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INTRODUCTION

This dissertation consists of three empirical studies that aim at providing new evidence in the field of public policy evaluation. In particular, the first two chapters focus on the effects of the European cohesion policy, while the third chapter assesses the effectiveness of Italian labour market incentives in reducing long-term unemployment.

The first study analyses the effect of EU funds on life satisfaction across European regions , under the assumption that projects financed by structural funds in the fields of employment, education, health and environment may affect the overall quality of life in recipient regions. Using regional data from the European Social Survey in 2002-2006, it resorts to a regression discontinuity design, where the discontinuity is provided by the institutional framework of the policy.

The second study aims at estimating the impact of large transfers from a centralized authority to a local administration on the incidence of white collar crimes. It merges a unique dataset on crimes committed in Italian municipalities between 2007 and 2011 with information on the disbursement of EU structural funds in 2007-2013 programming period, employing an instrumental variable estimation strategy that exploits the variation in the electoral cycle at local level.

The third study analyses the impact of an Italian labour market policy that allowed firms to cut their labour costs on open-ended job contracts when hiring long-term unemployed workers. It takes advantage of a unique dataset that draws information from the unemployment lists in Veneto region and it resorts to a regression discontinuity approach to estimate the effect of the policy on the job finding rate of long-term unemployed workers.

CHAPTER 1

Ever closer Union?
The effects of EU Regional Policy on citizens' well-being

Ilaria De Angelis

Abstract

European economic and political institutions, as well as academic economic literature, are devoting increasing attention to alternative measures of well-being that incorporate non-monetary aspects of quality of life. This paper points at identifying the impact of a peculiar public policy, the European Regional Cohesion Policy, on citizens' life satisfaction. It exploits a regression discontinuity design, where the discontinuity is provided by the institutional framework for the implementation of such policy. I find that, while the policy has no significant effect on life satisfaction, it has a positive effect on perceptions about democracy and education in recipient regions.

1 Introduction

It could be argued that the search for happiness is the ultimate motivation for many individual decisions, including economic ones. The need for an assessment of happiness measures has also been recently claimed by economists and politicians [1].

The recent political debate on alternative measures of well-being, along with the established economic literature on happiness measures, have brought about the need for a more complete and informative assessment of the impact of public policies on citizens' well-being, not only in terms of income and GDP, but also in terms of perceived satisfaction about their quality of life.

The relevance of European regional policy for citizens' well-being is also assessed in the periodical Reports on economic, social and territorial cohesion published by the European Commission [2].

In light of these considerations, the aim of the paper is to identify the impact of European regional development policies on citizens' life satisfaction. In order to identify a causal relationship between EU funds and well-being, I resort to a RDD strategy that exploits the exogenous variation in the assignment of funds across European regions. I find that EU funds do not have any effect on general well-being, measured as self-reported satisfaction for life as a whole, whereas they have a positive effect on citizens' perceptions about the state of the democracy and the state of education in their countries.

The paper is structured as follows. Section 2 discusses the contribution to the existing economic literature. Section 3 describes the institutional setting. Section 4 introduces the data and some descriptive statistics. Section 5 introduces the empirical strategy. Section 6 presents the results and provides some robustness checks and extensions and Section 7 concludes.

2 Related literature

This paper contributes to two different strands of literature: one on the economics of happiness and the other on evaluation of European regional cohesion policy.

The literature on evaluation of European regional cohesion policy focuses on the effect of the policy on convergence of GDP per capita across European regions.

Ederveen et al. (2006) want to assess whether *structural funds* (SF) are effective and what conditions affect their effectiveness. They in particular underline some critical aspects for

[1]The first attempt can be found in Stiglitz et al. (2009)

[2]For further details see European Commission

the assessment of SF's effectiveness. First, SF are assigned according to the so called *additionality principle*, therefore it is difficult to disentangle the effect of European SF from the effect of national expenditure on regional growth. Second, SF are often required to be invested in specific predetermined projects that need not be necessarily growth promoting but nonetheless they absorb complementary factors such as human capital that would otherwise be allocated towards more growth-enhancing activities. Third, corruption and other institutional factors may take away funds from more productive activities Then authors estimate the conditional effect of SF transfers on annual national GDP growth rate in thirteen European countries from 1960-1965 to 1990-1995, using a pooled cross-section regression and controlling for initial GDP per capita, average gross domestic savings rate, human capital accumulation rate, population growth rate, rate of technological progress and capital depreciation rate. The observational unit is country's performance average over a five-year period. SF transfers are measured by the amount of European Regional Development Fund received by the country and the conditioning factor is the institutional quality measured in different specifications by inflation, trust, openness and the corruption perception index. They find that structural funds have a negative impact on GDP growth if institutional quality is not taken into the right account.

Becker et al. (2010) determine the causal effect of Objective 1 status on regional GDP growth of treated regions, using a fuzzy regression discontinuity design on data from 1988 to 2006. They exploit the fact that during this period not all eligible regions have been assigned the funds whereas some non eligible regions have been assigned them. They find that on average Objective 1 status raises real GDP by 1.6% in the same programming period whereas it has not significant effects on employment growth rate. They justify this result by stating that Objective 1 transfers mainly stimulate the volume and the change in the structure of investment and that job creation takes longer than the programming period of seven years. To this extent, it is worth highlighting two aspects. First, many investments in Objective 1 regions concern environmental quality, urban and network infrastructures, education. This kind of investments contribute directly to GDP growth but not to employment growth in the short run, whereas they may have direct and indirect effects on the quality of life through GDP growth. Second, employment growth is explicitly addressed by Objective 2, that involves different European regions, i.e. all the regions not covered by Objective 1. Therefore, it would be interesting to assess whether the effect of structural funds on employment in these regions is significant. Third, the effect of Objective 1 transfers on economic growth and employment may vary according to the institutional capacity of Regions to use efficiently such funds. To this regard, Becker et al. (2013) show that the effect of Objective 1 status on growth and employment is heterogeneous and it crucially depends on regional absorptive capacity, measured in

terms of human capital and quality of government.
My contribution is closer to the one by Accetturo et al. (2014), who want to assess the effect of the EU regional policy on trust and cooperation, resorting, as I do, to a RDD strategy. They find that regional policy produces what they define as an unwanted outcome, that is a depletion of the civic capital in Objective 1 regions. This paper goes in the same direction, as it shows that EU regional policy has a negative effect on perceived quality of life in treated regions.

The empirical literature on happiness is mainly addressed at defining the more appropriate measure of subjective well-being and its determinants at individual and aggregate level.
To this extent, i Carbonell and Frijters (2004) provide an exhaustive review of the literature on the determinants of happiness and compare different estimation models commonly used in the empirical literature. In particular, they first estimate OLS regressions of life satisfaction on individual characteristics using specifications in levels and in differences. Their result is in line with the standard results in the literature. Satisfaction increases with age, income, health and being in a stable relationship and decreases with the number of children in both specifications. Then they estimate the same coefficients using an ordered probit and ordered logit models, stressing the fact that this estimation model does not allow to account for individual unobserved heterogeneity. Nonetheless, the sign and the significance of the coefficients are the same as those in the OLS specifications but the magnitude of their standard errors obviously changes. Finally, they estimate an ordered logit model with individual fixed effects and individual specific thresholds for life satisfaction and they find that results are similar to those obtained with linear first-difference estimation but are different from the ordered logit model. So they conclude that considering cardinal or ordinal measures of life satisfaction does not change qualitatively the results whereas the treatment of unobserved fixed effects does. Another relevant issue is the role of relative income/social status in determining life satisfaction. The relative position can be defined either with respect to the income/social status of a reference group or with respect to individual's own aspirations and expectations. Both aspects have been shown to be significant for self-reported life satisfaction.
Stutzer (2004) finds that higher income aspirations, measured by the level of income per month that people consider to be sufficient, reduce individual utility whereas they increase with individual income and the average income of the community they live in.
Ferrer-i Carbonell (2005) shows that there exists a significant relationship between the individual level of happiness and his/her income differential with respect to the reference group and that comparison effects are asymmetric, i.e. the effect of negative income

differentials on self-reported happiness is significant whereas the effect of positive income differentials is not. The main problem in determining the effect of relative income is to correctly define individuals' reference groups.

Finally, it is worth mentioning that the main problem in assessing individual happiness lies in the subjectivity of self-reported measures. However, recently some attempts to find objective measures of happiness using medical and psychological variables, such as blood pressure or psychological strain, have been done, among the others, by Blanchflower and Oswald (2008).
Moreover, Benjamin et al. (2010) provide evidence that measures of subjective well-being are good proxies for individual utility and if any reversal between choice and predicted well-being is present, it is systematic.
As for the determinants of life satisfaction at aggregate level, a broad literature on the relationship between macroeconomic variables such as GDP growth rate, unemployment, inflation and happiness has developed.
The founding work in that sense is the one by Easterlin. The author highlights what has then become famous as the Easterlin paradox, that is GDP growth trends in developed countries after the second world war do not correspond to increases in reported happiness. This result at the aggregate level actually contrasts with the evidence from microeconometric estimates about the relevance of income on life satisfaction. Many explanations, including that on relative income, have been provided by the recent literature in that sense. The more relevant is the review by Stevenson and Wolfers (2008), that try to test the validity of the Easterlin paradox comparing the results from different surveys within and between countries. They find evidence of a persistent positive relationship between gdp and life satisfaction as well as between GDP and happiness.
MacCulloch et al. (2001) estimate the effect of inflation and unemployment on life satisfaction in Europe using a two-step methodology. In the first step, they estimate an OLS regression of life satisfaction on standard individual characteristics (age, gender, marital status, number of children, education, working status and income) for each country in the sample. Then they compute mean residual life satisfaction for each country in each year and use it as dependent variable to estimate the effect of unemployment, inflation and country and time-specific variables.
In a more recent paper, Tella et al. (2003) estimate the effect of GDP, GDP growth and unemployment on life satisfaction in European countries from 1975 to 1992, controlling for individual characteristics and individual relative income. They find that GDP affects individual life satisfaction, contrary to what the Easterlin paradox states. They also test for endogeneity by using lagged measures of GDP and they still find such a significant

positive relationship.

Alesina et al. (2004) estimate the effect of inequality on life satisfaction in US and Europe. They run separate ordered logit regressions of individual life satisfaction on inequality at country level, other country macroeconomic variables such as inflation and unemployment, time and country fixed effects and standard individual characteristics for US and Europe.

Frey and Stutzer (2000) exploit institutional decentralization of suisse regions to assess the effect of political institutions, in particular the degree of direct democracy and local government's autonomy, on happiness. they find a sizable positive effect of the degree of individual political participation and the degree of government decentralization on happiness. A similar result, concerning the degree of political and fiscal decentralization is found by Diaz-Serrano and Rodriguez-Pose (2011) in cross-country analysis of life satisfaction in Europe.

To my knowledge, this would be the first paper trying to assess the effectiveness of European Cohesion policy in terms of aggregate life satisfaction.

The contribution of the paper would be twofold then. On the one hand, it would contribute to the economic literature on the measurement of life satisfaction by providing an alternative application of such measures to policy evaluation. On the other hand, it would deliver an innovative indicator for the performance of EU Regional Policy, beyond employment and GDP growth rates.

3 Institutional setting

Regional policy implemented by European Commission is aimed at reducing economic and social disparities across the 271 regions of the 27 Member States.

In 2007-2013 programming period around 25% of EU-27 regions show a GDP per capita lower than 75% of the European average and the largest part of total EU budget (35.7%) is assigned to cohesion policy in order to reduce this gap.

In 2000-2006 and 2007-2013 European regional policy was based on three priorities: Convergence, Regional Competitiveness and Employment and Territorial Cooperation.

The Convergence objective (Objective 1) absorbed the largest share of resources and it is addressed to promote growth in lagging regions with a per capita GDP less than 75% of the Community average and in phasing-out regions, with GDP per capita slightly above the threshold because of the statistical effect of enlargement.

The Competitiveness and Employment objective (Objective 2) was aimed at strengthening regional attractiveness and employment by promoting innovation, entrepreneurship, protection of environment and investments in human resources.

Finally, the Territorial Cooperation objective (Objective 3) pointed at reinforcing inter-regional cooperation and exchange of experiences.
The achievement of these objectives was financed through the so called *structural funds*, that is the European Fund for Regional Development (ERDF) , the European Social Fund (ESF) and the Cohesion Fund.
European Commission, in agreement with national States, renewed every six years its decisions on cohesion objectives and funds' allocation, whereas funds were delivered to regions every year or every two years within the same programming period.
Funds' allocation had to be in line with the *additionality principle*, according to which EU structural funds cannot replace national expenditure for development by Member States. Co-funding can only be applied to projects whose cost exceed 50 millions of euros and the share of co-funding by the European Commission varied according to the objective. In particular, for Objective 1 regions, the share of community co-financing was allowed to vary between 50% and 75% of total expenditure, whereas for Objective 2 such share ranged between 25% and 50% of total expenditure.
Financed projects in Objective 1 regions mainly concerned investments in employment and social inclusion, infrastructures and transports, R&D, education, environment, energy, health, tourism, urban and rural development, ICT. Such investments are likely to affect life satisfaction in recipient regions, not only through their effect on GDP growth but also by influencing directly the quality of life. However, despite the huge amount of resources spent for structural funds in the last years, they have proven to perform poorly in terms of GDP and employment growth in lagging-behind European regions. To this regard, it is worth remarking that in programming period 2000-2006, 195 billions of euros, corresponding to 69.7% of community expenditure for regional policy, have been devoted to Objective 1 against 11.5% and 12.3% of Objective 2 and Objective 3 respectively.

4 Data and descriptive statistics

4.1 Data

In programming period 2000-2006, lagging-behind regions, that were eligible for the assignment of Objective 1 funds, were defined by the European Commission regulations as those regions with a per capita GDP lower than 75% of European average in years 1996-1998. The 75% rule then provides the ideal framework for a regression discontinuity design to evaluate the impact of the policy on regional aggregate life satisfaction.
Data on life satisfaction are taken from the first three rounds of the European Social Survey (2002, 2004 and 2006). Life satisfaction is measured by asking individuals the

following question: *"All things considered, how satisfied are you with your life as a whole nowadays?"*. Answers are reported on a scale from 0 to 10, where 0 means that the individual is extremely dissatisfied and 10 means that the individual is extremely satisfied. I obtain a measure of life satisfaction at regional level by averaging the answers of all individuals living in the same region across the three survey rounds.
Data on regional GDP in purchasing power standard and additional socioeconomic information at regional level are taken from Eurostat.
Regions that joined European Union in 2004 are excluded from the analysis, as long as it would be difficult to disentangle the effect of Regional policy from the effect of joining the European Union, if any, should both effects have occurred at the same time in regions around the threshold. The following analysis will then focus on 130 regions belonging to EU-15 countries.

4.2 Descriptive Statistics

Table 1 reports average life satisfaction, GDP per capita and other socioeconomic variables in Objective 1 regions and other regions in the relevant periods. Treated regions display lower life satisfaction on average, whereas satisfaction for the state of democracy and education is higher in these regions.
In order to have a clearer figure about the distribution of regional life satisfaction according to GDP per capita, figure 1 represents first-order local polynomial functions of regional average life satisfaction against the average GDP per capita relevant for the assignment of Objective 1 funds, expressed as a percentage of the EU-15 average GDP in the same reference period.

5 Empirical strategy

The estimating equation is the following:

$$LS_{i,j,t} = f(GDP_{i,j,t-s}) + \beta T_{i,j,t} + f(GDP_{i,j,t-s})T_{i,j,t} + \varepsilon_{i,j,t} \tag{1}$$

where $LS_{i,j,t}$ is the average level of life satisfaction in region i, country j in period t (2000-2006); $T_{i,j,t}$ is a dummy that takes value 1 if the region is eligible for Objective 1 and 0 otherwise; $f(GDP_{i,j,t-s})$ is a $n-th$ order polynomial function of average GDP per capita in the relevant period, that approximates GDP trends far from the threshold.

The crucial assumption underlying this regression discontinuity framework is that regions around the threshold are similar in all relevant observable characteristics except for the assignment to treatment, that is equivalent to say that assignment to treatment is random conditional to any observable characteristic of the region. This only allow to estimate a local average treatment effect around the 75% threshold.

By comparing formal eligibility according to the 75% rule and effective assignment of funds as reported in the European Commission regulations, it emerges that there is partial compliance to the treatment. Figure 2 shows that partial compliance goes in both directions: in programming period 2000-2006 there have been two eligible non-recipient regions and 19 non-eligible recipient regions. This calls for a fuzzy regression discontinuity approach, as long as the probability of assignment to treatment for regions with a GDP just below the 75% threshold is smaller than 1 whereas the probability of assignment to treatment for regions with a GDP just above the 75% threshold is greater than 0. It is then possible to instrument the actual assignment to treatment with the eligibility for Objective 1 status and then use such predicted probability to estimate the discontinuity in life satisfaction. Indeed, the formal eligibility rule is positively correlated with the assignment to the treatment but it is reasonably uncorrelated with any other variable affecting life satisfaction. Next section will show the results.

6 Results

Table 2 report the IV estimates considering respectively 1st, 2nd and 3rd order symmetric polynomials (without the interaction terms). Surprisingly, the estimated effect is negative and statistically significant in the 1st order polynomial specification,that seems a good approximation of GDP trends in figure 2, whereas significance disappears when considering higher polynomials.

One might then be concerned that differences in regional life satisfaction are mainly explained by differences in individual characteristics of the population in regions just above and below the threshold.

In order to account for such differences I try an alternative approach, drawing from the contribution by MacCulloch et al. (2001). First, I predict self-reported life satisfaction depending on a set of individual characteristics (gender, age, education, marital status, working status, health status) and year and region dummies.

$$H_{i,j,t} = \alpha_\iota + \beta X_{i,j,t} + \gamma_j + \delta_t + \varepsilon_{i,j,t} \tag{2}$$

Then, I use the estimated residuals of equation (2) as dependent variable in equation (1), under the assumption that the residuals contain only the share of life satisfaction that is not explained by individual characteristics. Table 3 reports the results of the

first-step regression.
Coefficients are in line with the findings of related literature. In particular, males report lower life satisfaction with respect to women and married individuals are more satisfied on average. Unemployment and sickness have a negative effect on life satisfaction, whereas the marginal effect of education is positive.
IV results on life satisfaction residuals in table 4 show again a negative but non significant effect of the policy.

6.1 Heterogeneous effects

One possible concern is that life satisfaction as a whole may appear too a vague and broad concept to be directly affected by a policy that permeates into many different aspects of citizens' lives, whereas such policy may influence the perception that people have on those aspects that are mainly affected by the policy.
Thanks to the availability of data on life satisfaction in the ESS, I am able to restrict the concept of life satisfaction by looking at satisfaction for economic conditions, democracy, state of education and health services in the home country. Tables 5 to 8 show that, while the policy seems to have no effects on the satisfaction for economic conditions and health services, it affects positively the perceived satisfaction for the state of democracy and education.

6.2 Robustness

A major concern is that the above results are driven by the correlation between GDP and perceived economic conditions in a region and do not reflect the pure effect of the policy. If this is the case, I should find a significant jump in the satisfaction measures regardless of the assignment to treatment. Then, I estimate equation (1) at two fake thresholds (60% and 100% of average regional GDP). Tables 9 and 10 shows that the estimated coefficients at such thresholds are not significant.

7 Conclusions

This paper shows that the European regional policy, whose explicit objective is to promote economic and social cohesion, improves the perception people have about democracy and the schooling system in recipient regions, whereas the effect is null for satisfaction in other relevant aspects of life (economic conditions and health system). I have also proven that the estimated effects do not depend on differences in individual characteristics nor on regional differences in socio-economic variables.

In the last years international institutions are devoting increasing efforts to measure differences in regional well-being and quality of life [3].

In view of that, this paper suggests that subjective measures of well-being may represent a complementary indicator of the performance of development policies aimed at reducing economic and social imbalances.

[3] The most important example is OECD among others

Figure 1: Regional GDP and life satisfaction

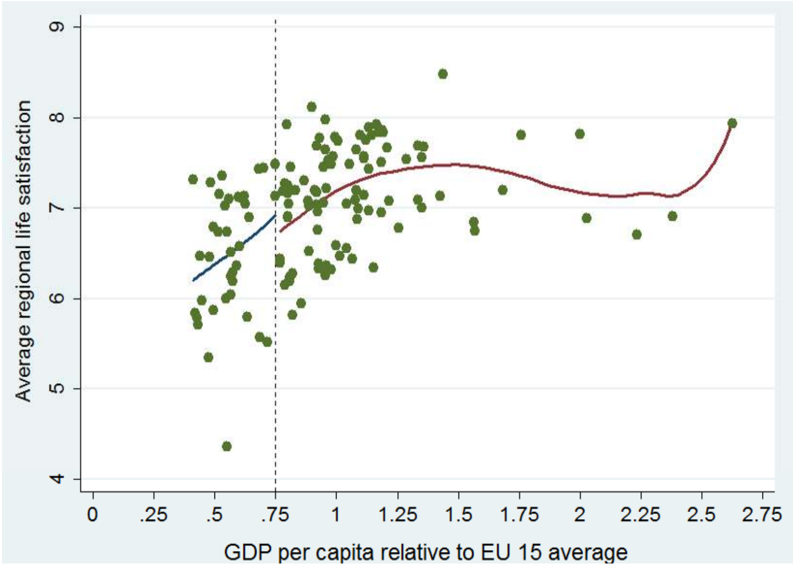

Figure 2: Compliance to the 75% rule

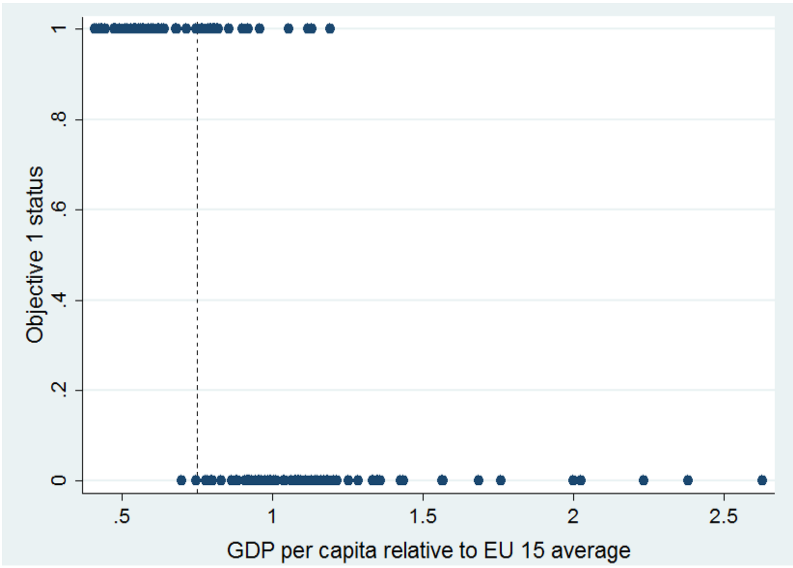

Table 1: Descriptive statistics

	(1)		
	Other regions	Objective 1 regions	Total
Average GDP per capita in 1996-1998 (euro)	22876.4	13257.6	18806.9
	(7306.5)	(3814.1)	(7711.8)
Average life satisfaction in 2002-2008	7.193	6.646	6.962
	(0.498)	(0.766)	(0.679)
Average satisfaction for the state of economy in the country	4.757	4.692	4.729
	(1.067)	(1.628)	(1.328)
Average satisfaction for democracy in the country	5.668	6.194	5.891
	(0.834)	(1.841)	(1.373)
Average satisfaction for the state of education in the country	5.677	6.280	5.932
	(1.265)	(3.524)	(2.491)
Average satisfaction for health service in the country	5.682	5.164	5.463
	(0.841)	(1.253)	(1.063)
Observations	130		

mean coefficients; sd in parentheses

Table 2: Main specification

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	-0.835***	-0.139	1.115
	(0.304)	(1.005)	(1.891)
Average GDP per capita in 1996-1998 (euro)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
GDP^2		-0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	0.117	0.269	-0.011
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Individual determinants of life satisfaction

	(1)
VARIABLES	
male	-0.057***
	(0.011)
age	-0.085***
	(0.002)
age^2	0.001***
	(0.000)
education	0.282***
	(0.012)
married	0.589***
	(0.012)
unemployed	-1.292***
	(0.028)
sick	-1.214***
	(0.032)
Observations	143,735
R-squared	0.177
Year FE	yes
Region FE	yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Alternative specification

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	-0.011	-0.038	0.033
	(0.023)	(0.086)	(0.136)
GDP	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
GDP^2		0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	0.007	-0.046	-0.032
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Satisfaction for the state of the economy in the country

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	1.003	4.244	7.939
	(0.646)	(3.273)	(7.966)
Average GDP per capita in 1996-1998(euro)	0.000*	0.001	0.002
	(0.000)	(0.000)	(0.002)
GDP^2		-0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	-0.043	-1.029	-3.692
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Satisfaction for the way democracy works in the country

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	2.570***	6.666	7.499
	(0.742)	(4.383)	(7.674)
Average GDP per capita in 1996-1998(euro)	0.000**	0.001	0.001
	(0.000)	(0.001)	(0.002)
GDP^2		-0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	-0.288	-2.401	-3.071
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Satisfaction for the state of health services in the country

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	-0.407	-1.418	-0.627
	(0.488)	(1.869)	(2.834)
Average GDP per capita in 1996-1998(euro)	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.001)
GDP^2		0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	0.072	-0.033	0.073
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Satisfaction for the state of education in the country

VARIABLES	(1) 1st order polynomial	(2) 2nd order polynomial	(3) 3rd order polynomial
Objective 1 regions	2.467**	6.146	14.645
	(1.239)	(5.562)	(14.993)
Average GDP per capita in 1996-1998(euro)	0.000	0.001	0.003
	(0.000)	(0.001)	(0.003)
GDP^2		-0.000	-0.000
		(0.000)	(0.000)
GDP^3			0.000
			(0.000)
Observations	130	130	130
R-squared	-0.091	-0.664	-3.723
F Statistic	36.51	3.008	1.171

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Placebo test at 60% threshold

VARIABLES	(1) Satisfaction for economic conditions	(2) Satisfaction for democracy	(3) Satisfaction for health system	(4) Satisfaction for education system
60% thershold	0.037	0.405	-0.080	0.907
	(0.329)	(0.339)	(0.257)	(0.611)
GDP	0.000	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	130	130	130	130
R-squared	0.012	0.022	0.060	0.032

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Placebo test at 100% threshold

VARIABLES	(1) Satisfaction for economic conditions	(2) Satisfaction for democracy	(3) Satisfaction for health system	(4) Satisfaction for education system
100% threshold	0.037	0.405	-0.080	0.907
	(0.329)	(0.339)	(0.257)	(0.611)
GDP	0.000	-0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	130	130	130	130
R-squared	0.012	0.022	0.060	0.032

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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CHAPTER 2

Take the money and...bribe! The effects of EU funding on corruption*

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Abstract

This paper analyzes the impact of transfers from a centralized authority to a local administration on the incidence of white collar crimes in that same area. We employ a unique dataset of crimes committed in Italy between 2007 and 2011, which allows us to precisely identify the types of crimes of interest and the exact municipality in which the crime was committed. Merging this information with that on the disbursement of EU structural funds and employing an instrumental variable estimation strategy, we find that a one percent increase in EU funds per capita increases the rate of white collar crimes by 1.4 percent in the recipient municipality. We further provide evidence that the effect is offset in the municipalities which are most efficient in the production of public goods, and in those where grassroots monitoring is higher.

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* *The views expressed in the article are those of the authors only and do not involve the responsibility of the Bank of Italy.*

1 Introduction

In April 2013 the mayor of Rome was prosecuted for having redirected major sums of EU funds aimed at agricultural activity to pay a number of consultants of his own nomination. In September 2014 the court of Palermo started a trial against the manager of a training company accused of having subtracted about 15 million euro of EU funds for fictitious courses. In October 2014, the director general for EU policies of the Abruzzo region was arrested for corruption. In June 2014, the former president of the region Veneto and the mayor of Venice were arrested for having accepted large bribes in relation to the realization of the largely EU funded project of the Mose of Venice.
These and other similar scandals have been appearing more and more on the Italian newspapers in the most recent years, pointing at the existence of large holes in the mechanisms of allocation and monitoring of the use of EU funds.

Yet the phenomenon is not confined to Italy, as similar cases have been reported in many other EU countries, especially in those of Central and Eastern Europe.[1] According to the European Commission, it would be around 5 billion euro per year the amount of EU funds that are lost in fraud and corruption (European Commission, 2014).

The present paper takes the moves from this anecdotal evidence to investigate the relationship between the receipt of EU funds and the incidence of white collar crimes in Italian municipalities over the period 2007-2011.

Our empirical analysis relies on a novel and detailed dataset of crimes at the municipal level and links these to the data on the disbursement of the EU structural funds over the period 2007-2013. In order to identify a causal relationship between transfers and corruption we resort to an IV strategy that exploits the timing of mayoral elections to predict the yearly amount of funds received by each municipality. The instrument thus builds on the existing literature which links public administrators' effort to the timing of elections with the idea that as elections come closer, local administrators who seek reelection exert more effort to receive public funds from the central authority (Persson and Tabellini, 2000). Our first stage results indeed confirm this idea, showing that each extra year in charge increases the amount of EU funds that the local administration receives by about 4 percent. Exploiting this exogenously generated variation in the amount of EU funds received by each municipality, we conclude that a one percent increase in transfers raises the white collar crime rate by 1.4 percent on average. Such effect turns out to be mainly driven, across the various types of funds, by transfers for public works but is offset in municipalities with more grassroots monitoring or higher efficiency in the

[1] For references on the newspapers see the following links: Venice, Rome, Palermo, Abruzzo, Slovakia, Romania, Bulgaria.

provision of public goods.

In the light of these results, the present work can provide some recommendations for the recent reforms aimed at increasing the effectiveness of the EU structural funds (see the EC webpage for reference). Our findings on the importance of local grassroots monitoring suggest that disclosure and transparency in the disbursement process will be crucial, therefore providing support for initiatives such as data web dissemination. Similarly, our results related to the efficiency of the local public sector are in line with the proposal to include in the EU scheme some elements of pre-requisites to receive the funds.

The paper is structured as follows: Section 2 discusses our contribution to the existing economic literature; Section 3 describes the institutional setting; Section 4 introduces the data and some descriptive statistics; Section 5 discusses the identification challenges we face and introduces our empirical strategy; Section 6 presents our results and provides some robustness checks and extensions; finally Section 7 concludes.

2 Related literature

Our study adds new evidence to the literature on the determinants of corruption, providing an empirical investigation of corruptive behaviors. The existing empirical contributions which investigated the determinants of corruption have highlighted the role of institutions (Fisman and Gatti, 2002), culture (Fisman and Miguel, 2007), gender of the administrators (Brollo and Troiano, 2013) and several contextual factors (Bai et al., 2013; Campante and Do, 2014). From this point of view our paper is closest to the work by Brollo et al. (2013) who estimate the effect of large transfers of public resources to local administrations onto the propensity of politicians and public officials to commit corruption crimes. Compared to their work, the main novelty of our paper is that we are able to analyze a non developing country context. Indeed, all the above mentioned studies, with the exception of Fisman and Miguel (2007) and Campante and Do (2014), used data from Brazil or from Indonesia. Moreover, we are able to rely on a more comprehensive measure of corruption in that our white collar crime rate is available for all municipalities nationwide and includes all crimes involving public officials and not only those arising from the audit of the local administrations' sheets as in the case of Brollo et al. (2013). Our data is also particularly robust to under-reporting of corruption crimes as it couples the information collected through the reports from the victims with that on criminal facts that the police department collects in its day-by-day activity. In the analysis of the determinants of corruption, we also add evidence to the arguments provided by Olken

(2007) as we explore the role of grassroots monitoring in preventing corruption. Finally, by uncovering the relationship between corruption and the electoral cycle, we build on the findings of Ferraz and Finan (2011) who provided evidence that corruption is lower when elections approach if the mayor can rerun for his seat.

A second strand of literature to which we contribute is the one aimed at understanding the political mechanism behind the allocation of funds from the central to lower levels of government. The political economy literature has explained such decisions in the light of electoral interests, so that it may be optimal to target either core support areas (Cox and McCubbins, 1986) or swing districts (Dixit and Londregan, 1996). But personal interests and connections also seem to play a role, for instance Carozzi and Repetto (2014) show that Italian municipalities which are birth towns of politicians in the national Parliament tend to receive higher transfers per capita from the central government, while Barone and Narciso (2013) showed that the presence of organized crime in the area would boost transfers to the local administration. Our paper will highlight the role of electoral cycles and political alignment with the central government in determining the amount of resources received by a local administration.

Finally, from a more policy oriented perspective, this study provides additional insights to the recent debate on the desirability of the EU Cohesion policy. The impact of structural funds in terms of employment and GDP growth has been widely measured (Becker et al., 2010; Busillo et al., 2010; Becker et al., 2013) and the consensus view is that there seems to be an (EU-average) positive effect, even though the administrative capacity of the recipients regions is likely to make a big difference in terms of effectiveness. Some recent literature revealed that there may be undesirable side effects of the policy: Accetturo et al. (2014), for example, showed that the disbursement of EU funds negatively affects the degree of civicness and social cooperation in the receiving area. Our results will add increased corruption to the list of such unwanted side effects.

3 Institutional setting

The European regional policy[2] is aimed at promoting growth and investments and reducing economic and social imbalances among European regions. The policy is implemented through the so called *structural funds*, which are allocated by the European Commission to the member states on a 7 year basis. The allocation mechanism is based on common guidelines and bilateral agreements between the Commission and each member state.

[2]Details on the functioning of the policy can be found on the EC dedicated webpage.

Once funds have been assigned to member states, a crucial decision becomes the level of decentralization at which to manage the funds: a more decentralized management may leave more room for discretionality and hence misbehaviors on the part of local politicians (Mauro, 1998; Tanzi and Davoodi, 2000), whereas a fully centralized system would reduce the degree of accountability of the local politicians.
To limit the outbreak of rent seeking behaviors the European Commission has set a mechanism of automatic withdrawal of funds that takes place whenever member states do not report and certify the total spending by the end of the programming period; this threat of withdrawal should push national authorities to impose heavier regulations and requirements over EU funds, making corruption more costly. Moreover, in order to make the national policy makers feel more responsible about the use of the EU funds, it is established that member states and regions have to guarantee an adequate share of co-financing of the projects implemented with EU funds as a condition to receive the funds.

On the other hand, several economists and policy makers suggested that the possibility of funds misuse should not be disregarded because the architecture of the EU funds allocation and spending is complex and involves many levels of government, this leads to major bureaucratic redundancies and to a high fractionalization of the expenditure which undermine the possibility of national authorities to adequately monitor the funds use; moreover, although most projects are managed at the local level, more than 90 percent of co-financing comes from national resources and not from the local ones, this reduces the incentives for local authorities to monitor the spending of the funds and the implementation of the projects.[3]

Yet, as of today, the EU funds represent a major source of funding for most local administrations which have suffered from severe spending cuts from the central government following to the crisis outbreak. For the 2007-2013 programming block, Italian regions were assigned funds for around 60 billion euro, including national co-financing for around 31 billion.

[3]The main concerns of the debate can be found on lavoce.info: Perotti and Teoldi (July, 2014), Nannariello (September, 2014), Perotti and Teoldi (September, 2014).

4 Data and descriptive statistics

4.1 Data

To retrieve the effect of EU funds on corruption we build a balanced panel dataset of 8.092 Italian municipalities (the lowest level of government in Italy) for the years 2007-2011.

Our dependent variable, the rate of white collar crimes, is derived from SDI (Sistema d'Indagine), the archive of the Ministry of Interior which contains records of all the crimes committed in all Italian municipalities over the period of interest taken directly from the IT system used by the police for investigation activities.[4] This dataset presents two major advantages: first, because it reports all the open cases which are under investigation by the police, it provides an instantaneous picture of the criminal activity in the municipality, whereas most datasets on crimes only report arrests or convictions which are likely to occur with delay with respect to when the crime is committed. Secondly, our dataset is less subject to problems of underreporting of crimes because, on top of the reports filed by the victims of the crimes, it also contains records of all the investigations opened by the police forces themselves. This is a particularly valuable aspect in the case of corruption crimes in that in such crimes neither of the parties involved has any interest in reporting the crime because they would both be guilty of a criminal offense.

From the SDI dataset we construct the rate of white collar crimes as the yearly number of such crimes divided by the population size of the municipality. The classification of crimes is made directly by the Ministry of Interior on the basis of the respective applicable law. We thus identify as white collar crimes all crimes committed against articles 314-323 (crimes against public administration) and 479-481 (crimes against public faith) of the Italian penal code: these include corruption, bribery, embezzlement, abuse of authority and fraud.

To build our explanatory variable of interest, we exploit information on disbursements of EU structural funds published on Opencoesione, an on-line portal created in 2012 which contains all information on the use of EU cohesion policy funds in Italy for the programming block 2007-2013. The website is constantly maintained and updated by the Ministry of Economic Development and is considered a ground-breaking example of transparency of the public administration. Opencoesione reports detailed information on type, localization, beneficiaries and payments relative to all projects financed through structural funds in Italy during the 2007-2013 programming period. The data are re-

[4]The SDI contains confidential data. It is used by the Bank of Italy exclusively for research purposes on the basis of a special agreement with the Italian Ministry of Interior.

ported bimonthly but, to link them with our crime data, we aggregate them on a yearly basis. In the data each payment is associated to a single project, which can be realized by either private or public firms and associations or by local authorities. The geographic localization of the projects refers to the municipality where the work is realized. Therefore payments do not only measure transfers to the local authority but they also include transfers from the public administration to other subjects residing in the municipality. In some cases a single project may involve more than one municipality, in which case it is not possible to recover the share of payment received by each single municipality. In these cases we imputed an equal share of payment to each municipality involved. Finally, to merge this information with our crime data, which are currently only available until 2011, we used only data on payments made between 2007 and 2011.

Finally, in our empirical analysis we will resort to an instrumental variable approach in which we exploit the timing of local elections to predict transfer of EU funds. Data on electoral outcomes are taken from the records of the Ministry of Interior, which provides information on the date, candidates, coalitions, results and turnout at municipal mayoral elections.
Additional socio-economic, geographic and demographic information at municipal, regional and local labor system level are taken from the Italian National Bureau of Statistics (ISTAT).

4.2 Descriptive Statistics

Table 1 reports the average criminal incidence per municipality by year and type of crime: the first two lines split the total crime rate between white collar crimes and other types of crimes. It is clear that white collar crimes represent a minor fraction of the total criminal activity in Italy: in 2007-2011 the average white collar crime rate amounted to only 0.05 per 1,000 inhabitants. This incidence, nevertheless, increased over time, while the total crime rate slightly decreased.The lower panel of the table reports the incidence of the most common types of white collar crimes: corruption, bribery and abuse of authority; these three categories cover around 60 percent of all white collar crimes on average, with corruption alone making up for about 30 percent.

In table 2 we report the average number of projects started per year and municipality and the amount of EU funds transfered. The table shows that average number of projects was close to zero in the first two years of the programming period, reflecting the fact that these years have mainly been dedicated to the assignment of public tenders and other preparatory activities. As a consequence, the amount of transfers more than doubled between 2008 and 2009 and continued growing during the following years. More than 50 percent of the funds financed public works; around 20 percent were assigned to firms and local authorities for the purchase of goods and services, which include physical assets as well as IT devices, consultancy and training services; less than 10 percent were used to subsidize R&D investments by firms or to sustain employment.

Finally, table 3 reports descriptive statistics on the instrumental and control variables we use in our empirical estimation by area. All variables rates are measured at the municipal level, with the exception of GDP growth, measured regionwise, and employment rate, measured at Local Labor Market level. In table 3 we further provide evidence on the amount of funds that municipalities have received from the national government. These numbers clearly show how resources from the central government to local municipalities have progressively been reduced: not only they grew less and less in the first years, but they even eventually fell dramatically in 2011.

5 Empirical strategy

We are interested in estimating the effect of an increase in EU funding going to a certain municipality in a certain year on the rate of white collar crimes involving public officials in that same municipality and year. The estimating equation is thus of the following type:

$$C_{mt} = \alpha + \beta F_{mt} + \gamma X_{mt} + \delta_t + \delta_r + \epsilon_{mt} \quad (1)$$

Where the rate of white collar crimes per one thousand inhabitants is our dependent variable and β our coefficient of interest. F_{mt} is the log of EU payments per capita. We include in the regression a set of municipality and time varying control variables and year and region fixed effects to difference out all the time invariant and region invariant characteristics. We prefer to include region rather than municipality fixed effects because our dependent variable presents high serial correlation so that controlling for both time and municipality fixed effects would leave us with too little variation to identify an effect of the EU funding.

Despite the inclusion of control variables and time and region fixed effects, one may be concerned that the coefficient β may capture some omitted variable bias due to the existence of unobservable characteristics which are correlated with both the amount of funding received by a certain municipality and the rate of white collar crimes in that same municipality. Ex-ante one may think this might lead to either upward or downward bias. Estimates may be upward biased if, for example, the EU allocates more money to more disadvantaged municipalities, and in such municipalities the citizens have moral principles which make them find it more acceptable to live on a criminal activity such as bribing the local government (Putnam et al., 1993); if this was the case then the observed positive correlation between EU funds and corruption would be driven by differences in moral principles rather that by the receipt of large amounts of funding from the EU. On the other hand, one could imagine that more funds are received by areas in which the local politicians exert more effort in trying to attract external sources of funding for their municipality; if this is the case more money would go to more "virtuous" local administrators, who would thus be less prone to accept bribes, so that the OLS coefficient would overall be downward biased.

Another concern is related to the possibility that measurement error in the independent variable may attenuate the magnitude of the coefficient of interest (attenuation bias). There are indeed several potential sources of measurement errors in our data: the first derives from the fact that some large projects are not localized in one single municipality, but in a group of them, in this case we split the full amount paid by the EU in equal shares among the different municipalities involved (section 4); secondly, there is a matter of timing of accounting in that we are attributing all the payments made during the year to the same time unit, but it may be the case that, for example, payments made towards the end of the year where actually recorded in the following year.

To rule out these concerns, we turn to an instrumental variable approach so as to isolate the effect of the funds from other possible omitted variable effects and correct our estimates for measurement error. In this paper we propose to exploit the variation in the allocation of funds due to the electoral cycle, thus imagining that, as local elections get closer, the incumbent administration will exert more effort in trying to attract more financial resources and thus increase its electoral favor.

There are many reasons to suspect the existence of a positive link between mayoral elections and transfers. One explanation may be that obtaining funds from the EU requires time and experience, so that the older is the local administration the higher its ability to attract EU funds to its territory. An alternative explanation would then be that in years in which the national public debt crisis has imposed more and more binding constraints to public spending, especially at local level, politicians may want to resort to opportunistic transfers to increase their political credit during the pre-electoral period. A similar argument would be consistent with evidence coming both from the existing empirical economic literature (Kendall et al., 2014) and from real life anecdotes. For example, the mayor of Rome Gianni Alemanno, who was rerunning for elections in May

2013, claimed to have approved, on the 7th of March 2013, an Action Plan for Sustainable Energy (PAES) which would immediately bring to Rome EU funds for 2 million euro. He also announced in October 2012 that in 2014-2020 programming period part of the EU funds will be directly addressed to and managed by big metropolitan areas, such as Rome, to improve sustainable development (news).
On the 9th of January 2013, the mayor of Bari, Michele Emiliano, who would not run for the following elections of June 2014 having been in charge for already two mandates, announced the approval of a Plan for Bari, entirely funded through the EU funds for a total amount 1.6 billion euro.

The idea that local politicians try to attract more funds as elections approach is also consistent with a well-established strand of literature which shows that there exists a positive relationship between the timing of elections and public spending decisions. From a theoretical point of view, this relationship dates back to the models of electoral cycles of Rogoff and Sibert (1988) and Rogoff (1990) and to those in Alesina et al. (1992). More recently then, similar arguments have been at the root of a series of empirical works which, as we do, exploited the electoral cycles to predict variations in the amount of public spending: for example, Levitt (1997) used the timing of mayoral and gubernatorial elections in the U.S. to predict changes in local public spending for police and thus identify the effect of changes in police hiring on crime rates.

6 Results

6.1 Main results

In this section we first retrieve the estimate of the effect of transfers on the rate of white collar crimes and then check whether this effect varies depending on the efficiency of local public good production and on the degree of grassroots monitoring.
Table 4 reports our main results. We first estimate the effect trough simple OLS adding a set of control variables which include population density, regional GDP growth rate, employment rate in the local labor system and some geographical characteristics of the municipality such as altitude, slope, coastal location. We observe that, no matter whether we include or not our set of control variables, we fail to find any correlation between transfers of EU funds and white collar crimes. As discussed in the previous section, this could be due to several sources of bias: measurement error, omitted variable bias. For this reason we resort to a two stage least squares estimation exploiting the variation in transfers determined by the electoral cycle. Our first stage estimates show that the approaching of

local elections significantly increases the amount of funds transferred to the municipality: each extra year since the last elections increases the amount of funds to the municipality by 4 percent. Not surprisingly our first stage estimates also show that more funds are allocated to areas with lower economic growth and employment rates. Exploiting this variation, our IV estimation reveals that a one percent increase in per capita transfers raises white collar crime rate by 0.07 crimes per 1,000 people, that translates into a 1.4 percent increase in the average white collar crime rate in the same year. The comparison between our OLS and our IV estimates thus suggests that, among the various possible sources of bias of the OLS regressions discussed in section 5, those leading to downward bias, among which measurement error, tend to predominate.

Recent empirical literature on the economic impact of structural funds has shown that differences in the quality of local institutions contribute to produce heterogeneous effects of the policy on GDP growth and the local level of civicness (Becker et al., 2013; Accetturo et al., 2014). We are thus interested in checking whether the effect of EU transfers on corruption changes according to the municipality's efficiency in the provision of the public good. Indeed, more efficient municipalities show a higher level of administrative capacity and a lower degree of bureaucratic complexity that make corruption more costly. For instance, an entrepreneur wanting to build a EU-funded plant in a municipality that is relatively inefficient in the provision of public goods would find it more convenient to bribe local public officials rather than bearing the burden of red tape.
In order to measure the efficiency of local public administrations we consider the share of local public expenditure for administrative activities over total public expenditure in 2006, normalized for the population. A similar indicator identifies those administrations which are most cost effective in the production of their services to the public (assuming these are constant for all municipalities). Data on local public expenditure are taken from the database of Italian municipalities' balance sheets maintained by the Italian Ministry of Interior. As reported in table 3 the average value across all Italian municipalities was about 4.5 percent. To account for differences in the efficiency of public good production in our estimates, we split the sample in two so that efficient municipalities will be those whose share of per capita expenditure for administrative activities is below the median value (1.6 percent) and inefficient ones are those above the median. Results, reported in table 5, show that in more inefficient municipalities a one percent increase in EU transfers raises white collar crime rate by 0.18 crimes per 1,000 inhabitants, an increase that is more than double the average effect, whereas among more efficient municipalities the effect is close to zero and it is not significant, provided that the instrument is still valid.

The effect of transfers on white collar crimes might also depend on the level of grassroots monitoring (Olken, 2007). In particular, we expect that in those municipalities where citizens are more interested in politics and put more effort in monitoring local politicians' behavior, the latter face a higher probability of being discovered grabbing public resources and thus reduce their corruptive activities.

To proxy for the level of grassroots monitoring we use the municipal rate of turnout at the 2011 referendum. Indeed this is a measure of interest for the public good which is exempt from particularistic interests and patronage motivations (Putnam et al., 1993). Moreover Giordano and Tommasino (2011) show that the degree of citizens' interest in politics affects local public sector efficiency due to citizens' pressure on politicians. Because the referendum contained four questions[5] and people could respond to some of them only, we take the municipality average turnout among the four questions. Estimation results in table 6 confirm our prediction that in municipalities where grassroots monitoring is lower, the probability of public officials being involved in white collar crimes is higher: a one percent increase in transfers raises white collar crime rate by 0.09 crimes per 1,000 people, whereas in municipalities with high levels of grassroots monitoring the effect of transfers on white collar crime rates is much lower (around 0.03) and not significant. Moreover, the relationship between timing of elections and transfers is almost twice as large in municipalities with low grassroots monitoring suggesting that this social pressure on policy makers also decreases the discretionality in the allocation of EU funds to municipalities.

In conclusion, we find that on average increasing EU funds determines a higher propensity to extract private rents among public officials in the recipient municipality and that this effect disappears when municipalities are more efficient in the production of the public good and is significantly reduced by local grassroots monitoring. In the next sections we provide first some evidence on the robustness of our instrumental variable and then further results and some specification checks to complete our empirical analysis.

6.2 Robustness

We start with a placebo exercise. If the estimated impact of EU transfers on white collar crimes is driven by (omitted) differences across municipalities, then we should obtain similar estimates when using a fake outcome - that is, a variable that is not affected by the

[5]In 2011 national referendum citizens were asked to express their preferences on four relevant topics in the political debate at that time: 1. whether to liberalize the provision of local public services; 2. whether to modify charges for the provision of water; 3. whether to install nuclear plants in Italy; 4. whether to allow the Prime Minister and the members of the government to be taken to trial while in charge.

transfers but, similarly to white collar offenses, captures the omitted heterogeneity. Our falsification exercise uses the rate of all crimes excluding white collar ones as dependent variable. Indeed, there is no reason to suspect that the disbursement of EU funds should increase the rate of violent or private property crimes (if not, in the long run, through a mechanism of social capital depletion as in Accetturo et al. (2014)). The relative results are reported in table 7. Differently from the case of white collar crimes, the OLS coefficient is now largely positive and significant: this goes in the direction of saying that more EU funds are allocated to more disadvantaged areas, which are characterized by higher crime rates. As a matter of fact this coefficient becomes smaller when we add some variables to control for the economic characteristics of the area. On the other hand, when we run our two stage least squares estimation, it turns out that EU funds have no causal effect on the overall crime rate that does not include white collar crimes, confirming the hypothesis that omitted heterogeneity plays no role for our findings.

While the power of our instrumental variable, i.e. years from last mayoral elections, appears to be reasonably high, with values of the first stage F statistic systematically above the rule-of-thumb threshold value of 10, one may be more concerned about the excludability of the instrument from regression 1.

A potential issue refers to the role of non EU transfers. Local politicians have access to extra-EU public resources (public money from national sources). If these resources also increase corruption, then there could be a direct link between timing to election and white collar crimes, which is a problem for the excludability. To corroborate the hypothesis that the timing of elections only affects corruption through the disbursement of EU funds, we run the same regressions using transfers from the national government to local administrations instead of EU funds (table 8). When we run an OLS regression between transfers from the national government and white collar crimes we find, as a matter of facts, a strong positive relationship between the two variables. Yet, timing to election does not enter into our first-stage while the F statistic is extremely low. The lack of a relationship between the timing of elections and white collar crimes when we analyze the channel of non EU funds might be due to both the fact that starting from the financial crises national transfers were severely reduced and the circumstance that these funds are allocated with much less discretion than EU funds, as long as they mostly follow rule-based assignments.

Note that according to our definition of white collar crimes, corruption arises when public funds are grabbed by local public officials; therefore, this specific crime activity

requires the availability of public resources to be accomplished (no public money, no corruption).
However, a more subtle failure of the excludability refers to the incentives of the local politicians, that might change over the electoral cycle. For instance, in a model in which the policy maker allocates the available resources either to the provision of a public good or to the extraction of private rents, when elections approximate he will want to spend more for the public good so as to increase his, or his party's, probability of re-election [6]. If, on the other hand, the incumbent administrator has no interest in him or his party being reelected, he would face a stronger incentive to grab the public funds. Similar mechanisms would pose a threat to the excludability of our instrument in that the approaching of local elections would directly affect the incumbent government's incentives to extract rents, irrespective of the amount of EU funds available (provided that it is not zero). To give a first cut to this issue, we estimate the effects of EU funds separately for the municipalities in which the incumbent mayor was at his first mandate (hence could be reelected) and the municipalities in which he was at his second mandate (hence he could not be reelected). Our results, reported in table 9, suggest that the increase in corruption might be more pronounced for second-term mayors; however, our estimated coefficient of 0.07 is very close to the one estimated for first-term mayors, whose incentives to grab driven by the electoral cycle should be low.[7]

Our instrument is based on a political mechanism through which the local mayor is able to get more EU funds as the elections get closer. The plausibility of this mechanism can be further substantiated by considering the political alignment between the local administrators and those who share some responsibility for the EU spending at the higher level of jurisdictions (EU financing is indeed provided within a framework of multi-level governance: see Section 3). The idea is that aligned local administrations should get EU money more easily. This idea is exploited in table 10, where we make use of an alternative instrument, defined as the interaction between the time elapsed since last elections and the political alignment of the municipality mayor with the higher level of administrations. To be sure, we construct a binary variable which equals one when the municipality is aligned with either the regional or the national government and zero

[6]Similar mechanisms, however, are not necessarily in place in our setting, where the local government has the opportunity of increasing the production of public good without decreasing the amount of private rents extracted because it can count on an increase in the total available budget.

[7]The plausibility of electoral incentives argument for Italian mayors should not be taken for granted. For instance, among those municipalities in which the incumbent mayor could not run for elections, because he had already been in charge for two mandates, and in which the party of the incumbent mayor wins the elections, most of the times at least one member of the former municipal board (giunta comunale) belongs to the new board even if the mayor has changed; moreover many mayors who cannot be elected for the third time in their municipality run for higher level political seats and thus still have incentives to not misbehave at the end of their mayoral mandate.

otherwise and then interact this variable with our main instrument of distance from last elections.[8] This choice is supported by the findings in Brollo and Nannicini (2011), who argue that central government incumbent politicians have incentives to transfer more financial resources to aligned municipalities in the years before local mayoral elections so as to increase the political consensus for their party. Because we only have information on the political alignment of the local administrators for a small subsample of municipalities,[9] we also replicated our baseline regressions on such subsample. As shown in table 10 the effect of transfers on white collar crimes is robust to the use of the alternative instrument, the coefficient being still positive and significant, though slightly smaller in magnitude. Moreover the first stage estimates confirm the idea that aligned municipalities are those which discretionally receive more funds from the central government as local elections approach. Also, the test of over identifying restriction reported at the bottom of the table rejects the hypothesis that our main instrument is not excludable.

A final concern is the possibility that there might be some delays between the moment in which money is transferred to the municipality and that in which the white collar crime is committed. While the recent study by Mironov and Zhuravskaya (2014) partially reassures us on this respect, as it shows that the tunneling of dirty money is instantaneous as soon as the money is received, we still prefer to investigate this aspect in our data, by introducing a lagged independent variable in our specification. As shown in table 11, the coefficient associated to transfers received by the municipality in the previous year do not seem to have any impact on the current rate of white collar crimes.

6.3 Extensions and further specification checks

An important extension of the empirical results concerns the type of project funded. We are interested in knowing whether the emergence of corruption is more related to some specific types of projects. We thus split transfers in the three categories described in table 2 and estimated separately the effect of each type of EU transfers on white collar crime rates. Table 12 reports the results. We find that the overall effect of transfers on white collar crimes is mainly driven by transfers for the purchase of goods and services

[8] The instrument is thus equal to zero when the municipality is not aligned with either central administration and it equals the years since last elections for those municipalities aligned with either higher level of government. The alignment binary variable will have sufficient time variability because it potentially changed when the national government changed in 2008, and each time the regional or municipal administration was elected.

[9] The excluded municipalities are governed by mayors belonging to civic parties, that are not politically aligned by definition.

and transfers for the realization of public works. This is consistent with the idea that for those categories of expenditure the degree of discretionality of the local administration is higher.

7 Conclusions

This paper shows that the receipt EU structural funds contributes to the rise of white collar crimes. However, it also suggests that this relation is by no means inescapable. Corruption driven by EU funding is going to be a lesser concern when local public authorities are highly efficient in the provision of public goods. Accordingly, conditioning the transfers on (ex-ante) measures of local public sector effectiveness might be a step forward to reduce the unwanted effects of the EU financing. In this respect, note that the EU rules for the 2014-2020 programming period include pre-requisites for local administrations, that is, threshold levels of administrative capacity that should be in place before disbursements arrive.[10] Also, our findings suggest that in the localities with high level of grassroots monitoring the impact of the funding on white collar crimes vanishes. In view of that, an important implication from our work refers to transparency. Making available to the general public the data on EU funded projects is going to enhance the ability of the local community to monitor the good use of the resources. Finally, our estimates suggest that the elasticity of white collar criminal activities to the receipt of the money also varies with the typology of programs. In this respect, programs that include the purchase of goods and services and those referring to public works are the ones most likely to be problematic. Therefore, anti-fraud investigations should concentrate in those areas.

[10] Regulation No 1303/2013 of 17 December 2013 of the European Parliament and of the Council

Table 1: Criminal incidence in Italian municipalities, by year and type of crime.

	2007	2008	2009	2010	2011	Total
Crime rate, excluded white collar	29.949	27.244	26.716	26.200	28.366	27.695
	(23.302)	(21.264)	(21.786)	(19.962)	(21.209)	(21.572)
White collar crime rate	0.006	0.053	0.067	0.0622	0.056	0.049
	(0.074)	(0.296)	(0.425)	(0.613)	(0.343)	(0.392)
By type (in share of white collar crimes):						
Corruption	0.457	0.189	0.216	0.189	0.420	0.266
	(0.376)	(0.346)	(0.360)	(0.336)	(0.426)	(0.384)
Bribery	0.047	0.014	0.010	0.020	0.029	0.020
	(0.161)	(0.084)	(0.070)	(0.117)	(0.135)	(0.109)
Abuse of authority	0.456	0.200	0.106	0.231	0.547	0.305
	(0.369)	(0.355)	(0.348)	(0.370)	(0.431)	(0.405)
Observations	8,092	8,092	8,092	8,092	8,092	40,460

mean coefficients; sd in parentheses

Crime rates are computed per thousand inhabitants.

Table 2: EU funds disbursements to Italian municipalities, by year and type of project.

				(1)		
	2007	2008	2009	2010	2011	Total
Number of projects per municipality	0.0798	0.619	3.750	13.34	16.24	6.805
	(0.492)	(2.897)	(17.03)	(43.60)	(78.63)	(41.49)
EU funds per municipality	10.21	56.16	134.89	190.61	247.38	127.85
	(262.90)	(789.46)	(1,264.78)	(3,088.55)	(3,701.61)	(2,261.32)
By type of project:						
Purchase of goods and services	0.20	5.65	31.59	37.44	53.15	25.60
	(6.13)	(113.52)	(441.50)	(543.49)	(666.78)	(435.82)
Public works	1.60	29.89	65.50	113.98	133.73	68.94
	(43.21)	(462.79)	(784.48)	(2,649.80)	(3,021.03)	(1,843.38)
Subsidies to firms and workers	5.081	8.00	14.80	13.95	17.81	11.93
	(51.73)	(102.77)	(166.91)	(120.33)	(230.55)	(147.53)
Observations	8,092	8,092	8,092	8,092	8,092	40,460

mean coefficients; sd in parentheses

Funds are all reported in thousand euros.

Table 3: Instrumental and control variables, by year

	2007	2008	2009	2010	2011	Total
Years since last mayoral elections	2.284	2.955	1.330	1.797	2.005	2.074
	(1.148)	(1.337)	(1.610)	(1.458)	(1.429)	(1.505)
Political alignment	0.814	0.918	0.903	0.818	0.818	0.854
	(0.389)	(0.275)	(0.296)	(0.386)	(0.386)	(0.353)
GDP growth rate, regional	1.549	-1.171	-5.729	1.743	0.387	-0.644
	(0.645)	(1.125)	(1.463)	(1.859)	(0.930)	(3.028)
Employment rate, LLM	46.15	46.13	45.22	44.78	44.76	45.41
	(6.787)	(7.017)	(7.069)	(7.120)	(7.071)	(7.041)
National funds, 1,000 euros	1,438.90	1,846.06	1,988.00	2,003.92	506.27	1,556.63
	(15,882.0)	(21,116.6)	(22,853.8)	(20,509.2)	(7,852.10)	(18,461.5)
Turn out at 2011 referendum	-	-	-	-	-	56.72
						(7.449)
Efficiency of local administration	-	-	-	-	-	0.0448
						(0.0981)
Observations	8,092	8,092	8,092	8,092	8,092	40,460

mean coefficients; sd in parentheses

Table 4: Main specification

	(1) OLS	(2) IV	(3) First stage	(4) OLS	(5) IV	(6) First stage
Log EU transfers per capita	-0.000	0.071**		-0.001	0.070**	
	(0.001)	(0.04)		(0.001)	(0.04)	
Log population density				-0.013***	-0.013***	0.006
				(0.002)	(0.00)	(0.006)
Regional GDP growth rate				-0.002	0.001	-0.050***
				(0.002)	(0.00)	(0.006)
SLL employment rate				-0.001	-0.001	-0.008***
				(0.001)	(0.00)	(0.003)
Log altitude				-0.001	0.001	-0.034***
				(0.003)	(0.00)	(0.009)
Log slope				0.001	-0.002	0.048***
				(0.002)	(0.00)	(0.006)
Coastal				0.045***	0.032***	0.175***
				(0.009)	(0.01)	(0.031)
Years since elections			0.040***			0.040***
			(0.005)			(0.005)
Observations	40300	40300	40300	40300	40300	40300
R^2	0.012	-0.05	0.281	0.014	-0.04	0.285
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
F Statistic		70.35			72.46	

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Public spending efficiency

	(1)	(2)	(3)	(4)
	High efficiency		Low efficiency	
	IV	First Stage	IV	First stage
Log EU transfers per capita	0.012		0.178**	
	(0.02)		(0.08)	
Years since last mayoral elections		0.046***		0.036***
		(0.007)		(0.006)
Observations	19982	19982	20010	20010
R^2	0.05	0.320	-0.16	0.274
Controls	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
F Statistic	45.77		31.51	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Grassroots monitoring

	(1)	(2)	(3)	(4)
	Low Monitoring		High Monitoring	
	IV	First Stage	IV	First stage
Log EU transfers per capita	0.086*		0.027	
	(0.05)		(0.06)	
Turn out at 2011 referendum	-0.002***	0.003	-0.001	-0.003
	(0.00)	(0.002)	(0.00)	(0.002)
Years since last mayoral elections		0.051***		0.029***
		(0.007)		(0.007)
Observations	20147	20147	20153	20153
R^2	-0.05	0.339	-0.00	0.222
Controls	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
F Statistic	58.46		18.23	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Total crime rate

	(1) OLS	(2) IV	(3) OLS	(4) IV
Log EU transfers per capita	0.505***	-0.770	0.374***	-0.175
	(0.080)	(1.91)	(0.077)	(1.81)
Observations	40300	40300	40300	40300
R^2	0.033	0.03	0.106	0.11
Controls	no	no	yes	yes
Year FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
F Statistic		70.35		72.46

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: National transfers

	(1) OLS	(2) IV	(3) First stage
log national transfers pc	0.012***	2.752	
	(0.003)	(6.02)	
Years since elections			0.001
			(0.002)
Observations	38728	38728	38728
R^2	0.014	-18.76	0.690
Controls	yes	yes	yes
Year FE	yes	yes	yes
Region FE	yes	yes	yes
F Statistic		0.218	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Career concerns

	(1)	(2)	(3)	(4)
	First Term		Second Term	
	IV	First Stage	IV	First stage
Log EU transfers per capita	0.065		0.119	
	(0.05)		(0.07)	
Years since last mayoral elections		0.041***		0.034***
		(0.006)		(0.008)
Observations	24792	24792	14163	14163
R^2	-0.03	0.292	-0.15	0.271
Controls	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
F Statistic	47.61		17.44	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Alternative instrument

	(1) OLS	(2) IV	(3) First stage	(4) IV	(5) First stage
log EU transfers pc	0.003***	0.042*		0.034*	
	(0.001)	(0.02)		(0.02)	
Years since elections			0.051***		-0.007
			(0.011)		(0.026)
Alignment					-0.068
					(0.076)
Alignment*Years since elections					0.070**
					(0.029)
Observations	7713	7713	7713	7713	7713
R^2	0.035	-0.12	0.342	-0.06	0.343
Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes
F Statistic		20.83		9.647	
Hansen J Statistic				0.451	

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Lagged effects

	(1) OLS	(2) IV	(3) OLS	(4) IV
log EU transfers per capita	-0.001	0.070**	-0.002	0.100*
	(0.001)	(0.04)	(0.002)	(0.06)
log EU transfers t-1			0.001	-0.012
			(0.002)	(0.04)
Observations	40300	40300	32181	32166
R^2	0.014	-0.04	0.013	-0.09
Controls	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
F Statistic		72.46		15.63

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

CHAPTER 3

Long-term unemployment and firms' occupational choices An evaluation of hiring incentives in Italy

Ilaria De Angelis

Abstract

This paper analyzes the impact of an Italian labor market policy introduced in the early '90s (L. 407/1990) targeted to long-term unemployed. It allows firms to cut their labor cost when hiring long-term unemployed with open-ended contracts. Using a unique dataset drawn from unemployment lists in Veneto and exploiting the source of variation provided by the institutional framework of the policy, I apply a Regression Discontinuity Design to estimate the effect of being eligible for the policy on the job finding rate of long term unemployed workers.

1 Introduction

The last Italian budgetary law, approved at the end of 2014, abolished article 8 of law 407/1990. This article allowed firms to benefit from hiring workers who have been unemployed for at least 24 months.

The aim of the policy was to raise employment opportunities for disadvantaged workers, especially in poorer areas. This paper takes the moves from the abolition of the program to evaluate its effects on the rate of entry into employment of long-term unemployed in the last 25 years.

The empirical strategy relies on a unique dataset on individual unemployment spells in Veneto region from 1990 to 2011 and it exploits the discontinuity in the unemployment duration required for the application of the incentive in order to identify the effect of the policy. I find that the job finding rate raises in the 24th month of unemployment. The effect is stronger for women, aged and low educated workers. The paper proceeds as follows. Section 2 summarizes evidence from previous studies. Section 3 presents the institutional setting of the policy. Section 4 discusses data and descriptive statistics. Section 5 illustrates the empirical strategy. Section 6 shows the results and some robustness checks and Section 7 concludes.

2 Related literature

To my knowledge, this is the first attempt to provide evidence on the effectiveness of the employment incentives contained in the Italian law 407/1990. Anecdotal evidence suggests that, since its entry into force, the program has been widely applied all over Italy; however, after 30 years of implementation, there isn't any empirical evidence on its effects. Previous research in labor policy evaluation in Italy has mainly focused on the so called *Liste di mobilita'* (LM), a program targeted to dismissed workers, that was introduced in the same years. Linking the Veneto Lavoro dataset to records from the Italian social security agency, Rettore et al. (2008) exploit the variation in the eligibility duration according to the age of the worker to evaluate the impact of an additional year in the LM on the re-employment probabilities and wages of enrolled workers. They find that a longer eligibility period has no effect on re-employment rates, except for workers aged 50 or more.

This paper is closest to the work by Rettore et al. (2008) in that it exploits the same Veneto Lavoro dataset in a regression discontinuity framework.

Concerning hiring incentives, the work by Cipollone et al. (2004) evaluate the effects of a tax credit introduced in 2001 for firms hiring on a permanent basis workers aged at least 25 years old who did not hold a permanent position in the 24 months preceding the hiring. Using data from the Italian labor Force Survey, they find that the tax credit increased labor force participation of eligible inactive people by about 1.4 percent in the first year of implementation and by 2.1 percent in the second year, the increase being mostly concentrated among 35-54 years old, low educated workers. the authors argue that the result is driven by the regularization of previous "'under the table"' job contracts rather than by new entries into the labor force. Differently from their work, I can exclude that the policy I am evaluating provides firms with incentives to move out of the black economy, as long as workers have to officially declare their working status via enrollment in unemployment lists in order to be eligible for the program. Other works look at the role of temporary work agencies on the probability of finding a permanent job in Italy (Ichino et al., 2005), at the effect of a compulsory job assistance program for young people in UK (Blundell et al., 2004) and at the displacement effects of job placement assistance in France (Crepon et al., 2013).

3 Institutional setting

Article 8 of law 407/1990 allowed firms to benfit from hiring workers who have been unemployed or have received unemployment insurance for at least 24 commercial months. The incentive consisted in a 50 per cent reduction in social security contributions (SSC) payed by the firm on open-ended contracts related to eligible workers for 36 months, provided that they are not hired in substitution of dismissed workers. The incentive amounted to a 100 percent reduction for firms located in the south or for artisan businesses. In order to be eligible for the incentive, the worker has to submit a "declaration of immediate availability to work"(DID) to the provincial employment center. The unemployment duration required for eligibility is then computed since the date of submission of the DID.
In general, firms hiring dependent workers have to register the job contract to the provincial employment office for the payment of SSC to the worker. Then, as soon as the hiring is registered, the DID relative to the hired worker ceases to be valid for the computation of the unemployment duration. The DID is only suspended if the individual is hired as a dependent worker for less than 5 months or if she is paid less than 8 thousands euros per year. However, if the worker enters again into unemployment after a period of employment, the computation of the unemployment duration is not automatic and she has to submit a new DID.
The so called *"Fornero law"* (law 92/2012) extended similar incentives to other categories of long-term unemployed. In particular it provided the following subsidies: (i) 50 percent rebate on SSC for 12 months on temporary contracts to women in unemployment for at

least 24 months; (ii) 50 percent rebate on SSC for 18 months on open-ended contracts to women in unemployment for at least 24 months; (iii) 50 percent rebate on SSC for 12 months on temporary contracts to workers aged 50 or more in unemployment for at least 12 months; (iv) 50 percent rebate on SSC for 18 months on temporary contracts to workers aged 50 or more in unemployment for at least 12 months.

Even after law 92/2012, the incentives provided by law 407/1990 still remain the largest and most used ones. However, since 2012 it is no longer possible to disentangle the effect of the two policies. Then, I only restrict the sample to DID ended before 2012.

4 Data and descriptive statistics

4.1 Data

Data are taken from unemployment lists in Veneto, provided by Veneto Lavoro, and cover the period from january 1990 to june 2013. The dataset contains individual characteristics for more than 2 million workers enrolled in the list (gender, age, education, citizenship), the date of enrollment in the lists, the expiration date of the enrollment and the motivation for expiration. The dataset also provides information on the date and the type of job contract for hired workers. I restrict the sample to workers whose DID is no longer valid because of hiring. Other motivations for the invalidity of the DID are compulsory retirement, death, change of province of residence, inclusion in the LM for dismissed workers, unmotivated rejection of job offers.

The final sample is then composed by more than 586,634 individuals in the years 1991-2012.

The running variable for the implementation of the RDD is the unemployment duration. I consider the individual unemployment duration as the number of commercial months between the date of submission of the DID and its end date of validity. Then, among all unemployed workers enrolled in the unemployment lists, the treatment group is composed by workers reporting an unemployment duration equal or larger than 24 commercial months. Following the rules imposed by the law, commercial months are computed as follows: if the effective unemployment spell includes at least 15 days in a month, the entire month is computed in the total spell; if instead the unemployment condition lasts less than 15 days in a month, that month is excluded from the computation.

Treated individuals amount to 112,474. The dependent variable is the job finding rate in the n-th month. It is computed as the number of workers finding a job over the total number of workers enrolled in the unemployment lists in that same month.

4.2 Descriptive Statistics

Table 1 reports the relevant individual characteristics and the average unemployment duration for treated and non treated workers. The gender variable takes value 1 for males and 0 for females, whereas the education variable takes value 1 for primary or lower secondary education, 2 for upper secondary education, 3 for university degrees and 4 for postgraduate diplomas. As it is clear from table 1, the main difference between treated and control group is in the unemployment duration, while there isn't any relevant difference in the observable characteristics between treated and control group. Major differences do not emerge even focusing on workers in their 23rd month unemployment against workers in their 25th month (Table 2). This evidence is also in favor of the identification assumption that unemployed workers do not differ significantly in those observable characteristics that are relevant for the assignment to treatment.

5 Empirical strategy

Following Rettore et al. (2008), in order to estimate the effect of the policy for treated workers, I compare the mean job finding rate Y^T of the treatment group ($D = 1$) with that of the control group ($E[Y^{NT}|D = 0]$) and get the following identity by adding and subtracting the (unobserved) term $E[Y^{NT}|D = 1]$:

$$E[Y^T|D = 1] - E[Y^{NT}|D = 0] = E[Y^T - Y^{NT}|D = 1] + E[Y^{NT}|D = 1] - E[Y^{NT}|D = 0] \tag{1}$$

where $E[Y^T - Y^{NT}|D = 1]$ is the average treatment effect on the treated (ATT) and the difference in brackets is the selection bias due to the different composition of the two pools with respect to unemployment duration.

However, since the assignment to treatment deterministically depends on unemployment duration expressed in motnhs (U), by conditioning on $U = 24$, the selection bias becomes:

$$E[Y^{NT}|D = 1, U = 24] - E[Y^{NT}|D = 0, U = 24] \tag{2}$$

which is reasonably equal to zero as long as the conditional mean $E[Y^{NT}|U]$ is a continuous function in a neighbourhood of U=24 (Hahn et al., 2001).

Then, the main assumption for the identification of the ATT effect is that treatment and control groups are similar with respect to those observable characteristics that may simultaneously affect eligibility status and employment outcomes around the 24 threshold. Indeed there might be some kind of sorting around the 24th month threshold: among the pool of eligible workers, those who get a job may also be more able in signalling their eligibility to firms in order to raise their chances of being hired. Then firms can manipulate the pool of treated individuals by choosing among the "best signalers". However, provided that the composition of the treatment and control group is similar with respect to observable characteristics that might simultaneously affect unemployment duration and employment outcomes, there is no reason why "signalling" ability should differ around the 24th month threshold, especially in the case of long term unemployed, which in general might show worse signalling abilities with respect to other workers.

I use a local polynomial regression of degree 1 to estimate the conditional expectations of the job finding rate for the treated and control groups at the 24th month threshold. Weights follow a standard epanechniknov kernel function, whereas the bandwidth is calculated using a rule-of-thumb bandwidth estimator. Confidence intervals are set at the 95 percent confidence level.

6 Results

6.1 Main results

Figure 1 plots the job finding rate over the average unemployment duration measured in months since the enrollment into the unemployment lists. It is evident that the probability of finding a job is around 10 percent in the first month and it drops dramatically afterwards. This is in line with the main predictions of standard job search models [1]. Such models suggest that the average unemployment duration is the inverse function of the so called *hazard rate*, that is the probability of accepting a job offer, which in turn depends on the contact rate between firms and workers and on the reservation wage distribution. The hazard rate is likely to be affected by many factors. In particular, the contact rate between firms and workers may decrease over time because of a reduction in the job search activity due to discouragement on the worker's side. On the other hand, in presence of unemployment insurance or alternative sources of income, the worker's reservation wage may raise, by reducing the probability of finding a job.

Then one would expect that, in the absence of any policy intervention, as we move on the left of the job finding rate distribution over time, the probability of finding a job approaches to zero.

On the contrary, in presence of an effective labor market policy aimed at raising employment, one should observe an upward shift in the job finding rate at some point. Indeed, figure 1 shows that there is a significant jump in the average job finding rate between the 23rd and the 24th of unemployment.

[1] A review of job search models can be found in Rogerson et al. (2004)

I am also interested in exploring whether the policy had heterogeneous effects on different types of workers. In general, one would expect that female, older or less educated workers are less likely to find a job.
Figure 2 plots the job finding rate for male and female workers. As expected, being eligible for the SSC discount raises the probability of finding a job by around xxx percent for women, whereas the increase is close to zero for men.
Nonetheless males' job finding rate shows a shift around the 12th month. That jump might be explained by other temporary labor policies implemented at local level around the 12th month threshold that are not reported the data, such incentives for apprenticeship contracts, which are very frequent in Veneto region. However, the confidence interval around that threshold is very large, reassuring on the fact that the jump is not significant.
Figure 3 and 4 confirm that the effect of the policy is larger for older (aged 35 or more) and less educated workers (holding a primary or middle secondary school diploma at most), respectively, compared to their younger and more educated counterparts.
Concerning education, it is worth noting that the distribution of the job finding rate for more educated workers enrolled in unemployment lists is highly dispersed in the first months of unemployment and the probability of finding a job is lower on average among eligible workers in this category, even though the unemployment duration seems to be shorter on the right of the threshold. This counterintuitive evidence might be partly explained by sample composition issues: as long as more educated workers have access to more job search channels (Blau and Robins, 1990), it is reasonable to think that they are less likely to apply for jobs at the provincial employment office. Therefore, those who enroll in the provincial unemployment lists may represent the left tail of the graduates' distribution.

6.2 Robustness

In order to validate the identification strategy, I carry out two specification checks. The main concern is about *selection into treatment*. In this framework unemployment duration may depend on those variables (e.g. age and education) that are affected by the same unobservables relevant for the job finding rate. For example, less educated workers may be more likely to remain in unemployment due to the lack of (unobservable) professional skills, whereas older workers may face lower re-employment probabilities due to (unobservable) firms' preferences towards young workers. If these are the cases, one should observe a jump in the education and age variable around the threshold, that would cast doubts on the validity of the main identification assumption. Figure 5 shows that age and years of education are continuous functions of the unemployment duration, reassuring on the fact that there hasn't been any selection in the assignment to treatment with respect to these observables.

As a second check, I run a placebo test by estimating the effect of interest at two fictitious thresholds for the implementation of the policy (20 and 30 months of unemployment). The idea behind the test is that, if the effect of interest is driven by omitted variables that are correlated both to the unemployment duration and to the job finding rate, then the latter should show a significant jump even in the absence of the policy. Figure 6 shows that this is not the case neither at the 20th nor at the 30th month threshold.

7 Conclusions

This paper shows that employment incentives in the form of SSC rebates contribute to reduce long-term unemployment by raising the job finding rate of eligible individuals. In particular, I have analyzed the effect of a specific labor market policy introduced at the end of 1990 by article 8 of law 407 and abolished in 2014, that provided a 50 percent rebate on SSC for three years to firms hiring workers in unemployment for at least 24 months.

The effect of the policy is heterogeneous across different types of workers. In particular, while the effect is null for young, males and high educated workers, it is larger for females, elderly and low educated workers, that are more likely to remain longer in unemployment.

I have also shown that the effect of interest is not driven by differences in age and education between the treatment and control group.

These findings suggest that, despite the magnitude of the effect is small, the policy is more effective for more vulnerable categories of workers.

Table 1: Individual characteristics of unemployed workers, by treatment status

	(1)		
	Not eligible	Eligible	Total
gender	0.465	0.372	0.447
	(0.499)	(0.483)	(0.497)
age	32.85	32.73	32.83
	(10.39)	(10.11)	(10.34)
education	9.269	9.536	9.319
	(5.029)	(5.017)	(5.028)
Duration of unemployment (in commercial months)	6.901	43.75	13.75
	(6.171)	(19.54)	(17.54)
Observations	501492		

mean coefficients; sd in parentheses

Table 2: Individual characteristics of unemployed workers around the threshold

	(1)		
	23rd month of unemployment	25th month of unemployment	Total
gender	0.428	0.424	0.426
	(0.495)	(0.494)	(0.494)
age	32.46	33.10	32.78
	(10.16)	(10.13)	(10.15)
education	9.041	9.043	9.042
	(4.985)	(4.878)	(4.930)
Duration of unemployment (in commercial months)	23	25	24.02
	(0)	(0)	(1.000)
Observations	9387		

mean coefficients; sd in parentheses

Figure 1: Job finding rate and unemployment duration

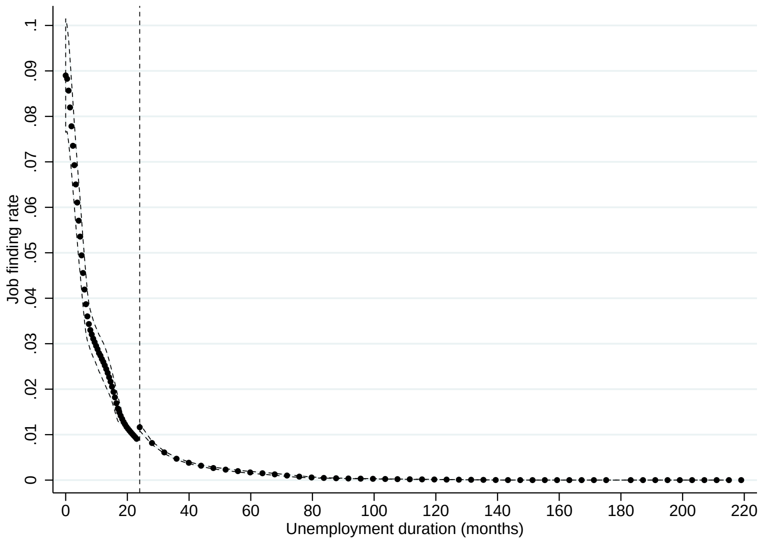

Figure 2: Job finding rate and unemployment duration, by gender

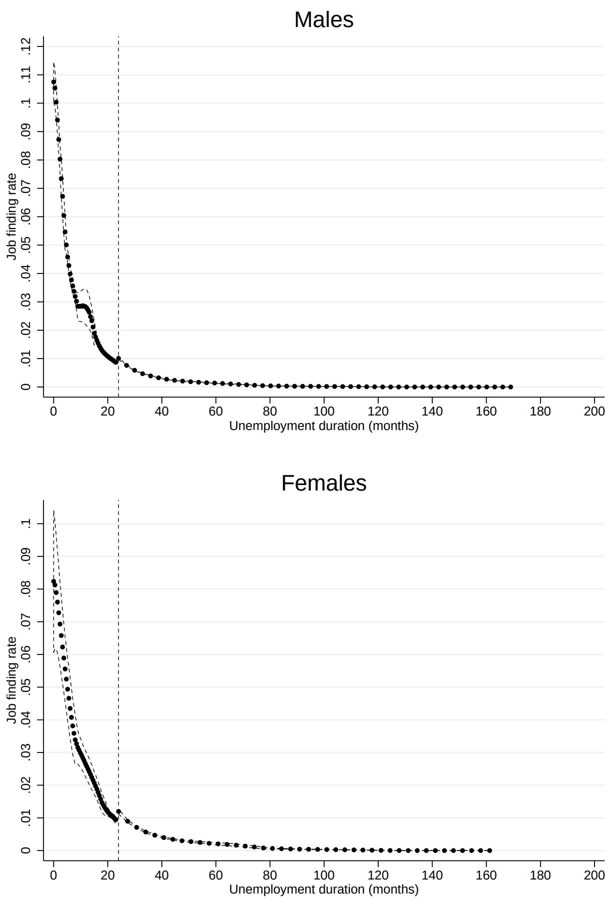

Figure 3: Job finding rate and unemployment duration, by age

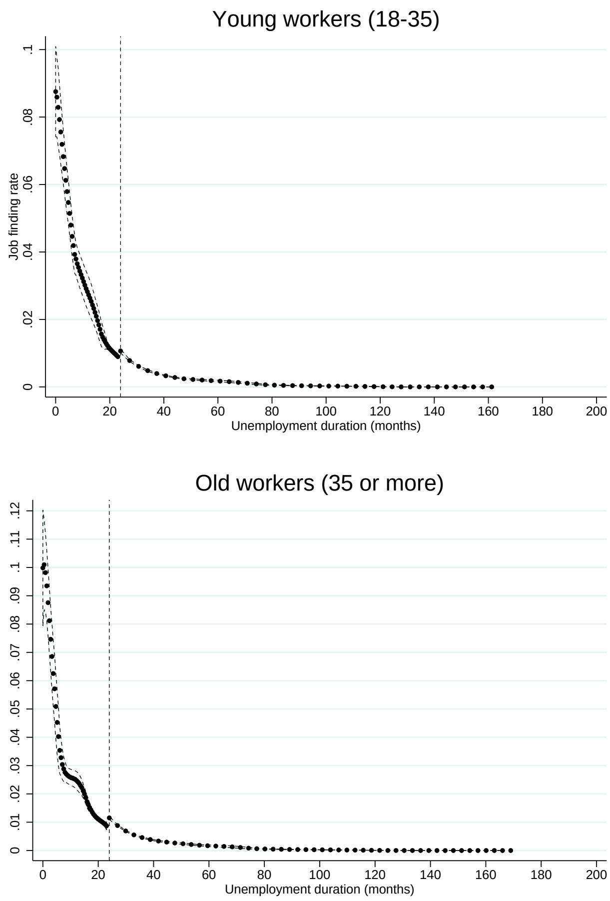

Figure 4: Job finding rate and unemployment duration, by level of education

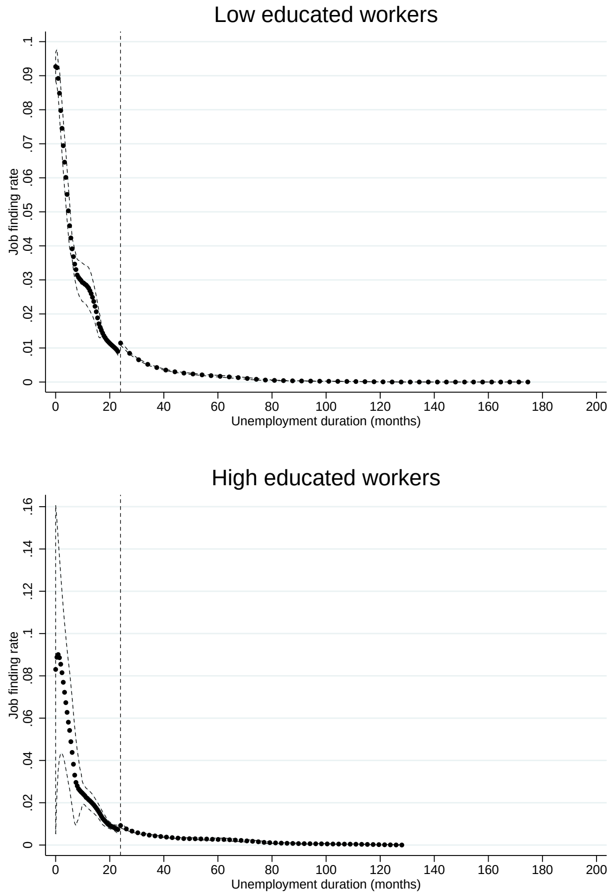

Figure 5: Age and unemployment duration