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Hard to Forget: Long-lasting Effects of Social Capital Accumulation Shocks

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Abstract

Very few contributions have dealt with the analysis of specific determinants of social capital accumulation and destruction. Even if limited in scale, the analysis of precise historical events can help in discerning the dynamics of social capital and its persistence. The case of Italian unification is here considered. I focus on three historical episodes of conflict, which caused the death of a big fraction of population in three specific locations. When towns in the areas surrounding these locations are considered, I show how each kilometer further from the hit town is associated with a significant increase in the electoral turnout in European Parliament elections held in 1979-1999. I believe these differences to reflect differences in social capital endowments across towns. The pattern is confirmed when World War I casualties are used as a measure of social capital at the beginning of the XXth century. Results are robust to the inclusion of a number of controls and to several robustness checks.

Keywords: social capital, political participation, social capital persistence, Italy.

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1 Introduction

War and conflict typically yield dramatic changes in the environment individuals live in and interact with. History can be surprisingly violent, and its manifestations can rapidly alter the economic and social structure of society. Exposure to war and conflict is likely to shape shared norms and beliefs in a highly persistent way. But, even when the turbulence of events reaches its peaks, communities may be exposed to it to different extents.

I suggest that differences in the level of awareness of specific historical episodes can lead to persistent differences in the way communities live and function. I focus on the history of unification of Italy in 1860-61, and the so called *war against brigandage* in the following years. I look at three main specific events: the siege of Gaeta, the massacres of Pontelandolfo and Casalduni and the ones of Latronico and Castelsaraceno. In all cases, a big fraction of the local population was killed. Memories of the same were likely to remain vivid well into the twentieth century (Duggan, 2007). These facts credibly shaped the environment, norms and beliefs of individuals in the local communities. The close contact with violence and death, perpetrated by the army of the new king, presumably lowered the expectations of trustworthiness towards the political authority, prevented the same authority from the implementation of credible rule enforcement mechanisms, and undermined the rise of generalized trust towards fellow citizens in the northern part of the country. In other words, the awareness of these events reasonably threatened the process of social capital accumulation and the spread of civicness over the unified country. I thus claim that proximity to one of the above locations is strongly correlated with social capital endowments as observed around 120 to 140 years later. Closeness is thought to be associated with a higher probability of direct testimony, and/or stronger connections between individuals and communities. Proximity to one of the stated locations is then coupled with deeper awareness of those violent events, bigger shocks to the environment and individuals' norms and beliefs, and thus more credible threats to the social capital accumulation process.

I focus on the towns located in the areas surrounding the three specified locations and hit towns. In the spirit of Putnam (1993), I use European Parliament elections turnout as measure of social capital and generalized trust. To some extent, I argue European elections turnout to be comparable with referenda turnout, a measure of social capital widely used in economic literature (Guiso, Sapienza and Zingales, 2004, 2008b). This because European politicians are far from being easily monitored, and citizens have little awareness of EU institutions and their activity (Bonet, Muñoz and Torcal, 2007). Voter participation is in this case particularly correlated with expected trustworthiness of candidates. Using data from elections in year 1979, 1989 and 1999, I show how, after controlling for population and other town level characteristics, each kilometer further from the hit town is significantly associated with a 0.2-0.5 percentage points increase in the electoral turnout. Results are reasonable in magnitude, and robust to the inclusion of additional

controls which capture differences in schooling and age distributions at around the time of the elections. The effect is also found not to be heterogeneous across areas and time periods. Causal interpretation of the above requires towns closer to the hit ones to be comparable with those further in terms of unobservables in 1860-61, and thereafter not to have been exposed to other shocks in a systematically different way, so that differences in social capital today can be interpreted as being the result of the suggested effect of conflict exposure. I claim this to be a reasonable assumption. First, all towns in the sample shared the same formal institutional arrangement after unification. Second, results are unaltered after controlling for population in 1861.

I also consider the total number of World War I casualties of individuals born in each town in the selected sample. Under the assumptions that the military ability distribution was constant across towns, and that the probability of death in war was the same for all individuals with the same ability and military grade, casualties can be used as a proxy for the number of individuals from each town who went to fight in World War I. The latter is presumably related to rule enforcement and social capital endowments at the beginning of the XXth century. I show how, given population and other town level characteristics, each kilometer further from the hit town is associated with a significant 0.4-1 units increase in the number of casualties per town: a confirmation of the suggested pattern and a further indication of social capital persistence.

The conceptual framework is developed in Section 2, together with discussion of the relevant literature. Section 3 provides the historical background and details on the events under investigation. The identification strategy is discussed in Section 4, while Section 5 presents the data, the details of the sample selection and variable descriptions. Section 6 deals with the empirical estimation, results and robustness checks. Section 7 concludes.

2 Social Capital Accumulation and Destruction

The concept of social capital has been shown to be able to partially explain a wide range of economic phenomena, from economic growth (Knack and Keefer, 1997) to institutions performance (Putnam, 1993) and financial development (Guiso, Sapienza and Zingales, 2004). In the devoted chapter of the Handbook of Social Economics, Guiso, Sapienza and Zingales (2010) provide a recent survey and deal with the subtle issues of definition and measurement. Throughout the paper, I will refer to their definition of social capital as civic capital, i.e. *those persistent and shared beliefs and values that help a group overcome the free rider problem in the pursuit of socially valuable activities.* In this section, I will explore the relationships which compose my argument, disentangling this definition in its fundamentals, and discuss the relevance of each one in light of the paper's objective.

Persistence is one of the most interesting features of social capital. Evidence of long-term persistence has been suggested in several recent contributions, starting with the insights of Putnam (1993). In the latter, differences in civic capital between the north and the south of Italy are imputed to the different degree of exposure to the free city-state experience that ended more than five centuries ago. Guiso, Sapienza and Zingales (2008b) empirically test this hypothesis by focusing on the cities in the center-north and identifying a set of historical determinants of the free city-state experience. Using the number of non-profit organizations per capita, turnout at major referenda, and presence of an organ donation organization as measures of social capital, when the free city-state experience is instrumented by the identified predictors, they find a positive effect of the free city-state experience on today's levels of social capital. They also perform a matched difference in difference estimation by comparing differences between free city-states and control cities in the north with differences between cities in the south which the historical determinants predict to have been likely to become free city-states (but didn't because of the power exerted by the Normans) and control cities. Overall results suggest that 47% of the difference in social capital between north and south in Italy are due to the free city-states experience. The same issue of very long term persistence in relationship with historical determinants is dealt with and extended in Tabellini (2010), where institutional arrangement in XVII and XVIII centuries is shown to partially explain today's differences in values across the regions of Europe. Durante (2010) calls for the role of geography and climate instead. Climate variability is thought to be the most relevant source of risk in rural pre-industrial economies. In the author's perspective, higher historical climate variability is thus associated with higher payoff from collective action and cooperative behavior in the past, which fostered the emergence of a highly persistent culture of trust. He shows how within- and between- areas climate variability are able to partially explain today's differences in social capital. All these contribution suggest persistence of social capital to be very high. It is thus not implausible to expect the historical episodes I put under investigation to have an effect on social capital as observed even 120 to 140 years later.

Here I argue that the events at stake threatened the social capital accumulation process. Few contributions have dealt with the issue of social capital destruction. One example, related with the issue of long term persistence, is the work by Nunn and Wantchekon (2009), who focus on the disruptive effects of slave trade in Africa. They show how individuals whose ancestors' ethnic groups were heavily exposed to slave trade are less trusting today. Another example is the one of Jacob and Tyrell (2010), who show how the density of Stasi informers in the former GDR is significantly related to social capital levels in today's East Germany. The logic behind the suggested links in both these contributions is once again that historical events can lead to the emergence of a culture of mistrust, and undermine the accumulation of social capital. In particular, as in Jacob and Tyrell (2010), it is possible to trace back today's differences in social capital to the means of exertion of political power. With this respect, the works of the sociologist Portes (1998) and the

political scientists Boix and Posner (1998) offer the most interesting insights. They both suggest the circular dimension of social capital or, in more familiar terms, the problem of reverse causality: social capital makes rule enforcement mechanisms and compliance less costly, but, relevantly, well-government enhances cooperation. In the framework of this paper, the considered historical episodes of conflict lowered the expectations of trustworthiness towards the political authority. This presumably prevented the same authority from the implementation of credible rule enforcement mechanisms, and undermined the rise of generalized trust towards fellow citizens in the northern part of the country. In other words, consistently with the circular argument above, I argue that these events have trapped local communities in a negative self-reinforcing spiral of low civicness. This mechanism is similar to the one explored by the social psychologists Zamperini and Menegatto (2011) in the analysis of the riots and police intervention in Genova, Italy, during the 2001 G8 meeting. The authors elaborate the concept of *psycho-political trauma*, which makes individuals who experience it feel delegitimated as citizens and victims of what they call *civic expulsion*. The question then becomes how big the impact of these events could have been. I claim they shaped the environment, norms and beliefs of individuals in the local communities. In the medical and sociological literature, war has been shown to have a significant impact on beliefs, behavior and structure of society[1]. The impact of war on beliefs has also been investigated in some empirical (Bianchi, 2010) and theoretical (Anderlini et al., 2010) contributions in economics. Even in absence of a precise answer about the magnitude of the effect, all these contributions point in direction of a significant effect of war on beliefs and behavior of individuals and communities.

Going back to the definition of social capital at the beginning of the section, it is worth noticing that it does not distinguish explicitly between norms and beliefs. A community may exhibit high levels of trust because individuals' preferences are biased towards cooperation, or because members have positive beliefs about others' trustworthiness, or both. The empirical literature has voluntarily escaped from a clear distinction between the two cases[2]. Nonetheless, the discussion is relevant in terms of the theoretical framework to think of. Bisin and Verdier (2001, 2010) and Bisin, Topa and Verdier (2004) model cooperation as a transmitted cultural trait, whose persistence is driven by a mechanism of intergenerational transmission where parents evaluate their children's payoff in terms of their own preferences. A similar framework is used in Tabellini (2008), with the additional modeling of limited vs. generalized trust. Guiso, Sapienza and Zingales (2008a) model instead intergenerational transmission of beliefs. Simulation results show how even a brief

[1] See among the others, Başoglu at al., 2005; Summerfield, 2002; Modell and Haggerty, 1991; Kiser, 2001.

[2] In Sapienza, Toldra and Zingales (2008), the authors use experimental evidence to try to distinguish between the *preferences*-related and the *beliefs*-related components of trustful behavior. Expected trustworthiness is shown to significantly increase the quantity sent by the sender in a modified trust game. They thus conclude the beliefs component of trust to be the driving determinant of trustful behavior. Furthermore, answers to the World Value Survey (WVS) trust-related type of questions, aimed at capturing generalized trust, are shown to be significantly positively correlated with expected returns.

shock in the payoffs from cooperation can have long-lasting effects on the beliefs and social capital of a community. In the background of my analysis, I ideally refer to this last contribution.

3 Narrowing Down the Unification of Italy

I focus on the history of unification of Italy in 1860-61, and the so called *war against brigandage* in the following years. In particular, I consider the war of annexation of the Kingdom of the Two Sicilies to the Kingdom of Sardinia, which started with the expedition of the Thousand and the landing of Garibaldi in Marsala (Sicily) on May 11, 1860, and lasted until right after the end of the siege of Gaeta and the proclamation of the creation of the Kingdom of Italy on March 17, 1861[3]. The war against brigandage is broadly referred to as the collection of battles and conflict episodes fought by the piedmontese army against bands of brigands in Southern Italy after unification. The one of brigandage is a highly controversial phenomenon, somehow in between organized resistance of former Bourbon officials, soldiers and supporters, and generalized crime, fostered by poverty, disillusionment towards the new order and political instability. According to Duggan (2007), local population was regularly in contact with the brigands, and the draw of a clear line of separation between resistance, crime and everyday fight for survival of peasants seems to have been at the time extremely hard. What is certain is that 100,000 soldiers, half of the Italian army, were deployed in the South in 1864 to maintain order.

Given the nature of the question at stake, it is worth highlighting that, even if the myth of the unified nation had been vivid in the minds of the elite long before 1861, the vast majority of the inhabitants of the peninsula were found unprepared by the events. From the very first moment after the piedmontese forces had occupied Southern Italy, it was undoubtedly clear to the average peasant that any hope of changes in the status quo was vain. In order to maintain order and stability at the local level, substantial continuity was guaranteed to the power of *galantuomini* in the majority of cases (Duggan, 2007). It is thus reasonable to think that the larger part of the population in Southern Italy had been relatively unaffected by the events of unification. This supports the credibility of my strategy, which consists in pointing at specific episodes and discussing proximity to the involved locations as one of the determinants of the outcome of interest.

3.1 Gaeta

The siege of Gaeta is usually recognized as the last relevant event in the war of annexation of the Kingdom of the Two Sicilies to the Kingdom of Sardinia. The siege started in November, 1860 and ended on February 13, 1861. After leaving the city of Naples in

[3]Duggan (2007) provides an extensive treatment of the history of the unification of Italy and the following years.

September, 1860, the Sicilian King Francis II and his wife Marie Sophie took refuge in Gaeta, a coastal fortress in the modern region of Lazio. In November, the piedmontese forces, led by the general Cialdini, started to bomb the city adopting new long-range cannons. Soldiers and inhabitants were massed in the old fortress. With soldiers and population killed by the bombs and epidemic typhus, and with foreign ships of Napoleon III abandoning the harbour of Gaeta in January, Francis II and his wife consigned themselves to the victors on February 14. Casualties amounted to a thousand among the sicilian soldiers, and between 200 and 300 among civilians (Di Fiore, 2010).

3.2 Pontelandolfo and Casalduni (PC)

The events at Pontelandolfo and Casalduni (henceforth, PC) are usually referred to as the most emblematic episodes of the war against brigandage. According to Duggan (2007), on the evening of August 7, 1861, as the annual celebrations in honor of San Donato were beginning, a local troop of brigands led by the former Bourbon soldier Cosimo Giordano joined the local community of Pontelandolfo to hear Vespers. Encouraged by the presence of the brigands, the people of Pontelandolfo ordered Don Epifanio, the priest, to go into the parish church and sing a *Te Deum* in honour of the deposed Bourbon king. They then marched towards the headquarters of the National Guard and the town hall, destroyed the Savoy symbols and registers of birth, looted the homes of the local *galantuomini* and murdered the local tax collector. As the echoes of these facts spread, a detachment of *bersaglieri* was sent to investigate the issue. The troops imprudently entered the town, and faced the anger of the local population. Forced to leave the city, forty-one soldiers were killed while crossing the neighborhoods of Casalduni. Informed of the facts, the piedmontese general Enrico Cialdini, newly installed military and civil commander in Southern Italy, ordered his officers to reduce the towns of PC to a 'heap of rubble' in retaliation. On August 14, the troops entered in Casalduni, but the vast majority of the population had left in advance. The three people left were murdered. The whole population of Pontelandolfo was instead caught on time. The town was terrorized during five or six hours. Many atrocities were committed. In the words of one *bersagliere* reported by Duggan (2007), soldiers were encouraged to loot at will and instructed to shoot everyone except women, children and the infirm. Troops set fire to the town. Estimates put the number of those killed at 400 (over a population of 3000), with many more being arrested and shot in the weeks that followed.

3.3 Latronico and Castelsaraceno (LC)

Out of the three, the events at Latronico and Castelsaraceno (henceforth, LC) are by far those which have been studied the least. According to Duggan (2007), in December 1860 a government decree was passed, which called up all those in their early twenties to join the new Italian army. As previously stated, after unification the majority of local *galantuomini* were assured a certain degree of continuity in the exercise of their power, irrespectively of whether they had been supporters of the Bourbons or not. In the years when the

brigandage phenomenon was widespread, local rich landlords usually played opportunistically on both sides. In this context, local authorities, under the threat of brigands, were reluctant to the enforcement of the decree. As a result, starting in the beginning of 1861, patrols of government troops travelled throughout Southern Italy searching for draft-dodgers. According to Pedio (1987) and Duggan (2007), in the morning of February 1st, the troops arrived in the area of LC. Anyone who was thought to be between 20 and 25 years old was arrested and summarily shot as a deserter, the vast majority of them being peasants.

4 Identification Strategy

Is proximity to one of the above locations within the determinants of social capital endowments as observed even around 120 to 140 years later?

In the spirit of Putnam (1993) and, more recently, of Guiso, Sapienza and Zingales (2010), I first use European Parliament election participation as measure of social capital. Voter participation entails a positive cost for the individual. Politicians are in general far from being easily monitored. This is particularly true for politicians elected in the European Parliament, as european citizens have in general little awareness of EU institutions and their activity (Bonet, Muñoz and Torcal, 2007). As costs of monitoring politicians increase, voter participation is more correlated with expected trustworthiness of candidates. Moreover, the supranational nature of the European Parliament, together with its limited scope of intervention with respect to national parliaments, makes electoral turnout less prone to contamination problems due to the so called "exchange vote"[4]. I thus argue European elections turnout to be comparable with referenda turnout, a measure of social capital widely used in economic literature (Guiso, Sapienza and Zingales, 2004, 2008b). Furthermore, recent experimental evidence shows expected trustworthiness to be highly positively correlated with the answer to the standard World Value Survey (WVS) trust-related type of questions, aimed at measuring generalized trust[5] (Sapienza, Toldra and Zingales, 2008).

In order to measure social capital endowments earlier in time, I then come to consider casualties in World War I. The willingness to risk life fighting for a war is closely related to the levels of civic engagement and generalized trust towards fellow citizens. I thus believe the number of World War I casualties of individuals born in a given town to work as a

[4]The exchange vote is a widespread phenomenon in Italy, and relatively more present in southern regions. In presence of exchange vote, citizens are asked to vote for a particular candidate in exchange for personal benefits, gained through political intervention within or outside the legal spectrum. With the introduction of new generation mobiles, endowed with photo cameras, the problem has received increasing attention. On April 1, 2008, the italian government passed the Decree-Law n. 48, which prohibited the introduction in the electoral cabin of any mobile or facility capable of capturing or recording images.

[5]The WVS does not provide enough information to perform the analysis as the town level, so I cannot use answers to the trust-related questions as a measure of social capital.

proxy for the number of individual who went to fight in World War I, a credible measure of social capital endowments at the beginning of the XXth century. This is true under a set of specific assumptions. First, the military ability distribution needs to have been constant across towns in the sample. Second, given the military grade, the probability of death in World War I needs to have been the same for all individuals with the same ability. To the best of my knowledge, this is the first time that World War I casualties are used as a measure of social capital in Italy in the 1910s.

I claim that closeness to one of the towns where the above reported events occurred is coupled with deeper awareness of the same, and thus bigger shocks to the environment and individuals' beliefs. This because proximity is thought to be associated with a higher probability of direct testimony, and/or stronger connections between individuals and communities. Here I explore the relationship between distance to the hit town and social capital as measured by electoral turnout and war casualties. In order to attach to the findings a causal interpretation, it is crucial to frame the distance measure as being as good as randomly assigned. Is this credible? While it is undoubtedly hard to believe the specific hit towns within the area to have been randomly chosen, it is easier to think at the remaining towns in the sample as having been not sistematically different at the time of the conflict in terms of unobservable determinants of social capital. It is useful to frame distance from the hit town as being a measure of *treatment intensity*, with the treatment being (indirect) exposure to the mentioned violent historical events. In the empirical analysis, I compare those towns exposed to higher treatment levels, closer to the hit towns, with control towns, further away from the hit ones. After controlling for altitude and surface of the town's territory, I claim the two groups to be comparable in terms of unobservables in 1860-61, and thereafter not to have been exposed to other shocks in a systematically different way, so that differences in social capital endowments can be interpreted as being the result of the suggested effect of conflict exposure. Notice that all towns in the sample shared the same formal institutional arrangement after unification. In order to shed further light on the validity of this underlying assumption, I include in the analysis data on population in 1861 for the whole sample. This is supposedly as far I can possibly get in terms of 1860 observables, and results are shown to be robust to the inclusion of this variable as a control.

I also include in the analysis a wide range of additional controls, measured at around the same time of European Parliament elections[6]. These variables control for the schooling and age distributions in each town, which are likely to have an impact on social capital endowments.

[6]I focus on elections in years 1979, 1989 and 1999, using as controls Census data from 1981, 1991 and 2001 respectively. The Data section (5) provides detailed data information.

5 The Data

5.1 The Sample

Having focused on the three specified locations, I constructed the sample to include for each area around 40 towns closest to the hit towns in terms of geographical distance. Selected towns, together with their administrative borders, are shown in Figure 2. The final sample counts 120 observations, with towns belonging to 7 different provinces and 5 different regions. Figure 1 shows the political map of the Italian peninsula between 1815 and 1870. Comparing Figure 2 with Figure 1, we can see how the selected towns surrounding Pontelandolfo-Casalduni and Latronico-Castelsaraceno were all incorporated in the Kingdom of Two Sicilies in 1815, and were annexed to the Kingdom of Sardinia without any change in domination occurring until unification[7]. The area surrounding Gaeta is shown to be possibly more problematic, as towns in the norther part were part of the Papal State, together with the papal enclave of Pontecorvo. Nonetheless, since no relevant historical events are recorded in the area prior to unification, I still consider the area to be sufficiently homogeneous in terms of town-level unobservables. Moreover, differences across towns in the area should not be systematically related with distance from Gaeta.

5.2 Variables Description

In the empirical analysis, I use data on European Parliament elections in June of years 1979, 1989 and 1999. Town-level data on *electoral turnout* are available from the Elections Historical Archive of the Italian Ministry of Internal Affairs[8]. Data on World War I casualties are extracted from the *Albo d'Oro* of World War I, a 26 volumes work edited by the Ministry of War during Fascism. It provides a complete record of all World War I casualties of italian citizens, with place of birth, military grade and information on the death circumstances. Figure 6 shows as an example the cover of the second volume and its first page, and one record in details. For each town in the sample, I identified all entries belonging to individuals born there, and counted the total number of casualties and casualties among infantrymen.

Given the nature of the question at stake, the motivation for the adopted *distance* measure is crucial. I chose to discard travel distance and use geographical distance instead as contemporary road networks are mostly subsequent to unification. This could possibly confound identification. On the other hand, it would be more appropriate to use travel distance in around 1860, but data are extremely hard to collect. I will thus rely on geographical controls as capable to net out possible ruggedness-related confounding factors.

[7]The town of Benevento, on the boundary of the correspondent area of Latronico-Castelsaraceno, is excluded from the sample. First, being a county seat (*capoluogo di provincia*), confounding factors might work in unpredictable directions. Second, as shown in Figure 1, its territory was a papal enclave before 1806 and after 1815, until it was annexed to the Kingdom of Sardinia in 1860.

[8]Archivio Storico delle Elezioni, Ministero dell'Interno.

Data on *population* at the town level are Census data, collected and distributed by ISTAT, the Italian National Institute of Statistics. 1861 and 1871 Census population data are provided in ISTAT (1997). Data from Census 1981, 1991 and 2001 are accessible through the Communes Statistical Atlas (Atlante Statistico dei Comuni) (ISTAT, 2009), together with data on *altitude*[9] in meters and *surface* in hectares of territory under the jurisdiction of each town. From the same source, I also use data on *schooling* and *age* distributions in each town as additional controls. In particular, I use data on: number of individuals having university degree or high school diploma as highest qualification obtained, number of individuals aged 0-19, 20-39, 40-59, 60 or more.

6 Estimation and Results

The first part of the empirical analysis is a cross section analysis of 1979 European Parliament elections town-level turnout data. Observations are reduced to 119, as one town in the sample was not an autonomous municipal unit yet, as it comes to be by 1989. Figure 3a presents a graphical representation of the relationship between 1979 electoral turnout and distance from the closest hit town for the whole sample. Figure 3b provides the same picture, adopting different colors to identify observations belonging to the three different areas. The graph shows how, consistently with the suggested link, the distance variable is positively correlated with electoral turnout in the sample. However, Figure 3b also suggests that towns belonging to different areas might be unevenly distributed according to the distance measure. Given the way I constructed the sample, including almost the same number of towns per location, this reflects a higher or lower density of towns in the three different areas. This may induce an over- or underrepresentation of towns closer or further away from the closest hit towns, lowering the sample variability of the distance measure, making identification of a linear effect more complicated. Looking at the same graph, I conclude this is not the case, as differences in density of towns per area seem to cancel out in the overall sample. Moreover, possible differences in the effect of interest across areas are explored and tested in the second part of the analysis.

The empirical analysis in this part is based on the following regression specification:

$$euro_{ij} = \alpha + \beta\ distance_{ij} + \lambda\ pop_{ij} + \mathbf{X}'_{ij}\varphi + \gamma_j + u_{ij}, \tag{1}$$

where $euro_{ij}$ is 1979 European Parliament elections turnout in town i in area j, with $j \in \{Gaeta, PC, LC\}$. *distance* is distance from the closest hit town, *pop* is population in 1981 and $\mathbf{X}$ is a set of geographical controls, such as *altitude* and *surface*. γ_j are area fixed effects. Summary statistics for all the variables used throughout the analysis are

[9] Data on altitude at the municipality administrative seat, and minimum and maximum altitude within the town's territory are provided.

provided in Table 1.

Table 2 provides the estimation results. In column (1), a simple OLS regression of 1979 electoral turnout over distance is run using data from the whole sample. Results confirm the pattern in Figures 3a and 3b, reporting a 5% level significant coefficient of the distance variable: according to the baseline estimate, each kilometer further away from the hit town is associated with a significant 0.24 percentage points increase in the electoral turnout. Column (2) reports results from the same regression but excluding hit towns, whose distance measure is clearly equal to 0. The number of observations is thus reduced to 114, and the coefficient of interest slightly increase in magnitude (0.26) and remains highly significant. All regressions in columns (3) to (8) are run on the restricted sample, excluding hit towns[10]. In column (3), area fixed effects are included, to net out differences in the average town-level turnout across areas. The coefficient of interest is still significant at the 5% level, and it rises to 0.31. Population size may have an impact on electoral turnout, due to spillover effects in political awareness and participation or higher representativeness of national parties. When population in 1981 is included in the regression, results in column (4) show how the coefficient of the distance measure becomes even higher in magnitude and still significant at the 5% level, with each kilometer further away from the hit town being associated with a significant 0.34 percentage points increase in the electoral turnout. Notice that the population coefficient is, significant at the 5% level, consistently positive and reasonably low in magnitude.

Results could possibly be driven by the fact that hit towns are located in particular, less accessible geographical areas. Isolation might have fostered a culture of resistance towards political authority, leading to the massacres. In order to rule out this possibility, I include as controls both altitude and surface of territory belonging to each towns. Results are reported in column (5) of Table 2. We can see how the coefficient of interest slightly increases (0.36), and remains significant at the 5% level. Both altitude and surface have a reasonably negative coefficient, with only the altitude one being significant at the 5% level. As an additional check, in column (6) I use minimum and maximum altitude within the territory of the town as controls. Again, the distance coefficient remains significant at the 5% level, even if it decreases in magnitude (0.29).

Using the same specification as in column (5), I then check for the possibility of heteroskedasticity of residuals. The Breusch-Pagan/Cook-Weisberg test for heteroskedasticity returns a p-value of 0.69. I cannot thus reject the hypothesis of homoskedastic residuals. Nonetheless, I run the same regression as in (5), but with robust standard errors, adopting the White covariance estimator for the variance covariance matrix of coefficients. Results in column (7) are substantially unaltered. Finally, consistently with Guiso, Sapienza and

[10]Results do not substantially change if hit towns are included. I decided to exclude them in this version for consistency with the rationale for the identification strategy, which relies on the comparison of towns closer to the hit ones with those further away.

Zingales (2008b), I run again the same regression as in (5), but weighting observations with population in 1981. This because one might be worried that the unobservable determinants of electoral turnout and social capital in general might be noisier for towns with smaller population. Weighted least squares coefficients, together with standard errors, are reported in column (8). The distance coefficient falls in magnitude (0.26), but it is now significant at the 1% level.

Overall, results from Table 2 show how each kilometer further away from the hit town is associated with a significant 0.24-0.36 percentage points increase in the electoral turnout. Next, I show estimation of the distance coefficient is shown to be robust to a number of additional checks.

Adopting the same specification as in column (5), I first include province fixed effects. Since the towns in the sample belong to 7 different provinces, one might be worried that towns further away from the hit ones are more likely to belong to different provinces, confounding the results. If so, province fixed effects should capture the variability in the electoral turnout previously captured by the distance measure, making the relative coefficient no longer significant. Results when province fixed effects are included are reported in column (1) of Table 3. Not only the coefficient of the distance measure keeps being significant at the 5% level, but it is also substantially unchanged in magnitude with respect to the estimation results in column (5) of Table 2. Furthermore, the schooling and age distributions in each town are likely to be associated with electoral turnout in a systematical way. Even if results after the inclusion of geographical controls makes it less likely to find any reason for schooling and age distribution to vary systematically with distance, I control for the number of individuals with University Degree and High School Diploma, and for the number of individuals in different age groups, all in 1981. Results are reported in column (2) of Table 3: the coefficient of the distance measure keeps to be significant at the 5% level and equal to 0.35, consistently with previous findings.

My identification strategy relies on the assumption that towns closer to the hit ones were comparable with towns further away in terms of unobservables in 1860-61, and have not been exposed to other shocks in a systematically different way thereafter. Even if all towns in the sample shared the same formal institutional arrangement after unification, one might be worried about pre-existing differences. One possibility to partially test the validity of this assumption is to include population in 1861 as a control variable in the main regression: if the inclusion of the 1861 population variable yielded dramatical changes in the coefficient of interest, concerns about unobservable differences in 1860-61 would increase, threatening the overall validity of the empirical analysis. Column (3) in Table 3 provides the result for the augmented regression. The coefficient of interest drops to 0.27 but it keeps to be significant at the 10% level. Given that 1861 population data are not available for 6 towns in the sample, the same regression is run using population in

1871 as additional control instead. Column (4) shows results to be substantially unchanged with respect to the main specification in column (5) of Table 2.

6.1 Panel Data Analysis

In this part of the analysis, I consider three different time periods. I use European Parliament elections data from in years 1979, 1989 and 1999, and population data from years 1981, 1991 and 2001. I thus construct a panel dataset, with 119 units observed in 3 time periods, and one observed in 2.

I base the empirical analysis on the following baseline regression:

$$euro_{ijt} = \alpha + \beta\ distance_{ij} + \lambda\ pop_{ijt} + \mathbf{X}'_{ij}\varphi + \delta_t + \gamma_j + u_{ijt}, \tag{2}$$

where $euro_{ijt}$ is now European Parliament elections turnout in year t for town i in area j. *pop* is population as recorded two years later. Consistently with the preferred specification from the previous part, $\mathbf{X}$ is geographical controls which include *altitude* and *surface*. Area fixed effects are still included, together with δ_t, time period fixed effects.

Retaining the assumption of the distance measure being orthogonal to unobservables, I use a Random Effects estimator, which assigns an equicorrelated structure to the variance covariance matrix of residuals belonging to the same unit but to different time periods. In order to clean further within town correlation of residuals, I also cluster standard errors per town. Results from the baseline specification including the whole sample are reported in column (1) of Table 4, while column (2) reports results when hit towns are excluded. Results are very similar to the ones obtained in the first part of the analysis: each kilometer further away from the hit town is associated with a significant 0.32-0.38 percentage points increase respectively in the electoral turnout. Notice that the coefficient of the distance measure is now significant at the 1% level.

Given the increased number of observations, it is now appropriate to run the same regression separately for each area, in order to shed some light on the heterogeneity of the effect across the three locations[11]. Results from the area of Gaeta, LC and PC are reported in columns (3), (4) and (5) respectively. Results for the areas of Gaeta and LC are highly significant (1% level), and the coefficient of interest is higher in magnitude than what obtained in column (2) (0.51 and 0.48 respectively). When the area of PC is considered, the coefficient of the distance measure turns out to be almost zero, and no longer significant. I believe this result to belong to one specific feature of the PC area. A national level politician, born in the area, member of the Italian Parliament from 1976 to 2008

[11]Note that the adoption of clustered standard errors here is problematic, as the number of clusters per area is not sufficient to guarantee consistency of the cluster robust estimator of standard errors. Nonetheless, I report results using clustered standard errors. Results from the same random effect estimates without clustered standard errors are substantially unchanged.

and former Minister of Labour, was running for european elections in 1999. The personal character assumed by the act of voting in the area presumably biases the electoral turnout as measure of social capital, confounding the effect of distance and mining the results in column (5). In order to investigate the heterogeneity issue further, I include in the regression the interaction of the distance variable with the area dummy. Results are reported in column (6). The interactions coefficients are insignificant, so that I reject the hypothesis of heterogeneity of the effect of interest across areas.

Finally, I test whether the effect of interest has changed over time. I thus include in column (7) the interaction of the distance variable with the time period dummies, together with town fixed effects. Results show the coefficients of the interaction variables to be not significant. I can thus reject the hypothesis of heterogeneity of the effect across the three considered time periods.

Results from the panel data analysis are in general consistent with what found performing the 1979 cross section analysis in the previous part. I thus claim the reported effect to be highly persistent and homogeneous across the three locations.

6.2 World War I Casualties

Finally, I check whether the above resulting pattern is confirmed when we go back earlier in time, using World War I casualties as a measure of social capital endowments per town at the beginning of the XXth century. Consistently with the previous findings, Figures 7a and 7b present a graphical representation of the positive relationship between the total number of casualties and casualties among infantrymen with distance from the closest hit town.

The empirical analysis is based on the following regression specification, homologue to (1):

$$casualties_{ij} = \alpha + \beta\ distance_{ij} + \lambda\ pop_{ij} + \mathbf{X}'_{ij}\varphi + \gamma_j + u_{ij}, \tag{3}$$

where $casualties_{ij}$ is the number of World War I casualties of individuals born in town i in area j, and pop is now population in 1911.

Estimation results for this part are reported in Table 5. I first consider the total number of casualties, independently from the military grade. In column (1) a simple OLS is run, with the distance variable as the only explanatory variable. The correspondent coefficient turns out to be significant at the 5% level and equal to 1.05. Each kilometer further away from the hit town is thus associated with a 1 unit increase in the number of total World War I casualties. Column (2) reports the results from the adoption of the full regression specification, with the hit towns included in the sample. The coefficient of interest is now

significant at the 1% level, even if slightly lower in magnitude (0.89). Results when hit towns are excluded are reported in column (3). The coefficient of interest becomes here insignificant. However, Figure 7a motivates the adoption of weighted least squares, as dispersion in the number of casualties seems to increase with distance from the closest hit town. Column (4) reports results from the same specification as in (3), but adopting WLS, weighting observations with the inverse of the distance measure, and excluding hit towns. The coefficient of interest is now significant at the 1% level and equal to 0.83.

The assumptions needed to justify the adoption of World War I casualties as a measure for social capital endowments make it appropriate to restrict the attention to casualties given the military grade. This because, given ability, the probability of death in World War I is likely to be more homogeneous across individuals with the same military grade. For this reason, in columns (5) to (8), I repeat the previous analysis but using as output the number of World War I casualties among infantrymen only. The previous findings are substantially confirmed, with the additional positive result of the distance coefficient being significant now at the 10% level when the full specification is adopted using OLS and excluding hit towns, even if it halves in magnitude (0.46).

Overall, results show how, given the specified controls, each kilometer further from the hit town is associated with a 0.4-1 units increase in the number of World War I casualties and casualties among infantrymen, suggesting the positive relationship between distance and social capital endowments to be in place in the 1910s.

6.3 Placebo Treatments and Sample Selection

In order to shed further light on the validity of the overall analysis, I first provide results from a placebo treatment exercise. I use panel data on European Parliament elections turnout in 1979, 1989 and 1999 for the whole sample of Italian municipalities. To ensure consistency with the framework and reasoning above, I first extract the sample of municipalities whose population is recorded to be lower than 40,000 over the considered time period. I end up with a slightly unbalanced panel of 7,453 municipalities observed in three points in time. From this sample, I randomly select 500 towns, and assign them a *placebo treatment* status. I then select the 40 towns closest to the selected one, and run the random effects regression specification adopted in the panel data section above separately for each of the derived 500 samples (excluding the initially selected town). The density of the estimated distance coefficients from these regressions is shown in Figure 8, together with the smoothed kernel estimate of its density function. The distribution is centered at zero, providing evidence of a non-existing relationship between European Parliament elections turnout in neighboring towns and distance for the average italian municipality. In particular, the probability of finding a density coefficient estimate at least as positively large and significant as the one found in the above analysis in the panel data section is of

1.4% in the derived sample.

As a final check, I test the robustness of the derived distance coefficient estimate to different sample selection criteria. I consider the baseline specification at the beginning of this section, and estimate the correspondent distance coefficient using different samples as derived by varying the number of towns closest to the hit one (excluding the latter). In Figure 9, I plot the coefficient estimate and t-statistic for the null hypothesis of a zero coefficient as a smoothed function of the number of towns closest to the hit one per episode. Both the coefficient estimate and the derived t-statistic are shown to be remarkably stable over the support. In particular, the t-statistic is higher than the critical value corresponding to the 5% significance level.

7 Conclusions

In this paper, I show how differences in the awareness of specific historical events can lead to highly persistent differences in the life of communities.

I consider the case of the unification of Italy in 1860-61, and the war against brigandage in the following years. I focus on three specific episodes of conflict which caused the death of a big fraction of population in specific locations. I claim that the awareness of these events reasonably threatened the process of social capital accumulation. This makes possible to trace back today's differences in social capital to differences in proximity to the hit towns. Closeness is thought to be associated with a higher probability of direct testimony, and/or stronger connections between individuals and communities. Proximity to the hit locations is then coupled with deeper awareness of those violent events, bigger shocks to the environment and individuals' norms and beliefs, and thus more credible threats to the social capital accumulation process.

I look at all towns in the areas surrounding the locations where the historical facts happened. In the spirit of Putnam (1993), I first use European Parliament elections turnout as measure of social capital and generalized trust. Using data from elections in year 1979, 1989 and 1999, I show how, after controlling for population and other town level characteristics, each kilometer further from the hit town is associated with a significant 0.2-0.5 percentage points increase in the electoral turnout. Results are reasonable in magnitude, and shown to be robust to the inclusion of additional controls which capture differences in schooling and age distributions at around the time of the elections. Moreover, the effects is found not to be heterogeneous across areas and time periods. In order for the found effect to be interpretable in causal terms, towns closer to the hit ones have to be comparable with those further in terms of unobservables in 1860-61, and thereafter not to have been exposed to other shocks in a systematically different way. I claim this to be a reasonable

assumption. First, all towns in the sample shared the same formal institutional arrangement after unification. Second, results are unchanged after controlling for population in 1861 and 1871. In order to provide evidence of the robustness of the suggested relationship across time, I also focus on World War I casualties of individuals born in each town as a measure of social capital endowments at the beginning of the XXth century. Results are consistent with the main hypothesis, as, given population and other town level characteristics, each kilometer further from the hit town is shown to be associated with a 0.4-1 units increase in the number of World War I casualties and casualties among infantrymen. Finally, I show results to be robust to the adoption of different sample selection criteria, and find them consistent with the results from a random placebo treatment exercise.

It is worth noticing that, if computed in terms of the output standard deviation, the magnitude of the effect is quite similar when comparing results from the analysis of European Parliament elections turnout and World War I casualties. In both cases, a 10 km increase in the distance measure is associated with a 0.2-0.4 standard deviations increase in the considered social capital measure.

In the direction of further improvement, it would be interesting to try to replicate the reported results using other measures of social capital, such as referenda turnout or number of individuals active in voluntary associations. In order to provide additional evidence of the suggested channels for persistence of the effect, one could also find birth records data and study the evolution over time of the distribution of surnames in the towns in the sample. If the distribution is found to have remained approximately constant, the channel of intergenerational transmission of norms and beliefs would gain credibility. In light of the results by Alesina and Giuliano (2010a, 2010b), it would also be of some interest to focus on family ties as outcome, in order to further research the issue of substitutability of social capital and family ties.

The historical treatment of the years following the unification of Italy is still far from being completed. This prevents the analysis from being expandable to other locations and episodes within the context of those years, since information becomes more fragmented as the relevance of events decreases. Nonetheless, the empirical framework adopted here is presumably generalizable and applicable to a number of historical context, such as the 1936-39 Spanish civil war.

Despite the lack of unique definition and measurement, the concept of social capital has given birth to a solid and promising line of research. I believe that the focus in the analysis on a small scale can credibly shed some light on the process of social capital accumulation and destruction, opening the way to further, deeper investigation.

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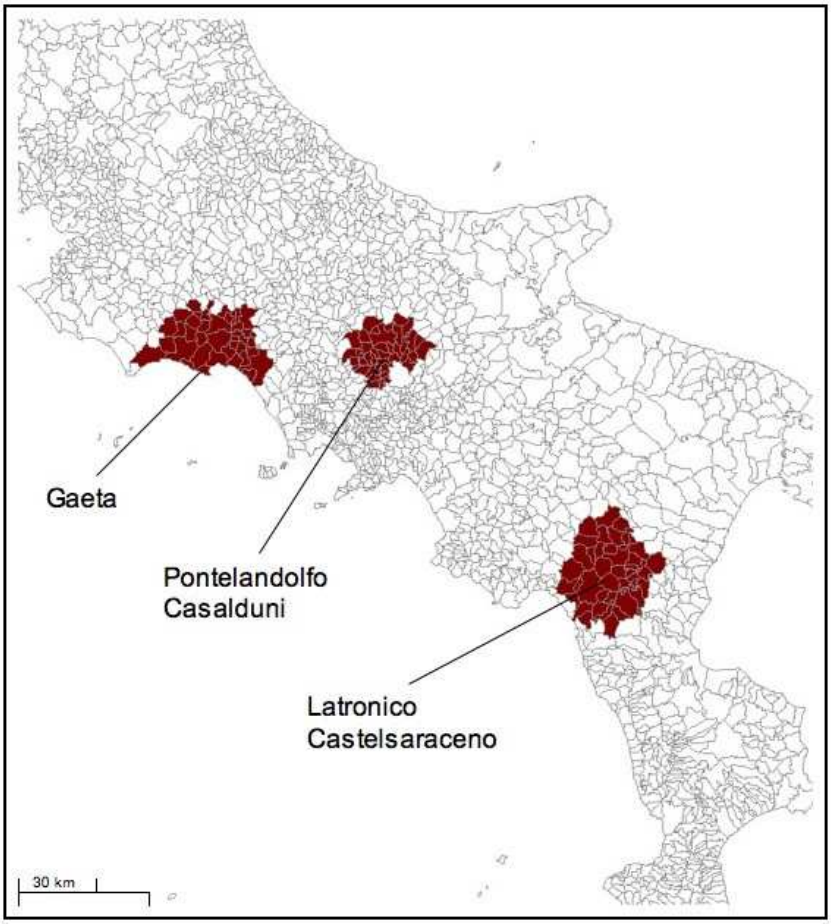
Tables and Figures

Figure 1. The Unification of Italy 1815-1870

From Shepherd, W. (1911), *Historical Atlas*, Henry Holt and Company.

Figure 2. Areas and Towns in the Sample



Elaborated using ISTAT (2009), *Communes Statistical Atlas (Atlante Statistico dei Comuni)*.

Figure 3a. 1979 Elections and Distance from Closest Hit Town

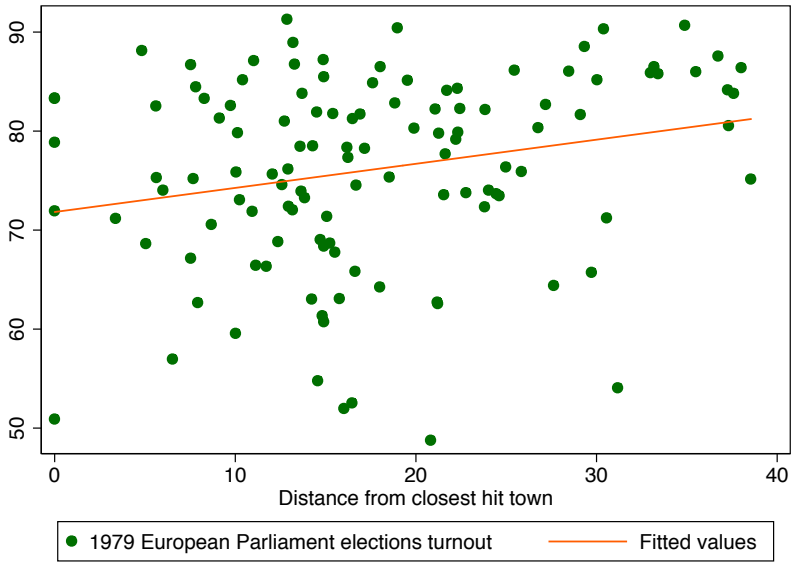


Figure 3b. 1979 Elections and Distance (by Area)

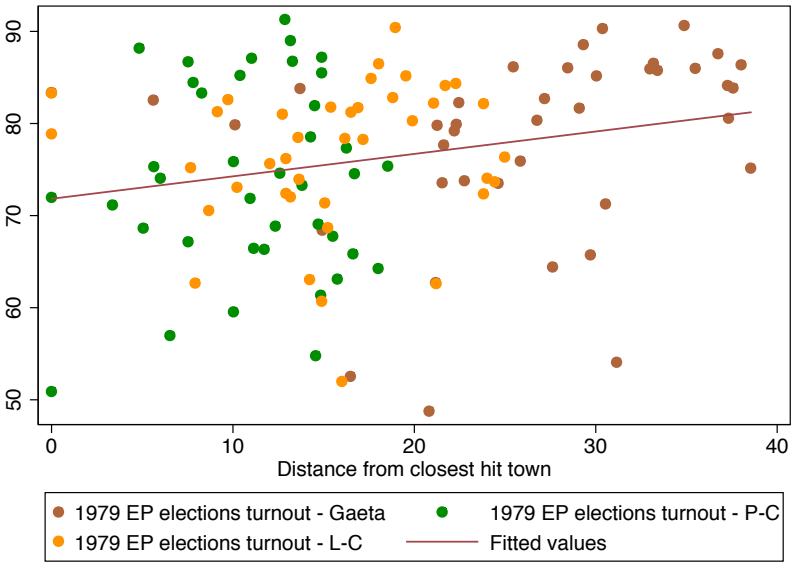


Figure 4. 1989 Elections and Distance from Closest Hit Town

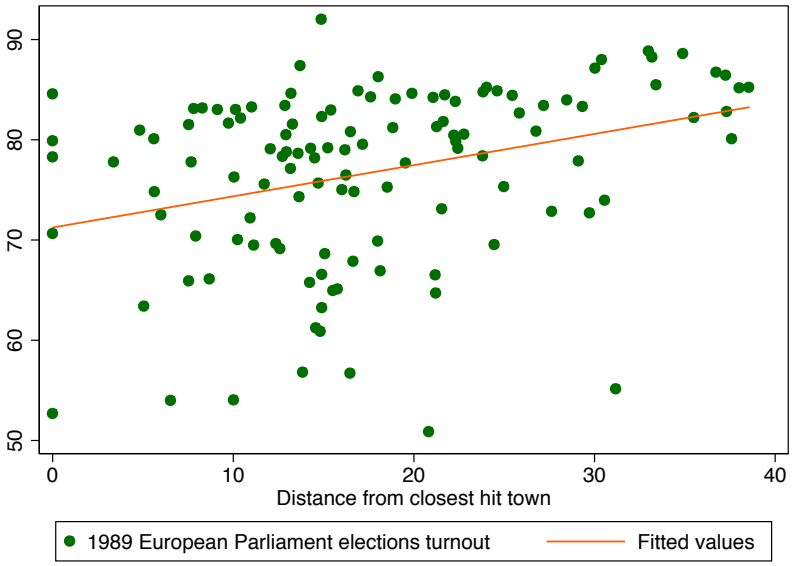


Figure 5. 1999 Elections and Distance from Closest Hit Town

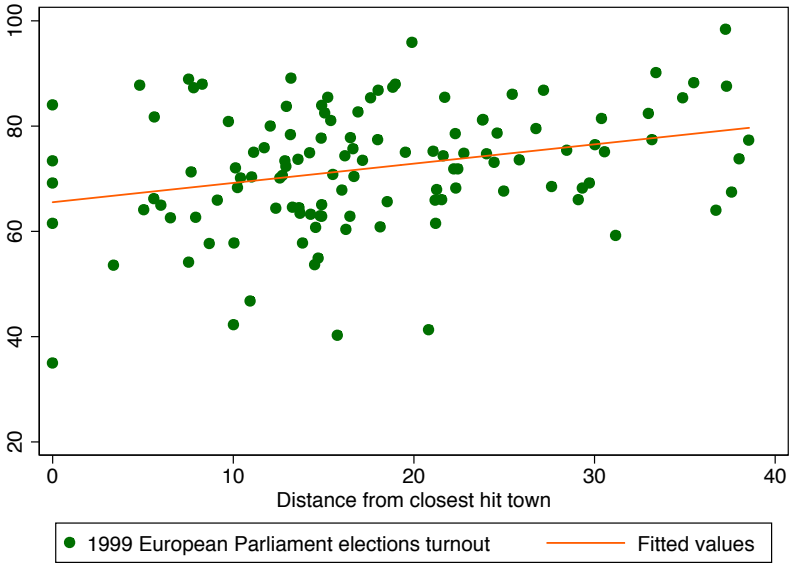
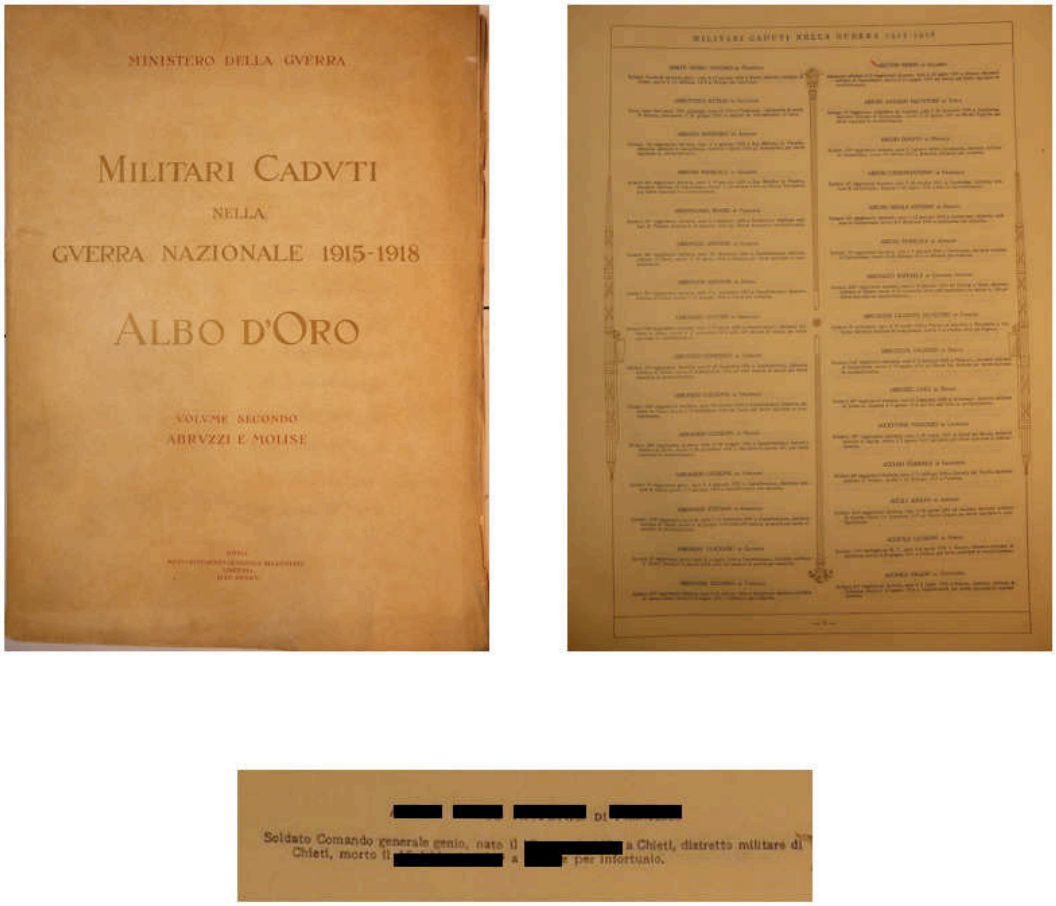


Figure 6. The *Albo d'Oro* volumes



The cover, first page and an entry in details, from Volume 2 of the *Albo d'Oro*.

Figure 7a. Total WWI Casualties and Distance from Closest Hit Town

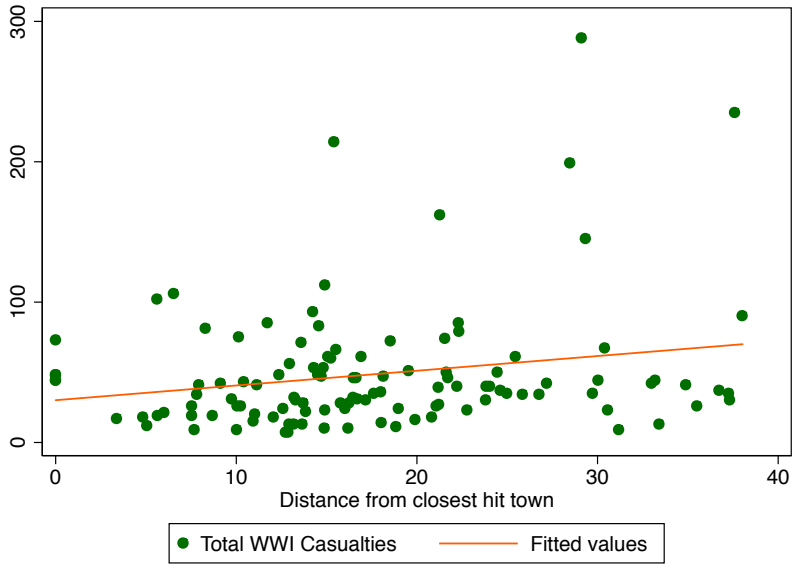


Figure 7b. WWI Casualties among Infantrymen and Distance from Closest Hit Town

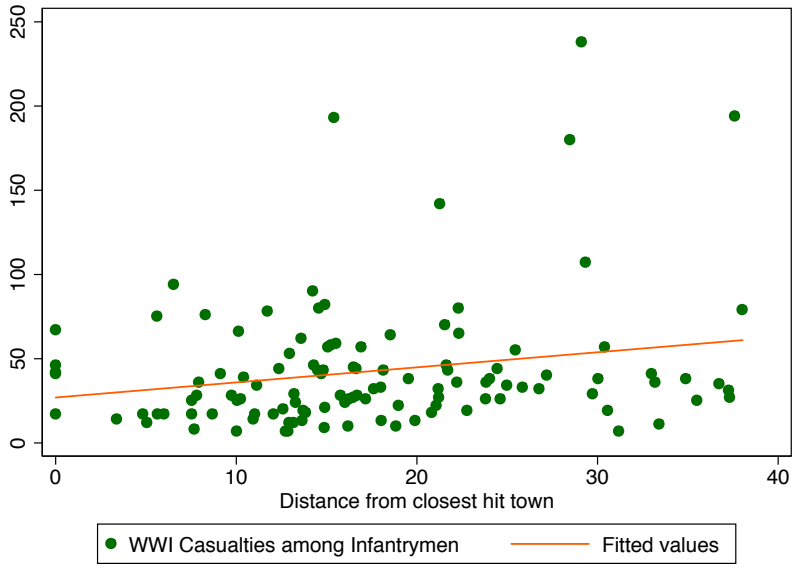


Figure 8. Distribution of Placebo Distance Coefficients

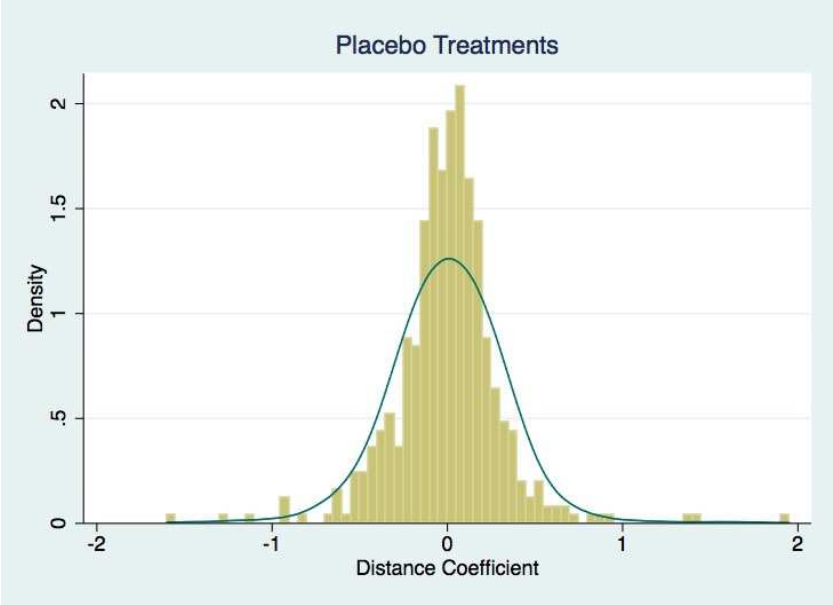


Figure 9. Sample Selection and Distance Coefficient

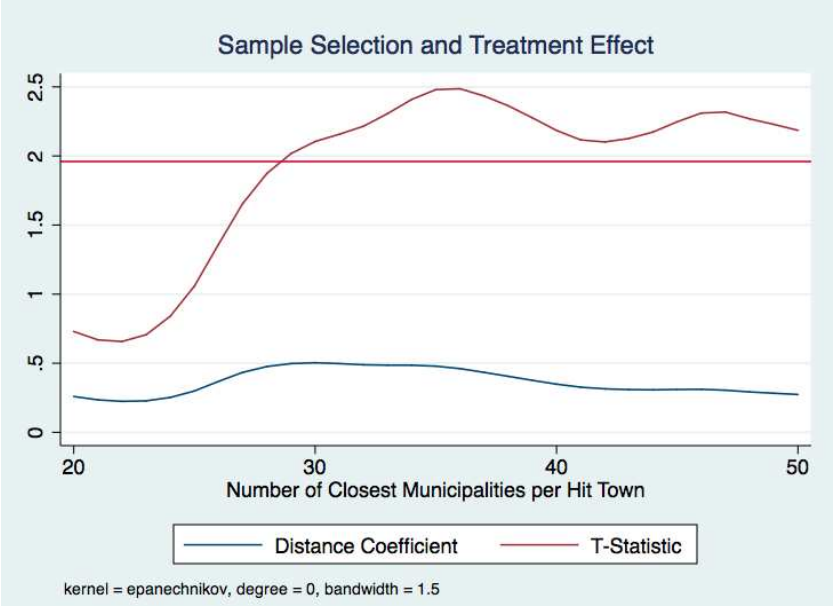


Table 1. Summary Statistics

Overall Sample

Variable	Obs.	Mean	Std. Dev.	Min	Max
Distance from hit town (km) (hit towns excluded)	115	18.49	8.63	3.37	38.56
Altitude (m) (Municipality building)	120	419.44	253.51	2	975
Altitude (m) (Minimum)	120	193.43	171.48	-1	659
Altitude (m) (Maximum)	120	1051.44	477.00	36	2247
Surface (ha)	120	4452.19	3312.86	895	17566
Electoral turnout 1979	119	76.14	9.69	48.75	91.28
Electoral turnout 1989	120	76.76	8.86	50.85	92.01
Electoral turnout 1999	120	72.03	11.58	34.89	98.34
Population 1861	114	3248.22	2688.22	471	18326
Population 1871	120	3542.96	3132.89	513	20327
Population 1911	120	3733.68	3177.29	822	19472
Population 1981	120	4429.72	6230.49	578	36840
Population 1991	120	4538.76	6563.96	587	37077
Population 2001	120	4425.73	6541.90	584	36633
Individuals with University Degree (1981)	120	77.15	164.45	1	1042
Individuals with High School Diploma (1981)	120	390.94	734.91	15	4263
Individuals aged 0-19 (1981)	120	1399.85	2135.59	141	12040
Individuals aged 20-39 (1981)	120	1169.98	1799.41	128	10470
Individuals aged 40-59 (1981)	120	1082.87	1462.21	158	9133
Individuals aged 60 or more (1981)	120	777.01	864.08	150	5197
Total Number of Volunteers in NPOs	120	161.03	354.56	0	2684
Total Number of NPOs	120	13.37	24.21	0	203
Total Number of Voluntary Associations	120	12.43	22.53	0	188
WWI Casualties among Infantrymen	116	42.58	38.33	7	238
WWI Total Casualties	116	48.41	44.96	7	288

Table 2. Estimation Results from Baseline Specification

	1979 European Parliament Elections turnout							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance	0.244**	0.263**	0.314**	0.341**	0.358**	0.288**	0.358**	0.255***
	(0.09)	(0.10)	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)	(0.09)
Population 1981				0.000**	0.000*	0.000*	0.000**	0.000
				(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Altitude					-0.014**		-0.014**	-0.014***
					(0.01)		(0.01)	(0.01)
Surface					-0.000		-0.000	-0.000
					(0.00)		(0.00)	(0.00)
Altitude (Minimum)						-0.022***		
						(0.01)		
Altitude (Maximum)						-0.003		
						(0.00)		
Constant	71.827***	71.378***	70.984***	69.677***	80.721***	81.968***	80.721***	81.148***
	(1.88)	(2.06)	(2.90)	(2.92)	(4.42)	(4.64)	(4.69)	(3.72)
Area Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	119	114	114	114	114	114	114	114
R^2	0.054	0.057	0.060	0.095	0.181	0.202	0.181	0.293

* p-value < 0.1; ** p-value<0.05; *** p-value<0.01.

Hit towns excluded in all specifications but (1); (1)-(6) Ordinary Least Squares;
(7) Eicker-Huber-White robust standard errors; (8) Weighted Least Squares, weight is Population in 1981.

Table 3. Robustness Checks

	1979 European Parliament Elections turnout			
	(1)	(2)	(3)	(4)
Distance	0.364**	0.394***	0.270*	0.360**
	(0.17)	(0.14)	(0.15)	(0.14)
Population 1981	0.000*	0.000	0.001*	0.000**
	(0.00)	(0.00)	(0.00)	(0.00)
Altitude	-0.015***	-0.005	-0.014**	-0.013**
	(0.01)	(0.01)	(0.01)	(0.01)
Surface	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Pop. with Uni. Degree		-0.047		
		(0.04)		
Pop. with HS Diploma		0.015		
		(0.01)		
Pop. aged 20-39		-0.019		
		(0.02)		
Pop. aged 40-59		-0.005		
		(0.01)		
Pop. aged 60 or more		-0.008		
		(0.01)		
Population 1861			-0.001	
			(0.00)	
Population 1871				-0.000
				(0.00)
Constant	73.718***	100.133***	82.449***	80.390***
	(5.88)	(28.56)	(4.62)	(4.44)
Province Fixed Effects	Yes	No	No	No
Area Fixed Effects	Yes	Yes	Yes	Yes
Observations	114	114	108	114
R^2	0.223	0.264	0.172	0.187

* p-value < 0.1; ** p-value<0.05; *** p-value<0.01.

(1)-(4) Ordinary Least Squares, hit towns excluded.

Table 4. Panel Data Analysis

	European Parliament Elections turnout						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance	0.320***	0.374***	0.505***	0.476***	-0.052	0.411**	
	(0.12)	(0.12)	(0.17)	(0.18)	(0.27)	(0.20)	
Population	0.000	0.000	-0.000	0.001**	0.002	0.000	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Altitude	-0.014***	-0.016***	-0.012	-0.001	-0.016**	-0.016***	
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.00)	
Surface	-0.000	-0.000	0.000	-0.001***	-0.002**	-0.000	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Distance * δ_{89}							0.073
							(0.09)
Distance * δ_{99}							0.099
							(0.09)
Distance * γ_{Gaeta}						0.085	
						(0.26)	
Distance * γ_{PC}						-0.557	
						(0.34)	
Constant	84.253***	83.908***	65.765***	72.492***	83.052***	82.759***	79.957***
	(3.60)	(3.69)	(6.07)	(5.92)	(5.10)	(4.49)	(3.86)
Town Fixed Effects	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	Yes
Area Fixed Effects	Yes	Yes	No	No	No	Yes	No
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	359	344	117	114	113	344	344
R^2	0.228	0.233	0.226	0.200	0.363	0.250	0.789

* p-value< 0.1; ** p-value<0.05; *** p-value<0.01.

Standard errors are clustered per town in all specification but (7); hit towns are excluded in all specifications but (1);

(1)-(2) Random Effect estimation; (3)-(5) Random Effect estimation performed separately for each area: Gaeta, L-C and P-C respectively;

(6) Random Effect estimation; (7) Ordinary Least Squares with town Fixed Effects.

Table 5. World War I Casualties

	All Casualties				Casualties among Infantrymen			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance	1.048**	0.889***	0.409	0.827***	0.895**	0.871***	0.460*	0.863***
	(0.45)	(0.29)	(0.27)	(0.25)	(0.38)	(0.28)	(0.26)	(0.25)
Population 1911		0.009***	0.013***	0.011***		0.007***	0.011***	0.009***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Altitude		-0.007	-0.005	-0.003		-0.004	-0.003	-0.001
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Surface		0.004***	0.001	0.002***		0.004***	0.001*	0.002***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Constant	30.136***	-19.597**	-6.698	-12.932*	26.965***	-18.167*	-6.757	-13.168*
	(8.87)	(9.86)	(8.18)	(7.47)	(7.56)	(9.33)	(7.89)	(7.29)
Area Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	116	116	111	111	116	116	111	111
R^2	0.045	0.801	0.879	0.853	0.045	0.755	0.844	0.810

* p-value< 0.1; ** p-value<0.05; *** p-value<0.01.

(1)-(3) and (5)-(7) Ordinary Least Squares; (4) and (8) Weighted Least Squares, weight is $1/Distance$.
Hit towns are excluded in all specifications but (1), (2), (5) and (6).