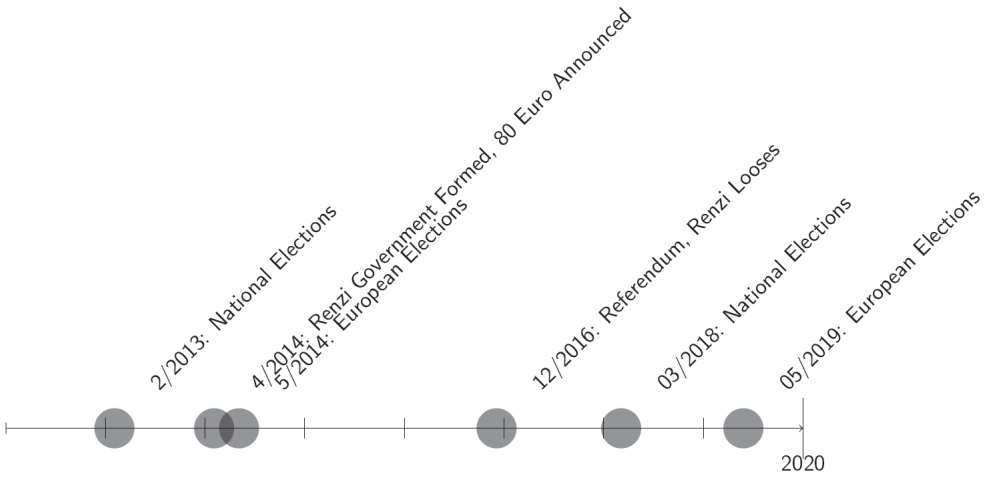
Table 5
The Effect of the 80 Euro Bonus on Democratic Electoral Performance, Survey Evidence

	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	-0.00165	-0.221***	-0.0433	-0.0385	0.0226	-0.0733
	[0.0500]	[0.0527]	[0.0784]	[0.0740]	[0.0718]	[0.0698]	[0.0498]
Eligible Income	-0.0491	0.0441	-0.0915	-0.00966	0.0194	0.0347	-0.0440
	[0.0312]	[0.0424]	[0.0682]	[0.0565]	[0.0592]	[0.0487]	[0.0311]
80Euro Recipient	0.110**	-0.0337	0.166**	-0.0270	0.0545	-0.0952	0.101*
	[0.0548]	[0.0571]	[0.0828]	[0.0756]	[0.0768]	[0.0719]	[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 election. Columns (3) and (4) look at shifts in the evaluation of the Democratic Party leader, Matteo Renzi, or on the evaluation of the incumbent government activity, in the pre vs. post election survey of 2014. In both cases, the responsent is asked to give an evaluation in a scale from 1 to 10. In columns (5) to (8), I look at the assessment of the economic situation. Columns (5) and (6) look at the assessment of the national economic situation, both in the past and in the future year, while columns (7) and (8) look at the own economic situation. Finally, column (9), repeats the same specification of column (1), but including also two dummies, $EconomicAssessmentWorsened$ and $EconomicAssessmentImproved$, capturing the change in views on the national economy.

Appendix A: Additional Tables and Figures

Figure A1
Timing of the relevant events



Timing of the relevant electoral events.

Figure A2
Evolution over Time of Outcomes in Municipalities with High vs. Low-Treatment Intensity

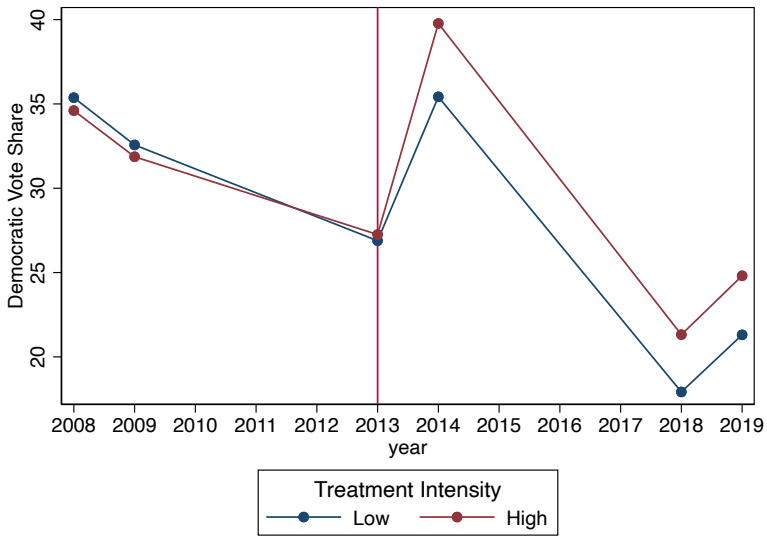


Figure A3
Local identification - thought experiment

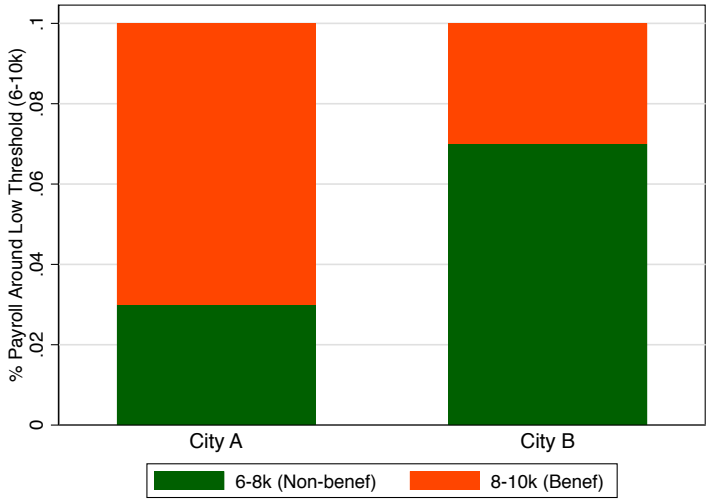
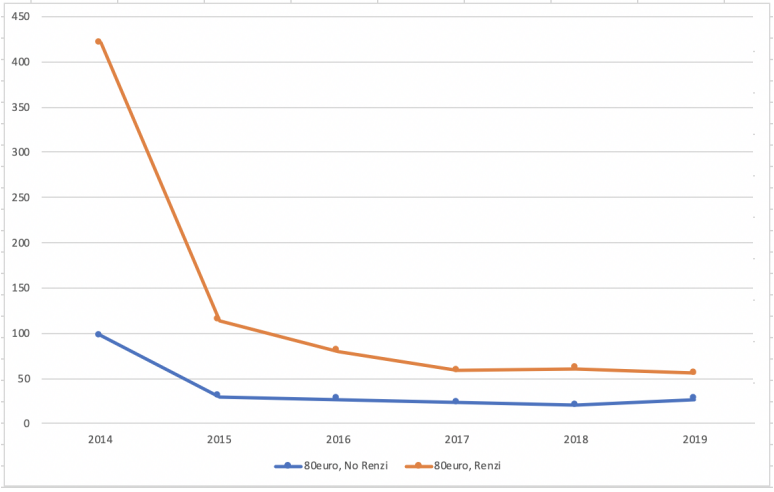


Figure A4
News containing "80 Euro" and "Renzi"



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 word "80 Euro", while the orange line represents the volume of news titles including both the term "80 Euro" and the term "Renzi".

Figure A5
Distribution of average Bonus amount per capita

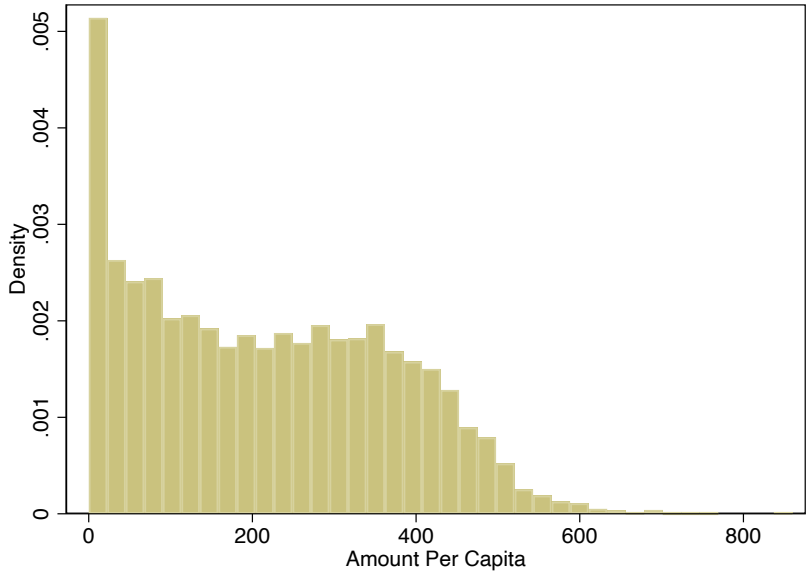


Figure A6
Dynamic Difference-in-differences estimates, Alternative Specification

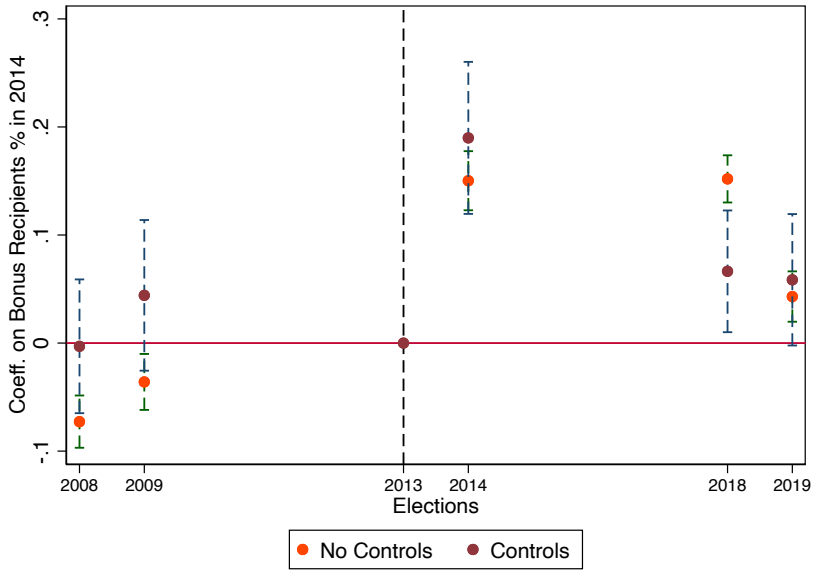
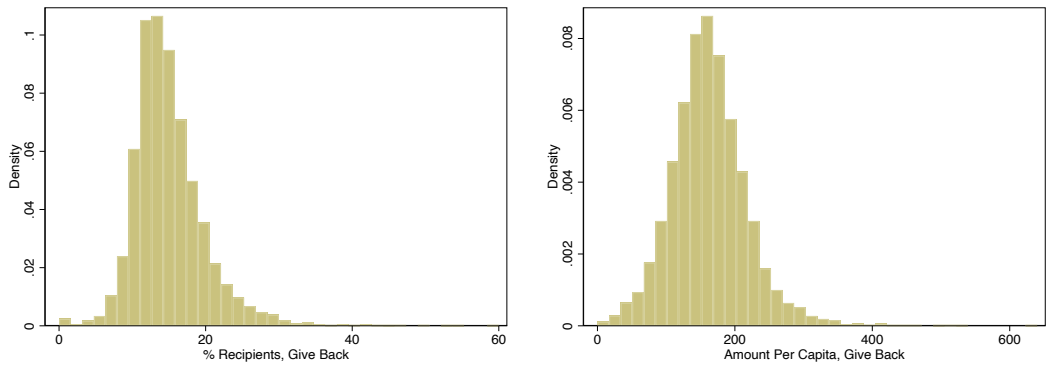
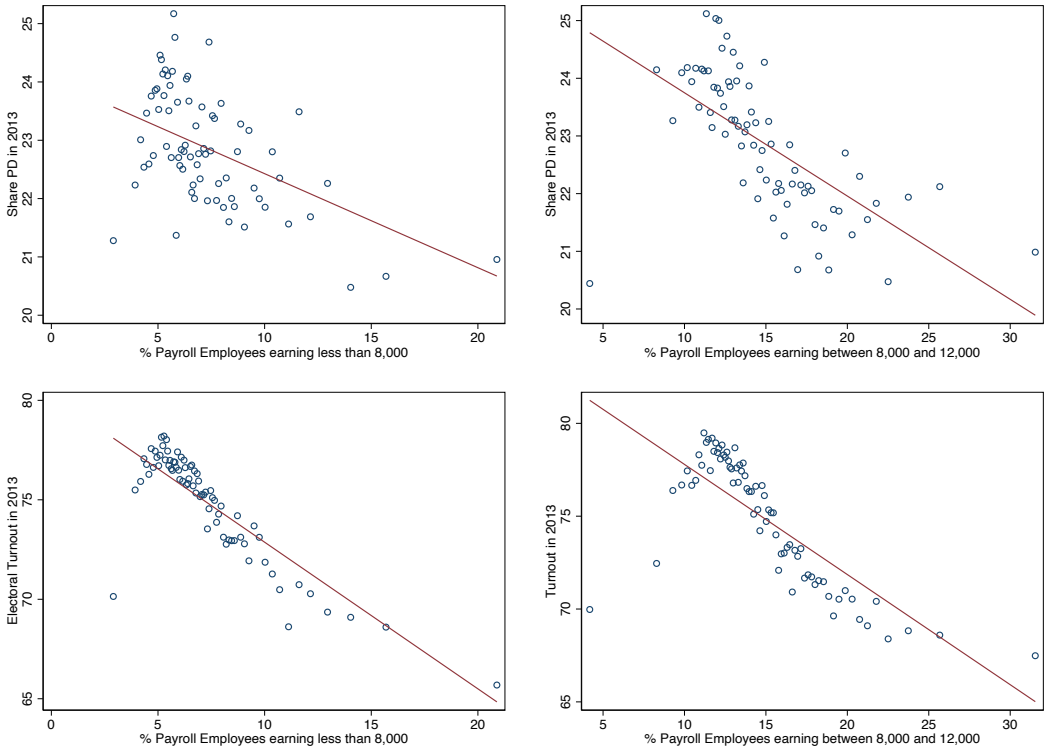


Figure A7
Distribution of the share of restitutions and average restitution amount per capita across municipalities



The histogram on the left represents the distribution of the average share of recipients who have to give back the bonus in the municipality, while the one on the right represents the distribution of the average amount that each recipient had to give back, for the subsample of municipalities in which at least one recipients had to give back some share of the bonus.

Figure A8
Understanding Limited Responses among Low-Income Individuals



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
2013	22.72	22.70	6.84	0.20	56.46	7,712
Share Right	16.73	15.92	6.60	0.00	71.85	7,712
Share Extreme Right	7.65	5.26	7.79	0.00	50.93	7,712
Turnout	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

Table A2
Determinants of Treatment Intensity

	% Recipients					
	(1)	(2)	(3)	(4)	(5)	(6)
% Eligible Income	0.251***	0.272***	0.148***	-0.0179	-0.0265	-0.0368*
	[0.00615]	[0.0111]	[0.0113]	[0.0202]	[0.0203]	[0.0203]
% Payroll	0.341***	0.336***	0.435***	0.156***	0.138***	0.125***
	[0.00678]	[0.00868]	[0.00888]	[0.0371]	[0.0372]	[0.0359]
% ≤ 0			-0.121	-0.0189	-0.0361	-0.104
			[0.127]	[0.127]	[0.125]	[0.126]
% 0-10,000			-0.207***	-0.0933***	-0.0926***	-0.106***
			[0.0120]	[0.0157]	[0.0159]	[0.0164]
% 26-55k			-0.269***	-0.171***	-0.166***	-0.191***
			[0.0206]	[0.0207]	[0.0208]	[0.0222]
% 55-75k			-0.188*	-0.0386	-0.0270	-0.0653
			[0.103]	[0.100]	[0.0977]	[0.101]
% 75-120k			-0.320***	-0.159	-0.156*	-0.168*
			[0.0967]	[0.0979]	[0.0946]	[0.0973]
% Above 120k			-0.283*	-0.140	-0.145	-0.188
			[0.168]	[0.164]	[0.157]	[0.152]
Predicted % Recipients				0.629***	0.644***	0.642***
				[0.0785]	[0.0789]	[0.0769]
% Recipients, Give Back					0.218***	0.229***
					[0.0281]	[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: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$. 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 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
	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, Local Difference-in-Differences Design

	Dependent Variable: Δ2013 − 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. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. 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 A5
The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, IV Estimates

	$\Delta 2013-2014$ Share PD				$\Delta 2009-2014$ Share PD
	(1)	(2)	(3)	(4)	(5)
% Recipients	0.149***	0.195***	0.195***	0.128***	0.0982***
	[0.0156]	[0.0195]	[0.0504]	[0.0219]	[0.0228]
Pre-T Mean	22.90	22.90	22.90	22.90	22.90
Observations	7712	7712	7712	7712	7697
R2	0.0190	0.0392	0.0392	0.833	0.0723
First Stage Coef	1.240	1.071	1.071	1.037	1.071
First Stage F-stat	19432.4	10167.4	1214.8	8644.9	10131.3
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Cluster Prov. S.E.	No	No	No	Yes	No
RegXYear FE	No	No	Yes	No	No
Weighted	No	No	No	No	No

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. 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 Performance and Turnout, Levels Specification

	(1) $\Delta 2013-2014$ Votes	(2) $\Delta 2013-2014$ Votes	(3) $\Delta 2013-2014$ Votes	(4) $\Delta 2009-2014$ Votes	(5) $\Delta 2013-2014$ Votes	(6) $\Delta 2009-2014$ Votes
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 A7
The Effect of the 80 Euro Bonus on Turnout and Votes to Other Parties

	Δ2013 − 2014 Share 5Stars	Δ2013 − 2014 Share Right	Δ2013 − 2014 Share Extreme Right	Δ2013 − 2014 Turnout	Δ2013 − 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. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. 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 the 2016 Referendum

	Δ2013 − 2016 Share PD			
	(1)	(2)	(3)	(4)
Bonus Recipients (Share)	0.441***	0.150***	0.136***	0.130***
	[0.0231]	[0.0477]	[0.0493]	[0.0498]
% Recipients, Give Back			-0.0375	-0.0466*
			[0.0257]	[0.0258]
Amount Per Capita, Give Back				-0.00860
				[0.257]
Pre-T Mean	22.90	22.90	22.90	22.90
Observations	7709	7709	7085	7057
R2	0.0690	0.342	0.342	0.344
Year FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes

Notes: Standard errors clustered at the municipality level in brackets. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$. 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 A9
The Effect of the 80 Euro Bonus on Democratic Party Electoral Performance, Mechanisms, Alternative Definition of Restitutions

	Δ2013 − 2014 Votes PD		Δ2013 − 2018 Votes PD		Δ2013 − 2019 Votes PD	
	(1)	(2)	(3)	(4)	(5)	(6)
% Recipients	0.189***	0.176***	0.0664**	0.0736**	0.0583*	0.0570*
	[0.0359]	[0.0365]	[0.0287]	[0.0293]	[0.0310]	[0.0313]
Amount Per Capita	-0.00358		0.000122		-0.00237	
	[0.00311]		[0.00238]		[0.00253]	
% Recipients, Give Back		0.187***		-0.0804*		0.0391
		[0.0608]		[0.0464]		[0.0484]
Amount Per Capita, Give Back		-0.0172*		0.00331		-0.00967
		[0.00914]		[0.00737]		[0.00768]
Pre-T Mean	22.90	22.90	22.90	22.90	22.90	22.90
Observations	7697	7697	7696	7696	7630	7630
R2	0.0594	0.0607	0.112	0.113	0.112	0.112
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	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)-(2) is the change in the vote share of the Democratic party between the 2013 and 2014 elections, in columns (3)-(4) it is the change in the vote share of the Democratic party between the 2013 and the 2018 elections, and in columns (5)-(6) 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 (VEP), Give Back* is the number of Bonus recipients that had to return the Bonus in 2015 because they turned out to be ineligible, divided by the voting eligible population. *Amount Per Capita* is the average Bonus amount per recipient, in a given municipality. *%AmountPerCapita, GiveBack* is the average amount that individuals had to give back. 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.

Table A10
Voting and Participation Patterns by Income and Employment Status

	Income Group			**Employment Status**					
	Low Income mean	Middle Income mean	High Income mean	Payroll mean	Self-Employed mean	Unemployed mean	Housewife mean	Student mean	Pensioner 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

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 I 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?