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Chapter 1

Introduction

When people outside of academia ask me to define what political economy is about, for a matter of simplicity I often present it as being the field that studies the many connections between politics and economics, and in particular how on one side politicians can take economically relevant decisions driven by their desire of being re-elected, and on the other side how the economy, and economic shocks, can affect voters' political and electoral decisions.

The two papers that constitute this thesis relate to both of these dimensions of political economy, as they tackle empirically questions concerning both the strategic behaviour by politicians induced by re-election incentives, and the political repercussions of a large trade shock. In particular, both of the works concentrate on the Italian political landscape, whose many peculiarities and excesses come extremely useful to researchers working on these topics.

The work presented in Chapter 2 extends the literature on political budget cycle to a previously unexplored dimension, that of electoral promises. In particular, I exploit the fact that provisional budgets, which include the political agenda of the incumbent administration relatively to expenditures and revenues, are released shortly before elections due to chronical governmental delays, and that they are well publicized by local media. This allows to obtain high-frequency, objective measures of electoral promises, and results show that mayors engage in over-promising of investments in the election year by 20% more than in other years. A number of robustness checks confirm that this is due to strategic behaviour, and that this behaviour is driven by re-electable mayors who are not under the control of national parties, and already engage in standard political budget cycle behaviour, opportunistically raising investments in the year preceding elections.

Chapter 3 instead builds on a recent surge in interest, both in society and in related literature, on the link between globalization, trade shocks and a radicalizing effect on politics. I use the Chinese surge in productivity over the last 20 years as an exogenous trade shock, and then follow recent works to map this shock to local labour markets based on their historical patterns of industry specialization, therefore inducing heterogeneity in exposure to the net import shock at the local level. Results show that this has not caused a fall in support for mainstream parties, nor has it caused increased vote shares for more radical or populist parties, even when using indexes that account for shifts in the whole ideological distribution of the electorate. This goes

against empirical findings of the last years, and opens new questions on the role of globalization in causing the recent populist backlash.

Chapter 2

Fulfillment of Electoral Promises and the Political Budget Cycle: Evidence from Italian Municipalities

Abstract

This work exploits the timing and public circulation of budget forecasts in Italian municipalities in order to produce empirical evidence of strategic behaviour by mayors on their promised investments. In particular, using a province fixed-effects model, mayors are shown to over-promise investments in election years by 20% more than over the rest of the term. The results are driven by mayors who are not term-limited, and also by those unaffiliated to national parties and with greater levels of education, in line with previous evidence produced over political budget cycles. Moreover, mayors who over-promise more in election years are those who engage in higher pre-electoral increases in investments, thus creating a bridge between traditional political budget cycle and this strategic behaviour. Lastly, I present some suggestive evidence of a positive effect of the strategic behaviour on the probability of re-running and of being re-elected.

Keywords: Political Budget Cycle, Budget Forecasts, Public Finance, Investment Promises, Italy

2.1 Introduction

While they tend to capture the public attention and the media's headlines, political promises tend to carry relatively little interest and credibility within the political economy literature. In fact, most of the debate concerning such promises is confined on the theoretical grounds of prospective and retrospective voting and the relative weight that each should carry, while empirical research on the matter has mostly been lacking. A clear challenge in trying to tackle empirically the question of whether there is strategic behaviour relatively to political promises is that it is hard to obtain data that is sufficiently high-frequency, since such promises are generally identified as those that are done at the end of the term, which does not allow to observe any variation occurring within the term. The goal of this paper is to tackle empirically exactly the issue of electoral promises and to do so in relation with political cycles, so as to allow any strategic behaviour on promises to emerge clearly.

This work aims at overcoming this challenge by using data on the balance sheets and budget forecasts of Italian municipalities over the period between 2003 and 2012, exploiting some characteristics of budget forecasts relatively to their timing and diffusion that allow to interpret them as being spending promises. In this way, it's possible to establish whether the degree to which promises are fulfilled varies over the electoral term and across different kinds of politicians. Results suggest the existence of such political cycle, as the difference between what is promised and what is actually realized increases vastly in the election year with respect to the two previous years, implying that politicians behave strategically across the cycle relatively to the promises they make and the degree to which they fulfill them.

More in detail, the results are driven by mayors with re-election incentives who are not term-limited, by those not affiliated to national parties, and those with higher levels of education. A link is also provided with the more prolific literature on political budget cycles on realized expenditures, since the cycle on promise fulfillment appears to be driven by those same mayors behaving strategically relatively to pre-electoral investments. Lastly, there is some suggestive evidence in favour of a positive effect of strategic behaviour on promises on the probability of being re-elected.

The paper is structured as follows. Section 3.2 discusses some literature related to the current work, while Section 3.4 presents background information on Italian municipalities and the process of budget forecasts. Section 3.5 introduces the data. Section 3.3 presents the empirical strategy. Section 3.6 reports the main results. Section 2.7 provides results on the robustness of the empirical evidence. Section 2.8 then discusses the mechanisms through which the strategic manipulation occurs. Section 2.9 concludes. Finally, the appendix contains additional tables and graphs discussed in the main text.

2.2 Related Literature

By focusing on the existence and characteristics of an electoral cycle in promise fulfillment, the current work is related to different literatures. In the first place, it is clearly connected to research concerning the role of electoral promises and the relevance of prospective voting,

that is, to use the definition given by Persson and Tabellini (2000), to pre-election models. In this framework, electoral promises are considered to be binding, explaining why voters would trust them wholeheartedly, and shifting all electoral competition to a competition on promises. Seminal works in this field include the median voter models, in line with Downs (1957), and probabilistic voting models following Lindbeck and Weibull (1987, 1993). While in some cases promises tend indeed to be delivered for the most part[1], though, in practice there is no legal obligation to fulfill them completely once in power. More recent models on prospective voting, such as Harrington (1993), Aragones et al. (2005) and Panova (2017), have tried to go beyond this assumption while still introducing some elements of prospective voting, by treating promises as non-costly signals over the policies that are then implemented if elected. In these models, even though promises are cheap talk, politicians deliver them in equilibrium since otherwise they would be voted out in the following election. Therefore, promises hold a key role in that they act as a coordinating mechanism for voters through which they can then sanction politicians, resulting in non-babbling equilibria. Nonetheless, these equilibria require repeated elections in order for voters' sanctioning to be effective, and therefore the above results are strongly weakened once considering setups with term limits, both at the national level, such as presidential elections in USA, Russia or Mexico, and at the local level, such as Italian municipalities.

Another peculiarity of existing models is that, while differing over whether they consider promises to be binding or not, they agree in that promises are cheap talk, therefore not affecting directly neither the voters nor the politicians' payoffs. Recent experimental evidence by Corazzini et al. (2016) suggests that, as already shown for common citizens, lying is not costless for politicians. Such costs, which are not pecuniary but rather psychological, affect the nature of electoral promises, making them costly, and therefore hinting that in equilibrium they don't necessarily have to carry either full or no information over the future policies, as instead suggested by the previous works in the literature mentioned before.

By providing evidence of a cycle in promise fulfillment, this work tackles the issue of the relevance of prospective voting from an empirical point of view. In particular, existing empirical research on the topic, which is concentrated in the field of political science, looks at whether governments do fulfill their promises once in power[2], or, from a voter's perspective, whether the prospective or retrospective component of voting dominates.[3]

Regardless of which of the two is more influential, prospective and retrospective voting are not mutually exclusive, as in practice voters make their choices based on both aspects.[4] This paper's contribution is to focus on the less well-studied politician's side, and in particular to understand not only whether the promises are fulfilled, but whether the degree to which they are changes over time and across the electoral cycle, which would be indicative of strategic behaviour

[1]Thomson et al. (2014) study the fulfillment of electoral pledges of 47 governments in 11 countries, concluding that parties mostly fulfill their promises.

[2]See Thomson et al. (2014)

[3]MacKuen et al. (1992), Hsieh et al. (1998) and Ellinder et al. (2015) provide evidence mostly in favour of prospective voting, which in the latter paper is modelled together with retrospective voting in a probabilistic voting framework. Lanoue (1994) and Norpoth (1996) find instead the retrospective component of voting to be dominant. Lastly, Lewis-Beck (1988) concludes that both components of voting are equally important.

[4]Ellinder et al. (2015) formalize this idea in a probabilistic voting framework, where voters' expectations are affected both by electoral promises and by past performance.

relatively to promises. One of the challenges in doing so is to obtain an objective measure of electoral promises and their fulfillment that is sufficiently high-frequency as to observe any cyclical pattern, and therefore any strategic behaviour on the politician's behalf.[5] In this sense, this work exploits the timing and characteristics of the Italian local budget forecasts in order to identify them as being promises, and therefore, by comparing them to the balance sheets, to observe any cycle in the degree of promise fulfillment.

This paper is also linked to models of retrospective voting, which are instead based on the idea that voters choose whom to vote according to the policies implemented previously while in power. These models include sanctioning mechanisms (see Barro 1973, and Ferejohn 1986), while most of the recent literature has focused on political budget cycles (PBC hereafter), that is, on increases in certain expenditures before elections, building on Nordhaus (1975) and, with a selection mechanism in a rational voters setup, on Rogoff and Sibert (1988), Rogoff (1990) and Shi and Svensson (2006).[6]

The empirical literature has produced sizeable evidence on strategic pre-electoral increases at the sub-national level, helped in part by the refinement of econometric techniques on dynamic panel data model, such as the difference-GMM estimator developed by Arellano and Bond (1991), which allows to consider more uniform institutional backgrounds, therefore reducing fears about endogeneity. Drazen and Eslava (2010) find evidence of a PBC in capital expenditures in the year preceding elections in Colombia; Veiga and Veiga (2007) also observe an increase in the most visible expenditure items in the electoral year for Portuguese municipalities; Petterson-Lidbom (2003) finds evidence of PBC in Swedish municipalities, while Brender et al. (2016) obtain similar results over electoral year rises in expenditure for municipalities in Israel.

There is empirical evidence of PBC also for Italian municipalities, which are the focus of the present work; in particular, Cioffi, Messina and Tomassino (2012) find evidence of PBC driven by mayors not belonging to national parties, who are more inclined to raise capital expenditures in the period before elections. Repetto (2017) uses a difference-in-difference approach to show the existence of a PBC for capital expenditures and, in particular, a decrease in the size of strategic maneuvering following a reform which made the municipalities' balance sheet of the pre-electoral year available before elections, therefore highlighting the importance of balance sheets and voters' information in determining the size of the political budget cycle. Lastly, Alesina and Paradisi (2015) use a Regression-Discontinuity Design exploiting a recent tax reform finding evidence of strategic behaviour by mayors, who set up significantly lower tax rates when running for re-election. Overall, thus, evidence seems to suggest that also in Italy at the local level there is strategic behaviour on capital expenditures aimed at influencing the retrospective component of voting.

[5] As an example, Ellinder et al. (2015), one of the few other works on the subject, focus on a specific case study in Sweden concerning two promises by the governing party in successive election, exploiting survey data on voters in a difference-in-difference framework to capture their causal response.

[6] Asymmetry of information over the politician's competence between him and the voters drives pre-electoral increases in the most visible categories of expenditure, which act as signals of the politician's level of competence, leading to an equilibrium in which competent incumbents raise pre-electoral expenditures and get re-elected. Drazen and Eslava (2008) propose a model which is similar in spirit, but where politicians only change the composition of spending rather than its overall level.

The current work is related to the above literature on retrospective voting on a few dimensions. First of all, it takes inspiration from its focus on the cyclical variations across the electoral term. Moreover, through the results showing that the mayors most involved in the electoral increase in unfulfilled promises are those who also perform pre-electoral increases in capital expenditures, it also suggests that strategic manipulation of the prospective and retrospective component of voting is strictly linked, and that there is an additional moment before elections in which mayors behave strategically on the policy variables available to them. Lastly, it is connected to the works [7] showing that civic, local parties are more prone to strategic behaviour on expenditure relatively to national partisan, presumably because of lower control on the politicians and lower reputation concerns.

Finally, the existing literature on budget forecasting and bias in forecasting errors is also related to this work, even though it mostly goes in a different direction. In fact, the bulk of this literature focuses on the manipulation of revenues' forecasts as a way to avoid having to forecast budget deficits in electoral times[8], whereas this work focuses on spending forecasts and on the varying degree to which they are fulfilled as elections draw closer. In particular, in the current work spending forecasts are interpreted as electoral promises due to the fact that they are public documents and widely reported by the local media, and therefore can have an impact on the electorate.

2.3 The Italian Framework

2.3.1 Background Information

In the Italian institutional framework there are three levels of sub-national government: regions, provinces and municipalities. Regions are mostly responsible for the provision of health services, while provinces have some functions related to road maintenance and the natural environment, but they are about to be cancelled as a governing body following recent reforms. Municipalities are the smallest administrative unit in Italy: there were 8,109 municipalities in 2010, but the number tends to vary slightly over the years due to mergers between some of the smaller towns. Municipalities are responsible for a number of services, ranging from waste disposal to urban roads' maintenance, primary schooling, local transports and social services (such as assistance to elderly people). Their total expenditures account for 10% of all the government's primary

[7] Akhmedov and Zhuravskaya (2004) for Russian regions and Cioffi et al. (2012) for Italian municipalities.

[8] Brogan (2012) finds a significant impact of the electoral cycle and of political institutions on budget forecasting errors and, in particular, lead to under forecasting revenues. Bischoff and Goutout (2006) study West German states between 1992 and 2002, finding that political factors (such as popularity of the incumbent governing party) lead to over forecasting revenues, but don't find any specific evidence on the role of elections. Lago-Penas and Lago-Penas (2008) perform a similar analysis for Spanish municipalities, finding that in election years revenues are under forecasted and that deficits increase even though forecasted deficits do not. Couture and Imbeau (2009) find instead that revenues are over forecasted in election years in Canadian provinces, but that the forecasting error is lower in provinces with anti-deficit laws, even though endogeneity biases pose a threat to the latter results. Goeminne et al. (2008) find that in Flemish municipalities there is no effect of elections on budget forecasting errors, while political fragmentation has, somehow counterintuitively, a negative effect on forecasting errors. Lastly, Bruck and Stephan (2006) concentrate on countries belonging to the Eurozone and conclude that national governments systematically under forecast their budget deficits prior to elections in order to avoid being fined under the Stability and Growth Pact in the immediately following months.

expenditures and 4% of GDP, whereas municipalities' capital expenditures account for 30% of capital expenditures at the national level. Municipalities are financed with both transfers and own revenues. The former can come from the national or regional government or from the European Union, whereas own revenues include taxes, fees (from the provision of public services or building permits), penalties, capital transfers, the disposal of public assets, and loans.

Municipalities are headed by a mayor, who in turn appoints an executive body (*Giunta Comunale*). The legislative body is the municipal council (*Consiglio Comunale*), whose members are elected directly by voters through preferences. Mayors are elected directly since 1993, when a law was passed with the objective of increasing their accountability and strengthening their power [9]. In the same year term limits were imposed on mayors, who cannot run for more than two consecutive mandates, unless an early termination occurs before half the term has expired. Since 1999, mayoral mandates have been extended from 4 to 5 years and can terminate early in case of resignation of the mayor or of more than half of the municipal councillors, or if the balance sheet is not approved by the set date.[10] A plurality system is in place for mayoral elections, with a majority premium of 60% of seats awarded to the party or coalition endorsing the winning candidate, and a run-off if none of the candidates obtains an absolute majority in the first round.[11] Table 2.1 shows how the timing of local elections differs across all municipalities, typically occurring in "waves", so that each year a different subset of municipalities always holds elections. For the purposes of this analysis, this reduces the possibility of year effects influencing our results. It must also be noted that, both as a general rule and in the time period considered in our study, elections are always held in the first half of the electoral year, between March and June.

Since 1999, municipalities have also been subject to the Domestic Stability Pact (DSP from hereafter), which was introduced after the Stability and Growth Pact was unveiled across Euro-zone countries. The underlying principle of the DSP is to limit expenditures of both municipalities and regions by imposing a balanced budget rule, but the details of the Pact have changed on a year-to-year basis, with certain items being excluded from the computation, while in other years there were limitations on total expenditures regardless of the revenues. Lastly, from 2001 onwards municipalities with population below 5,000 inhabitants were excluded from the rules of the DSP.

2.3.2 Budget Forecasts and Balance Sheets

Municipalities are required to prepare budget forecasts, that is, documents establishing both the amount and the distribution of expenditures for the following years, and how they will be financed. Therefore, budget forecasts are planning documents that set out on paper what the political agenda of the local administration is. Balance sheets instead simply report the amounts which have actually been spent and received in the previous solar year.

[9] Law 81/1993
[10] Law 120/1999
[11] Slightly different rules apply for municipalities with a population below 15,000, with a simple plurality system with no run-off, and a majority premium of 66% of the council seats.

There are certain requirements imposed by the Italian law on both provisional budgets and balance sheets. Balance sheets need to be approved by a set date in the months which follow the year over which they report.[12] Failure to do so has severe consequences, since it leads to an early termination of the mayoral term and to new elections. Budget forecasts instead have to be approved by the municipal council by the 31st of December: failing to do so triggers a 2-months period during which expenditures are limited to one twelfth of the previous year's amount until the budget forecast is eventually approved. The law also establishes that any expenditure in excess of the forecasted amount has to be explicitly approved by the municipal council.

Whereas the formal deadline for approving budget forecasts of year T is the 31st of December of year T-1, in practice it is always postponed to later months,[13] mostly because of last-minute changes to the national budget law potentially affecting municipalities. This is relevant since it means that mayors can officially announce their planned expenditures for a given year well into that same year. In particular, budget forecasts on the electoral year can potentially be publicly released well into the year itself[14], becoming effectively electoral tools for incumbent mayors. Similarly, there are occasions in which the delays are such that budget forecasts of electoral years could potentially be approved only after elections are held. This is taken into account with specific robustness checks in section 2.7 when analysing electoral promises.

One of the key identifying elements of the current work is the interpretation of budget forecasts as electoral promises. This is due mainly to the public relevance of budget forecasts, which define the political agenda of the incumbent administration and are widely reported by the media. In order to provide a better intuition, I have collected all the articles regarding budget forecasts of municipalities published in the 19 newspapers of one of the main Italian editors (Gruppo L'Espresso). The findings are shown in figure 2.1.[15] Each year around 2,000 articles concerning budget forecasts are published. Moreover, the distribution of articles across months confirms that budgets are approved, and more importantly talked about, in specific periods of the year. The majority of the articles are published in December, which is the theoretical legal deadline. Another spike occurs in March, which in almost all years is the initial postponed deadline established by the government. The number of articles related to budget forecasts increases from an average monthly value of 195 to around 500 articles in the month of December, when the first deadline is set. The month in which the delayed deadline is set also shows an increase in all years in the number of articles on budget forecasts with respect to the previous month. This effect only becomes less pronounced after 2009, when the final budget deadlines were set between September and November. Therefore evidence seems to suggest that budget forecasts are documents publicy reported by the media, and that these reports increase

[12] Before 2008 it was the 30th of June; afterwards, following Law 189/2008, balance sheets have had to be approved by the 30th of April. (Repetto, 2017)

[13] Between 2003 and 2012, which is the time period we study, the final deadline was moved to May of the following year for 4 times, while in one occasion it was moved to April, June, September, October and November; see table 2.1.

[14] Unfortunately the exact dates in which budget forecasts and balance sheets are approved by the municipal council are not publicly released, since it is not required by the law.

[15] The blue line marks the month of December, which is the initial deadline for approving budget forecasts; the orange line marks the month in which the first postponed deadline is established; the black line marks the month in which elections are held.

around both the original and the delayed deadlines, thus meaning that in the majority of years citizens are shown the provisional budget shortly before the elections.

2.4 Data

2.4.1 Sample

The original dataset contains information on both budget forecasts and balance sheets of 8,104 municipalities for a 10-year time span, from 2003 to 2012. Both budget forecasts and balance sheets for all municipalities are gathered from the Ministry of Internal Affairs' website, with specific details about the composition of both revenues and expenditures.[16]

As mentioned in section 3.4, before elections mayors are expected to manipulate those variables which are more visible to voters. In Italy, current spending consists mostly of salaries rather than transfers, so that while it is a relevant component of expenditure, it is hard to manipulate and divert towards specific groups of individuals. Similarly, mayors tend to have reduced control over revenues, with limited scope for taxing powers. On the other side, capital expenditures are both visible and targetable, as they include investments in roads, schools and parks. Therefore, the main dependent variables in the current work are per capita capital and total spending. Current spending is also included in some regressions as a placebo test, in order to confirm that it is not subject to strategic behaviour across the cycle.

The work first looks at realized expenditures contained in the final balance sheets, in order to confirm the presence of a standard PBC in Italian municipalities. Then, the analysis shifts from the existing literature by focusing on the promised expenditures contained in the budget forecasts.

In order to capture potential strategic manipulation by incumbent mayors, the focus is on the unfulfilled promises, that is, the difference between forecasted and realized expenditures. In particular, unfulfilled promises are defined as the percentage deviation with respect to the realized value observed in the final balance sheet:

$$UnfulfilledPromise = 100 * \frac{(ForecastedValue - RealizedValue)}{RealizedValue}$$

where both forecasted and realized values are expressed per capita. This definition aims at capturing the size of the forecasting error, be it over-forecasting, in which case the value is positive, or under-forecasting. Such errors capture the degree to which politicians keep their word and "walk the walk", and can thus be interpreted as measures of promise fulfillment, and in particular, as elections draw nearer, of unfulfilled electoral promises. Around 70% of forecasting errors are due to over-forecasting by the running administration; this is in line with a cautious behaviour by mayors when redacting the budget forecast, but may also be driven by strategic manipulation of promises in order to influence elections. It is possible that other non-electoral factors, such as education and partisanship, contribute to the changes in forecasting errors, leading to additional robustness checks being run in section 2.7 in order to confirm the above

[16] While data on balance sheets is available from 1999, budget forecasts are only available from 2003 onwards.

interpretation of forecasting errors.

In order to obtain the final sample, I first drop all 5,272 observations belonging to terms which have lasted less than the 5 years established by law, since they could be exhibit systematic differences with respect to the rest.[17] Because of differences in the administrative system and in some of the local budgeting laws, I also drop all municipalities belonging to Special Statute Regions, which amount to 10,372 observations. I then drop a number of observations defined as outliers based on the dependent variables presented above. First, I drop 3,352 observations with forecasted or realized capital expenditures per capita exceeding 15 times the respective standard deviation.[18] Then, I further drop 627 observations falling in the 99th percentile of unfulfilled promises for capital expenditures, which includes observations with unfulfilled promises equal to or above 6,063 %. This leads to a final sample including 62,097 observations.

2.4.2 Summary Statistics

Figures 2.2 and 2.3 give an overview on the realized expenditures of Italian municipalities, taken from their final balance sheets. Over the 10-year period taken into consideration, total expenditures grow from 71 to 78 billion euros, about 3 percent of Italian GDP. Most expenditures are current ones, which increase from an initial amount of 35 billion euros in 2003 to 51 billions in 2012. Their overall share of total expenditures has also grown at the expense of capital expenditures, which instead have halved from 25 to 12 billion euros. Both loan repayments and services have significantly lower shares than current expenditures.

Figure 2.4 gives a similar overview but related to forecasted expenditures. The difference with respect to the previous figure is sharp: total forecasted expenditures are, on average, almost double the realized expenditures, and in particular capital expenditures hold a much more relevant role. Figure 2.5 shows that, until 2010, capital expenditures account for about half of total expenditures, while the current expenditures' share is significantly lower than in the final balance sheets.

The difference between forecasted and realized values is also visible in Table 2.2, which reports the mean values of both forecasted and realized expenditures per capita. The average forecasted capital expenditures are more than twice as large as the realized ones, whereas the difference between forecasted and realized average current expenditures is minimal. Table 2.3 instead shows the distribution of capital expenditures across the years of the mayoral term, and in particular the increase in the pre-electoral year, which is consistent with standard PBC. These findings are further confirmed by Table 2.4, which focuses on the unfulfilled promises divided by category of expenditure. The mean unfulfilled promise on capital expenditures is 40 times as large as current expenditure's, and also larger than that on loan repayments or services. This is partly due to the predictable nature of current expenditures, which make forecasting bound

[17] Since this sample could be self-selected, as a robustness check I also ran all regressions including these terms, and results do not change signiicantly. In section 2.7 the baseline regression on unfulfilled promises with all such terms is included.

[18] Dropping observations which exceed expenditures by 5 or 10 times the respective standard deviation does not affect the results; this specification was preferred since it was the most conservative, and affected the dataset the least.

to be more accurate; uncertainty regarding investments is notoriously higher, as they can be cancelled altogether, or costs can be larger than initially forecasted; at the same time, though, it is also indicative of possible strategic behaviour by mayors. This is further confirmed by Table 2.5, showing the distribution of unfulfilled promises across the term and the vast increase occurring in the election year.

Threfore, in conclusion, the possibility of the cost of investments increasing beyond initial expectations may indeed explain the fact that forecasts concerning capital expenditures tend to be more inaccurate, as mayors may want to account for greater uncertainty then when preparing the budget forecast. Moreover, as mentioned earlier, municipal councils are required to approve distinctly any expenditure which goes above the forecasted level, so that municipalities may be incentivised to "play it safe" and intentionally produce higher forecasts. Nonetheless, while these provide possible explanations to the consistent difference *in levels* between forecasted and realized capital expenditures, they do not account for variations in unfulfilled promises over time, and in particular over the electoral cycle. If forecasts were indeed simply inaccurate, even if accounting for differences between mayors, one would expect them to remain so across the whole term. The following section presents the empirical strategy, which builds on this idea.

2.5 Empirical Strategy

The main goal of the current work is to establish whether mayors engage in strategic behaviour on their electoral promises. In particular, if they were caused by factors other than strategic manipulation, unfulfilled promises should remain constant across the mayoral term. In order to test this, I employ a standard province-fixed effects model:

$$Y_{it} = X_{it}B + \sum_{j=1}^{T} B_j + \omega_p t + \epsilon_{it}$$

where subscript i stands for municipalities, p for provinces, j for years from the next election (where the baseline year is the pre-electoral one) and t for years. Y_{it} is the fiscal variable being analyzed, such as capital or total expenditures, X_{it} is the set of controls, ω_t represents the province-by-year fixed effects, while ϵ_{it} is the residual. In order to capture changes occurring for each year of the electoral cycle, I introduce four dummies δ rather than a single election-year dummy. Each dummy is equal to 1 according to the number of years left until the next elections, using the pre-electoral year as the reference one. This allows to observe more readily differences not only between the pre-election and election years, but also between the pre-electoral year and the year before that, and so to have a better understanding of the degree to which promises are fulfilled throughout the whole term.

This specification is the preferred one since, within each province, it is possible to observe enough variation both within and across electoral terms. This would not be possible, instead, at the municipal level, since the sample considers 10 years and therefore most municipalities only present two full terms, limiting any analysis, particularly those concerning term-limited mayors. Nonetheless, the baseline regression with municipality fixed effects is shown as a robustness

check, and results still hold.

The controls that are used in this specification include: a municipality's population, the square of the population, the number of firms and employees in 2001 (to account for its lagged level of economic activity), dummies for whether the municipality is a province capital or whether it is on the sea or montaneous, the altimetric zone, the share of population under 35 and that over 65 (to account for greater expenditures due to the demographic structure), the share of inhabitants with an university degree, the share of unemployed, and lastly the average taxable income of the municipality. [19]

Before looking at the cycle on unfulfilled promises, though, I first analyse the political budget cycle on both realized and forecasted expenditures alone. In order to do this, I implement, as standard in the related literature, a dynamic panel-data, Arellano-Bond difference GMM specification:

$$Y_{it} = \sum_{j=1}^{T} B_j Y_{it-j} + \delta B_{j+1} + X_{it}B + u_i + \omega_t + \epsilon_{it}$$

where i stands for municipalities and t for years; Y_{it} is the fiscal variable being analyzed, such as capital or total expenditures, and j is the number of lags of Y_{it} (which varies between 2 and 3 according to what the dependent variable is); X_{it} is the set of controls; and u_i and ω_t represent respectively municipality and year fixed effects. As in the previous model, the four δ dummies capture the whole electoral cycle.

This model could be estimated with a fixed-effects regression, which would remove the municipality fixed effects and year effects, and should therefore ensure the identification of the parameters of interest, B_{j+1}. But because the dependent variable is also a function of its own lags, which appear on the right hand side, performing the regression with a fixed-effects specification produces the Nickell Bias, determined by the correlation between the lags of the dependent variable and the error term ϵ_{it}, which persists even after differencing or demeaning all variables. This bias is of order 1/T, where T is the length of the panel, so that for a dataset with a fixed length it's not possible to reduce the size of the bias.

A solution to this problem has been offered by Arellano and Bond (1991), who introduce the Difference GMM. This estimator allows for a consistent and efficient estimate of the parameters by transforming the model through first differencing, and then instrumenting the transformed first lag of the dependent variable with all of its available lags, which act as "internal"instruments. These instruments are valid as long as there is no serial correlation in the errors ϵ_{it}; this can be tested through the Arellano-Bond test that looks at whether there is second-order correlation in differences, which in turn would imply first-order correlation in levels.

In the current work T=10, implying that fixed-effect estimates would indeed suffer from the Nickell Bias, so I follow the most recent literature and run Difference-GMM regressions to test the existence of PBC on realized expenditures, since they are dependent also on their lagged values. I adopt the same approach for promised expenditures as well, since also budget forecasts

[19] All variables related to the geo-social characteristics of the municipalities are obtained from the National Statistical Office and the Atlas of Italian Municipalities, while all political variables have been computed from the Electoral Database of the Ministry of Internal Affairs.

are redacted building on the previous years' amounts. In this specification, which is effectively a municipality fixed-effects one, only variables that vary over time at the municipal level are considered, which are: population, population squared, term-limited mayor, and the average taxable income.

2.6 Empirical Results

2.6.1 Political Budget Cycle

In the first place, I verify whether Italian municipalities are subject to PBC; that is, whether there are increases in capital expenditures occurring around elections, which would indicate strategic manipulation by mayors aimed at affecting the retrospective component of voting. Table 2.6 shows the results of regressions using per capita realized expenditures as the dependent variable. Results confirm that capital spending increases sharply before elections, by 100 euros per capita, which corresponds roughly to a 16% increase with respect to the previous year, and a 20% increase with respect to the average across the whole term. At the same time, the rise in spending does not occur suddenly in the pre-electoral year, but rather begins two years away from elections and then remains at the same level over the following year. The results also suggest that term limited mayors spend less on investments, which is in line with their reduced electoral incentives, even though the result is barely not significant at the 10% level. Current expenditures also appear to vary across the electoral cycle, but to a much smaller degree. In fact, pre-electoral year current spending is only 8 euros per capita higher than the electoral year's, that is, around 1% the average level of current expenditures. Evidence thus seems to confirm that it is indeed capital expenditures that are subject to strategic manipulation. As for total expenditures, results are in line with the previous ones on capital expenditures, since pre-electoral total spending is 103 euros per capita higher than in electoral years, and again not significantly different from the previous year's.

Therefore results on the PBC for capital expenditure seem to suggest that spending is higher in the year before elections, but also that the spending increase occurs in the middle of the term; moreover, as expected, the cycle is much stronger for the most visible expenditure components, namely investments.

Table 2.7 displays the results when the dependent variables are forecasted expenditures.[20] As in the previous case, the outcomes of the regressions are highly statistically significant, and in pre-electoral years forecasted capital expenditures are 56 euros per capita greater than in electoral years, a 5 percent increase with respect to the average capital forecast. Total forecasted expenditures display a similar behaviour, even though changes are smaller - pre-electoral total forecasted expenditures are 62 euros per capita higher than in election year. Lastly, promised current expenditures appear to be higher two years before election rather than in the year before elections, but the coefficient, while significantly different from zero, is below 1% of the average forecasted current expenditures. Figure 2.6 provides a graphical representation of the results

[20]The only difference in the model is given by the number of lags of all expenditures, following the criteria of using the smallest number of lags satisfying the Arellano-Bond test for autocorrelation of errors.

discussed above, showing the electoral cycle for both realized and promised capital expenditures.

Overall, these results suggest that, over the election cycle, the path followed by promised expenditures is similar to the one of real expenditures. This is to be expected, since budget forecasts are strictly linked to balance sheets, and it is unlikely that their trends diverge radically. In order to obtain evidence of strategic manipulation of promises, therefore, rather than looking at a "PBC on promises", it is necessary to look at evidence of a cycle on unfulfilled promises, meaning the differential between what is promised and what is effectively realized in a given year.

2.6.2 Unfulfilled Electoral Promises

After having looked at whether there is a PBC for realized and forecasted expenditures individually, I now try to establish whether there is a political cycle on unfulfilled promises (as defined in section 3.5). If mayors were behaving strategically with respect to budget forecasts in order to overpromise relatively to what is actually spent, this would result in increases in unfulfilled promises in election years, since in those years budget forecasts are (in the vast majority of cases) published before the elections, while elections take place before the end of the year, so before all spending is actually observed. Table 2.8 displays the results of the province fixed-effects regression with all the municipality controls previously described; as previously explained, capital expenditures are the most likely to be subject to strategic behaviour by the mayor, and indeed results show unfulfilled promises on investments decreasing by 20 percentage points two years from elections, remaining low in the pre-electoral year, and increasing by a whole 45 percentage points in the electoral year, which correspond respectively to a 9% decrease and a 26% increase with respect to the average unfulfilled promises of the previous year, and a 9% decrease and 20% increase with respect to the average level of unfulfilled promises across all years of the mayoral term. This seems to confirm the initial hypothesis that unfulfilled promises increase substantially during elections years as a consequence of strategic behaviour, whereas there are no substantial variations over the rest of the cycle except for a decrease in unfulfilled promises two years before elections, which could signal a more credible behaviour by the incumbent mayors. These results are statistically significant at the 99% confidence level, while instead control variables are not significant; an exception is given by term limited mayors apparently making greater unfulfilled promises across the cycle, but this will be studied more in detail in the next section. Unfulfilled promises on current expenditures, which can be used as a "placebo test" since mayors are not expected to make promises on them, vary significantly across years, including the electoral year, but all changes are extremely limited in size and below one percentage point, therefore confirming that substantial variations in election year unfulfilled promises only occur for the more visible and flexible capital expenditures.

In table 2.9 I introduce the level of realized capital expenditures as an additional control variable. In this way, it is possible to establish whether mayors make more unfulfilled promises on relatively smaller or larger investments. Results confirm the cyclical pattern described above, with an electoral increase of 36 points in capital unfulfilled promises, amounting to a 20% increase on an yearly basis, and a 16% increase with respect to the term average. Interestingly, the bigger

the investment, the lower the degree to which the promise goes unfulfilled: going from the 25th to the 75th percentile of realized investments leads to a decrease in the unfulfilled promise of 35 points, which is approximately equal to the size of the election-year strategic increase. Therefore, mayors appear to promise more capital expenditures with respect to whay they actually realize when the investment level is lower. To this end, further controls on identification are done in section 2.7.

2.7 Robustness

The results from the previous section suggest that there is indeed a political cycle in the degree to which mayors fulfill their investment promises. Nonetheless, the interpretation of the results is threatened by three different factors. First of all, I run robustness checks on the specification of the empirical model. I then ensure that the results are robust to different sample selections, including municipalities with terms terminating early and belonging to Special Statute Regions. Lastly, the identification of the strategic manipulation could also be threathened by issues related to the institutional setup and the timing of elections. I tackle each of these individually, in order to provide further evidence in favour of the original interpretation of the results.

2.7.1 Identification checks

As discussed in section 3.3, the province-by-year fixed-effects specification is preferred because it allows to observe greater heterogeneity across electoral cycles with respect to the municipality fixed-effects model. Nonetheless, even though a vast number of municipality-specific controls are included, there could still be time-varying factors at the municipal level that are not included and might be influencing the results. To this end, I also run the baseline regression on the cycle on unfulfilled promises in a setup with municipality and year fixed effects. As shown in table 2.10, results still hold and have a very similar magnitude, with an election year increase of 43 points with respect to the two previous years.

As an additional robustness check on the empirical specification, I also apply the Arellano-Bond GMM model to unfulfilled promises. Standard PBC analysis on realized expenditures employs this specification due to incremental budgeting leading to auto-regressiveness of expenditures that does not allow for a standard fixed-effects approach, as mentioned in section 3.3, but in the case of unfulfilled promises, which are the difference between observed and realized expenditures, this is not the case. Nonetheless, as a further check, I also verify the results on the cycle of unfulfilled promises when considering this specification and, as table 2.11 shows, results still hold.

2.7.2 Sample checks

In section 3.5 I discussed the selection of the final sample, which excludes all terms that terminate before the deadline set by law and all those belonging to Special Statute Regions, and then further excluding outlier observations. Here I check whether this choice affects the main results.

First, I only exclude municipalities whose term does not last 5 years, and then proceed to remove all outlier observations exceeding both the standard deviation requirement (more than 15 times the standard deviation of realized or forecasted capital expenditures) or the 99th percentile requirement (capital unfulfilled promises belonging to the 99th percentile of the distribution). Results in table 2.12 confirm the results described above, with an electoral increase in unfulfilled promises equal to 31 percentage points. I then include also municipalities that have mayoral terms lasting less than five years and only remove those defined as outliers according to the above classification, and results in table 2.13 confirm all those obtained previously, therefore suggesting that the selection of the final sample does not affect the results neither qualitatively nor quantitatively.

2.7.3 Institutional factors

The increase in the difference between forecasted and realized electoral expenditures could also be caused by institutional factors, and in particular the timing of elections. Local elections in Italy are generally held in the first half of the year, and in the current sample are always held between March and June. One of the consequences of this unwritten rule is that not all election year expenditures may always be attributed to the incumbent mayors, since, if the mayor is term limited or is not re-elected, a significant part of the expenditures will be managed by another administration. This implies that the increase in unfulfilled promises in the electoral year could be driven by non-strategic factors, thus threatening my interpretation of the cyclical pattern in unfulfilled promises.

In order to control for this possibility, I verify whether the month in which elections are held can contribute to determining the unfulfilled electoral promises effect. The idea is that the earlier the elections are held, the less the realized expenditures will be in the hands of the incumbent administration over the course of the electoral year, which in turn could lead to higher unfulfilled promises as a result of this mistiming rather from any strategic manipulation. In order to check for this possibility, I run a province-fixed effects regression in which, unlike the previous specification, I replace the election-year dummy with four different election-year dummies, one for each of the months in which elections are held in the 10-year span captured by the sample. Table 2.14 shows that the cycle on capital unfulfilled promises, even though greater when elections are held in March, remains significant and large also when elections are held in May or June. Viceversa, when looking at total unfulfilled promises, they tend to be larger in June than in the other months, which, if anything, goes against the previous suggestion that higher forecasting errors could be expected when elections are held earlier.

Following the same logic, if the rise in election-year unfulfilled promises was mostly driven by a change in the administration, then one would expect the effect to be larger when there is a change in mayor. To this end, I consider two different sub-samples including mayors who are re-elected and all other cases in which there is mayoral turnover. If the results on the election year were driven by the change in the administration, then one would expect the results to only be significant in the latter sample but, as shown in table 2.15, the increase in electoral unfulfilled promises is significant for both subsamples, and is actually significantly higher for the

subsample of reelected mayors. There are clearly potential endogenity issues, since the strategic behaviour may in turn affect the probability of reelection, but this evidence, together with the one presented above, points to the fact that the results are indeed due to strategic behaviour.

Another key part of the identification strategy is the interpretation of budget forecasts as being promises made by the incumbent mayor. As seen in table 2.1, in most years the deadline for providing budget forecasts is before or immediately after the date in which elections are set, which justifies this interpretation also for election years. The only exceptions are given by the years 2010, 2011 and 2012, in which budget forecasts could be approved up to 6 months after the elections, in which case they may be released after the elections and potentially by a new administration, therefore losing their status as electoral promises. Since the day in which municipalities approve their budget forecasts is not publicly available, I run the baseline regressions excluding all years after 2009. Table 2.16 shows that all previous results still hold and coefficients remain almost the same, with the exception of unfulfilled promises on total expenditures, which now become insignificant in the electoral year.

As a further check, I also control for the winning margin with respect to the second-placed candidate in the previous election, in order to understand whether the cycle is driven by the past competitivity of the municipality. As shown in table 2.17, the main results are not affected, while the past competitivity of the municipality does not have any impact on the strategic behaviour.

Lastly, I run a further robustness check by restricting the sample to municipalities with population above 5,000, since municipalities below this threshold are exempted from the restrictions of the Domestic Stability Pact and therefore mayors could exhibit a different behaviour.[21] Nonetheless, results show that the cycle is significant for both classes of municipalities, thus rejecting the idea that the balanced budget rule has a significant impact on the mechanism of unfulfilled electoral promises which has been shown in the current work.

2.8 Discussion

2.8.1 Term limits and re-election incentives

The above sections have focused on showing the existence of a political cycle in unfulfilled promises on capital expenditures, and its robustness to different empirical specifications and to any influence due to electoral timing or sample selection. Having proven that there is indeed strategic behaviour related to promises and their fulfillment, it is then interesting to fully understand the mechanisms at work, and which kind of mayors are more prone to act strategically.

To this end, I first set out to exploit the Italian law limiting mayors to two consecutive terms, which allows to disentangle the effect of re-election incentives on the politicians' behaviour. Mayors who are in their first-term are expected to have additional incentives in behaving strategically in order to obtain electoral gains, so that the sample is split between mayors who are re-electable and those who are lame ducks. Table 2.18 shows the results for unfulfilled promises on different kinds of expenditures, and indeed the effect is significantly larger for re-electable mayors, who

[21] Bonfatti and Forni (2017) show that the PBC is reduced by two thirds in municipalities that are not subject to the Domestic Stability Pact.

increase unfulfilled promises on capital expenditures in the election year by 30 percentage points more than the mayors who are on their second consecutive term. For other kinds of expenditures, instead, there is no significant variation between first and second-term mayors, confirming that their strategic behaviour is limited to investments.

Since the rule introducing mayoral term limits was established before the beginning of the sample, there are no fears about mayors modifying their behaviour only following its introduction, and thus affecting the results. Overall, this result is highly significant since it confirms that there is a clear divide between mayors who have re-election incentives and those who do not, with the former driving, as expected, the strategic behaviour on electoral promises.

2.8.2 Education level

Another interesting aspect is whether mayors differ in their strategic behaviour according to their education level, since education has been shown to be a relevant factor in the politician's ability to develop adequate policies.[22] In this setup it is not clear ex ante what the impact of education on the cyclical variations in promise fulfillment should be. On the one hand more educated politicians, through increased sophistication, can have a greater understanding of the effect that electoral overpromising can have on the electorate, and therefore be more prone to engage in strategic behaviour on promise fulfillment. On the other side, if indeed the observed cycle was not the result of strategic engagement but rather that of errors and miscalculations, more educated mayors should be expected to have lower levels of unfulfilled investment promises in election years.

In order to investigate this, I exploit data provided by the Database of Local Administrators ran by the Ministry of Internal Affairs, which contains each mayor's education level. I then divide mayors in two categories that are roughly equal in size: those who have at most a high school diploma, and those who have at least obtained an university degree. Table 2.19 presents split sample regressions on different kinds of unfulfilled promises, and shows that mayors who have at least an university degree present unfulfilled electoral promises on capital investments that are more than twice as large as those of mayors who have obtained at most a high school diploma. In fact, the former increase election year unfulfilled promises by 30 percentage points more than the latter, and this difference is highly statistically significant.

The results therefore clearly show that more educated mayors are actually more prone to engaging in strategic behaviour on promise fulfillment. This equilibrium outcome fits with a selection mechanism a la Rogoff and Siebert (1988), in which more competent types are forced to engage in electoral strategic behaviour in order to separate from less competent politicians, and further reassures that the results are due to politicians' strategic behaviour.

2.8.3 National versus civic parties

In addition to understanding how the personal characteristics of the mayor affect the cycle on promise fulfillment, it is also relevant to understand whether party membership is a significant

[22] For instance, Besley et al. (2011) exploit natural deaths and terminal illnesses to show that more educated leaders lead to higher economic growth.

determinant of strategic behaviour on promises. In recent years, some studies have concentrated on the divide between older and national parties, and the ones that are younger and more civic. For instance, Akhmedov and Zhuravskaya (2004) show that, in Russian regions, the efficiency in the provision of public goods depends on how strong the national parties are, while both Cioffi et al. (2012) and Gamalerio (2017) find that the PBC on realized expenditures in Italian municipalities is driven by civic lists (that is, movements that are born in municipalities and only run for local elections). Also Hanusch and Keefer (2013) find similar results at the cross-country level, with younger parties being less likely to limit PBC.

In order to establish whether this divide has an effect also on strategic behaviour on promise fulfillment, I separate mayors between those belonging to parties that are organized at the national level, regardless of their ideology, and those that instead run for a Lista Civica, that is, a civic list. In the Italian framework it is a difficult task to separate mayors according to their ideology, since in smaller municipalities mayors may have an ideological inclination that is known to the local citizens but is not explicited in the list's name; in this case, though, the objective is not that of identiying the ideology of the party, but rather whether the mayor is affiliated or not to a national party, which simplifies the task at hand.

Table 2.20 shows the results of the split sample regressions, confirming the hypothesis derived from the existing literature that the strategic behaviour is stronger in parties lacking a national affiliation, which present significantly larger increases in the electoral unfulfilled promises on capital expenditures. Therefore it appears that the lack of control usually provided by a party on its local affiliates, together with presumably lower career concerns, leads to greater strategic behaviour over the course of the electoral terms, and to more unfulfilled investments in election years.

2.8.4 Unfulfilled promises and the standard PBC

Lastly, I investigate the link between the cycle in unfulfilled promises and the standard PBC on realized expenditures, for which there is large empirical support both in the current paper and in the related literature. In fact, the above results indicate that mayors who engage strategically on unfulfilled promises share some characteristics with those who have been shown to behave strategically relatively to realized investments, such as party membership. A natural follow-through question is whether the two cycles are actually linked, that is, whether the mayors who fulfill less investment promises in the year of elections are also those who increase capital expenditures more in the pre-electoral year.

In order to investigate this, I select the quartile of mayoral terms in which there have been the highest pre-electoral increases in capital expenditures, which can be considered to be the drivers of the PBC on realized expenditures, and separate them from all other observations. Then, I run split sample regressions in order to establish whether the cycle in unfulfilled promises is actually driven by mayors who behave more strategically in accordance with standard PBC. Table 2.21 confirms the above intuition, showing that mayors who increase pre-electoral expenditures then engage in much higher strategic behaviour on unfulfilled promises, increasing them by 216 percentage points with respect to the previous year, whereas mayors belonging to the

other 75 percentiles of the distribution actually show the opposite behaviour, reducing unfulfilled electoral promises.

While the results must be handled with care (for example, such a substantial increase can be due to the fact that mayors increasing pre-electoral expenditures the most are also likely to have had lower unfulfilled promises in the pre-electoral year), they are nonetheless striking, as they suggest that the two cycles are indeed connected. Mayors therefore appear to behave strategically not only in the year before elections, by increasing capital expenditures in order to impress voters, as already accepted by the empirical literature on PBC, but then reinforce their strategic engagement, measured in terms of unfulfilled promise, in another moment before elections, when they publish their final budget forecast, which corresponds to an electoral promise.

2.8.5 Unfulfilled promises and voting

The focus of this work has been to highlight empirically that electoral promises are subject to strategic behaviour by politicians, and in particular this has been shown through the degree to which they fulfill such promises across the electoral cycle. Nonetheless, while focusing on the "supply side" provided by politicians, the expected outcome is for this behaviour to have an electoral impact. In fact, in the standard PBC literature, politicians engage in increased spending in the pre-election year in order to obtain an electoral advantage. Similarly, in this framework, the reason why mayors would be more inclined to engage in strategic manipulation and overpromise on capital expenditures in the election year is to obtain an electoral advantage, by convincing voters about their spending intent. In order to verify whether increased overpromising on capital expenditure favours mayors' reelection, I first run fixed-effects logit regressions, which aim at capturing the impact on the probability of re-election of the deviation in capital unfulfilled promises in the election year with respect to the term average. This specification, which has been used with realized capital expenditures in the literature on PBC [23], allows to focus on the change in election-year unfulfilled promises rather than on their level, and it is the change in capital unfulfilled promises which is expected to matter the most on the voters' choice.

Table 2.22 shows the results of these regressions. Columns 1 and 3 focus on the impact of the election year deviation in unfulfilled promises on the probability of re-election, showing respectively the coefficients and the odds ratios, while the dependent variable is equal to 1 if the mayor is re-elected for a second consecutive term and 0 otherwise. In columns 2 and 4, instead, the dependent variable is equal to 1 if the incumbent mayor is seeking re-election and 0 otherwise. I include the latter because it is expected that first-term mayors will be more likely to engage in strategic manipulation of unfulfilled promises only if they are running for re-election, so as to obtain an electoral advantage, and also in order to look at a larger sample, since there are 1091 instances of first term mayors having to decide whether to re-run or not, but only 391 cases in which they decide to do so. Results show that an increase in the election year deviation in capital unfulfilled promises does have a positive and statistically significant impact on both the probability of being re-elected and the probability of seeking re-election. In particolar, in order to provide a better sense of the magnitude of this effect, going from the 25th to the 75th

[23] See Drazen and Eslava (2008), Sakurai and Menezes Filho (2008), Veiga and Veiga (2007) amongst others.

quartile in terms of election year deviation in unfulfilled promises, amounting respectively to -63 and 98 percentage points, leads to an increase in the odds ratio of being re-elected of 12.8%, and an increase in the odds ratio of seeking re-election of 8.94%.

2.9 Conclusion

This work has aimed at showing empirically that expenditure promises follow a political cycle, much like the literature on PBC has shown to be the case for realized expenditures. This suggests that indeed promises tend not to be seen as purely non-binding, while lending support to the role of prospective voting.

This evidence suggests that politicians expect voters to value both past performance and electoral promises and try to manipulate strategically both components, and that the strategic behaviour is driven by the same set of mayors in both cases. In order to study manipulation of promises, I have exploited some peculiarities of the Italian framework, such as the fact that mayors have a two-terms limit and so there cannot be any long-term sanctioning as envisioned by some theoretical models on prospective voting, and also the fact that budget forecasts, both because of their timing of release and resonance in the local media, can be interpreted as being promises, and in particular electoral promises when close to elections. Further research on the topic in different sub-national environments would allow to shed further light on the role of political strategic manipulation of both policies and promises, while it would also be of interest to study further the role of voter information in political cycles and in the selection of politicians.

2.10 Appendix

Table 2.1: Municipal Elections and Budget Deadlines

Election date	Municipalities involved	Initial postponed budget deadline	Final ”
25 May 2003	309	31 March	30 May
13 June 2004	4222	31 March	21 June
3 April 2005	361	31 March	31 May
28 May 2006	1111	31 May	31 May
27 May 2007	744	31 March	30 April
13 April 2008	411	31 March	31 May
7 June 2009	3973	31 March	31 March
28 March 2010	452	30 April	30 September
15 May 2011	1139	31 March	3 October
6 May 2012	744	31 March	30 November

Table 2.2: Expenditures per capita

Variable	Mean	Std. Dev.
Forecasted Capital	1173	2675
Forecasted Current	788	514
Forecasted Debt	161	351
Forecasted Services	221	454
Forecasted Total	2344	3053
Realized Capital	535	960
Realized Current	769	471
Realized Debt	92	185
Realized Services	118	298
Realized Total	1515	1367

Table 2.3: Capital expenditures per capita by years from election

Variable	25th percentile	Median	75th percentile	
Forecasted- 4 years from elections		210	433	1014
Forecasted - 3 years from elections		208	433	1004
Forecasted - 2 years from elections		198	423	997
Forecasted - 1 year from elections		234	469	1098
Forecasted - election year		193	414	952
Realized - 4 years from elections		157	277	543
Realized - 3 years from elections		147	266	521
Realized - 2 years from elections		141	266	534
Realized - 1 year from elections		184	323	636
Realized - election year		124	237	481

Table 2.4: Unfulfilled promises

Variable	Mean	Std. Dev.
Capital	221	574
Current	3	20
Total	54	118

Table 2.5: Capital unfulfilled promises by years from election

Variable	25th percentile	Median	75th percentile	
4 years from elections		-11	42	182
3 years from elections		-9	47	190
2 years from elections		-8	43	190
1 year from elections		-14	31	151
Election year		-7	56	227

Table 2.6: Baseline PBC for realized expenditures

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
First Lag of Dependent Variable	0.044*	0.783***	0.023
	(0.026)	(0.184)	(0.031)
Election Year	-100.744***	-8.646***	-103.807***
	(12.296)	(2.222)	(13.002)
Two Years Before Elections	-16,337	-3.993**	-22.858*
	(12.477)	(1.661)	(13.025)
Three Years Before Elections	-25.486**	-5.191***	-31.764**
	(12.280)	(1.738)	(13.252)
Four Years Before Elections	-50.140***	-4.517***	-67.458***
	(13.737)	(1.685)	(14.639)
Term Limited Mayor	-14.214	1.632	-14.244
	(11.078)	(1.412)	(11.635)
Observations	46178	46178	46130
Municipality Controls	Yes	Yes	Yes
Arellano-Bond test for AR(2)	0.184	0.110	0.112
Sargan test	0.193	0.000	0.087

Standard errors in parentheses. Dependent variable is the realized expenditures per capita, expressed in 2011 euros. Two-step Difference-GMM estimator with Widmeijer robust standard errors. p-values are reported for the Arellano-Bond test and the Sargan test. Municipality and year-fixed effects included in all regressions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Baseline PBC for forecasted expenditures

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
First Lag of Dependent Variable	0.275***	0.299**	0.229***
	(0.054)	(0.125)	(0.056)
Second Lag of Dependent Variable	-0.003	0.068	-0.004
	(0.022)	(0.071)	(0.021)
Election Year	-56.235**	4.405	-46.602**
	(22.789)	(2.750)	(23.122)
Two Years Before Elections	28.507	4.750**	35.370*
	(20.083)	(2.135)	(20.834)
Three Years Before Elections	0.372	5.676	* 8.913
	(21.212)	(2.903)	(22.501)
Four Years Before Elections	28.334	1.061	35.366
	(26.022)	(3.260)	(26.872)
Term Limited Mayor	-38.436***	3.692*	-42.940**
	(24.987)	(2.024)	(25.403)
Observations	39287	39287	39287
Municipality Controls	Yes	Yes	Yes
Arellano-Bond test for AR(2)	0.437	0.484	0.651
Sargan test	0.012	0.000	0.010

Standard errors in parentheses. Dependent variable is the realized expenditures per capita, expressed in 2011 euros. Two-step Difference-GMM estimator with Widmeijer robust standard errors. p-values are reported for the Arellano-Bond test and the Sargan test. Municipality and year-fixed effects included in all regressions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Political cycle in unfulfilled promises

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	45.089***	0.431	1.456
	(7.460)	(0.290)	(1.123)
Two Years Before Elections	0.263	0.317**	1.881
	(8.018)	(0.141)	(1.789)
Three Years Before Elections	20.682*	0.285	4.455
	(11.257)	(0.331)	(2.899)
Four Years Before Elections	22.282***	0.678*	4.296**
	(7.097)	(0.319)	(1.522)
Term Limited Mayor	13.904*	0.210	2.954*
	(7.108)	(0.229)	(1.626)
Observations	61415	61415	61363

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.9: Political cycle in unfulfilled promises controlling for realized expenditures

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	35.828***	0.423	0.676
	(6.338)	(0.291)	(1.395)
Two Years Before Elections	-1.003	0.307**	1.630
	(8.530)	(0.143)	(1.753)
Three Years Before Elections	18.235	0.280	4.193
	(10.423)	(0.334)	(2.742)
Four Years Before Elections	16.584**	0.664*	3.661**
	(6.765)	(0.318)	(1.387)
Realized Expenditures	-0.092***	-0.001*	-0.007**
	(0.028)	(0.001)	(0.003)
Term Limited Mayor	13.770*	0.211	2.980*
	(6.729)	(0.229)	(1.586)
Observations	61415	61415	61363

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Unfulfilled promises cycle with municipality fixed effects

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	42.980***	0.419*	1.552
	(7.552)	(0.222)	(1.308)
Two Years Before Elections	1.751	0.903***	0.795
	(5.078)	(0.273)	(0.653)
Three Years Before Elections	13.482***	0.118	1.902**
	(4.041)	(0.196)	(0.662)
Four Years Before Elections	7.755	0.686**	2.258***
	(6.025)	(0.282)	(0.637)
Realized expenditures	-0.146***	-0.012***	-0.024***
	(0.040)	(0.002)	(0.006)
Term Limited Mayor	9.706**	0.167	1.612*
	(4.503)	(0.150)	(0.759)
Observations	61415	61415	61363

Standard errors in parentheses, clustered at the regional level.

Dependent variable is the forecasting error expressed in percentage points.

Year and municipaliy fixed effects included in all regressions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Unfulfilled promises cycle with Arellano-Bond

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
First lag of dependent variable	0.021	0.099*	0.196***
	(0.022)	(0.060)	(0.023)
Second lag of dependent variable	0.009		0.047**
	(0.015)		(0.023)
Election Year	62.301***	0.567***	5.099***
	(9.907)	(0.177)	(1.176)
Two Years Before Elections	9.561	0.827***	1.837
	(9.130)	(0.174)	(1.206)
Three Years Before Elections	31.541***	0.555***	3.060**
	(9.109)	(0.151)	(1.253)
Four Years Before Elections	33.759***	0.573***	5.607***
	(9.973)	(0.174)	(1.421)
Term Limited Mayor	10.252	0.146	0.290
	(10.404)	(0.151)	(1.415)
Municipality Controls	Yes	Yes	Yes
Arellano-Bond test for AR(2)	0.48	0.23	0.14
Sargan test	0.43	0.00	0.12

Standard errors in parentheses.

Dependent variable is the realized expenditures per capita, expressed in 2011 euros.

Two-step Difference-GMM estimator with Widmeijer robust standard errors. p-values are reported for the Arellano-Bond test and the Sargan test. Municipality and year-fixed effects included in all regressions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: Unfulfilled promises cycle keeping Special Statute Regions

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	31.693***	0.407	0.846
	(5.705)	(0.279)	(1.132)
Two Years Before Elections	-2.519	0.350**	2.181
	(9.162)	(0.141)	(1.720)
Three Years Before Elections	16.721	0.338	3.926
	(9.941)	(0.310)	(2.566)
Four Years Before Elections	16.813**	0.661**	3.593**
	(6.166)	(0.286)	(1.364)
Realized expenditures	-0.088***	-0.001**	-0.007**
	(0.026)	(0.000)	(0.003)
Term Limited Mayor	11.043	0.221	2.736*
	(6.569)	(0.217)	(1.436)
Observations	67285	67285	67228

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Unfulfilled promises and mayors' credibility

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	26.013***	0.237	1.312
	(5.819)	(0.236)	(0.898)
Two Years Before Elections	-5.024	0.331**	1.805
	(8.255)	(0.156)	(1.629)
Three Years Before Elections	11.353	0.141	2.920
	(6.618)	(0.223)	(1.966)
Four Years Before Elections	6.239	0.429	2.623*
	(5.429)	(0.340)	(1.297)
Realized expenditures	-0.093***	-0.001*	-0.006**
	(0.028)	(0.000)	(0.003)
Term Limited Mayor	11.870	0.286	3.084*
	(7.509)	(0.284)	(1.726)
Observations	72528	72528	72461

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: Unfulfilled promises cycle - different election months

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Elections in March	78.110**	-0.834	-4.943
	(32.089)	(1.164)	(9.573)
Elections in April	-29.177	-1.025*	-15.992
	(39.661)	(0.580)	(11.869)
Elections in May	36.131**	1.183	0.182
	(12.622)	(0.760)	(1.953)
Elections in June	44.075***	-0.037	5.829*
	(11.601)	(0.276)	(3.022)
Two Years Before Elections	-0.396	0.282*	2.189
	(9.497)	(0.151)	(2.191)
Three Years Before Elections	18.111	0.329	4.429
	(11.392)	(0.359)	(3.009)
Four Years Before Elections	16.099**	0.575	3.390**
	(6.674)	(0.354)	(1.233)
Realized expenditures	-0.092***	-0.001*	-0.007**
	(0.028)	(0.001)	(0.003)
Term Limited Mayor	13.989*	0.221	2.976*
	(6.757)	(0.231)	(1.582)
Observations	61415	61415	61363

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.15: Unfulfilled promises cycle - same and different mayors in election year

	(1) Capital - Not reelected	(2) " - Reelected	(3) Current - Not reelected	(4) " - Reelected	(5) Total - Not reelected	(6) " - Reelected
Election Year	27.945***	64.618***	0.574	0.318	-0.912	2.654
	(7.684)	(9.347)	(0.385)	(0.198)	(1.715)	(3.553)
Two Years Before Elections	-2.863	-1.843	0.145	0.763**	1.808	-1.724
	(9.521)	(13.861)	(0.169)	(0.307)	(2.007)	(2.221)
Three Years Before Elections	12.083	32.097	0.175	0.866***	4.179	2.582
	(10.962)	(20.428)	(0.407)	(0.257)	(3.223)	(2.827)
Four Years Before Elections	8.615	35.815**	0.240	1.667**	3.928	3.133
	(8.123)	(16.024)	(0.393)	(0.565)	(2.245)	(2.489)
Realized expenditures	-0.095***	-0.081**	-0.002***	-0.000	-0.008**	-0.006
	(0.029)	(0.029)	(0.001)	(0.001)	(0.003)	(0.004)
Term Limited Mayor	14.046		0.029		3.668	
	(9.718)		(0.183)		(2.538)	
Observations	46013	15402	46013	15402	45974	15389
Chi-square test on election year coefficients	8.96		0.83		1.38	
Prob¿chi	0.01		0.36		0.24	

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions.
Dependent variable is the forecasting error expressed in percentage points.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.16: Unfulfilled promises cycle excluding post-2010 years

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	32.451***	0.418	1.717
	(9.514)	(0.431)	(1.664)
Two Years Before Elections	-5.411	0.592**	1.342
	(5.561)	(0.203)	(1.101)
Three Years Before Elections	24.580**	0.342	5.038**
	(9.413)	(0.321)	(2.288)
Four Years Before Elections	18.336**	1.119***	3.530***
	(6.229)	(0.338)	(1.078)
Realized expenditures	-0.083***	-0.001	-0.008**
	(0.025)	(0.001)	(0.003)
Term Limited Mayor	17.454**	0.375	3.315**
	(7.155)	(0.342)	(1.453)
Observations	43251	43251	43232

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions. Dependent variable is the forecasting error expressed in percentage points.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.17: Unfulfilled promises and past winning margin

	(1) Capital Expenditures	(2) Current Expenditures	(3) Total Expenditures
Election Year	54.649***	0.354	2.878
	(16.977)	(0.469)	(2.079)
Two Years Before Elections	4.092	0.298**	3.979
	(12.911)	(0.112)	(3.097)
Three Years Before Elections	21.433	0.386	4.734
	(15.759)	(0.400)	(4.102)
Four Years Before Elections	14.311	0.693**	5.154*
	(9.512)	(0.237)	(2.545)
Past election winning margin	38.469	1.916	14.864
	(43.424)	(1.204)	(12.007)
Realized expenditures	-0.110***	-0.001*	-0.008**
	(0.034)	(0.001)	(0.003)
Term Limited Mayor	0.909	-0.319	-1.503
	(7.957)	(0.198)	(1.290)
Observations	40286	40286	40242

Standard errors in parentheses.

Dependent variable is the forecasting error expressed in percentage points.

Year and province fixed-effects with standard errors clustered at the region level.

"Past election winning margin" accounts for the vote difference (in share) between the top two candidates in the previous electio

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.18: Unfulfilled promises and term limits

	(1) Capital - Reelectable	(2) " - Term Limited	(3) Current - Reelectable	(4) "- Term Limited	(5) Total - Reelectable	(6) " - Term Limited
Election Year	46.209***	14.073*	-0.231	1.629	1.172	-1.217
	(10.509)	(7.351)	(0.238)	(0.957)	(1.494)	(3.358)
Two Years Before Elections	5.570	-13.395	0.216	0.500	2.735	-0.780
	(13.327)	(10.918)	(0.208)	(0.413)	(2.114)	(1.228)
Three Years Before Elections	20.758	13.694	0.097	0.695	3.590	5.707
	(12.799)	(10.390)	(0.316)	(0.538)	(2.850)	(3.873)
Four Years Before Elections	18.956**	14.621	0.729*	0.509	3.716**	3.514
	(6.795)	(12.144)	(0.380)	(0.511)	(1.702)	(2.624)
Realized Expenditures	-0.089***	-0.096**	-0.001**	-0.001	-0.007**	-0.008**
	(0.026)	(0.034)	(0.000)	(0.001)	(0.003)	(0.003)
Observations	40007	21408	40007	21408	39976	21387
Chi-square test on equality of election year coefficients	5.09		3.15		0.54	
Prob¿chi	0.02		0.08		0.46	

Standard errors in parentheses, clustered at the regional level. Dependent variable is the forecasting error expressed in percentage points.

Province-by-year fixed effects included in all regressions.

Samples split according to whether the mayor is re-electable (in his first term) or not.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.19: Unfulfilled promises and mayors' education level

	(1) Capital - High School	(2) " - University	(3) Current - High School	(4) " - University	(5) Total - High School	(6) " - University
Election Year	20.935**	50.108***	0.856	0.184	-0.029	0.657
	(9.781)	(6.286)	(0.696)	(0.228)	(2.102)	(1.883)
Two Years Before Elections	2.001	1.539	0.639**	0.240	1.781	1.977
	(8.359)	(17.509)	(0.296)	(0.256)	(1.384)	(2.612)
Three Years Before Elections	13.403*	23.669	0.816	-0.185	3.223*	4.727
	(7.139)	(19.157)	(0.653)	(0.308)	(1.818)	(3.537)
Four Years Before Elections	24.269**	12.257	0.860	0.783	4.651***	3.302
	(8.902)	(11.533)	(0.517)	(0.498)	(1.079)	(2.373)
Cons_cap_pc	-0.072***	-0.131***	-0.001	-0.002***	-0.006**	-0.012**
	(0.020)	(0.040)	(0.001)	(0.001)	(0.002)	(0.006)
Term Limited Mayor	3.276	25.992*	0.396	0.009	2.151	3.826
	(4.548)	(14.118)	(0.453)	(0.167)	(1.259)	(2.195)
Observations	33505	24780	33505	24780	33475	24765
Chi-square test on equality of election year coefficients	5.71		0.79		0.54	
Prob¿chi	0.02		0.37		0.46	

Standard errors in parentheses, clustered at the regional level. Province-by-year fixed effects included in all regressions.
Dependent variable is the forecasting error expressed in percentage points.
Samples split according to whether the mayor has obtained an university degree or not.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.20: Unfulfilled promises and mayors' party allegiance

	(1) Capital - National	(2) " - Civic	(3) Current - National	(4) " - Civic	(5) Total - National	(6) " - Civic
Election Year	23.657	54.204*	0.252	1.902	-1.568	6.470
	(24.167)	(25.944)	(0.484)	(1.760)	(3.147)	(5.207)
Two Years Before Elections	11.289	13.405	0.592	1.012**	2.386	5.033*
	(27.026)	(11.865)	(0.368)	(0.365)	(2.584)	(2.371)
Three Years Before Elections	-7.100	48.604**	-0.034	1.208	-1.507	8.098**
	(20.015)	(21.077)	(0.548)	(0.819)	(2.959)	(3.634)
Four Years Before Elections	12.088	44.727**	0.264	2.055***	-0.240	9.672***
	(21.943)	(18.250)	(0.463)	(0.534)	(2.553)	(2.909)
Realized expenditures	-0.164***	-0.085***	-0.001	-0.001	-0.019**	-0.007*
	(0.044)	(0.027)	(0.001)	(0.001)	(0.009)	(0.003)
Term Limited Mayor	-35.843	19.989	0.112	0.313	-10.206	2.813
	(26.228)	(12.126)	(0.178)	(0.425)	(8.079)	(2.022)
Observations	7342	23074	7342	23074	7330	23064
Chi-square test on equality of election year coefficients	3.10		0.86		1.71	
Prob¿chi	0.09		0.35		0.19	

Standard errors in parentheses, clustered at the regional level. Dependent variable is the forecasting error expressed in percentage points.
Year and municipaliy fixed effects included in all regressions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.21: Unfulfilled promises cycle and realized political budget cycle

	(1) Capital - top 25th	(2) " - others	(3) Current - top 25th	(4) " - others	(5) Total - top 25th	(6) " - others
Election Year	216.247***	-20.516*	1.154**	0.234	26.063***	-5.862**
	(34.878)	(11.588)	(0.450)	(0.271)	(2.839)	(2.193)
Two Years Before Elections	99.037***	-22.128**	0.976**	0.232*	19.318***	-1.627
	(17.666)	(9.918)	(0.440)	(0.132)	(3.781)	(1.676)
Three Years Before Elections	160.202***	-9.351	-0.205	0.368	27.131***	-0.174
	(35.481)	(7.708)	(0.960)	(0.333)	(5.188)	(2.366)
Four Years Before Elections	106.481***	-3.047	2.548**	0.301	23.387***	-0.260
	(27.312)	(6.567)	(1.176)	(0.398)	(3.985)	(1.257)
Realized expenditures	-0.064***	-0.101***	-0.000	-0.002**	-0.005**	-0.008**
	(0.020)	(0.031)	(0.001)	(0.001)	(0.002)	(0.003)
Term Limited Mayor	27.647**	12.121	0.414	0.135	9.212**	1.721
	(10.612)	(7.216)	(0.612)	(0.206)	(3.358)	(1.396)
Observations	11351	50064	11351	50064	11342	50021
Chi-square test on equality of election year coefficients	31.04		6.44		53.06	
Prob¿chi	0.00		0.11		0.00	

Standard errors in parentheses, clustered at the regional level. Dependent variable is the forecasting error expressed in percentage points.
Year and municipaliy fixed effects included in all regressions.
Top 25th refers to observations belonging to terms in which the highest pre-electoral increase in realized capital expenditures has occured.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.22: Effect of unfulfilled promises on probability of re-election and seeking re-election

	(1) 1	(2) 2	(3) 3	(4) 4
Election Year Deviation	0.000577**	1.000577**	0.0004064***	1.000407***
	(0.0002925)	(0.0002926)	(0.0001468)	(0.0001469)
Average Unfulfilled Promises	0.0024126***	1.002416***	0.0003568	1.000357
	(0.000735)	(0.0007367)	(0.0002841)	(0.0002842)
Observations	370	370	1091	1091

Fixed effecs logit regressions. Coefficients are expressed in regressions 1 and 3, odds ratios in regressions 2 and 4.

Dependent variable is equal to 1 in regressions 1 and 3 if the mayor is re-elected while in regressions 2 and 4 if the mayor is seeking re-election; 0 otherwise.

Election Year Deviation refers to the deviation in the unfulfilled promise on capital expenditures relatively to the average over the rest of the term, which is captured by Average Unfulfilled Promises.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 2.1: Number of Articles Concerning Budget Forecasts

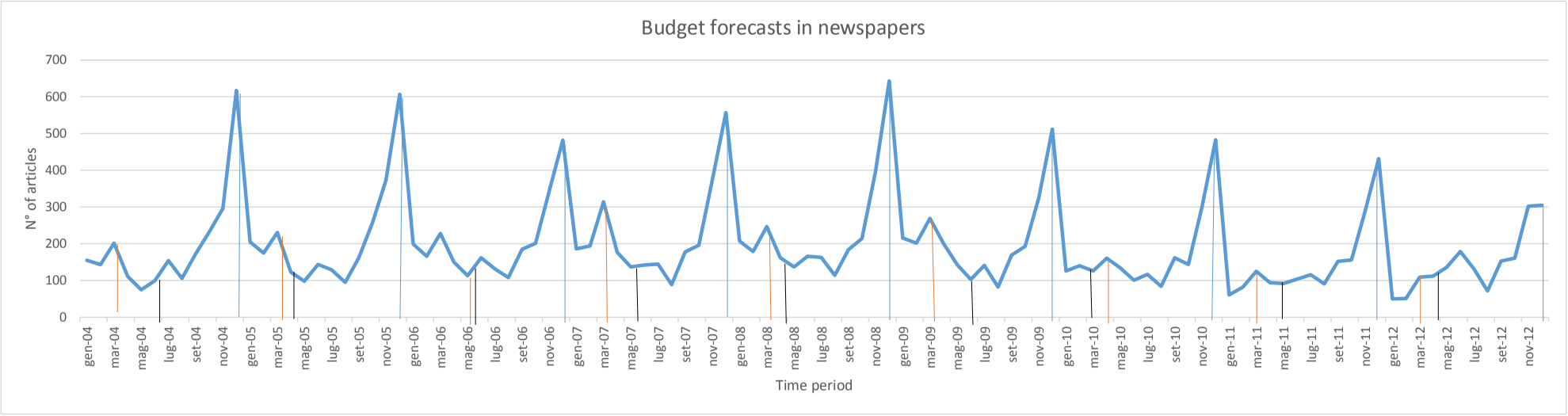


Figure 2.2: Realized Expenditures

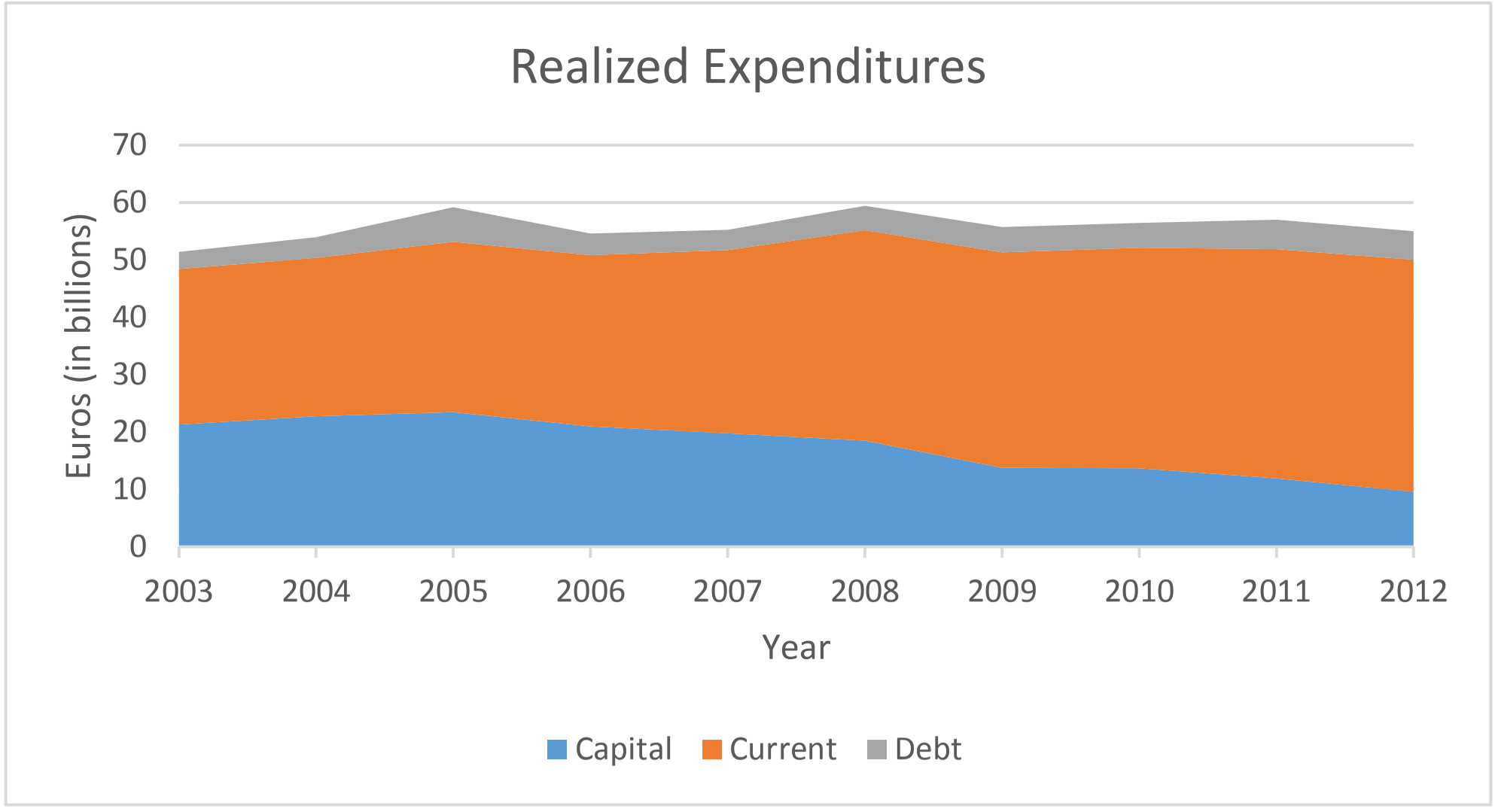


Figure 2.3: Realized Expenditures' Share

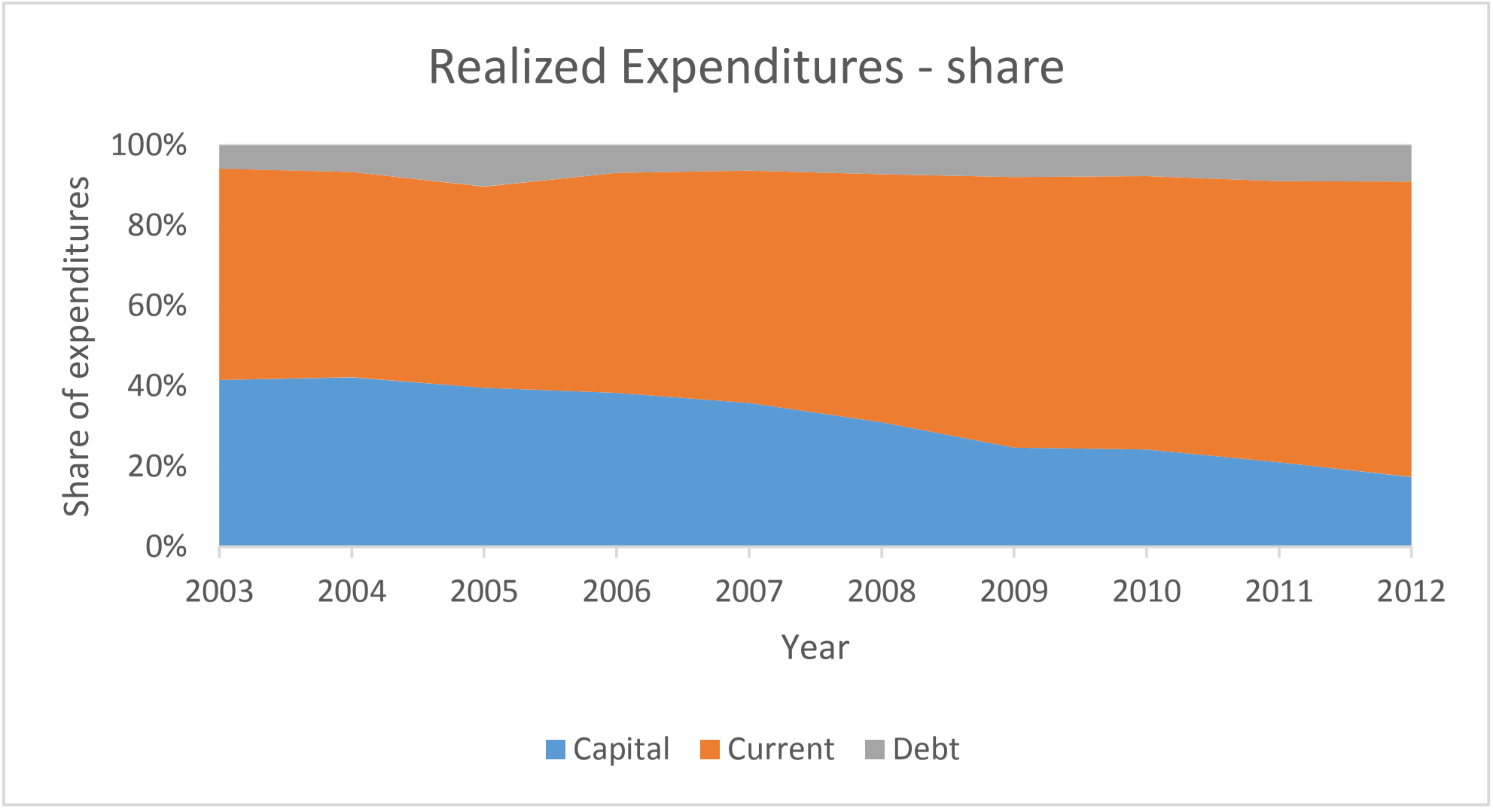


Figure 2.4: Forecasted Expenditures

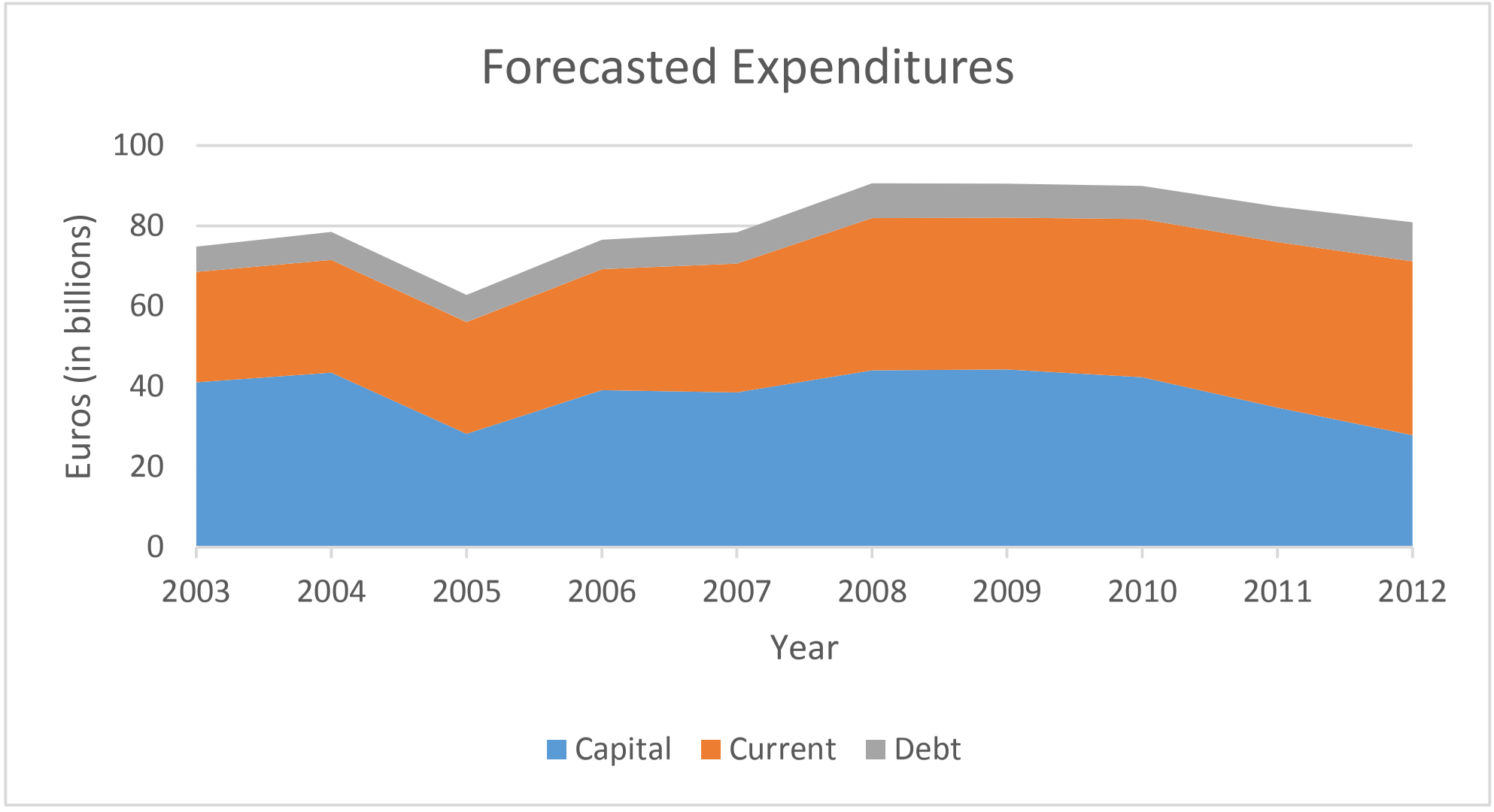


Figure 2.5: Forecasted Expenditures' Share

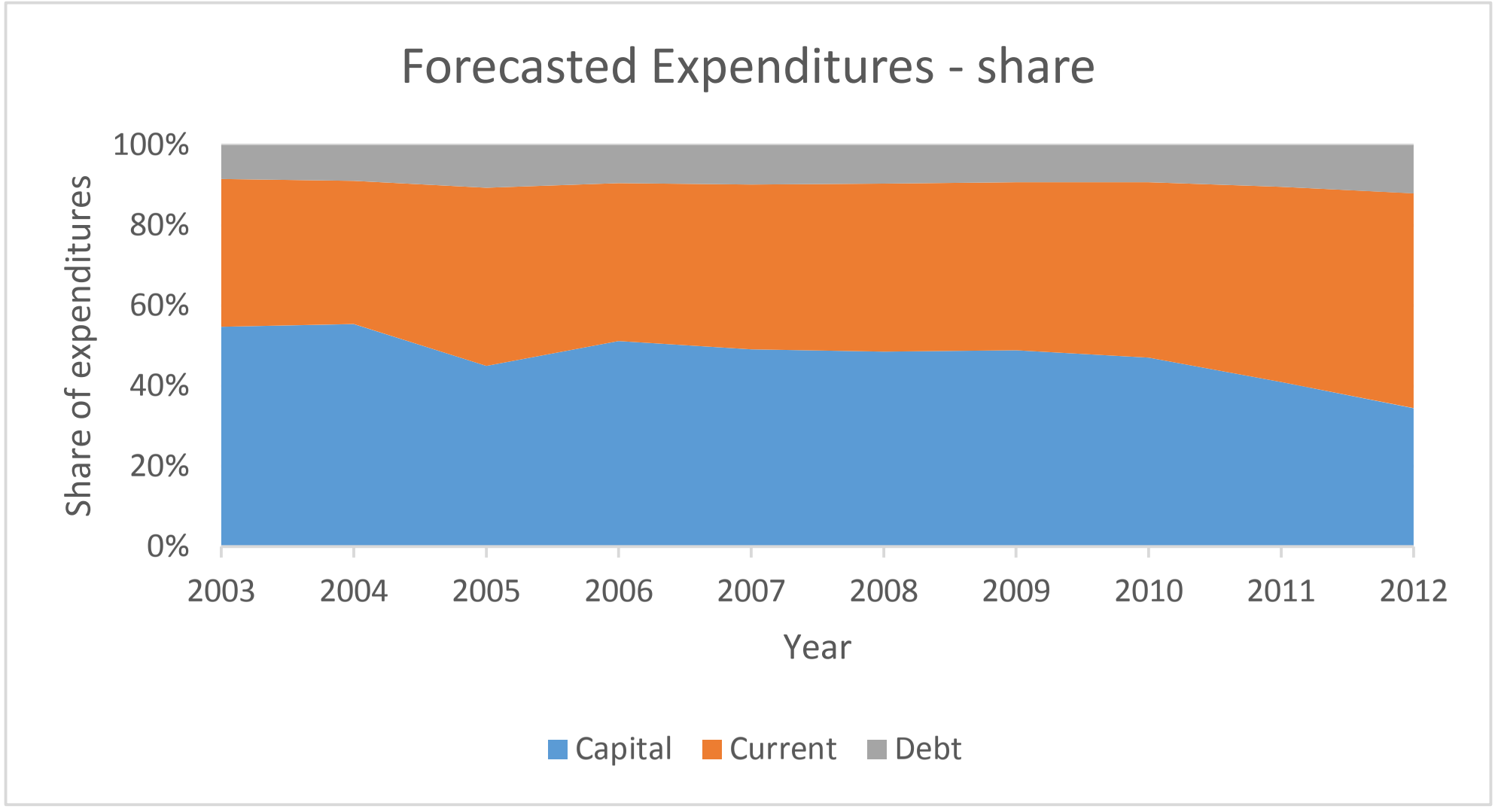


Figure 2.6: Political Budget Cycle for Capital Expenditures

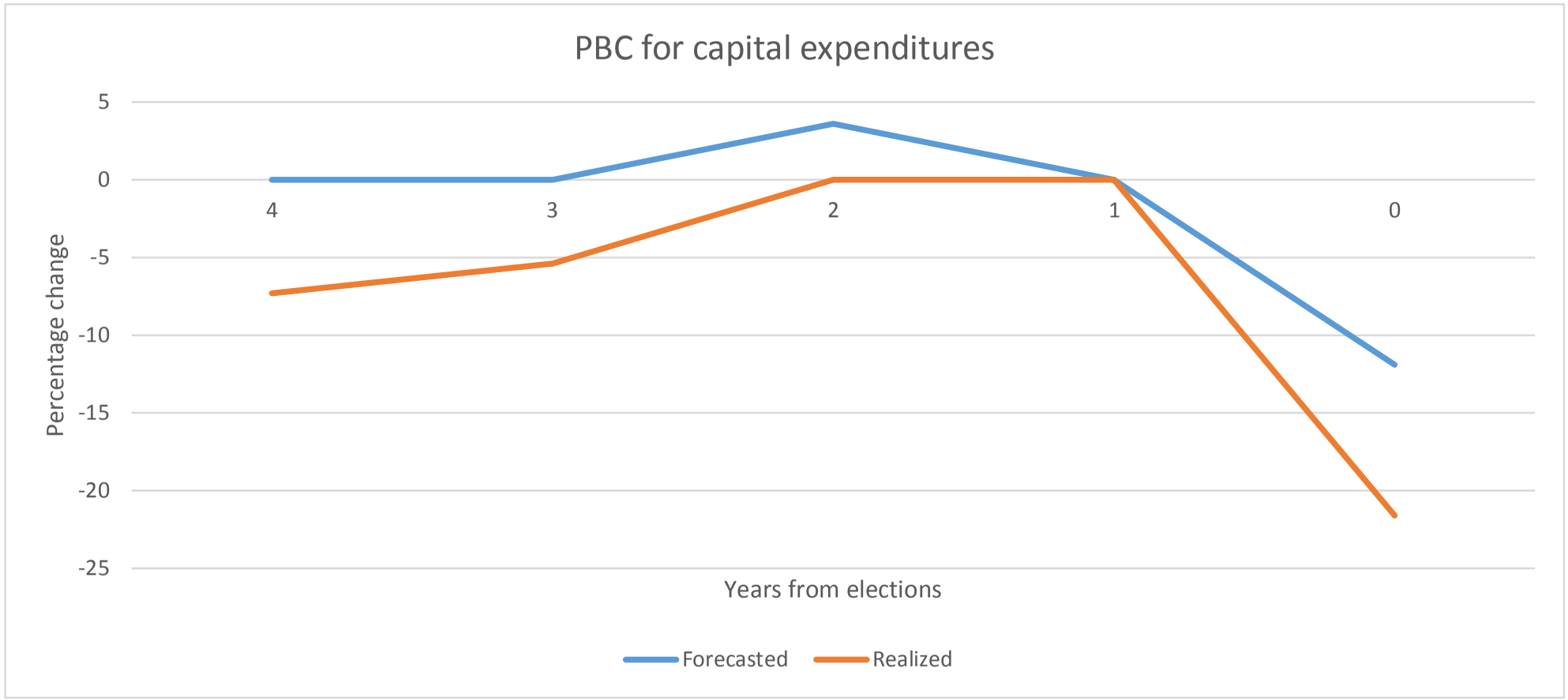
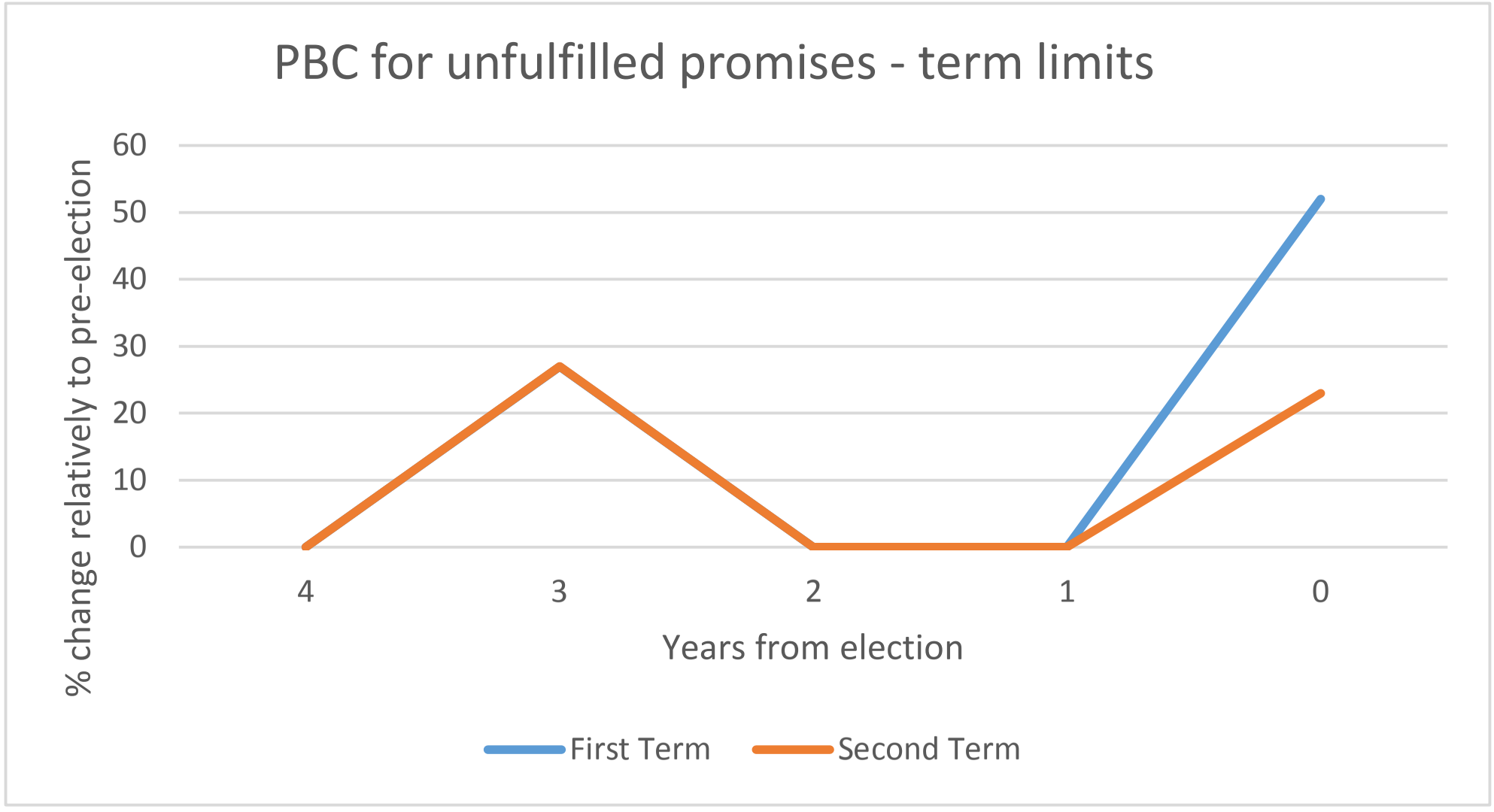


Figure 2.7: PBC for Unfulfilled Promises and Term Limits



Chapter 3

Lose from Globalization, Win at the Electoral Polls? Discording Evidence on the China Shock from Italy

Abstract

There has been a recent surge of interest in the link between globalization and the political repercussions caused by increased exposure to low-cost import competition, which has found it to cause increased political polarization and a surge in vote for radical right-wing parties. This work focuses on Italy, which is an interesting case study given its highly-exposed economy, and its multi-party system which includes radical right- and left-wing parties and also populist movements. In order to investigate the political effects caused by the trade shock determined by China's exogenous growth in productivity I construct an exhaustive database on the period between 1994 to 2016 and find that, in contrast to the related literature, there is no causal positive effect on the support for radical parties, nor is the rise in populist politics caused by increased exposure to the Chinese trade shock.

Keywords: Political Economy, Globalization, Trade Shock, Populism, Italy

3.1 Introduction

In the 2016 presidential elections, Donald Trump won the rural manufacturing counties in the Midwest through claims that *"Globalization has made the financial elite who donate to politicians very, very wealthy...but it has left millions of our workers with nothing but poverty and heartache"*[1]; in the United Kingdom's Brexit referendum the Leave side campaigned on *"Let's take back control over our economy and trade"*[2], while the campaign that saw Marine Le Pen's hard-right Front National gain a historical place in the French presidential runoff was launched with claims that *"The divide is not between the left and right any more but between patriots and globalists...Past leaders chose deregulated globalization, it turned out to be atrocious..."*.[3]

Closing the borders, protecting local industries from foreign competition, and a general rejection of globalization as it is are the recurrent themes of these campaigns, labelled by the media as the "populist backlash", that have swept across Western countries with surprise successes in a number of high-profile elections. This wave has broken lines with past ideological divides, as anti-establishment candidates strongly attacked the political and economic status quo of the globalized society, putting the role and future of globalization into serious question.

Following the sudden rise in prominence of an anti-globalization political message, and after many years in which the literature had mostly overlooked the effect of globalization on electoral politics[4], a number of recent studies have tried to establish the extent to which trade competition from low-income countries, which is amongst the main consequences of globalization, has effectively determined this political upheaval in Western countries, finding evidence of increased polarization in the US Congress (Autor et al., 2016), increasing vote shares for extreme-right and nationalist parties (Dippel et al. (2017), Malgouyres (2017), Colantone and Stanig (2017)), and a positive effect on the Leave vote share in the Brexit referendum (Colantone and Stanig, 2016).

In this work I extend this newly born literature to Italy, which represents an interesting case to study because of both its economy and its diverse political supply. Italy's manufacturing-heavy and export-oriented economy exposes it to supply shocks in low-income countries, and in fact globalization was seen as a direct threat by 39% of respondents in a 2017 survey;[5] on the other side, its multi-party sistem allows to explore variations in the vote-share of both extreme-right and extreme-left parties and also, more specifically, the impact of trade exposure on the rise of populist parties, which in Italy has began earlier than in other countries. This is done by using a very rich dataset including both general and European elections, popular referenda, online engagement with the populist Five Star Movement, and also different indexes that investigate different dimensions of the electorate's ideological leanings. The results show that while exposure to trade with China impacts local labour markets in the pre-crisis years

[1] https://www.politico.com/story/2016/06/full-transcript-trump-job-plan-speech-224891

[2] http://www.voteleavetakecontrol.org/our_case.html

[3] https://www.reuters.com/article/us-france-election-fn/frances-le-pen-launches-election-bid-with-vow-to-figh
il=0

[4] In his review of the literature on politics and globalization, Kayser (2007) wrote that "the sheer volume of literature in this area has made it easy to overlook an important fact: very little of it addresses the effect of economic globalization on actual politics, understood more narrowly as electoral politics."

[5] Demos (2017)

by increasing the overall share of unemployment and decreasing employment in manufacturing, it does not affect the vote-shares of extreme parties nor does it explain the increase in the degree of populism. This is in sharp contrast to all the previous studies focusing on different Western countries, raising questions on whether globalization, through trade exposure, has had a homogenous, disrupting impact on both the economies and political systems of developed countries, and in turn on the role that the lack of economic compensation to the "losers of globalization" may have had in causing the populist backlash.

In particular, I follow the approach pioneered by Autor et al. (2013), exploiting the exogenous productivity shock that has occurred in China across the last two decades. I then map the change in net imports between Italy and China, that occur at the national level and are divided by industry, to the Italian local labour markets, based on their historical patterns of sectoral specialization; in this way, local labour markets that have a relatively higher employment level in those industries that have witnessed the largest surge in net imports are the most affected by the trade shock. By exploiting this heterogenity across local labour markets in Italy, and using an instrumental variable approach to avoid internal demand shocks endangering the identification of the model, I am able to capture the causal effect of increased exposure to net import competition on a number of dependent variables that relate to different aspects of Italian politics.

The paper is structured as follows. Section 3.2 introduces the literature related to this work. Section 3.3 explains the identification strategy based on the Chinese productivity shock, while Section 3.4 presents background information on both Italian politics and trade. Section 3.5 introduces the data and the main summary statistics. Section 3.6 reports and discusses the main results. Section 3.7 concludes. Finally, the appendix contains the tables and graphs discussed in the main text.

3.2 Related Literature

This work is related, and contributes to, different branches of literature, and in particular to that on trade integration and politics. Originally, these works were mostly focused on cross-country studies aimed at capturing the effects of economic openness on national politics, or on studying the effect of trade exposure on individual preferences by exploiting either individual self-reported voting behaviour from surveys or legislators' voting records. The former found that greater economic openness, both commercially and financially, decreases turnout[6] and affects negatively economic voting[7], consistently with the idea that higher economic openness would constrain national policymakers, and thus reduce both the importance of voting and the weight of economic outcomes when evaluating the incumbent government. Works looking at the impact

[6]Steiner (2010) uses a cross-country study of OECD countries between 1965 and 2006 to conclude that higher economic openess reduces turnout. Marshall and Fischer (2014) find similar results for OECD countries between 1970 and 2007, extending the globalization measure to include financial openness. Steiner and Martin (2012) perform a cross-country study of 24 countries and conclude that higher financial openness reduces political polarization and this leads to lower turnout. Using individual-level data on the 2001 UK general elections, instead, Steiner (2016) concludes that those who feel that globalization reduces the government's influence on the economy are less likely to vote, especially if they are politically more left-leaning.

[7]Hellwig and Samuels (2007) study elections in 75 countries, finding that higher trade and financial openness reduces the impact of the economy on the incumbents' vote share.

of trade integration on individual preferences instead have found that it generally led to higher demand for protectionist tariffs[8] and more redistributive policies[9]. Recently, this literature has shifted its focus on capturing the effects of specific trade shocks, and in particular of the"China shock", which is how the dramatic increase in Chinese manufacturing productivity has come to be known in the trade literature, also following the key contribution by Autor et al. (2013).[10] The clearest advantage of focusing on individual, exogenous variations in trade is that it allows to capture the causal impact of import exposure. Works in this area originally focused on the US, finding that higher trade exposure may increase turnout[11] and political polarization[12], while the effect on support for the incumbents seems to be negative.[13], with evidence at the party level also being mixed.[14] In recent years the innovative approach first introduced by Autor et al. (2013) in their work on the US local labour market and later applied in Autor et al. (2016) on congressional electoral data, which captures heterogeneity in sub-national trade exposure based on historical employment composition and industry specialization, has been employed to study the political effects of trade exposure in Western high-income countries to low-income competitors.[15] Dippel et al. (2017), following the work by Dorn et al. (2014) on the German local labour markets[16], find that between 1987 and 2009 import competition leads to higher vote-shares for extreme right-wing parties, in the order of a 20% increase in the vote share for a one-standard-deviation increase in net exposure, and use mediation analysis in order to demonstrate that the effect is mostly explained by the local labour market dynamics. Malgouyres (2017)'s work on France and the extreme right-wing Front National party also finds that import competition from low-income countries has a radicalizing effect, in the order of an 8% increase in the change in the party's vote share for a one-standard-deviation increase in net exposure,

[8]Scheve and Slaughter (2001) find that, as predicted by the Heckscher-Ohlin model of international trade, factor type dominates industry of employment in explaining support for barriers to trade; see also Rodrik (1995) for a related and thorough survey.

[9]See Cusack et al. (2006) and Margalit (2013) amongst others.

[10]Exploiting the different within-industry specialization in local labour markets, they found that local labour markets hit hardest by the Chinese import competition suffered greater job losses in manufacturing and greater unemployment, in sharp contrast with workhorse models of international trade that suggest that any loss in employment in low-skilled sectors should be counterbalanced by employment gains in high-skilled-intensive sectors.

[11]Che et al. (2016) find that US counties hit hardest by the trade shock report higher turnout, using a diff-in-diff identification strategy based on the granting of a Permanent Normal Trade Relations status to China in 2008.

[12]Autor et al. (2016) look at the US congress between 2002 and 2010, after China's entry in the WTO, and conclude that local labour markets hit hardest by Chinese import exposure are more prone to elect more estreme congressmen of either side of the aisle.

[13]Autor et al. (2016) find some evidence of a decrease in support for congressmen of the governing party, while Margalit (2011) uses plant-level data on layoffs to find that trade-related job losses lead to lower support for the incumbent president. Jensen et al. (2016) instead define US counties as tradable or non-tradable based on their industry Gini coefficient, and find that higher concentration of economic activities in sectors with a comparative disadvantage, such as low-skill manufacturing, lower their support for the incumbent, and highlight the importance of work vulnerability in affecting voting choice. An exception is given by Feigenbaum and Hall (2015), who instead find no effect on the congressmen's probability of being re-elected or challenged.

[14]Che et al. (2016) find that, between 1992 and 2010, higher trade exposure increases the votes cast for Democrats and the probability of a Democrat being elected, in contrast to the findings of Autor et al. (2016) who fail to find any party effect.

[15]Other applications of this methodology to other fields include the work by Che et al. (2017), who find that counties with higher trade exposure experience higher crime rates.

[16]They find that import competition from China and Eastern Europe has a negative impact on local labour markets, as in the US, but that export access has an opposite effect

and that the effect is stronger between 2007 and 2012. Colantone and Stanig (2017) extend this work to 15 Western European countries between 1988 and 2007, even though they focus on regions at the NUTS-2 level rather than on the much smaller local labour markets of the studies previously mentioned. They find that net exposure to China leads to an increase in support for right-wing parties and a shift towards more nationalist and right-wing ideological positions, as measured through the center-of-gravity and median voter position indexes that rely on the Comparative Manifesto Project (Volkens et al. (2016)) that I also implement in the current work.[17] Lastly, Colantone and Stanig (2016) apply this methodology to the Brexit referendum, finding that a one-standard-deviation increase in import exposure between 1990 and 2007 leads to a 2% increase in the Leave vote share.

This work, which is closely related to the latter studies, contributes to the literature on trade and politics in three ways. First, by focusing on Italy, it widens the set of countries for which the relation between trade exposure and politics is specifically investigated, currently including USA, Germany and France, in addition to the Brexit referendum and Western Europe as a whole. Italy's multi-party system is in sharp contrast to the US' long-lasting bipolarism, and is more fluid and volatile than the ones in place in France or many other Western European countries, and therefore allows to consider whether and how the impact of trade exposure on voting is affected by a different political background. It is also the first study to apply the methodology introduced by Autor et al. (2013) to study the impact of trade exposure on populism, as measured through newly created indexes. Moreover, by including in the same framework results of both national and European elections, constitutional and abrogative referenda, data on membership to a specific populist party such as M5S, and the use of other indexes aimed at measuring the degree of populism, nationalism and right-left ideology of the whole political system, it allows to capture any variation occurring in Italian politics to a much larger degree than in the studies previously mentioned, which only included at most one of these measures. Lastly, by including elections up to December 2016, it also considers a larger time framework than previous studies, thus allowing to consider whether the impact of globalization on politics through trade has shifted in the post-financial crisis years.

This work is also related to the recent but growing literature focusing more specifically on populism. Both economists and political scientists have tried to isolate the drivers behind the rise in populist parties of recent years, with the former focusing on demand[18] while the latter have mostly taken supply factors into account[19]. The main recent contributions to this field include the work by Inglehart and Norris (2016), who focus on the demand side and find that cultural variables, such as anti-immigrant attitudes and mistrust for national and global governance, explain voters' behaviour more than economic variables, including occupational

[17] Another study focusing on the link between trade openness and right-wing populist parties at the European level is that by Swank and Betz (2003), who find no effect of trade exposure on these parties' vote share. An important difference is given by their measure of trade openness, given by each country's trade-to-GDP ratio, which weakens the causal interpretation of their result.

[18] Acemoglu et al. (2013) look into the causes for short-term protection demand by voters; Di Tella and Rotemberg (2016) propose that incompetent leaders are the result of betrayal-averse voters who want to minimize the chance of suffering from betrayal.

[19] See Norris (2005) and Golder (2016)

status and income, do. Guiso et al. (2017) tackle both demand and supply factors in their work on populist parties in Europe, with economic insecurity causing voters to be angry and demanding for more protection, and politicians claiming to be on the people's side against the elite, leading to an equilibrium outcome of more short-term protectionist policies. These three elements - voters' fear, the contrast between people and the elite, and short-term protectionist policies - are the criteria used in the current work in order to create a populism index for Italy, which they mention in their paper as a relevant "case study" because of the decreasing trust in institutions and increasing vote share for populist parties. Empirically they use the European Social Survey to find, in contrast to Inglehart and Norris (2016), that economic shocks have a significant and two-fold effect on voters' decision relatively to populism, since they decrease the voters' trust in parties and institutions, which in turn decreases their propensity to vote, while, for those voting, increasing the probability of casting a vote for more populist parties. They also find that increased exposure to globalization increases the supply of populism, and that mainstream parties adopt more populist agendas when such parties enter the scene, thus amplifying the aggregate supply of populism. Another relevant contribution is that by Rodrik (2017), who focuses explicitly on the link between globalization and populism, and concludes that the success of an anti-globalization message, both in its right and left-wing versions, is due to the unfavourable ratio between political and distributive costs, and net economic gains, together with a lack of global governance of this process, which lead to mass discontent and election results that could halt globalization as a whole. This work contributes to this growing literature by estimating the causal impact of a trade shock on the degree of populism in Italy, which has a politcal landscape marked by populist and anti-establishment parties and, as already mentioned in Guiso et al. (2017), is an interesting case to study. Moreover, I detach myself from the common approach focusing on individual surveys and self-reported voting, looking instead at actual changes in both vote shares and party manifestos. The results, indicating that trade exposure does not have an effect on the growth of populism, are in contrast with both Guiso et al. (2017) and Rodrik (2017), suggesting that exposure to trade and economic globalization may be secondary factors in explaining the rise of populism.

Lastly, this work is also linked to the literature on political economy in Italy, including works concerning electoral effects of other aspects of globalization such as immigration. Italy has long been used for empirical tests on its politics, and its politicians have been shown to be prone to implement strategically economic policies[20], while voters are electorally responsive to economic and social policies[21]. For what concerns immigration and Italian politics, Barone et al. (2016) have uncovered the role of immigration in shaping voter preferences, finding that immigration generates sizable increases in the vote share for the right-wing, and more immigration-averse, coalition, both at the local and national level, while also decreasing turnout and increasing protest votes. In a related work, Bracco et al. (2017) find that the election of mayors belonging

[20] Alesina and Paradisi (2015) shows that Italian mayors lower taxes before local elections, coherently with the Political Budget Cycle theory.

[21] Casaburi and Troiano (2016) provide evidence of electoral gains for mayors who implement more strictly a tax evasion program providing economic incentives to non-evaders, while Drago et al. (2016) find that voters are more likely to punish the incumbent government and vote for the rightwing coalitions in areas with the higher increase in crime rates following a collective pardon bill.

to the anti-immigration Lega Nord party reduces the presence of foreign immigrants. Current evidence thus seems to suggest that immigration is a politically salient topic, affecting voter preferences and inducing them to turn towards anti-immigration parties, and, in turn, that the political effects of these parties impact the presence of foreigners and thus the degree to which cultural globalization takes place in Italy. This paper contributes to this literature by focusing on the electoral effects of a specific economic shock, complementarily to the works on more cultural features of globalization.

3.3 Identification Strategy and the China Shock

Between 1990 and 2012, China's share of the world manufacturing value added grew from 5% to 24%, while its share of manufacturing exports grew from 2% to 12%.[22] Similarly, as seen in Figure 3.1, in Italy, Chinese imports went from being less than half of Italian exports to China in 1991, 0.9 versus 1.8 billions, to leading to a substantial Italian trade deficit of 6.8 billions in 2011, following a 17-fold increase of Chinese imports up to 17 billion euros.[23] The reasons for China's explosive trade growth in the last decades lie in its extraordinary increase in manufacturing productivity, which in turn has been determined by internal, political reasons rather than to global technological shocks. The affirmation of the reformist branch of the Chinese Communist Party in the early 1990's, after the tumultuous post-Tienanmen Square years in which doubts had been cast over the future of the Chinese market transition, led to the enhancement of internal reforms, such as the increase in the number of Special Economic Zones, and eventually to its entry into the WTO in 2001. The unexpectedness of China's economic reforms, together with the fact that its productivity improvement mostly represented a catch up following the distortions of the Maoist era, and coupled with China's comparative advantage in the manufacturing sector, all suggest that its trade expansion represents an exogenous economic shock, disconnected with contemporaneous shocks in high-income countries. (Autor et al. 2016) The identification of the Chinese trade rise as an exogenous supply shock, combined with its relative importance in the global market, has then led to numerous works focused on measuring its effects on the labour markets both in the US (Bernard et al. (2006), Autor et al. (2013), Acemoglu et al. (2016)) and in Europe (Dauth et al. (2014), Balsvik et al. (2015)).

In the current work I focus on the variations in trade between Italy and China in order to estimate the causal effect of a significant trade shock on the Italian local labour markets, and in turn on various political outcomes measured at the same geographical level. This is done by employing the methodology first introduced by Autor et al. (2013) who provide a theoretical model where they consider local labour markets as sub-economies, which are then subject to heterogeneous trade shocks according to the historical patterns of industry specialization. Empirically, this measurement approach consists in building indicators specific to each local labour market (in Italy defined as "Sistema Locale del Lavoro", SLL from hereafter) in order to quantify their exposure to trade. This measure accounts for both the increased import competition

[22]Source: World Development Indicators, (http://databank.worldbank.org/data/reports.aspx?source=world-development-indicators)

[23]Source: UN Comtrade Database. Amount normalized to 2005-constant euros.

and the access to added export markets could play a counterbalancing effect over a period of time is given by:

$$\triangle NetExposure_{Iit} = \sum_j \frac{L_{ijt}}{L_{jt}} * \frac{\triangle N_{Ijt}}{L_{it}} \tag{3.1}$$

where subscript I denotes Italy, i denotes local labour markets, j identifies the industry, and t denotes the time period. This measure maps the national shock, expressed by the change in net imports from Italy to China $\triangle N_{Ijt}$, to the various SLLs based on their industry composition, so that a sector will be weighted more if the SLL's national share of workers $\frac{L_{ijt}}{L_{jt}}$ in that sector at the beginning of period t is higher, while it will be weighted less the larger the SLL's overall workforce L_{it} is.

One concern about the above empirical strategy is that changes in imports and exports between Italy and China may be driven by domestic demand or supply shocks rather than to the Chinese supply shock, in which case $\triangle ImportExposure_{Iit}$ and $\triangle NetExposure_{Iit}$ could suffer from endogeneity bias, if the Italian shocks were also correlated to national electoral and political outcomes, or to labour market measures. For instance, a positive demand shock would increase imports from China, and therefore the trade exposure measure, while possibly inducing higher support for the incumbent government, resulting in a higher estimated impact of the trade shock on the electoral outcome even when not determined by the Chinese supply shock. In order to tackle this issue I follow the approach by Autor et al. (2013) and instrument Italian imports and net imports from China $\triangle M_{Ijt}$ and $\triangle N_{Ijt}$ with those of other similar high-income countries, from and to the same trading partner, defined respectively as $\triangle M_{Ojt}$ and $\triangle N_{Ojt}$. This set of countries closely resembles that chosen by Autor et al. (2013) and includes Australia, Canada, Finland, Japan, Korea, Norway, Sweden, and the United Kingdom, with some changes having to be made in order to exclude any member of the Eurozone, since its demand and supply conditions would likely be correlated to Italy's, violating the exclusion restriction assumption.

A last endogenity concern regards the possibility that anticipations on the Chinese trade shock may reflect on the current levels of employment, thus leading to a simultaneity bias. To address these concerns, initial employment shares in each sector j at time t are lagged by a decade to *t-1*. The instrument is thus defined as:

$$\triangle NetExposure_{Oit} = \sum_j \frac{L_{ijt-1}}{L_{jt-1}} * \frac{\triangle N_{Ojt}}{L_{it-1}} \tag{3.2}$$

3.4 Background Information on Italian politics

3.4.1 Italian institutions and electoral laws

The Italian Parliament is divided in a lower chamber, the *Camera dei Deputati*, and an upper chamber, the *Senato della Republica*. While a government needs to pass a vote of confidence on both chambers, the Senate is only elected by citizens who are 25 or older, so that in this work I concentrate on the results for the *Camera dei Deputati*, which is more representative of

the whole electorate. A legislature lasts for 5 years, but early elections may be called by the President of the Republic if there is no possible majority in the Parliament.[24]

Until 1993, a purely proportional system without explicit thresholds was in place. This was changed after the *Tangentopoli* corruption scandal exploded in the early 90's that redefined the whole Italian political system. From 1994 until 2001 a mixed majoritarian/proportional law, commonly known as the *Mattarellum*, was in place, with two thirds of deputies elected in uninominal seats and one third from the proportional vote given to parties, with a threshold set at 4%. Then the *Porcellum* electoral law was in place from 2006 to 2013, which granted proportional representation with a 4% threshold for individual parties and 2% for parties running in coalitions that received at least 10% of the votes, but its defining element was a majority premium granted to the coalition receiving the relative majority of votes for the chamber of deputies. While having relevant differences, both of these electoral laws favoured the formation of coalitions, in order to win more uninominal seats in the *Mattarellum* and to reach the majority premium in the *Porcellum*; this factor is going to be exploited when dividing mainstream parties from outsiders in the remainder of the paper.

A proportional system, instead, is still in place for European elections, occurring every 5 years, and with a national threshold set at 4%. By including the results of these elections it is thus possible to observe more "ideological" voting without any distortion induced by the existence of uninominal seats or pre-electoral coalitions.

3.4.2 The Italian political landscape

The Italian political system has historically been characterized by the presence of numerous parties from all sides of the political spectrum, including both radical right-[25] and left-wing parties[26]. At the beginning of the 1990's, the whole system was upset by the *Tangentopoli* corruption scandal, which led to the demise of all major parties, including the christian-democratic *Democrazia Cristiana* party that had been in government for almost 50 consecutive years, while the fall of the Soviet Union led to numerous fractures in left-wing parties, including the main opposition party, PCI. The main effect of this shock was to create a whole new party system with respect to before, with parties mostly cooperating through pre-electoral coalitions. In the period under consideration both center-right (1994, 2001, 2008) and center-left (1996, 2006) coalitions won general elections and governed the country. While more than thirty parties have contested each election in this time period, it is possible to divide the most important ones in few macro-categories, which I will also use in the next sections for the empirical results on the causal effect of the Chinese trade shock on the electoral outcomes.

The center-right field has been dominated by *Forza Italia*, the party founded by Mr Berlusconi in 1994, originally branded as the party of the "great liberal revolution", eventually it be-

[24] This occurred twice in the period under consideration: in 1996 and 2008, only 2 years after the previous elections. In two other cases, following the 1996 and 2008 elections, no early elections were called even though there was a change in the governing majority.

[25] The main one being the *Movimento Sociale Italiano*, which especially in the post-WWII years included many former members of the previous regime's Partito Nazionale Fascista.

[26] The Italian Communist Party, PCI, was more moderate than many of its European counterparties, but there were also many small parties to its left.

came a conservative party relying heavily on its leader. To its right there is *Alleanza Nazionale*, a post-neofascist party linked directly to the MSI, and which Mr Berlusconi contributed to bring to the mainstream. The *Lega Nord* party was also founded in the early 1990's and over time shifted from a northern regionalist pro-indipendence party to a right-wing populist party, with the stance against immigrants as one of its defining policy isues. There are then centrist parties, born out of the collapse of the *Democrazia Cristiana*, which over time have been in center-right coalitions and also ran independently in 2008 and 2013, with the most important party being *Unione di Centro*. In the leftist coalition, I consider the *Partito Democratico*, or PD, which is the result of the merge occurred in 2008 between the post-communist *Democratici di Sinistra* and the more left-leaning former members of DC, the *Margherita*. Both parties ran jointly for the Senate also before 2008, and were essentially seen as interchangeable coalition allies, so that I choose to consider them jointly for the whole analysis. I then aggregate all minor center-left parties who have entered coalitions with PD, which range from christian democrats to hardcore communists. Lastly, I focus on parties at the extremes of the political spectrum, both on the right and the left. The former are all minor parties [27] outside of the mainstream center-right coalitions and with extremely negative attitudes towards both immigration and globalization in general, which is seen as a threat to traditional values (Caiani, 2011). On the opposite side of the political spectrum are the radical-left, explicitly communist parties, which unlike the radical right-wing parties have not been marginalized and have also been part of governing center-left coalitions, but which are nonetheless very close to the no-global social movements, and critical of globalization. Lastly, there is the *Movimento 5 Stelle*, or M5S, which at the national level has only contested the 2013 general elections and 2014 European elections. In 2013 the Movement, founded by former comedian Beppe Grillo, won 25.5% of the votes, the highest vote share in Italian electoral history for a newly founded party, and has since then emerged as one of the most relevant actors in Italian politics in recent years. The Five Star Movement originated from Mr Grillo's blog and, especially in its early stages, relied heavily on the use of online platforms[28], benefitting from the spread of broadband access to internet (Campante et al. (2017)), and primarily voicing widespread discontent with the lack of transparency and honesty of mainstream parties and of institutions. Policy wise, the M5S has long refused to be framed on the typical left-right political spectrum, combining environmentalist and pro-welfare policies typical of left-wing parties, with anti-immigrant attitudes closer to those of right-wing movements. This, together with the anti-establishment sentiment it has had since its birth, and the blaming of globalization for the 2007 financial crisis, characterize the M5S as appealing to "losers" of globalization.

Overall, there are four main takeaways from the recent decades' Italian political landscape that are connected to the present work. The first one is that the *Tangentopoli* political shock led to a completely revolutionized party system, which favoured a more fluid electorate, not bound anymore to the parties that had governed the country for almost 50 years, which in turn should allow to observe more easily any changes in electoral outcomes induced by an external economic

[27] In the 22 years taken into account in this work, only one passed the 1% threshold.
[28] In particular, activists used *Meetup.com* to coordinate local activities and create thematic groups.

shock. The second one is the presence of parties from all extremes of the political spectrum, both inside and outside of the mainstream coalitions, with similarly negative views of the globalizing process from an economic point of view. The third one is the appearance of populist politics already in the 1990's due to Mr Berlusconi's innovative and controversial style[29], which in turn prepared the Italian landscape and electorate for the emerging of new populist parties such as M5S (Verbeek and Zaslove (2016)). Lastly, the presence of the Five Star Movement, with its anti-establishment rhetoric and wide electoral success, allows to consider whether trade exposure was determinant not only in increasing the vote share of fringe right-wing parties as in Dippel et al. (2017), but also that of populist parties that are more widely supported and with real chances at governing.

3.4.3 Abrogative and constitutional referendum

A peculiarity of Italian politics is the relevance attributed to both abrogative and constitutional referenda. Since 1946 there have been 70 abrogative referenda and 3 constitutional referenda, called in twenty different occasions, which over the course of the 20th century mobilized millions of voters in order to decide on issues such as abortion and divorce. In more recent years, turnout has dropped sharply, since abrogative referenda need 50% turnout in order to be valid, leading those who are against the abrogation of the law to abstain in order to maximize the probability of achieving their desired outcome. In this work I include four popular referenda called at different dates[30], three of which are abrogative. The first one, in 2009, concerned changes to the electoral law and only had a 23% turnout. Then, in 2011, four abrogative referenda called on the same day and with almost identical turnout, on topics ranging from the ban of nuclear energy and privatization of water distribution, to the abrogation of a judicial law personally related to Mr Berlusconi, leading it to be seen widely as a message against the sitting government. Lastly, in 2016 there were two referenda: an abrogative one in April aiming at banning oil drilling, which was widely supported by environmentalists and also those opposing the government led by PD's Matteo Renzi, and a constitutional one in December, which aimed principally at changing the balance of power between the Lower Chamber and the Senate, capturing the center of the stage for many months and recording an unexpectedly high 68% turnout, and whose defeat was also seen as a message against the sitting government and the political and economic establishment. Therefore, by including data on these referenda, I aim to capture protest, anti-establishment votes, which may be expressed more clearly through a yes or no vote than through votes towards a specific party at a general election.

3.5 Data

In order to construct the measure of trade exposure to China, I require data on bilateral trade flows between China and both Italy and the set of instrument countries, and also data on Italian

[29] Following his victory in the 2001 general elections, *The Economist* titled "The Triumph of Populism".

[30] Data on the other referenda occuring throughout this period is not available at the municipality or SLL level from any source.

employment by industry at the local labour market level. Data on trade is obtained from the UN Comtrade Database, which records it at the SITC Rev.2 commodity level, and all trade flows are converted from US dollars into 2005 euros using exchange rates provided by the US Fed. In order to match this data with that on employment, which is at the Nace Rev 1.1 industry level, I need to aggregate the trade data from the commodity to the industry level, which I do by using the correspondence tables provided by Eurostat[31]. Table 3.1 shows changes between 1991 and 2011 in the share of Italian imports from China as a share of overall imports for the ten most exposed industries, which all experience double-digits percentage increases.

Data on employment by industry at the Nace Rev 1.1 level by local labour market is obtained from the Italian Statistical Office (ISTAT). Italian *Sistemi Locali del Lavoro* are commuting areas comprising geographically contiguous municipalities, which are designated by ISTAT based on their characteristics.[32] Municipalities from a given labour market may belong to different provinces, but never to different regions; nonetheless, because of SLLs spread across different provinces, the regressions' fixed effects have to be set at the regional level.[33] Istat periodically redefines local labour markets; I focus on *Sistemi Locali del Lavoro* as defined in 2001, which includes 686 different SLLs, with an average of around 14 municipalities per local labour market. The number of SLLs is sufficiently high to avoid losing any statistical power, and is also similar to that used by Dippel et al. (2017) and higher than those of Colantone and Stanig (2016, 2017). All control variables relative to the socio-demographic and economic structure of the SLLs (which include the share of the working force who are employed in manufacturing, the share of foreigners, the share of women, the share of workers with an university degree, and the share of people above 65 years of age), are also taken from Istat and from the *Atlante Statistico dei Comuni* (Municipalities' Statistical Atlas). Table **??** provides summary statistics about the local labour market's economic variables that will be amongst the y_{it} in next section's regressions.

Table 3.2 summarizes the distribution of the net import shock across SLLs over the two main time periods taken under consideration, which are 1991-2001 and 2001-2011. The choice of these two time frames is done in order to clearly divide the years before and after China's entry into the WTO, which contributed to the further propagation of its productivity shock through lower barriers to trade and in turn higher Chinese exports to Italy; moreover, by focusing on a ten-year timespan, the results are comparable to those obtained by others in the literature. In addition to these reasons, focusing on these two timespans is also convenient given the nature of electoral outcomes: differences in the vote share of parties are computed every 10 years for European elections but every 5 or 7 years for national elections, and data on populist and ideological indexes and on popular referenda is observed in levels rather than in differences, so

[31] More specifically, the correspondence tables are on RAMON (Reference and Management of Nomenclatures), which is Eurostat's metadata server.

[32] Because the areas are designed according to commuting habits, there are no worries over possible differences between the resident and commuting populations, as instead was the case for Dippel et al. (2017) and Malgouyres (2017).

[33] There are 20 regions and 107 provinces in Italy, each province being fully enclosed within a region. Regions have stronger administrative powers, while a provinces' responsibilites mostly concern road maintenance and the schooling system.

that I apply the 1991/2001 shock to all electoral outcomes that are set before 2001, and the 2001/2011 measure to all political variables in following years.

The two tables show that the effect of the Chinese shock is much larger in the post-2001 period, with the net exposure of the median SLL equal to 18.46 euros per worker versus the 4.26 euros per worker of the previous decade. Moreover, the spatial distribution of the shock is highly unequal in both decades, as shown by standards deviation of respectively 14.21 and 37.63 euros per worker (versus average import exposure of 7.76 and 28.74). Figures 3.2 and 3.3 show the geographic distribution of the import shock across SLLs for both of the time periods, and in particular how, even though the most exposed areas are mostly concentrated in Northern Italy, there is variation in exposure to the trade shock across both SLLs and between periods. The instrument variable presents, for both periods, high correlation with the net exposure measures, equal to 0.44. The first-stage results from the regressions in the next section will further reject the hypothesis of weak instruments.

The choice of focusing on the 2001-2011 timespan has one main downside, which is that of including both pre- and post-financial crisis years within the same measure. In order to avoid focusing only on the years before 2007, which would greatly reduce the available political data, and to take into account the possible changes occurring in trade exposure because of the crisis, I also decompose the 2001-2001 into two sub-periods, one ranging from 2001 until 2007, at the very start of the financial crisis, and another one from 2007 to 2011.[34] Table 3.3 focuses on these sub-periods and confirms that exposure to the net trade shock is much larger in the pre-crisis years. Therefore, by using these additional timespans, it's possible to understand whether results on both local exposure to the trade shock and its effect on the electoral outcomes between 2001 and 2011 are purely driven by the post-crisis period. In particular, I will apply both the 2001/2011 measure and its two sub-periods to all political variables that follow those years.

Electoral data on national and European elections and on popular referenda is available from the Ministry of Interior. Between the beginning of the *Second Republic* and today there are 6 general elections (1994, 1996, 2001, 2006, 2008, 2013) and 5 European elections (1994, 1999, 2004, 2009, 2014). Electoral outcomes include change in turnout and in the share of spoiled and blank votes, the change in the the vote share for the individual parties and for all incumbent parties, and the change in vote for mainstream and outside parties. Tables 3.5 and 3.6 list the parties included in each category. Summary statistics on the electoral outcomes, which are the independent variables in the regressions presented in the next section, are shown in table 3.7 and 3.8 for general and European elections respectively. Differences are computed over a period of 10 years for European elections and 7 years for national elections (except for 2008 and 2013). Changes in the share of incumbent governments instead are only computed between consecutive elections, given that the electoral backlash againt incumbents, if present, would occur at the first election after they have been in government.

I also include data on turnout for the 2009, 2011 and 2016 abrogative referenda, and on the

[34] As a robustness check, I also experimented with a trade exposure measure for the period from 2007 to 2015, but both its distribution and the results from the next section are very close to those obtained with the 2007/2011 variable. I prefer to use the latter in order to remain consistent with the sub-division of the 2001/2011 period in pre- and post-2007 years.

"No" vote share for the 2016 constitutional referendum. Popular referendums were also held in 7 other occasions before 2009, but data is only available at the province rather than municipal level, therefore not allowing to aggregate at the SLL level. Lastly, data on membership to the online *Meetup.com* groups is also included, for the years before 2005 and 2012, following Campante et al. (2017). Summary statistics are provided in table 3.10.

3.5.1 Ideological measures

In order to have an additional measure of the SLLs' ideological and populist leaning, I also use data from the Comparative Manifesto Project (Volkens et al. (2016)), which provides human coding of the manifesto of parties for each general election. In this way, I am able to create 3 different indexes at the local labour market level, following Colantone and Stanig (2017), in order to define the SLLs' positioning in terms of populism, nationalism and on the standard left/right divide. With respect to the parties' vote shares, this offers an additional instrument in order to capture any political effect caused by the trade shock.

The first party positioning score refers to nationalism, where I follow Lowe et al. (2011) and, for each party p and election year t, compute:

$$Nationalism_{pt} = log(0.5 + n_{pt+}) - log(0.5 + n_{pt-}) \tag{3.3}$$

with n_{pt+} being the number of claims in a nationalist direction, such as a negative view of multiculturalism and a positive view of the national way of life, whereas n_{pt-} are claims contrary to these positions. [35]

A similar index is used to refer to the classical left-right position, given by:

$$RightPositioning_{pt} = log(0.5 + r_{pt+}) - log(0.5 + r_{pt-}) \tag{3.4}$$

where r_{pt+} and r_{pt-} are claims respectively close and far from right-wing positions, as identified by Laver and Budge (1992).

Following these two indexes, I also create a novel index measure of populism:

$$Populism_{pt} = log(0.5 + s_{pt+}) - log(0.5 + s_{pt-}) \tag{3.5}$$

with s_{pt+} and s_{pt-} being claims favourable and contrary to the populist views, which are identified following Guiso et al. (2017)'s approach. They focus on voters' fear, contrast between people and the elite, and short-term protectionist policies as the defining elements of modern populism, so that I define as more populist views that are negative on EU integration and internationalism, and positive views on anti-imperialism, direct democracy, protectionism, nationalism and welfare state expansion.[36].

Then, I combine these scores with electoral data from each labour market, in order to create

[35] In particular, n_{pt+} refers to claims coded as 601, 603, 605 an 608, whereas n_{pt-} refers to 602, 604 and 606.

[36] s_{pt+} refers to claims coded as 103, 109, 110, 202, 304, 406, 409, 413, 504, 601, 608, while s_{pt-} refers to codes 107, 108, 407, 414, 602, 607

three different measures of each SLL's ideological leaning. They are the SLL's ideological center of gravity, the ideological position of the median voter, and the aggregate share of votes for parties above the national median position.

The ideological center of gravity corresponds, for each SLL, to the average of the party's ideological scores weighted by their vote share obtained in the SLL:

$$CenterOfGravity_{it} = \frac{\sum_{p=1}^{n} v_{pit} Score_{pt}}{\sum_{p=1}^{n} v_{pit}} \tag{3.6}$$

where v_{pit} corresponds to party p's vote share in local labour market i at period t, and n is the number of parties for which manifesto scores are available at time t.

A second measure is given by the ideological position of the median voter. This is obtained by ranking all parties according to their ideological score (from most populist, nationalist or right-wing to least), cumulating their vote shares, and looking at the ideological score of the median voter.

A last measure is given by the vote share of parties with ideological scores above the national median; in this case, all parties are ranked based on their ideological score, as explained above, and then one observes the vote share at the SLL level of parties with positions above the median.

These three indexes provide a complementary approach with respect to the raw parties' vote shares, since they all capture different aspects of variations in the ideological distribution induced by a trade shock. In particular, the center of gravity is an index that is sensible to shifts in the whole distribution of scores and vote shares, and can be heavily affected also by changes limited to one extreme party. Viceversa, the other two measures capture changes occurring at the center rather than at the extremes of the electorate, which could theoretically leave the measure unaffected.

Table 3.9 shows the mean and standards deviation for each measure and for each election. Since manifestos are only available for general elections, for European elections I attribute to each party their manifesto score from the closest general election.[37] The table shows how the populist average center of gravity gradually increases over the years, as does the median voter's populist position, which is in line with the background information on Italy presented in section 3.4. On the other hand, the nationalism measures show no clear-cut trend, whereas the measures on right-left positioning steadily decrease over time, implying a leftward shift in the political party system.

3.6 Results

For regressions on electoral and economic outcomes, I estimate the following equation:

$$\triangle\, y_{it} = \alpha_1 + \beta_1 * \triangle NetExposure_{Iit} + X'_{it}\gamma + \theta_r + \epsilon_{it} \tag{3.7}$$

[37] For the 1999 election, the 2001 scores are used; for the 2004 EU election, 2006 scores are used; for the 2009 EU election, 2008 scores are used; and lastly, 2013 scores are used for the 2014 EU election.

where $\triangle y_{it}$ refers to either changes in local labour market economic variables, in electoral turnout, or in the parties' vote share, and $\triangle NetExposure_{Iit}$ is the measure of net exposure to the Chinese import shock, as described in equation (1). X'_{it} is a set of economic and demographic controls relative to the share of workers employed in manufacturing, the share of unemployed, and the share of people over 65 years of age, women, foreigners and people with university degrees in the workforce, all referring to the beginning of the period. θ_r denotes the fixed-effects at the level of administrative regions r, while ϵ_{it} is the error term. The use of regional fixed-effects means that I identify the effect of the trade shock only from changes occurring across local labour markets located in the same region; because some SLLs are inter-provincial, the standard errors also have to be clustered at the regional level. In order to account for the possible endogeneity of $\triangle NetExposure_{Iit}$, I instrument it with $\triangle NetExposure_{Oit}$, as defined in equations (2).

When using the ideological measures described in section 3.5.1, instead, the following equation is estimated:

$$y_i = \alpha_1 + \beta_1 * \triangle NetExposure_{Ii} + X'_i\gamma + \theta_r + \epsilon_i \tag{3.8}$$

with the dependent variable y_i being in levels rather than in differences, and where I attribute the shock measure $\triangle NetExposure_{Ii}$ that is closer to the period in consideration.

3.6.1 Import Shock and Local Labour Markets' Economies

In the first place, I focus on the economic impact that the Chinese trade shock had on the local economies, in order to establish whether it has caused labour market adjustments, which in turn could induce changes in the political outcomes. To do this, I consider five different economic outcomes that describe the main variations that can occur locally. Table 3.11 displays the results of both OLS and IV regressions across the two main periods, 1991/2001 and 2001/2011, and for the latter also considers the net shock in sub-periods 2001/2007 and 2007/2011. The first regressions concern the share of workers employed in manufacturing, which should be the most exposed ones to the trade shock, since the Chinese productivity shock affected manufacturing first and foremost. The OLS results from the 2001/2011 period suggest a negative impact on manufacturing employment, but the effect loses significance in the IV specification. When looking more specifically at the shock in the two post-2001 sub-periods, the shock emerges as highly significant in both timespans, although with opposite effects. Between China's entry in the WTO and the beginning of the Great Depression, the shock has a negative effect, as a one-SD increase in the net import shock causes a fall in manufacturing employment equal approximately to 1% (12.32 *-0.00067) , or 5% of the average change between 2001 and 2011. Viceversa, the effect of the shock in the post-crisis years cancels out this impact, explaining why there appears to be no overall impact of the trade shock on manufacturing when looking at the 2001/2011 decade as a whole.

The shock does not seem to affect the growth of overall employment, as seen from the regressions in the second row on the log change in employment, while the effect on the local unemployment rates resembles that described above for manufacturing employment, as the post-

crisis years effect cancels out the negative impact observed in the previous sub-period. In particular, when looking at the net shock between 2001 and 2007, a one-SD increase in trade exposure leads to a 5% increase in the average change in the local labour markets' unemployment rate (equal to 3%), in line with the prior given the nature of the shock and results by previous similar works. The effect, though, is fully counterbalanced by the post-crisis years, leading to an overall statistically insignificant effect over the 2001/2011 period.

Results on the effect of the trade shock on migration, measured by the change in the log of the SLL's population, are included to account for possible increased mobility induced by the shock on the most exposed voters, but show no statistically significant effects. I also look at the effect of the net trade shock on the composition of the workforce by skill, as measured by the change in the share of high-skilled workers, and the result is positive only for the 2001/2011 period, but is very small and barely statistically significant at the 10% threshold, so that the last two regressions suggest that there has been no compositional effect of the shock on local labour market's workforces.

These preliminary results on the economic effect of the trade shock on local labour markets suggest therefore that there has been a negative, even though not very large, effect on employment in manufacturing and on the general unemployment rates, but that this can only be captured by focusing on the pre-crisis 2001/2007 period rather than on the 2001/2011 decade. This is in line with Dauth et al. (2014) and Dippel et al. (2017), who find that the shock disrupts local labour markets in the 1998/2009 decade.

3.6.2 Electoral outcomes

The results above suggest that the Chinese trade shock has had a disruptive effect on employment in Italian local labour markets, although with significant heterogeneity over time. I then turn on the main object of the current work, which concerns the political repercussions of the Chinese shock. To this end, I first focus on what might be defined as *protest measures*, which include the change in turnout, the change in the sum of blank and spoiled ballots, and the change in support for incumbent governing parties. The common trait of these variables is that they reflect general disappointment by voters and are not linked to ideological aspects, since voters may react to the shock not through increased support for more radical parties but also through decreased political participation (Guiso et al. 2017) or lower support for governing parties that may be held responsible for the economic shock. Table 3.12 shows the results on the change in turnout and in the sum of blank and spoiled ballots over total votes. The F-Test statistics suggest that weakness of the instrument is not a worry as they are safely above common threshold values. The results instead show that for both variables, and for both general and European elections, increased exposure to the net trade shock does not lead to a decrease in participation, through either lower turnout or higher spoilt or blank ballots. In fact, while most coefficients are of the expected sign, negative for turnout and positive for protest ballots, no regression for either election or sub-period shows any statistically significant result. Table 3.13 presents results for regressions on the vote share of all incumbent parties. In this case, the change in the vote share of incumbents is different from the previous variables on two dimensions, since it is limited to

consecutive general elections, since vote on incumbent governments is more accurately measure at the end of the governments' mandate rather than halfway through it, as would be the case if European elections were included. Also in this case there are no warnings of possibly weak instruments, and results show that between 1996 an 2001, and 2001 and 2006, the trade shock has no impact on vote shares of incumbent parties, and the coefficients' positive signs are in contrast with the prior. Between 2008 and 2013, instead, the coefficient of the net exposure measure reduces the vote share of incumbent parties, and the effect is mostly driven by the post-crisis period. Nonetheless, the effect is extremely limited in size, as a one-SD increase in the trade exposure shock for the 2001/2011 decade only leads to a decrease in support for incumbents equal to 0.3% its decade-average change.

I then turn to the effect of the net import shock on the party vote shares. In table 3.14 shows the results of the regressions on the vote shares of all party groups, as defined in table 3.5. The table includes some blank cells since not all parties ran in the elections included in the sample; for example, the the Five Star Movement first ran nationally in the 2013 general elections. The results show that no party has systematically benefited or suffered from the trade shock, as confirmed for different elections and different shock-periods. Amongst the few, significative results, even though only at the 10% confidence level, there is the negative effect on *Forza Italia*'s vote share between the 2004 and 2014 general elections, with a one-SD increase in net exposure leading to a fall in support for Berlusconi's party equal approximately to 1% (37.63*0.00023), which is roughly one third of its average change in vote share over that decade, and as for previous results the effect is mostly driven by the post-crisis years. The only other (barely) significant results concern the centrist parties, which benefit from increased exposure to the trade shock between 2001 and 2008, and 2008 and 2013. This seems counterintuitive, as centrist parties are very much in line with the political and economic establishment, and with no real stance on globalization. When looking at the magnitude of the centrists' results, though, the impact appears to be negligible, as a one-SD increase in net exposure (measured between 2001 and 2011) increases their support between 2001 and 2008 by 0.5%, equal to 18% of their average change in vote share between the two elections; for the 2008/2013 elections, instead, the effect is equal to 0.6%, or 13% of the average change in support.

The support for more ideologically extreme parties, both on the right (*Lega Nord* and extreme-right parties) and on the radical-left, and also the new populist parties such as M5S, does not appear to be influenced by increased net exposure of SLLs to the Chinese trade shock. This is consistent across different specifications looking at both general and European elections, and across different time-periods for both elections and shocks. This suggests that no single party benefited, or suffered, from increased exposure to globalization. In order to understand whether the effect is only noticeable on mainstream parties as a whole rather than individually, I also include the change in the vote share of both mainstream and outsider parties, as defined in table 3.5, that is, dividing parties belonging to the main coalitions (right, center and left) and those outside of said coalitions. Nonetheless, also in this case there seems to be no effect of the trade shock, as support for mainstream parties remains unaffected by increases in net exposure.

Even though the shock seems to have no effect on the vote share of parties, it could be

changing the Italian political landscape through a change in the ideological leanings of both parties and voters, through a combined demand and supply effect. For instance, while the shock might not be leading the electorate to vote more for a given party, for instance M5S, different parties could be increasing their degree of populism, also in response to M5S's entry in the "party market", and voters could be rewarding these parties as a whole. In order to capture this possible mechanism, I run regressions using the indexes presented in table 3.9 as the dependent variables, and the results are presented in tables 3.15, 3.16 and 3.17.

First, I consider regressions looking at the impact of the trade shock on populism, based on anti-establishment and trade protectionist views, rejection of multiculturalism and support of direct democracy, using the indexes described in section 3.5. The first column of table 3.15 shows results relatively to the populist center of gravity, and all coefficients are not statistically different from zero. The median voter's populist position, on the other hand, appears to increase with net exposure to the trade shock in 1994, 1996, 2001 and 2009, while decreasing in 2004. The magnitude of these effects is mostly limited, though, as a 1-SD increase in the net import shock leads to increases between 1% and 8% of the center of gravity's average. The effect is more meaningful in 2009, when it leads to an increase equal to 48% of the index's mean, and in 2004, when instead the shock lowers the center of gravity by 18% of its average, with both coefficients significantly different from zero at the 10% confidence level. These results suggest that there has been a positive, yet limited, effect of the net trade shock on populism's success, and that this has been induced by shifts in the center of the electorate rather than in the whole of its distribution, given that the center of gravity index remains unaffected. The third set of regressions, where the dependent variable is the vote share of parties with populist views above the median level, presents no statistically significant coefficients, in contrast to this view. Evidence of an impact of the trade shock on the degree of populism of the Italian political landscape, both in terms of parties and voters, appears mixed at best, since the effect concerns center segments of the electorate, but is limited in size and measured only by one of the two dedicated indexes.

Table 3.16 shows the results of regressions on the measures of nationalism, which overlaps only partially with the above definition of populism. Results show that the nationalist center of gravity remains unaffected by the net import shock, with none of the coefficients statistically different from zero. The median voter's position relative to nationalism instead is affected by the net exposure to the trade shock, as shown by results in the second row, as higher trade exposure leads to more nationalistic position by the median voter in 1996, 2001 and 2008, with magnitude, measured as in previous regressions, ranging between 2 and 3% of its average. The effect is instead reversed in 2013, causing a 4% decrease in the median voter's nationalism. The vote share of more nationalistic parties remains instead unaffected by the trade shock, in line with regressions on populism. Evidence, therefore, suggests a limited impact of trade shock on nationalist positions, as areas that are more exposed to the net trade shock experience a persistent while small effect only in one of the three measures, rejecting the hypothesis of changes in the tails of the ideological distribution.

Lastly, table 3.17 shows results relatively to the positioning of parties on the right/left ideological scale. Only two regressions report significant results: the median voter's position in

1996 with a rightwards shift of 4% according to the previously defined magnitude measurement, and a leftward increase in the center of gravity by 4% in 2004. These two results suggest that the impact of the shock along the standard ideological divide is even smaller than that on the two other ideological dimensions, as there is no marked increase towards neither of the right and left extremes.

Overall, evidence on the effect of the net trade shock on these indexes is, at best, mixed. The only impact seems to be on the median voter's position, implying a populist and nationalist shift at the center of the electorate, but the magnitude is modest, and the effect loses its statistical significance when considering the vote share of parties with above-median populist and nationalist views. This is in contrast to Colantone and Stanig (2017), who find robust and large effects of the Chinese import shock when looking at nationalist and right-wing parties for the whole of Western Europe.

3.6.3 Trade shock and other forms of political participation

In order to assess whether the trade shock has influenced the Italian political landscape in ways that differ from those considered above, which are limited to general and European elections, I investigate the effect of increased net exposure to Chinese imports on other forms of political participation. In particular, I focus on the participation in popular referenda, and on online membership to the Five Star Movement meetup groups, following Campante et al. (2014)[38]. As described in section 3.4, popular referenda are a peculiarity of the Italian system, since they have been regularly called for more than five decades on a number of topics. In particular, in recent years, abrogative popular referenda have seen the general opposition of some, if not all, the mainstream parties, so that they provide a form of political participation to those who are at the margins of mainstream politics, but may still not identify themselves in either of the more radical or populist parties. I consider four popular referenda, three of which are abrogative and one constitutional. For the abrogative referenda, I focus on the turnout share, since they require a 50% turnout in order to be valid, and most of those opposed to the abrogation of the law in question are inclined, and often invited to by the parties opposed to the referenda asked voters not to participate[39], not to vote, since it gives them a higher probability of achieving their desired outcome than voting "No" would. The 2016 Constitutional referendum, instead, did not have any minimum threshold, and while the majority of parties, both mainstream and not, opposed it[40], support for "No" was widely seen to be an opposition to the establishment as a whole. Therefore, the dependent variables that I use are the overall turnout in the abrogative referenda of 2009, 2011 and 2016, and the vote share against the constitutional referendum of 2016, since they are all measures of political participation going beyond the party level. Results, shown in table 3.18, indicate that net exposure to the trade shock does not lead to increased

[38] They use the data in order to measure the impact of access to broadband on political participation, and only consider popular referenda up to 2011.

[39] In 2009 only PD explicitly supported the referendum. In 2011, Mr Berlusconi's party openly opposed the referendum, while PD and left-wing parties supported it, together with all parties with no parliamentary representation. In 2016, the governing PD and centrist parties were the only ones to openly call for voters to abstain, while FI did not take a stance.

[40] Only PD and its centrist governing allies were openly in favour of the referendum.

participation in the popular referenda as a form of protest against the political and economic establishment. All coefficients relatively to the 2001/2011 shock are negative, and the only ones that are statistically different from zero at the 10% confidence level refer to the pre- and post-crisis sub-periods for the 2009 referendum, and their magnitude is very limited in size, as a one-SD increase in exposure leads to respectively to a 3% increase and decrease in turnout with respect to its average. The fact that there is no effect in the most recent, and more anti-establishment[41], popular referenda suggests that trade exposure has not caused an increase in political participation through this channel.

An additional channel through which the trade shock could affect political participation is direct involvement in the activities of more populist parties. To this end I focus on the Five Star Movement, which as mentioned in section 3.4 was born as an online grassroots movement on the *Meetup.com* platform, where activists could coordinate their activities to be performed at the local level. The dependent variables are the number of meetup members between 2005 and 2012, and the number of days since the formation of the first group. Results are shown in table 3.18, and indicate that the net trade shock has not increased involvement in the meetups of the Five Star Movement, as all coefficients are not significantly different from zero, therefore refuting the hypothesis of increased direct political participation in anti-establishment parties caused by increased exposure to the trade shock.

3.6.4 Discussion

The results presented in this section do not align with the related literature on the political effects of trade shocks. While the increase in the relevance of Chinese imports is significant and comparable to that of other Western European countries studied by Dippel et al. (2017), Colantone and Stanig (2017) and Malgouyres (2017), the political repercussions of the trade shock appear to have been pretty limited. The vote share of almost all parties remains unaffected by changes in net trade exposure throughout the last two decades, and so does the degree of protest voting and the share of turnout, and alternative forms of political engagement. The only significant results concern the increasing populist and nationalist positions taken by the median voter, but are limited in size and not confirmed by the second measure used to assess shifts in the electorate's center. When put together, these pieces of evidence suggest that increased exposure to the trade features of globalization has not contributed to the success of populist parties and the emerging of populist views in the political landscape, nor has it caused a fall in support for mainstream parties, neither individually nor collectively.

3.7 Conclusion

The surge of populist parties, with agendas based on protectionism and opposition to an open society, has led to reconsider the effects of globalization in developed countries. This work joins the recently developed literature focusing on the causal effect of trade exposure, as measured by

[41] This view was widely held both in Italy and abroad; for instance, before the 2016 constitutional referendum, an article by The Economist was titled "After Trump, Italy could be the next anti-establishment revolt".

the Chinese trade shock, on politics, but its findings are in stark contrast to the related works, as confirmed by the numerous regressions exploring different dimensions of the Italian political landscape and its more radicalized and populist aspects, in the more thourough empirical study to date on the link between trade shocks and populism and ideological radicalization.

Two main conclusions can be drawn: the first is that the political effect of the trade shock induced by China, and representative of the economic changes brought by gloalization, is not homogeneous across countries. While recent works both in the US and in Western Europe have found evidence of increased polarization or support for radical parties caused by the trade shock, Italy appears to be mostly unaffected, therefore raising questions on whether the changes brought by free trade and Chinese import competition can be held responsible for the recent populist backlash also in countries that have not been directly studied. The second key finding is that the roots of the surge in populist politics in Italy are not purely economic, since a very large trade shock appears to be insignificant relatively to most measures of political change and radicalization. This suggests that cultural factors, such as those concerning immigration (Barone et al. (2015), Bracco et al. (2017)), may play a larger role in feeding populism rather than the openness of Italian economy and its exposure to low-cost import competition, in line with Inglehart and Norris (2016)'s cross-country results on populism, therefore calling for further research on this topic.

3.8 Appendix

Table 3.1: Share of Italian imports from China over total imports

Industry	Share in 1991	Share in 2011	Change 1991/2011
Manufacture of made-up textile articles	7.3%	59.5%	52.2%
Manufacture of fertilisers and nitrogen compounds	0.2%	33.1%	32.9%
Preparation and spinning of textile fibres	1.3%	29%	27.7%
Manufacture of instruments for measuring and navigating	0.7%	25.8%	25.1%
Manufacture of steam generators	0.3%	22.6%	22.3%
Manufacture of other textiles	1.2%	22.5%	21.3%
Manufacture of articles of fur	0.7%	21.6%	20.9%
Manufacture of machine-tools	1139	21.1%	20.6%
Manufacturing N.E.C. (toys, furniture, etc.)	3.4%	21.7%	18.3%
Textile weaving	1.4%	15.4%	14%

Table 3.2: Distribution of the net shock across SLLs and over time

1991/2001		2001/2011	
Percentiles	Net Exposure	Percentiles	Net Exposure
5th	0.99	5th	3.92
25th	2.67	25th	11.05
50th	4.26	50th	18.46
75th	7.77	75th	31.68
95th	24.96	95th	85.01

Table 3.3: Net shock distribution across SLLs for the two sub-periods

2001/2007		2007/2011	
Percentiles	Net Exposure	Percentiles	Net Exposure
5th	2.44	5th	0.23
25th	7.65	25th	1.72
50th	14.12	50th	3.32
75th	24.28	75th	6.81
95th	69.86	95th	23.33

Table 3.4: Summary statistics for economic outcomes and control variables

Economic Outcomes						
	1991/2001			2001/2011		
Variable	25th percentile	Median	75th percentile	25th percentile	Median	75th percentile
$\triangle ManufacturingShare$	-0.039	-0.010	0.020	-0.225	-0.162	-0.114
$\triangle LogEmployment$	0.020	0.063	0.113	0.042	0.087	0.130
$\triangle UnemploymentShare$	-0.009	0.001	0.021	0.017	0.031	0.044
$\triangle LogPopulation$	-0.046	-0.006	0.030	-0.029	0.018	0.061
$\triangle HighSkilledShare$	0.023	0.029	0.034	0.022	0.027	0.032

Table 3.5: Aggregation of political parties

Party	1994/2001	2001/2008
Partito Democratico *(PD)*	PDS, AD, Margherita	PDS, Margherita, PD
Forza Italia *(FI)*	FI	FI
Alleanza Nazionale *(AN)*	AN	AN
Lega Nord	Lega Nord	Lega Nord
Minor center-left parties	Rifondazione('94),Verdi,PSI,Rete,PDCI	Verdi,PDCI,IDV
Centrist parties	CCD/CDU	CCD/CDU, UDC
Extreme-right parties	Fiamma Tricolore,Forza Nuova	Fiamma Tricolore, Forza Nuova,Destra
Far-left parties	Rifondazione, PDCI	Rifondazione, PDCI, Sinistra Arcobaleno, Sinistra Critica, PCDL
Outsider parties	Rifondazione,Lista Pannella-Sgarbi,small parties; IDV('01)	" + Sinistra Arcobaleno,UDC,Destra,small parties

Table 3.6: Aggregation of political parties (bis)

Party	2008/2013
Partito Democratico *(PD)*	PD
Forza Italia *(FI)*	PDL
Alleanza Nazionale *(AN)*	PDL
Lega Nord	Lega Nord
Minor center-left parties	IDV ('08); SEL,CD
Centrist parties	UDC
Extreme-right parties	Destra, Forza Nuova
Far-left parties	Sinistra Arcobaleno, Sinistra Critica('08); PCDL, SEL, Rivoluzione Civile
Outsider parties	Sinistra Arcobaleno,UDC,Destra('08); M5S, Rivoluzione Civile,Fare

Table 3.7: Summary statistics for election outcomes in general elections

	Protest Measures								
	1994/2001			2001/2008			2008/2013		
Variable	25th percentile	Median	75th percentile	25th percentile	Median	75th percentile	25th percentile	Median	75th percentile
$\triangle Turnout$	-0.026	-0.015	0.004	-0.038	-0.028	-0.012	-0.087	-0.058	-0.043
$\triangle SpoiledBlankVotes$	0.003	0.009	0.027	-0.002	0.002	0.008	-0.011	-0.002	-0.005
	Parties								
△PD Vote Share	-0.053	-0.092	0.128	0.004	0.023	0.042	-0.108	-0.078	-0.046
△FI Vote Share	0.056	0.080	0.114	-0.340	-0.292	-0.246	-0.043	-0.035	-0.027
△AN Vote Share	-0.049	-0.001	0.020	-0.142	-0.112	-0.086	-0.043	-0.035	-0.027
△Lega Nord Vote Share	-0.073	0	0	0	0.008	0.059	-0.061	0	0.001
△Minor Center-Left Parties Vote Share	-0.111	-0.087	-0.065	-0.012	-0.002	0.008	-0.014	-0.004	0.009
△Centrists Parties' Vote Share	0.023	0.034	0.051	0.013	0.021	0.033	-0.051	-0.037	-0.028
△Extreme-Right Parties' Vote Share	0	0	0.009	0.013	0.022	0.032	-0.020	-0.014	-0.008
△Far-Left Parties Vote Share	-0.007	0.004	0.014	-0.034	-0.023	-0.014	0.001	0.011	0.024
△Mainstream Parties' Vote Share	0.049	0.090	0.144	-0.036	-0.012	0.012	-0.176	-0.135	-0.092
△Outsider Parties' Vote Share	0.078	0.097	0.124	-0.012	0.012	0.036	0.103	0.146	0.188
	1996/2001			2001/2006			2008/2013		
△Incumbents' Vote Share	0.030	0.006	0.021	0.009	0.029	0.052	-0.242	-0.190	-0.123

FI and AN merged in a single party in 2007, PDL, which then ran in the 2008 and 2013 elections.

Table 3.8: Summary statistics for election outcomes in European elections

	Protest Measures					
	1994/2004			2004/2014		
Variable	25th percentile	Median	75th percentile	25th percentile	Median	75th percentile
$\triangle Turnout$	-0.055	-0.009	0.048	-0.199	-0.137	-0.100
$\triangle SpoiledBlankVotes$	0.001	0.034	0.076	-0.102	-0.057	0.036
	Parties					
△PD Vote Share	-0.038	0.083	0.129	0.054	0.106	0.137
△FI Vote Share	-0.116	-0.077	-0.045	-0.074	-0.036	0.022
△AN Vote Share	-0.028	0.001	0.014	/	/	/
△Lega Nord Vote Share	-0.010	-0.003	-0.001	0.003	0.008	0.019
△Minor Center-Left Parties Vote Share	0.041	0-068	0-017	/	/	/
△Centrists Parties' Vote Share	0.038	0.061	0.010	/	/	/
△Extreme-Right Parties' Vote Share	0.015	0.020	0.027	/	-/	/-0.015
△Far-Left Parties Vote Share	0.022	0.038	0.063	-0.101	-0.062	-0.034

AN did not run in the 2014 elections, its former member splitting across various parties.

Table 3.9: Summary statistics for ideological measures

	General elections						European elections				
Variable	1994	1996	2001	2006	2008	2013	1994	1999	2004	2009	2014
Populism											
Center of Gravity	-0.131	0.770	0.995	0.373	0.977	1.042	-0.198	1.199	0.992	1.374	1.023
	(0.797)	(1.377)	(0.409)	(0.143)	(0.477)	(0.289)	(0.119)	(0.222)	(0.144)	(0.403)	(0.217)
Median Voter Position	-0.235	0.815	1.368	0.682	3.110	2.350	-0.210	1.265	0.739	3.528	0.165
	(0.516)	(0.408)	(0.334)	(0.177)	(0.911)	(1.147)	(0.518)	(0.315)	(0.154)	(0.487)	(0.169)
Vote Share above Median Position	0.171	0.201	0.384	0.392	0.079	0.173	0.109	0.079	0.303	0.111	0.055
	(0.360)	(0.469)	(0.878)	(0.831)	(0.241)	(0.270)	(0.104)	(0.100)	(0.164)	(0.178)	(0.114)
Nationalism											
Center of Gravity	2.412	1.252	2.009	1.688	0.992	1.008	1.751	1.868	1.778	1.326	1.193
	(6.541)	(1.781)	(0.712)	(0.512)	(0.385)	(0.164)	(0.345)	(0.332)	(0.240)	(0.352)	(0.208)
Median Voter Position	1.847	1.580	2.432	2.851	3.137	1.277	1.982	2.681	2.987	3.376	1.111
	(0.894)	(0.483)	(1.305)	(0.416)	(0.605)	(0.556)	(0.619)	(0.857)	(0.362)	(0.392)	(0.562)
Vote Share above Median Position	0.380	0.392	0.383	0.392	0.079	0.122	0.298	0.254	0.142	0.111	0.572
	(1.043)	(0.831)	(0.878)	(0.831)	(0.241)	(0.255)	(0.204)	(0.159)	(0.203)	(0.178)	(0.340)
Right-left positioning											
Center of Gravity	23.944	21.958	5.180	21.340	0.105	-13.644	18.560	9.664	19.587	0.655	-11.771
	(67.438)	(32.527)	(3.468)	(7.382)	(1.098)	(3.511)	(4.096)	(3.646)	(5.893)	(1.260)	(2.990)
Median Voter Position	14.871	18.493	4.707	21.89	4.627	-5.115	29.285	13.200	35.119	10.213	-6.085
	(14.517)	(9.349)	(17.011)	(32.296)	(5.871)	(4.371)	(13.769)	(11.070)	(25.532)	(5.998)	(2.471)
Vote Share above Median Position	0.291	0.304	0.337	0.392	0.036	0.165	0.252	0.240	0.319	0.073	0.200
	(0.881)	(0.649)	(0.798)	(0.831)	(0.055)	(0.274)	(0.170)	(0.148)	(0.171)	(0.152)	(0.123)

Table 3.10: Summary statistics for other forms of political participation

Variable	Mean	Standard Deviation
Turnout in 2009 abrogative referendum	0.18043	0.09235
Turnout in 2011 abrogative referendum	0.56096	0.06466
Turnout in 2016 abrogative referendum	0.26774	0.07037
"No" vote share in 2016 constitutional referendum	0.60248	0.09093
M5S meetup members per 100	.01443	.00050
Log days since formation of M5S online group	974.5038	369.3514

Table 3.11: Effect of net shock on local labour markets' economies

	1991/2001		2001/2011		2001/2007		2007/2011	
Variable	OLS	IV	OLS	IV	OLS	IV	OLS	IV
$\triangle ManufacturingShare$	0.00006	0.00005	-0.00027***	0.00007	-0.00040***	-0.00067***	0.00042	0.00084***
	(0.00012)	(0.00014)	(0.00008)	(0.00024)	(0.00011)	(0.00017)	(0.00026)	(0.00021)
$\triangle LogEmployment$	0.00009	-0.00020	0.00016	-0.00004	-0.00022**	-0.00017	0	0.00011
	(0.00062)	(0.00059)	(0.00028)	(0.00017)	(0.00008)	(0.00012)	(0.00028)	(0.00028)
$\triangle UnemploymentShare$	-0.00006	-0.00004	0.00002	-0.00004	0.00005*	0.00013**	-0.00017	-0.00020
	(0.00005)	(0.00004)	(0.00001)	(0.00006)	(0.00002)	(0.00006)	(0.00010)	(0.00007)
$\triangle LogPopulation$	-0.00016	-0.00011	-0.00007	0.00006	-0.00007	0.00002	-0.00002	0.00005
	(0.00018)	(0.00017)	(0.00022)	(0.00017)	(0.00005)	(0.00012)	(0.00028)	(0.00031)
$\triangle HighSkilledShare$	-0.00006	-0.00006	-0.00006	0.00003**	-0.00007	-0.00004	-0.00002	0.00005
	(0.00001)	(0.00002)	(0.00006)	(0.00001)	(0.00006)	(0.00009)	(0.00028)	(0.00002)
Kleibergen-Paap F-Statistic	-	13.66	-	48.80	-	158.87	-	1018.57
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Fixed-effects are set at administrative regions' level.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,
the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce,
all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Effect of net shock on measures of electoral protest

	1994/2001	2001/2008			2008/2013			1994/2004	2004/2014		
Variable	1991/2001	2001/2011	2001/2007	2007/2011	2001/2011	2001/2007	2007/2011	1991/2001	2001/2011	2001/2007	2007/2011
$\triangle Turnout$	-0.00024	-0.00005	-0.00004	-0.00004	-0.00001	-0.00005	-0.00006	0.00007	-0.00010	0.00006	-0.00022
	(0.00015)	(0.00004)	(0.00003)	(0.00007)	(0.00005)	(0.00005)	(0.00008)	(0.00037)	(0.00012)	(0.00010)	(0.00022)
$\triangle SpoiledBlankVotes$	0.00005	0.00002	-0.00002	0.00005	0.00004	-0.00004	0.00006	-0.00022	-0.00005	0.00003	-0.00011
	(0.00001)	(0.00003)	(0.00005)	(0.00002)	(0.00005)	(0.00003)	(0.00002)	(0.00017)	(0.00005)	(0.00003)	(0.00008)
Kleibergen-Paap F-Statistic	10.03	1083.10	140.58	1083.10	58.30	140.67	1092.02	10.67	57.23	144.51	1081.72
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Fixed-effects are set at administrative regions' level.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,
the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce,
all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13: Effect of net shock on support for incumbent governing parties

	1996/2001	2001/2006			2008/2013		
Net shock used	1991/2001	2001/2011	2001/2007	2007/2011	2001/2011	2001/2007	2007/2011
△ Incumbents' Vote Share	0.00010	0.00003	-0.00002	0.00006	-0.00020***	0.00002***	-0.00032***
	(0.00018)	(0.00010)	(0.00062)	(0.00017)	(0.00007)	(0.00005)	(0.00012)
Kleibergen-Paap F-Statistic	9.95	52.94	135.11	1061.91	57.03	135.49	1071.51
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
Control variables include the initial share of employment in manufacturing, share of women in the workforce, the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce,
all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Effect of net shock on vote shares of parties

Elections	1994/2001	2001/2008			2008/2013			1994/2004	2004/2014		
Net shock used	1991/2001	2001/2011	2001/2007	2007/2011	2001/2011	2001/2007	2007/2011	1991/2001	2001/2011	2001/2007	2007/2011
△ PD Vote Share	0.00004	-0.00007	-0.00004	-0.00007	0.00002	0.00002	0.00003	0.00014	0.00013	0.00005	0.00015
	(0.00024)	(0.00007)	(-0.00007)	(0.00012)	(0.00006)	(0.00005)	(0.00001)	(0.00025)	(0.00012)	(0.00008)	(0.00021)
△ FI Vote Share	-0.00024*	-0.00003	0.00002	-0.00007	0.00001	-0.00001	0.00002	-0.00010	-0.00023*	0.00007	-0.00042*
	(0.00015)	(0.00003)	(0.00002)	(0.00007)	(0.00002)	(0.00003)	(0.00002)	(0.00017)	(0.00012)	(0.00007)	(0.00018)
△ AN Vote Share	0.00010	0.00002	-0.00007	0.00004	0.00001	-0.00001	0.00002	0.00018			
	(0.00010)	(0.00002)	(0.00001)	(0.00004)	(0.00002)	(0.00003)	(0.00002)	(0.00018)			
△ Lega Nord Vote Share	0.00009	0.00008	-0.00003	0.00015	0.00003	-0.00002	0.00005	0.00010	0.00012	-0.000014*	0.00033
	(0.00010)	(0.00008)	(0.00067)	(0.00011)	(0.00002)	(0.00002)	(0.00004)	(0.00015)	(0.00015)	(0.00008)	(0.00023)
△ Minor Center-Left Parties Vote Share	-0.00007	0.00002	0.00002	0.00007	-0.00002	-0.00001	-0.00002	-0.000023			
	(0.00005)	(0.00003)	(0.00002)	(0.00004)	(0.00003)	(0.00002)	(0.00005)	(0.00018)			
△ Centrists Parties' Vote Share	-0.00004	0.00012*	-0.00003	0.000021***	0.00015**	0.00004	0.00024*	-0.00023			
	(0.00007)	(0.00007)	(0.00007)	(0.00007)	(0.00006)	(0.00004)	(0.00012)	(0.00018)			
△ Extreme-Right Parties' Vote Share	-0.00002	0.00001	-0.00004	0.00002	-0.00001	0.00001	-0.00002	0.00006			
	(0.00002)	(0.00001)	(0.00001)	(0.00002)	(0.00001)	(0.00001)	(0.00003)	(0.00014)			
△ Far-Left Parties Vote Share	-0.00004	-0.00002	-0.00004	0.00002	0.00009	0.00001	-0.00002	-0.00001	-0.00002	0.00004	-0.00007
	(0.00006)	(0.00002)	(0.00009)	(0.00003)	(0.00002)	(0.00002)	(0.00004)	(0.00016)	(0.00003)	(0.00003)	(0.00006)
△ M5S					0.00008	0.00003	-0.00003		-0.00001	0.00004	-0.00002
					(0.00002)	(0.00001)	(0.0003)		(0.00002)	(0.00001)	(0.00003)
△ Mainstream Parties' Vote Share	-0.00004	-0.00011	0.00002	-0.00019	0.00006	-0.00003	0.00013				
	(0.00011)	(0.00008)	(0.00008)	(0.00011)	(0.00010)	(0.00009)	(0.00016)				
△ Outsider Parties' Vote Share	0.00006	0.00011	-0.00002	0.00019*	-0.00003	0.00001	-0.00006				
	(0.00011)	(0.00008)	(0.00008)	(0.00019)	(0.00011)	(0.00009)	(0.00018)				
Kleibergen-Paap F-Statistic	8.79	53.97	126.94	1036.21	58.38	139.99	1085.68	10.27	289.22	727.57	5526.27
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
AN, minor center-left, centrist and extreme-right parties did not compete in the 2014 elections. In 2008 and 2013, FI and AN ran jointly as the PdL party, so results are identical for the two.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce, and the party's initial vote share, all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.15: Effect of net shock on measures of populism

	General elections						European elections				
Net shock used	1994	1996	2001	2006	2008	2013	1994	1999	2004	2009	2014
Center of Gravity	0.00108	-0.00039	0.00040	0.00010	0.00044	0.00022	0.00003	0.00015	0.00036	0.00075	-0.00014
	(0.00154)	(0.00125)	(0.00043)	(0.00016)	(0.00072)	(0.00040)	(0.00019)	(0.00045)	(0.00020)	(0.00082)	(0.00028)
Median Voter Position	-0.00141*	0.00212***	0.00129**	-0.00018	0.00046	0.00003	-0.0014127*	-0.00040	0.00003	0.00141*	0.00025
	(0.00076)	(0.00074)	(0.00062)	(0.00029)	(0.00133)	(0.00176)	(0.00754)	(0.00128)	(0.00026)	(0.00073)	(0.00026)
Share above Median	-0.00089	-0.00028	-0.00014	0.00018	0.00012	0.00023**	-0.00089	0.00022	0.00036	0.00017	-0.00001
	(0.00111)	(0.00039)	(0.00021)	(0.00020)	(0.00016)	(0.00011)	(0.00111)	(0.00014)	(0.00021)	(0.00017)	(0.00011)
Kleibergen-Paap F-Statistic	13.81	13.81	13.81	57.25	57.25	57.25	13.81	13.81	57.25	57.25	57.25
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
For years up to 2001, the 1991/2001 shock measure is used. For years beyond 2001, the 2001/2011 measure is employed instead.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce, all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.16: Effect of net shock on measures of nationalism

	General elections						European elections				
Net shock used	1994	1996	2001	2006	2008	2013	1994	1999	2004	2009	2014
Center of Gravity	-0.01086	-0.00148	0.00033	0.00008	0.00028	0.00021	0.00059	0.00064	0.00053	0.00085	0.00028
	(0.01227)	(0.00158)	(0.00063)	(0.00056)	(0.00055)	(0.00047)	(0.00089)	(0.00106)	(0.00041)	(0.00082)	(0.00037)
Median Voter Position	-0.00270	0.003581***	0.00418**	-0.00043	0.00172*	-0.00157**	-0.00271	0.00773	0.00008	0.00080	0.00037
	(0.00299)	(0.00082)	(0.00205)	(0.00068)	(0.00092)	(0.00079)	(0.00299)	(0.00504)	(0.00061)	(0.00061)	(0.00106)
Share above Median	-0.00382	-0.00079	-0.00014	0.00018	0.00012	0.00009	-0.00382	0.00002	0.00013	0.00017	0.35513
	(0.00507)	(0.00083)	(0.00021)	(0.00020)	(0.00016)	(0.00018)	(0.00507)	(0.00028)	(0.00017)	(0.00017)	(0.68465)
Kleibergen-Paap F-Statistic	13.83	13.83	13.83	58.27	58.27	58.27	13.83	13.83	58.27	58.27	58.27
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
For years up to 2001, the 1991/2001 shock measure is used. For years beyond 2001, the 2001/2011 measure is employed instead.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce, all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.17: Effect of net shock on measures of left/right ideological divide

	General elections						European elections				
Election	1994	1996	2001	2006	2008	2013	1994	1999	2004	2009	2014
Center of Gravity	-0.09797	-0.01357	-0.00248	0.01509	-0.00130	0.00350	0.00958	0.01737	0.02144*	-0.00209	0.00321
	(0.12845)	(0.02855)	(0.00907)	(0.0100)	(0.00130)	(0.00698)	(0.01273)	(0.01464)	(0.01119)	(0.00200)	(0.00549)
Median Voter Position	-0.00348	0.04735***	-0.01726	-0.03304	-0.00968	-0.00586	-0.00348	0.02867	0.04992	-0.00896	0.00254
	(0.01549)	(0.00205)	(0.02939)	(0.05280)	(0.01242)	(0.01187)	(0.04473)	(0.02075)	(0.03877)	(0.01123)	(0.00632)
Share above Median	-0.00322	-0.00064	-0.00016	0.00018	0.00003	0.00019	-0.00322	-0.00003	0.00038*	0.00002	0.00008
	(0.00447)	(0.00057)	(0.00026)	(0.00020)	(0.00005)	(0.00013)	(0.00447)	(0.00027)	(0.00021)	(0.00017)	(0.00016)
Kleibergen-Paap F-Statistic	13.83	13.83	13.83	58.27	58.27	58.27	13.83	13.83	58.27	58.27	58.27
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
For years up to 2001, the 1991/2001 shock measure is used. For years beyond 2001, the 2001/2011 measure is employed instead.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce, all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.18: Effect of net shock on popular referenda and M5S membership

Net shock used	2001/2011	2001/2007	2007/2011
Turnout in 2009 abrogative referendum	-0.00005	0.00026**	-0.00033*
	(0.00012)	(0.00026)	(0.00018)
Turnout in 2011 abrogative referendum	-0.00011	0.00019	-0.00036
	(0.00016)	(0.00010)	(0.00027)
Turnout in 2016 abrogative referendum	0.00014	0.00009	0.00010
	(0.00012)	(0.00010)	(0.00019)
"No" vote share in 2016 constitutional referendum	-0.00003	-0.00012	0.00008
	(0.00010)	(0.00008)	(0.00017)
M5S meetup members per 100	-0.00001	-0.00001	-0.00001
	(0.00001)	(0.00001)	(0.00001)
Log days since formation of M5S online group	-5.19458	-9.34035	15.52738
	(8.93110)	(9.34221)	(21.18056)
Kleibergen-Paap F-Statistic	57.29	144.40	1101.12
Socio-demographic controls	Yes	Yes	Yes
Regional Fixed-Effects	Yes	Yes	Yes

Each cell reports results from a separate regression. Different columns refer to different periods used for the Net Exposure measure. Fixed-effects are set at administrative regions' level.
Control variables include the initial share of employment in manufacturing, share of women in the workforce,the share of foreigners in the workforce, the share of people above 65 years of age, and the share of people with an university degree in the workforce, all set at their beginning-of-period values. Standard errors are in parentheses and clustered at the regional level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3.1: Trade between Italy and China

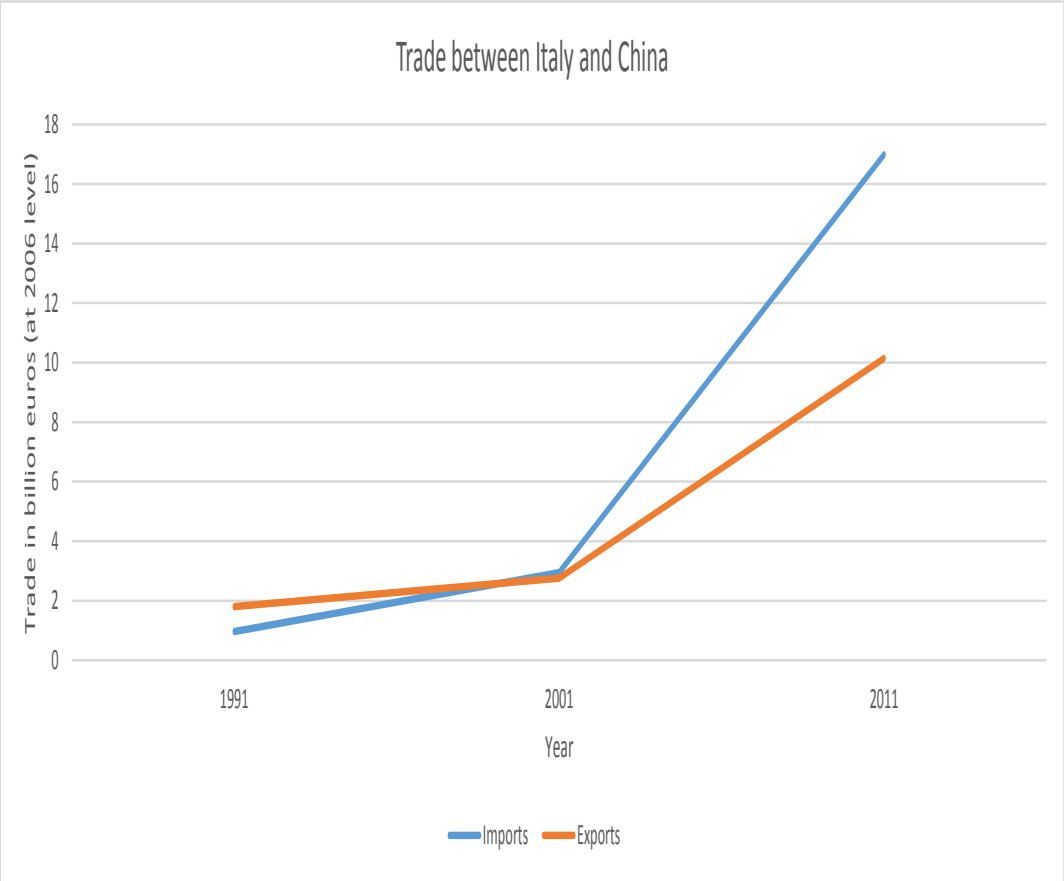


Figure 3.2: Net Exposure for 1991/2001

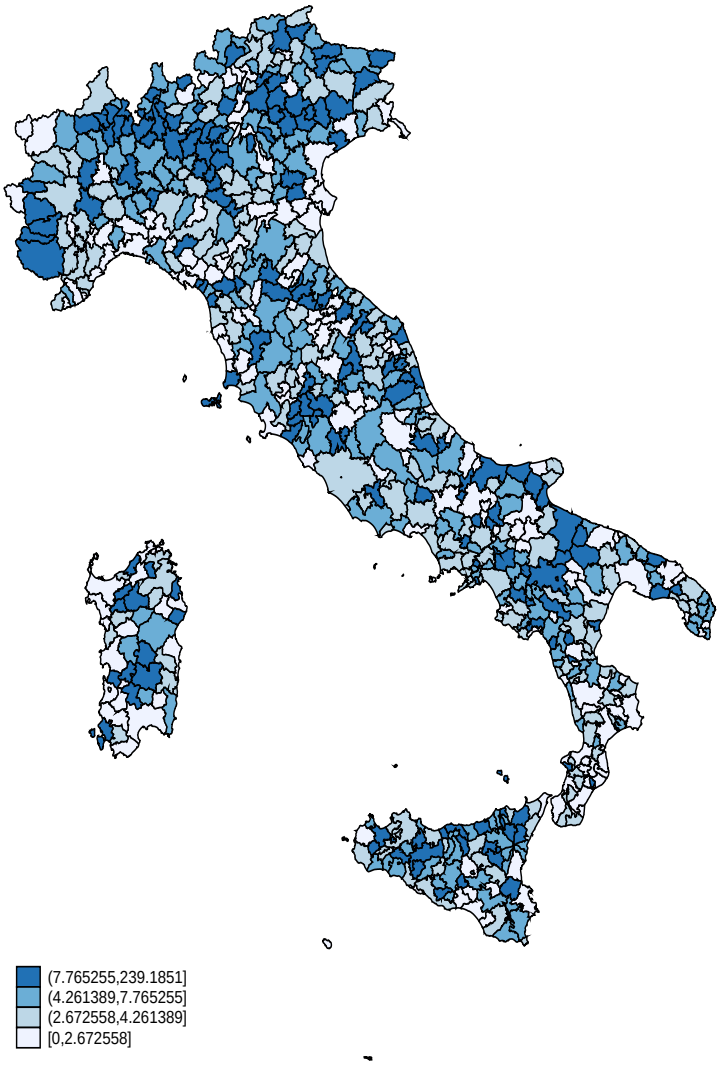
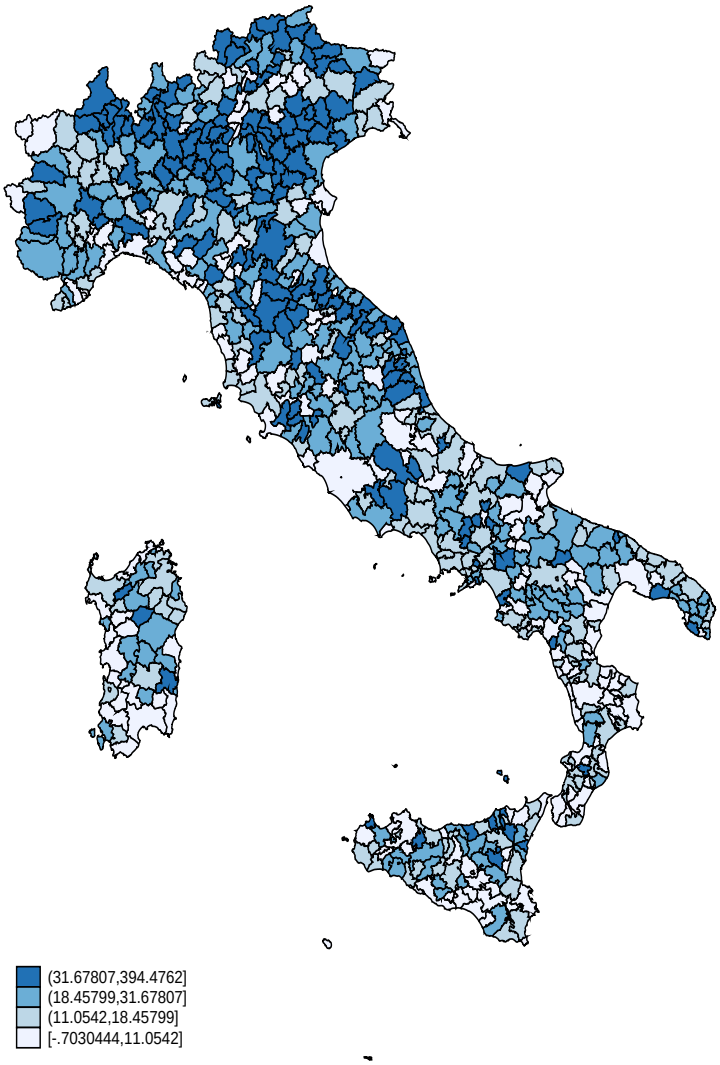


Figure 3.3: Net Exposure for 2001/2011



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All errors are and remain my own.