# ESSAYS ON ECONOMICS AND INSTITUTIONS

**Adele Grompone**

Thesis presented for the degree of
Doctor of Philosophy

Advisor: Prof. Antonio Acconcia
Department of Economics and Statistics
University of Naples Federico II
December 2019

# Contents


**1 Introduction** **6**

**2 How persistent is culture? Evidence from Southern Italy** **10**
2.1 Introduction . . . . . 10
2.2 Geography and history of the experiment . . . . . 13
2.3 Contemporary behavioral culture is worse southwards . . . . . 17
2.4 How old is this "worse"? . . . . . 21
2.5 At the upper border, cultural convergence: Was it all institutions? . . . . 25
2.5.1 Today, *Center* looks like *North* . . . . . 25
2.5.2 *North* used to be worse than *Center* (and *South*) . . . . . 26
2.5.3 Not much of a case for institutions and contagion . . . . . 28
2.6 Extending to controls . . . . . 31
2.6.1 Migration flows . . . . . 32
2.7 Robustness: Placebos treatments . . . . . 37
2.8 Determinants of cultural dynamics: some hints . . . . . 37
2.9 Conclusions . . . . . 39

**3 Long-term unemployment and subsidies for permanent employment** **42**
3.1 Introduction . . . . . 42
3.2 The subsidy granted by law 407/90 . . . . . 46
3.3 Data . . . . . 48
3.4 Trends in permanent hires . . . . . 49
3.5 Micro analysis . . . . . 50
3.5.1 Estimation strategy . . . . . 50
3.5.2 Defining the eligible and the non-eligible . . . . . 55
3.5.3 Main results . . . . . 57
3.5.4 Effect on wages . . . . . 64
3.6 Robustness checks . . . . . 64
3.6.1 Are results driven by substitution with other types of contract? . 66
3.6.2 Are results driven by substitution over time? . . . . . 68
3.6.3 Are results driven by substitution across areas? . . . . . 68
3.7 Cost-benefit analysis . . . . . 71

3.8 Discussion and conclusion . . . . . 73

**Appendix A** **74**
A.1 The data . . . . . 74
A.1.1 Provincial data . . . . . 74
A.1.2 Judicial district data . . . . . 74
A.1.3 Municipal data . . . . . 75
A.2 Specification robustness and covariates balancing across borders . . . . . 81
A.3 The economic implications of the reform . . . . . 83

**Appendix B** **86**

**Bibliography** **96**

# List of Figures


2.1 The region of study . . . . . 14
2.2 Turnouts at referenda in provinces of Central-Southern Italy . . . . . 18
2.3 Non-linear fit of contemporary referenda turnouts data around the New Border . . . . . 19
2.4 Non-linear fit of Pre-Reform municipal election turnout data around the New Border . . . . . 23
2.5 Non-linear fit of contemporary referenda turnout data around the Old Border . . . . . 26
2.6 Non-linear fit of Pre-Reform members of cooperatives and mutual aid societies data around the Old Border . . . . . 28
2.7 Baseline RD estimates of border dummies for referenda turnout . . . . . 40

3.1 Long-term unemployment rate . . . . . 43
3.2 Long-term unemployment rate across areas . . . . . 46
3.3 Permanent hires . . . . . 50
3.4 Permanent hires: South over Center-North . . . . . 51
3.5 Counterfactual analysis . . . . . 62
3.6 Cost-benefit analysis . . . . . 72

# List of Tables


2.1 Summary statistics over municipalities: Contemporary covariates . . . . 17
2.2 Summary statistics over municipalities: Contemporary outcomes . . . . . 19
2.3 RD on contemporary outcomes as function of location across the New Border . . . . . 22
2.4 RD on Pre-Reform outcomes as function of location across the New Border 24
2.5 RD on contemporary outcomes as function of location across the Old Border 27
2.6 RD on Pre-Reform outcomes as function of location across the Old Border 29
2.7 RD on past and contemporary literacy rates as function of geographic location across the New and the Old Borders . . . . . 30
2.8 Specification extensions . . . . . 33
2.9 Controlling for migration . . . . . 35
2.10 Interprovincial migration flows in 1955-2005 . . . . . 36
2.11 RD across placebo borders . . . . . 37

3.1 Cumulated three-year exemption from social security contributions for permanent hires . . . . . 48
3.2 Means and standard deviations computed over individuals in each category 56
3.3 Logit model — Odds ratios. Across areas comparison, year 2014 . . . . . 58
3.4 Logit model — Odds ratios. DDD, years 2014-15 . . . . . 60
3.5 Logit model — Odds ratios. Across areas comparison, year 2015 . . . . . 61
3.6 Logit model — Odds ratios. Falsification test: years 2013-14 . . . . . 63
3.7 Ordinary least squares regression . . . . . 65
3.8 Logit model — Odds ratios. Transitions to "long" fixed-term contracts . 67
3.9 Logit model — Odds ratios. Transitions to "short" fixed-term contracts . 69
3.10 Logit model — Odds ratios. Excluding the fourth quarter of 2014 and the first of 2015 . . . . . 70
3.11 Logit model — Odds ratios. Excluding individuals moving across areas . 71
A.1 List of municipalities in our sample by area . . . . . 78
A.2 Balancing properties for the baseline covariates across the New Border . 82
A.3 Balancing properties for the baseline covariates across the Old Border . . 82
A.4 Real per capita Provinces expenditures and government transfers . . . . 84
A.5 Works for marsh reclamation realized in concession in 1938 per province . 85

B.1 Percentage of permanent contracts receiving the subsidy in 2014, by eligibility status defined according to three different rules for computing non-employment duration . . . . . 87
B.2 Logit model — Odds ratios. Income rule . . . . . 89
B.3 Logit model — Odds ratios. Simplified rule . . . . . 90
B.4 Logit model — Odds ratios. Restricted non-employment duration intervals 91
B.5 Logit model — Odds ratios. Duration dependence: third order polynomial 92
B.6 Logit model — Odds ratios. Duration dependence: piecewise constant function . . . . . 93
B.7 Logit model — Odds ratios. Male sub-population . . . . . 94
B.8 Logit model — Odds ratios. Female sub-population . . . . . 95

# Chapter 1

# Introduction

The thesis is made of two chapters. The first one, co-authored with Luca Sessa, empirically investigates a case of social capital dynamics and endogeneity of culture by using an "exogenous" border shift, implemented within Central-Southern Italy by the Fascist regime, as a highlighting instrument to identify possible local variation. This is an interesting "natural" experiment, since it is quite established in the literature that poor current economic conditions in the South of Italy depend greatly on a low social capital and cultural endowment, which is in turn argued to be an heritage of the previous institutional history of the region. The experiment that we study allows us to highlight pure dynamics in culture as opposed to heritage with a direct application to that paradigmatically critical area.

As our experiment concerns a small region, the analysis can only rely on sub-provincial data, posing a heavy data-mining requirement. Aiming at assessing cultural dynamics over a wide spectrum, we looked for past and present data on the widest possible variety of local indicators of individual and institutional behaviors. We came up with a unique, rich data set for our area of study, though not a panel as the old variables which it was possible to assemble are mostly analogous, and not identical, to those suited for the current culture/social capital analysis.

The perturbation experiment performed by nature (by the hands of the legislator) that we analyze impinges on the shift of the border which, based on political and orographic factors, for more than eleven centuries separated the territories of southern Italy from the rest of the peninsula. Only recently with respect to that history, with Italian unification, that border has become just an administrative one. During the Fascist regime a reform abruptly shifted the southern border of the region of Rome down in an area entirely internal to what, that far, had been Terra di Lavoro, the province of Caserta included for centuries in the Kingdom of Naples/Two Sicilies. We perform spatial discontinuity exercises projecting pre- and post-reform outcome data on the changing "South" border. If today a cultural discontinuity is visible exactly on this new border, then this is evidence of local cultural dynamics, which in a reasonable span of time has overcome the weight of history and of cultural/institutional legacy. The same conclusions can be drawn if

today no discontinuity is found on the old, erased border, which used to be strong.

Cultural decoupling along the 20th century between separated southern zones will emerge, with in particular an adverse evolution for the part which did not change region, and assimilation for the part which did change region. We will also see that this assimilation cannot be explained by "cultural contagion" or by a usual institutional channel, given that previous to the reform the "adopting" northern part did not display more virtuous individual and institutional behaviors with respect to the "adopted" part.

We ask ourselves what could have triggered or be key to the (heterogeneous) cultural transformation that our experiment demonstrates. In particular, we wonder whether there was any role for other institutional or economic factors in shaping cultural evolution. A sequence of shocks with institutional and economic content did hit the areas with the reform and after. One shock was concomitant, consisting of local innovations in public expenditures associated to the creation of two provincial administrations and the abolition of one.

A second main shock was the local funding intervention by the Cassa per il Mezzogiorno, the public institution operative in the whole Southern Italy (slightly extended upward) between the 1950's and the 1980's. Results based on the data available to us showed that the Cassa expenditure, balanced across the two sides of the border, only had a limited impact on cultural divergence. Better data might however be more informative and improve this result.

A third, relevant shock was the institution of regional administrations in Italy, starting in 1970. With them, political decentralization brought to a local level the government of many functions that used to be a State prerogative. Regions' weight in public services functions has been ever-increasing since then, up to the new century. Before the reform, administrative efficiency was not worse in *South* than in the rest of the area under scrutiny, but the contemporary cultural discontinuity occurs exactly at the Latium-Campania regional jurisdiction limit. There follow hints to a sizeable, asymmetric impact of the newly-created institution of Regions for the detected cultural dynamics, but, given evidence on past data, the heterogeneity of such impact between Latium and Campania occurred for reasons unrelated to previous individual or institutional culture. Also, behaviors under scrutiny are not in the domain of those which the administrative activity of a Region may directly affect. One possible source of this differential impact of Regions might have been an indirect one, namely the adverse evolution of the levels of local services provided in Campania, and the effects on social cohesion that this might have entailed. This is definitely a direction worth exploring further.

The second chapter, co-authored with Emanuele Ciani and Elisabetta Olivieri, aims at studying the effectiveness of a subsidy introduced in Italy by law no. 407 of Dec. 29th, 1990 and in force until the end of 2014. The program targeted only firms that hired with a permanent contract a worker who had been unemployed for at least 24 months. Its amount was greater for firms in Southern Italy (100 percent of social security

contributions for 3 years; only 50 percent for firms in other Italian regions). The favorable treatment of Southern regions aimed at improving job opportunities of the long-term unemployed especially where this was more needed. Indeed, the incidence of long-term unemployment is strongly higher in the South than in Center-North. From 2008 to 2014 both areas experienced a sharp increase in long-term unemployment rate, but in the South it has been stably three times that in Center-North. This differential treatment in favor of Southern regions and long-term unemployed was abrogated in 2015 by the Financial Stability Law, which introduced exemption from social security contributions without distinctions between areas and duration of unemployment (although individuals with a permanent contract in the previous six months were excluded).

We use a sample of administrative micro-data about job flows (*Campione Integrato delle Comunicazioni Obbligatorie*, CICO) and select unemployed individuals that lost their job between 2009 and 2013, for whom we can observe the labour market history until the end of 2015. To achieve identification, instead of using simple diff-in-diff estimators (for instance comparing eligible vs. non eligible individuals across areas), we employ a triple difference estimator (DDD) that exploits variation in the relative cost of hiring with a permanent contract across time, regions and worker's unemployment length. Intuitively, the DDD approach exploits these three dimensions to remove (i) underlying differences between eligible and non-eligible unemployed; (ii) area-specific time trends and (iii) differential time-trends for the eligible. We implicitly assume that, without any targeted subsidy, the choice to hire with a permanent contract an eligible rather than non-eligible unemployed would have changed in 2015 in the same way in Northern and Southern regions.

The abolition of law 407/90 increased the relative cost of hiring with a permanent contract an individual unemployed for at least 24 months both in Southern and in Northern regions. On the contrary, the new 2015 subsidy introduced a strong cut in social security contributions for individuals who were not long-term unemployed and did not have a permanent contract in the previous six months. At the same time, the Jobs Act, a broader labour market reform package, was passed. Hiring trends between 2014 and 2015 were strongly affected by these interventions. However, the novelties introduced in 2015 applied to both our treatment and control groups homogeneously across the country. Therefore, if law 407/90 had never been present, the two groups would have been affected similarly by these job market interventions across different geographic areas, and this justifies the assumption of common *relative* trends that is behind our estimator.

For the triple difference estimator not to pick up spurious correlations, observable characteristics across these groups should be similar in the pre-treatment and post-treatment periods (and between areas). To check whether compositional changes affect our results, we control for a set of demographic characteristics, sector of previous job, incidence of irregular work and monthly dummies, together with their simple and double

interaction with temporal and geographic dummies, in order to be sure that the two groups can be considered identical in all observables.

A challenge that we face is that our DDD strategy is still only able to capture the differential effect on the eligible versus non eligible unemployed. The positive effect on the former might come at the expense of the latter, for instance if the unemployed will be pushed to wait until reaching the eligibility status in order to benefit from the policy. The estimate is also affected by possible issues of substitution over time (where firms anticipate the end of subsidies), across different types of contracts (where individuals have an advantage in avoiding short-term contracts that would end their eligibility status) and across areas (where individuals move to exploit the preferential treatment). Through a series of robustness checks we provide evidence that our results are not driven by these issues.

We find that, after the abrogation of law 407/90, eligible individuals in Southern Italy experienced a fall in their probability of finding a permanent job relative to non-eligible unemployed in Center-North. This implies that the targeted subsidy, in place until the end of 2014, was effective in rising their chances in the labor market. We estimate a 41 per cent higher probability of finding a permanent job in the subsequent week and argue that the effect is not driven by substitution over time or across areas, type of contract or category of jobseekers. Moreover, we find that the benefits deriving from jobs created thanks to the policy, measured by tax revenue and social security contribution paid by the employees, outweigh the costs of the policy, given by the amount of the subsidy.

# Chapter 2

# How persistent is culture? Evidence from Southern Italy

## 2.1 Introduction

From the seminal work by Putnam (1993) up to the empirical analyses by Guiso et al. (2016), Voigtländer and Voth (2012), Alesina et al. (2013) or Becker et al. (2015), various studies in economics have provided support for the great inertia or persistence of social capital and cultural traits. From there, it has been possible for a number of contributions to assess the economic impact of an inherited social capital/culture endowments.[1]

While a strong inertia in culture cannot be denied, cultural transformations do happen too, sometimes slowly, sometimes quite rapidly. As economists know well, people respond to incentives that act at an individual level. Each individual changes his conditional plans over *any* choice variable when he realizes that the structure of the model he is in, including the opportunity set, has changed around him. Seeing culture as the aggregation of individual behavioral best reply functions, one also sees that it may well change when the opportunity set changes for a non-zero part of the population. Even if potentially persuasive, arguments of this kind need sound theoretical and empirical foundations, as understanding ways of cultural dynamics is relevant for policy, and in particular for helping define actions to foster social capital improvements in depressed regions. By developing a central point in anthropology, namely that cultural change is more likely to emerge in unstable environments, Giuliano and Nunn (2017) give a contribution towards this understanding.

Identifying changing cultural traits may be just as difficult as it is for persistent ones. This paper empirically addresses the cultural persistence question by using an

1. In papers like De Blasio and Nuzzo (2009) or Tabellini (2010) persistence has been used to validate past institutions as an instrument to determine the impact of culture (including local informal institutions) on output.

"exogenous" border shift, implemented within Central-Southern Italy by the Fascist regime, as an highlighting instrument to identify possible local variation. A case of social capital dynamics and endogeneity of culture is of interest in itself, but additional interest stems from the place hosting the "natural" experiment, covering the (upper part of the) South of Italy. In fact, it is quite established in the literature that poor current economic conditions in that region depend greatly on a low social capital and cultural endowment, which is in turn argued to be an heritage of the previous institutional history of the region. But was it really inherited, or instead developed? The experiment that we study allows us to highlight pure dynamics in culture as opposed to heritage with a direct application to that paradigmatically critical area.

The perturbation experiment performed by nature (by the hands of the legislator) that we analyze impinges on the shift of the border which, based on political and orographic factors, for more than eleven centuries separated the territories of southern Italy from the rest of the peninsula. Only recently with respect to that history, with Italian unification, that border has become just an administrative one, separating the regions of Latium and Campania, later formalized as a sub-governmental institution in the 1970s. We find it instructive to analyze cultural dynamics in the neighborhood of the delicate "Banfield-Putnam border", by which we mean the border discriminating societies regarded as characterized by forward vs. backward, i.e., civic vs. regressive moral foundations. Of course, we are well aware that it is difficult to identify in a line the divide separating the "North" of Italy from the "South". The Papal State itself might have served as a large *limes* (a thick border) for centuries. Moreover, the experiment setup itself is not perfect: a long time elapsed, and many events occurred, since the Fascist reform. However, on the one hand we have that a reform abruptly shifted the southern border of the region of Rome down in an area entirely internal to what, that far, had been Terra di Lavoro, the province of Caserta included for centuries in the Kingdom of Naples/Two Sicilies. On the other hand, culture may take time to change, when it does, so that a time lag seems unavoidable to assess results of a natural experiment of this kind.

As our natural experiment concerns a small region, the analysis can only rely on sub-provincial data, posing a heavy data-mining requirement. Aiming at assessing cultural dynamics over a wide spectrum, we looked for past and present data on the widest possible variety of local indicators of individual and institutional behaviors. We came up with a unique, rich data set for our area of study, though not a panel as the old variables which it was possible to assemble are mostly analogous, and not identical, to those suited for the current culture/social capital analysis.

A local focus grants specific advantages, all related to continuity, when studying cultural dynamics of groups or areas. First, if culture is different in roots between macro-areas, a meaningful and useful comparison can only be drawn locally, i.e. for homogenous values scales and behavioral models, so that continuity ensures comparability. Secondly, a local approach overcomes the inconveniences - not commonly considered in the litera-

ture - which arise when comparing culturally heterogeneous populations over dimensions whose definition and interpretation are culturally biased towards one or the other standpoint. Finally, another feature which stems from localness and adds power and interest to our natural experiment is that it is not about cultural change of displaced people, like emigrants of first or subsequent generations for which the effect of the original culture has to be disentangled. Instead, it is about the behavioral impact of exogenous shifts in factors around people who are left where they or their families used to live.

Given data difficulties in applying diff-in-diff, we perform spatial discontinuity exercises projecting pre- and post-reform outcome data on the changing "South" border. For centuries, the southern region was subject to a single institutional history, which is thought to have prompted a specific, lower cultural stock. Unexpectedly and exogenously, in the first half of the 20th century a regime drew a new, merely administrative border inside this region. If today a cultural discontinuity is visible exactly on this new border, then this is evidence of local cultural dynamics, which in a reasonable span of time has overcome the weight of history and of cultural/institutional legacy. The same conclusions can be drawn if today no discontinuity is found on the old, erased border, which used to be strong. Cultures evolve and Southern Italy might be living an outcome, of possibly recent development, rather than an immutable datum from a remote past.

The light, administrative reform serves in the experiment as a perturbation instrument rather than as a treatment, as *by itself* unable to be a determinant of subsequent changes. Cultural decoupling along the 20th century between separated southern zones will emerge, with in particular an adverse evolution for the part which did not change region, and assimilation for the part which did change region. We will also see that this assimilation cannot be explained by "cultural contagion" or by a usual institutional channel, given that previous to the reform the "adopting" northern part did not display more virtuous individual and institutional behaviors with respect to the "adopted" part.

What did drive the dividing outcome? Modern data display worse cultural and institutional behaviors exactly in the Campania Region area, absent both in the pre-reform and in the intermediate periods. From this, one can conjecture a role for a relatively bad Campania subgovernmental local governance in determining the contemporary discontinuity. But such a role cannot stem from any previous wired-in cultural or institutional legacy, as we will show that local inefficiency in Campania is itself an outcome rather than a prior institutional characteristic. Other factors might have come into play, perhaps exactly with the creation of the Region institution, and possibly related to the political economy interplay between relative economic development and the multiplication of the levels of government.

More work is left to be done to answer the welfare-relevant question of what caused the cultural dynamics that we will report. Nevertheless, we believe that our research direction is potentially more instructive for policy action than the idle reference to cultural persistence and legacy of history, which, unsuited to shed light on determinants that it

places in the far past, leaves hopeless of recipes for improving a dimension so relevant for economic performance.

The paper is organized as follows: next section describes setup, extent and implications of the (exogenous) natural experiment. A first RD exercise in Section 2.3 formally confirms a contemporary cultural lag on the lower side of the Latium-Campania border. Replicating the exercise on pre-reform data in Section 2.4 yields instead cultural continuity across that border, before this existed. On the erased Latium-Campania border, now entirely in Latium, Section 2.5 finds contemporary assimilation, opposed to previous northern relative disadvantage, and evaluates the possible impact of known channels of cultural dynamics, like cultural contagion or institutions. Section 2.6 extends the first, main RD exercise to appropriate controls, with a focus on migration flows. A section on robustness "placebo" checks follows. Before concluding, Section 2.8 sketches some hints to possible drivers of the main results, which the scant "mid-term" data can help discriminate.

## 2.2 Geography and history of the experiment

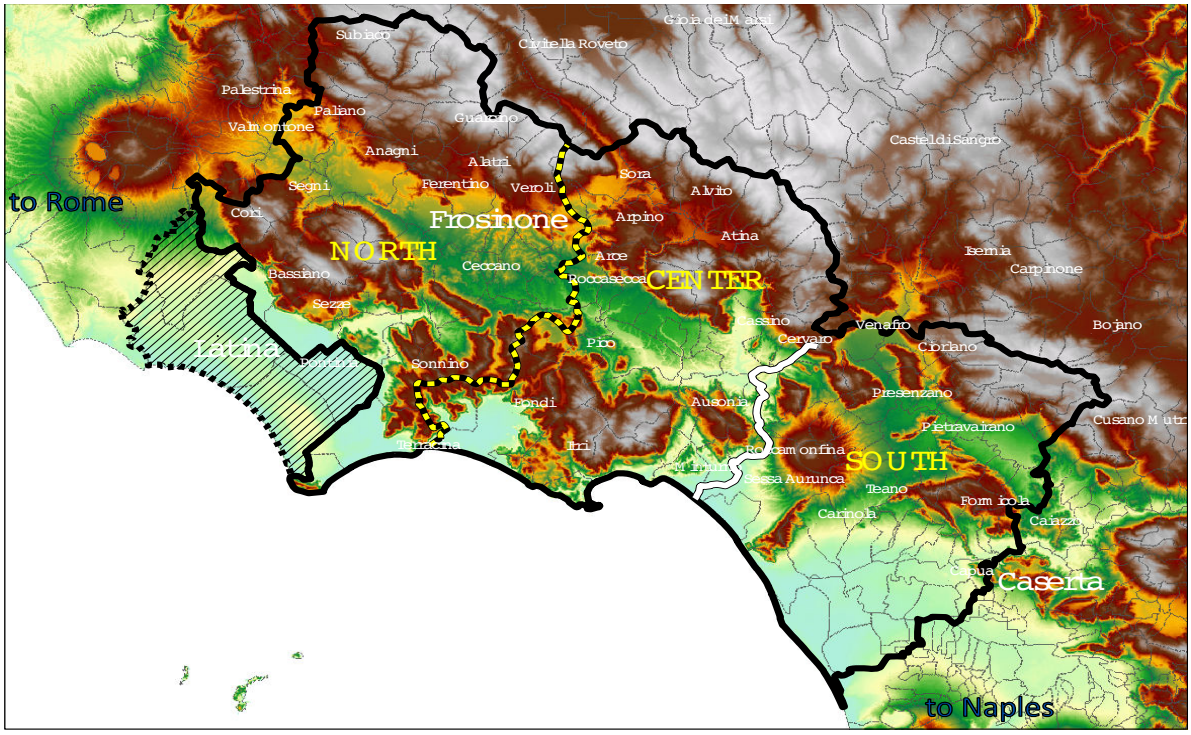
We focus on a region stretching for 120 km between Rome and Naples,[2] on the Tyrrhenian sea, reaching about 70 km inland (Figure 2.1). This territory has been hosting the State border with the longest effectiveness in the Italian peninsula: the south-western part of the border separating the Papal State and the Kingdom of the Two Sicilies, a political divide (yellow-dashed in the figure) which lasted virtually unmodified from the consolidation of the Lombard southern conquests in the 8th century, up to the 19th century.[3] Its millennial effectiveness also relied on geographical factors: for a large part, it ran over mountain ridges, coinciding with an orographic divide.[4] In 1870, troops of the recently founded Kingdom of Italy tore down for good this border as a national border. Nevertheless, it remained effective as an administrative delimitation of two wide Provinces (the main sub-national units in Italy for the following century), that of Rome from that of Terra di Lavoro (the wider in Campania, with Caserta as capital). As such, it also delimited the Region of Latium (i.e. the latest configuration of the Papal State territories, since then hosting the capital city in Italy) from that of Campania (the north-western section of the Two Sicilies, featuring Naples as capital city). At first, Regions were a merely statistical partition quickly set up by the young Italian government; starting in 1970, they became the main sub-national level of government.

2. The length of the straight segment joining Rome and Naples is 189 km, which can be covered by a minimum route of 227 km according to *www.viamichelin.com*. The municipality in the sample which is the closest to Rome by route is Paliano (63 km), while Casapesenna is the closest to Naples (30 km).

3. Actually, the first formal State on the southern side of this border emerged only in the 12th century, as Kingdom of Apulia. It later became Kingdom of Naples and, after 1815, of the Two Sicilies.

4. Unsurprisingly, given geography and history, linguists draw on this border an isogloss too, i.e. a local separation of languages/dialects, with their cultural contents (see Bruni 1984).

FIGURE 2.1: The region of study




*Note:* The region of study is delimited by a black thick continuous line. The new border between Latium and Campania is marked with a white continuous line, the old border with a yellow-black dashed line. A black thick dashed line delimits municipalities excluded from the sample. The thin reticle of municipal territories is shown in the background, together with elevation. Territories above the black thick line in the upper-right corner belong neither to Latium nor to Campania.

In December 1926, quite suddenly Mussolini abolished Terra di Lavoro and instituted 17 new Provinces. On Jan. 2, 1927, Frosinone became capital of a new, eponymous Province spanning over internal territories thus far belonging to the Provinces of Rome and of Terra di Lavoro. The border between Latium and Campania was shifted about 40 km southwards, tracking for its western two thirds the Garigliano river, in a quite flat area entirely internal to what was Terra di lavoro, a position of no geographical nor historical nor cultural discontinuity.[5] The villages of Terra di Lavoro south of this "new" border were assigned to Naples; the northern villages were assigned to Frosinone for the inland part, and to Rome for the coastal part. On Dec. 18, 1934, this latter strip of land, plus some other villages originally with Rome, became seat of another Province (Littoria, now Latina), following a massive campaign of marsh reclamation started in the second part of the 1920's, carried out by workers coming from many areas in Italy but mainly from the north-eastern regions of Veneto and Emilia. The reinstitution of the Province of Caserta after 19 years, at the end of 1945, did not shift the border back.

The official motivation that the Fascist regime set forth for the whole administrative reform was its intention to contrast mass urbanization, promoting instead ruralization. Nevertheless, the abolished Province of Terra di Lavoro was far from being a metropolitan area. This is taken by historians as proving that the real motivations behind this part of the reform were others: to resolve a local opposition internal to the regime between original fascists and nationalistic veterans;[6] to make a rhetorical claim of an expansion of the area of influence of Naples, while implementing a substantial enlargement of that of Rome instead. This is useful to our analysis inasmuch as it shows that the exact border shift did not seem to depend on unobservables which *jointly* explain both the position of the new border and the previous or subsequent pattern in local cultural indicators. This is true not only looking at the inspiring motivation, but also looking at the particular location where the new border was placed: no reason other than the presence of a partial line given by one river (historically, more a way of communication than a geographic impediment) seems able to give account of why the reform chose exactly that partition to tear apart administrative and jurisdictional districts with a long common tradition. Even geographers of the regime admitted that the extended Latium was lacking homogeneity.[7] We can therefore consider the border shift as *exogenous*.

5. "(Questo confine) è assai irregolare, non solo perché non ha alcuna rispondenza nella tradizione storica (la quale, come per le nazioni, così, in misura più ristretta, per le regioni, crea negli abitanti un particolare sentimento unitario), ma anche perché non è regolato da alcun criterio di carattere fisico" (Epifanio 1931). Significantly, the 1940 edition of the *Campania* guide book by the Italian Touring Club was still mentioning "the territory transferred in 1927 from the territory of Caserta to those of Littoria and of Frosinone" saying that it "holds more Campania features than Latium ones" (Bertarelli 1940).

6. In 1925, in light of the contrasts, the government decided to take direct control over the Terra di Lavoro branch of the Associazione Nazionale Combattenti, elsewhere typically aligned with the regime after the 1922 March on Rome.

7. "Esula dalla attuale circoscrizione amministrativa laziale il concetto geografico unitario. D'altra parte sopra siffatta considerazione, puramente speculativa, dovevano prevalere i criteri politico-

The reform allows identifying an area enclosed between the old and the new borders, of approximately 40x60 km, which includes the territories formerly in Campania and from 1927 aggregated to Latium: we call it *Center*. Then we flip this area over each of the two borders and draw two additional symmetric areas: below the new border, a *South* area, made of territories kept in Campania; above the old border, a *North* area in territories that remained in Latium. The three areas, similar in size, turn out to be similar also in a wide number of other features. Table 2.1 shows this for structural local covariates. These data are extracted from the innovative set that, by resorting to past and contemporary sources, we have assembled on a variety of sub-provincial characteristics and behavioral indicators, at individual and institutional levels. A detailed description is provided in Appendix A.

The contiguous areas of *Center* and *South* were exposed, for over one millenium until 1860-61, to the same pre-Italian political institutions (those of the Two Sicilies and previous kingdoms), and were still politically and administratively unified thereafter, until the fascist administrative border shift.[8] Cultural legacy can be taken as common between the two areas.

What did the border shift imply from an institutional point of view? With the reform, *Center* and *South* were decoupled as for the entity in charge of administrative tasks, i.e. the Province.[9] In unified Italy, Provinces have always been assigned very limited functions (local roads maintenance being the main one); nevertheless, they have been the main sub-governmental entity until the inception in the '70s of higher-level Regions, still administrative entities although over a wider domain. Needless to say, in the meanwhile all our areas underwent the same national institutions evolution. Therefore, the reform, quite void of potential impact on individual behaviors too, did *not* imply much of a *treatment*. Instead, the border shift can be taken as an exogenous perturbation able to draw a light, administrative separation of two "cultural twins" (leaving them where they had always been living, without dislocation) and allowing an experiment identifying, by comparison of individual and institutional behaviors between these twin areas, the weight of historical legacy for cultural development. If one twin behavior diverges from the other, there are forces stronger than history at play. Conversely, if today no discontinuity is found on the old *North-Center* border, which used to be strong, then this is evidence of local cultural dynamics, which in a reasonable time has overcome the

amministrativi dai quali lo Stato non può prescindere" (Caputo and Romero 1931, 438). Interestingly, Galasso (1996) rationalizes the border shift in terms of forward-looking geography, given the rising fate of Rome versus the declining gravitational attractiveness of Naples.

8. The literature on economic performance has put emphasis on the effect on contemporary cultural behaviors induced by having been exposed to the institutions of the Kingdom of Naples/Two Sicilies (e.g., De Blasio and Nuzzo 2009, or Di Liberto and Sideri 2015), even after periods of "dormancy" (D'Adda and De Blasio 2017).

9. Symmetrically, for the first time ever the reform unified (under the Provinces of Rome, later Latina, and of Frosinone) the *North* and *Center* areas, so that their historical border became void of any political or administrative content.

TABLE 2.1: Summary statistics over municipalities: Contemporary covariates

| Variable | *North* | *Center* | *South* | Variable | *North* | *Center* | *South* |
|---|---|---|---|---|---|---|---|
| Elevation | 465 (231) | 340 (245) | 209 (207) | Population | 7,371 (9,714) | 5,529 (8,300) | 5,667 (6,684) |
| Max diff. in elevat. | 895 (441) | 878 (463) | 627 (413) | Graduates | 20 (8) | 28 (12) | 28 (11) |
| Distance to coast | 37 (15) | 29 (15) | 29 (12) | Income tax base | 7,034 (1,009) | 6,479 (1,249) | 5,405 (1,063) |
| Time to Rome | 1.53 (0.29) | 1.90 (0.27) | 2.31 (0.22) | Res. property wealth | 39,371 (28,217) | 37,732 (22,160) | 36,575 (20,721) |
| Time to Naples | 2.25 (0.49) | 1.67 (0.28) | 1.15 (0.29) | Houses served by aqueduct | 0.91 (0.11) | 0.90 (0.13) | 0.86 (0.13) |
| Size | 44 (29) | 36 (26) | 32 (23) | Municipal personn. exp. | 194 (73) | 211 (86) | 202 (53) |
| Tot. Pop. | 449,622 | 364,941 | 396,660 | No. of Munic. | 61 | 66 | 70 |

*Note:* Averages over municipalities per 40 km-long area, except for the sum operator used in Total population, and for the Number of municipalities; standard deviations are in parentheses. The North sample excludes the (coastal) municipalities of Latina, Sabaudia and Pontinia (see Appendix A).

weight of institutional/cultural legacy. Then, the quest for determinants is open, and results from the experiment might help.

The setup is favorable to address the general, interesting question of cultural persistence vs. dynamics, but also to assess the extent to which local culture in Southern Italy can be considered "persistently bad". In fact, a local cultural lag is often regarded as a major determinant of the economic backwardness of Campania, a record-low per-capita income region in Italy: the region, and in particular its upper province of Caserta, rank last in a majority of social capital or cultural indicators surveys (e.g., Figure 2.2, from Guiso et al. 2004, or the principal component analysis in Tabellini 2010), and this characteristic is often ascribed to an institutional and cultural heritage. According to Guiso et al. (2016), low civic capital in Southern Italian regions can be traced back to Middle Ages developments.

## 2.3 Contemporary behavioral culture is worse southwards

Contemporary sub-provincial indicators confirm that cultural behavior is worse in Campania than in Latium. This is what emerges from virtually the whole set of social capital and individual and institutional cultural variables that we have collected at the sub-provincial level, when comparing our South area (today a province of Campania) with our Center and North areas (both in Latium today). On average, our South area is

FIGURE 2.2: Turnouts at referenda in provinces of Central-Southern Italy


*Note:* Provincial referenda turnout; 1946-1989 average. A thin (thick) line marks provinces (regions).

characterized by more crimes (particularly, economic-related ones) and by less turnout, non-profit activity, literacy and municipality efficiency (see Table 2.2). Graphically inspecting, Figure 2.3 displays the distribution of contemporary referenda turnouts at municipal level using a kernel-weighted local polynomial smoother to fit data against the distance from either side of the current Latium-Campania border. This non-parametric approach around the threshold allows us to gauge a visual impression of the discontinuity at the new border in a typical cultural indicator, unfavorable to *South*. It also hints to the need of a non-linear approach to figure out data content when performing a more formal econometric analysis, which is what we do next.

Assuming continuity in potential outcomes at a border, an observation unit in its left neighborhood might be taken as a valid counterfactual to one in its right neighborhood. Ideally, one would like to have enough data in each neighborhood to estimate the average treatment effect without having to rely on functional forms, using instead only variation at the discontinuity. Immediate border sides hardly display such data richness, as it is in our case too. In order to enlarge the data set, the researcher is bound to move away from the border. In our case, the distance between the old and the new border constrains our Center area to be no longer than 40 km. There follows an analogous dimensional constraint on the areas to compare. Our approach is therefore necessarily semi-parametric, in that it relies only on observations in bands extending 40 km from each border. This is also of advantage, as it limits the effect on the conditional functions of observations lying far from each border, preserving local information content instead. We try to maximize this effect by zooming also to a closer band, but sample size limitations do not allow us to go closer than 25 km.

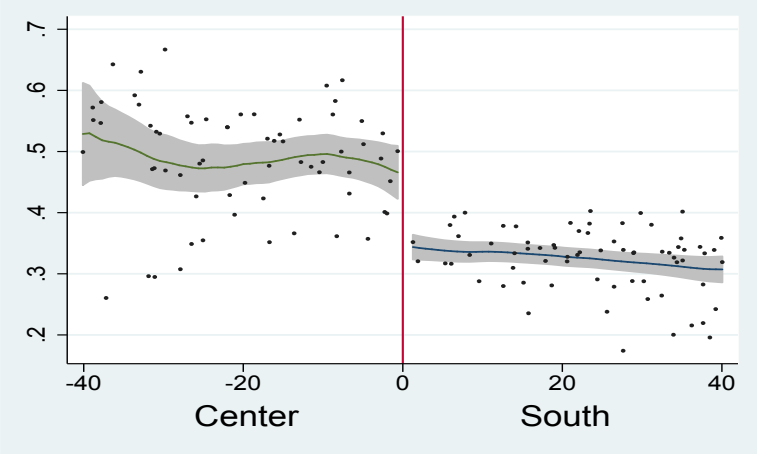
Moving away from the border, one is left with the problem of disentangling the treat-

TABLE 2.2: Summary statistics over municipalities: Contemporary outcomes

| Variable | *North* | *Center* | *South* | Variable | *North* | *Center* | *South* |
|---|---|---|---|---|---|---|---|
| Crimes | 133.7 | 131.9 | 164.5 | Turnout at | 0.80 | 0.74 | 0.66 |
| | (67.9) | (73.1) | (115.1) | Eur. elections | (0.07) | (0.08) | (0.13) |
| *Violent* | 23.7 | 21.2 | 20.5 | Non-profit | 4.2 | 4.0 | 2.6 |
| | (14.5) | (9.9) | (18.2) | organizations | (1.7) | (2.2) | (1.3) |
| *Other* | 109.9 | 110.7 | 143.9 | Non-profit | 3.5 | 1.6 | 1.9 |
| | (62.3) | (66.8) | (101.6) | employees | (7.5) | (3.1) | (3.0) |
| Mafia | 1.1 | 0.9 | 1.9 | Literacy | 97.7 | 97.2 | 96.4 |
| crimes | (0.9) | (0.8) | (1.7) | rate | (1.2) | (1.3) | (1.9) |
| Turnout at | 0.47 | 0.48 | 0.32 | Administrative | 1.5 | 1.5 | 2.7 |
| referenda | (0.10) | (0.09) | (0.05) | inefficiency | (1.2) | (1.1) | (1.5) |

*Note:* Averages over municipalities per 40 km-long area (61 municipalities in *North*, 66 in *Center* and 70 in *South*); standard deviations are in parentheses. The North sample excludes the (coastal) municipalities of Latina, Sabaudia and Pontinia (see Appendix A).

FIGURE 2.3: Non-linear fit of contemporary referenda turnouts data around the New Border



ment indicator (here, simply the condition of being north of one border), which is a non-linear and discontinuous function of geographic position, from the smooth effects of distance or coordinates. This problem can be solved through an appropriately specified regression model. Indeed, one necessary condition for correct specification is ability to distinguish a nonlinearity from a discontinuity. In analogy with Dell (2010), we set up a semi-parametric geographical (spatial) regression discontinuity (RD) model, aimed at evaluating the effect of being on either side of a border. It expresses cultural outcomes as functions of a number of georeferenced variables. Therefore, across each of our borders, we run an estimation equation over municipality $i$ variables of the form

$$y_i = \alpha + \gamma\, T_i + f(\text{geographic location}_i, T_i) + X_i'\, \beta + \Phi_i + \varepsilon_i \tag{2.1}$$

Here $y_i$ is an outcome variable, $\alpha$ is a constant, $T_i$ is our treatment dummy, valued 1 only if the municipality lies above the border, $X_i$ is a vector of covariates, and $\Phi_i$ is a set of fixed effects. The term $f(\text{geographic location}_i, T_i)$ is the RD polynomial which controls for smooth functions of geographic location. In it, we include interactions of our position measure with the area dummy. If in the data the treatment effect is a constant function of the distance from the border, the interaction term would be irrelevant and the estimated difference between realized and potential outcomes would apply to the whole domain. As the treatment effect might well vary with the distance, one can show that in this case including the interaction yields a correct estimate of the average treatment effect exactly at the threshold, which is where our comparison is most, or solely, effective: omitting this interaction would make our estimate imprecise at the point where we care.

We use this semi-parametric model to regress social capital and cultural outcomes over a number of polynomials in the forcing variable $d$ "algebraic distance from border".[10] We do so for a number of outcomes: on contemporary data, we use referenda and European elections turnouts, non-profit employees and associations, crimes and municipality inefficiency. Among crimes, we isolate the component which the literature regards as a valid (inverse) indicator of social cohesion, namely violent crimes (see Glaeser et al. 1996), and its complement, which is total economic-related crimes. By a particular intersection of them, we are also able to analyze *mafia* crimes across the border. Table 2.3 presents baseline estimates for the discontinuity exercise evaluated at the threshold of the current Latium-Campania (*Center-South*) border based on fully interacted polynomials in algebraic distance.[11] Given ex-ante uncertainty on the correct specification in geographic location, Panel I shows results for a linear polynomial with linear interaction, Panel II for

10. We also tried polynomials in deviations of municipalities geographic coordinates from their mean and, as in Dell (2010), in longitude and latitude and in time distance from Rome, finding similar results. Our baseline specification, however, has the advantage of being able to detect a potential discontinuity exactly at the border.

11. We define as "full" the interaction with the same degree of the polynomial. In the table, neither the coefficients of the geographic location, nor those of the control variables, nor of the constant are shown.

a quadratic-quadratic case and Panel III for a cubic-cubic one. The main finding is that, for all specifications, at this new border there is evidence of a quite strong and diffused discontinuity in individual and institutional cultural outcomes. This discontinuity concerns all aggregations of the crime rate, turnouts and local administrative inefficiency, and it is always unfavorable to *South*. Despite a millennial common legacy, *Center* and *South* live distinct cultural present times.

According to the quadratic specification in algebraic distance, that Appendix A shows to be preferred by data to alternative specifications,[12] in years 2004-2009 *South* had 196 intentional crimes more than *Center* for every thousand average residents; of these crimes, 62 were thefts (not shown), 36 were violent crimes, and 3 can be classified as *mafia* crimes. The total amount increases to almost 250 when restricting estimation to the information contained in the 25 km sample (but with only 10% statistical significance). Turnout at referenda was robustly 10-15 percentage points higher in *Center* than in *South*, while for European elections the difference ranges between 13 and 22 p.p. As for local administrative inefficiency, evaluated at the border, the average number of balance sheet parameters signaling structural deficit in southern municipalities in 2001 was 3.5 (over the 10 monitored by the Italian government), while it was 0.9 in *Center*. No difference in the number of employees (and local branches, not shown) of non-profit organizations emerges, but this should come as little surprise, for Table 2.2 shows that on average non-profit activity is equally low on both sides of the current Latium-Campania border: just 1.6 and 1.9 employees every 1,000 residents per municipality, in *Center* and in *South* respectively, vs. a national average of 11.5.

These results mark a clear cultural difference between areas which had long been sharing history and institutions. Today, cultural behavior happens to be worse when crossing southwards the contemporary *Center-South* border, created just a few decades ago.

## 2.4 How old is this "worse"?

How old is this discontinuity? How long ago does lower social capital in *South* trace back? How persistent is its worse culture with respect to *Center*? After all, *Center* and *South* had been sharing cultural traditions and institutional history for over a millenium.

To answer these questions, it is necessary to move back in time. Old sub-provincial behavioral data are typically scant, discontinuous, different from those in contemporary use, which undermines both a quest for a possible exact "inception date" of the *Center-South* discontinuity, and a kind of diff-in-diff exercise to check variation in time across the border, while controlling for major events occurred in the meanwhile. However, data mining in historical statistical bulletins prompts us with a number of pre-reform variables

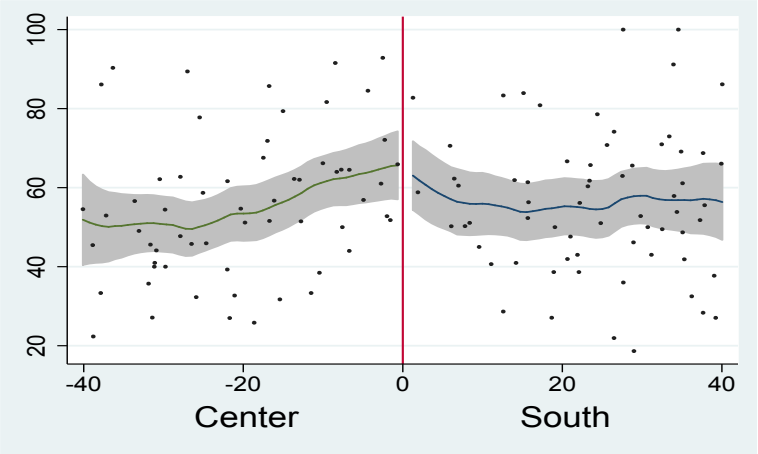
12. Appendix A also validates our RD exercises by showing that the necessary continuity in the covariates at the border is satisfied.

TABLE 2.3: RD on contemporary outcomes as function of location across the New Border

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | crimes | | | | refer. | Eur. el. | nonprof. | admin. |
| | total | *violent* | *other* | *"mafia"* | turnout | turnout | empl. | ineffic. |
| Panel I. Interacted Linear Polynomial in Algebraic Distance to the New Border | | | | | | | | |
| Center | -82.1 | -17.3** | -64.8 | -1.1* | 0.12*** | 0.01 | 2.29** | -0.85* |
| <40 km | (51.7) | (8.2) | (44.4) | (0.6) | (0.02) | (0.04) | (1.03) | (0.49) |
| $R^2$ | 0.08 | 0.13 | 0.08 | 0.13 | 0.58 | 0.15 | 0.04 | 0.17 |
| Center | -168.7** | -29.8** | -139.0** | -2.5** | 0.14*** | 0.08 | -0.31 | -2.03*** |
| <25 km | (80.3) | (12.6) | (68.9) | (1.0) | (0.03) | (0.05) | (1.09) | (0.59) |
| $R^2$ | 0.16 | 0.20 | 0.15 | 0.24 | 0.64 | 0.05 | 0.16 | 0.15 |
| Panel II. Interacted Quadratic Polynomial in Algebraic Distance to the New Border | | | | | | | | |
| Center | -196.0** | -35.6** | -160.3* | -2.7** | 0.15*** | 0.13** | -0.15 | -2.65*** |
| <40 km | (98.2) | (15.3) | (84.4) | (1.3) | (0.03) | (0.06) | (1.18) | (0.64) |
| $R^2$ | 0.13 | 0.19 | 0.12 | 0.18 | 0.58 | 0.18 | 0.08 | 0.22 |
| Center | -248.5* | -46.1** | -202.4* | -3.7* | 0.10*** | 0.22*** | -2.15 | -2.98*** |
| <25 km | (129.5) | (22.4) | (109.4) | (2.0) | (0.04) | (0.08) | (1.46) | (0.85) |
| $R^2$ | 0.18 | 0.24 | 0.17 | 0.27 | 0.64 | 0.14 | 0.18 | 0.17 |
| Panel III. Interacted Cubic Polynomial in Algebraic Distance to the New Border | | | | | | | | |
| Center | -264.7* | -48.2** | -216.5* | -4.6** | 0.10** | 0.23*** | -2.81* | -3.07*** |
| <40 km | (134.7) | (22.9) | (114.2) | (2.1) | (0.04) | (0.08) | (1.66) | (0.89) |
| $R^2$ | 0.14 | 0.21 | 0.12 | 0.23 | 0.59 | 0.21 | 0.11 | 0.23 |
| Center | -187.1 | -36.8 | -150.3 | -3.6 | 0.08* | 0.20 | 2.90 | -2.53*** |
| <25 km | (117.3) | (25.8) | (97.0) | (3.3) | (0.05) | (0.13) | (2.06) | (0.95) |
| $R^2$ | 0.20 | 0.28 | 0.18 | 0.27 | 0.66 | 0.16 | 0.22 | 0.18 |

*Note:* Estimated coefficients on "Center" dummy from regressions on polynomials in Euclidean distance from the municipality's townhall to the border, including interaction with the area dummy. "Center" equals 1 if the municipality is in *Center*, 0 if it is in *South*. The degree of the polynomial and of the interaction is linear in Panel I, quadratic in Panel II, cubic in Panel III. Constant and distance coefficients not shown. First lines in each panel consider observations within 40 km of the border (136 municipalities); second lines reduce this distance to 25 km (74 municipalities). Robust st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

FIGURE 2.4: Non-linear fit of Pre-Reform municipal election turnout data around the New Border




directly or indirectly related to those that we used for contemporary assessment. We use these pre-reform data to perform another, analogous RD test across what will later become the new Latium-Campania border, to check whether at those times there existed a discontinuity of the kind found in recent data.

Preliminarily, Figure 2.4 replicates for pre-reform local turnout data the fit across the *Center-South* border of Figure 2.3, necessarily replacing referenda with municipal elections:[13] there emerges visual confirmation of cultural continuity for territories which, that far, had never experienced a division for over a millennium.

An RD exercise over the broadest set of variables formally confirms the result. Table 2.4 highlights strong previous cultural continuity across the new Latium-Campania border, before its creation. This continuity holds for individual, collective and institutional behaviors, robustly with respect to functional forms or distance selection. It holds for available municipal level data (Part *a* of the table): local elections turnout, mutual aid and cooperative organizations and members, and main measures of local administrative inefficiency. Part *b* shows that this cultural continuity also holds for judicial district level data: crimes (all aggregations), litigations and both civil and criminal court efficiencies. Usual caveats when dealing with a limited sample size apply at this district level,[14] but on the one hand with them we refrain from using higher order polynomials, and on the other hand worries are quite offset by the stability of the results. Confirmation of local pre-reform homogeneity comes also from alternative measures (not shown) of first-degree court efficiency: no statistically significant discontinuity is found in the ratio of inves-

13. As for municipal elections, contemporary data (2002-2016) on turnout display a strong discontinuity in the cubic model, favoring *Center* over *South*.

14. Upon Italian unification, and for very few decades after, judicial jurisdictions were very fractionalized. Judicial districts typically pool groups of municipalities: we only consider those made up of municipalities strictly included in our areas.

TABLE 2.4: RD on Pre-Reform outcomes as function of location across the New Border

*Part a*: Municipal data within 40 and 25 km from the border

| | Dependent Variable | | | | | |
|---|---|---|---|---|---|---|
| | mun. el. turnout | mut./coop. members | mutual aid societies | cooper. | munic. self-adm. | mun. fin. auton. |
| Panel I. Fully Interacted Linear Polynomial in Algebraic Distance to the New Border | | | | | | |
| Center | 0.08 | -11.2 | -0.13 | -0.02 | -0.02 | 3.68 |
| <40 km | (0.06) | (12.9) | (0.10) | (0.05) | (0.03) | (2.30) |
| $R^2$ | 0.04 | 0.06 | 0.07 | 0.04 | 0.05 | 0.03 |
| Center | 0.10 | -17.1 | -0.12 | -0.03 | 0.03 | 0.73 |
| <25 km | (0.08) | (18.5) | (0.15) | (0.05) | (0.04) | (2.64) |
| $R^2$ | 0.11 | 0.08 | 0.01 | 0.04 | 0.09 | 0.11 |
| Panel II.a Fully Interacted Quadratic Polynomial in Algebraic Distance to the New Border | | | | | | |
| Center | 0.07 | -28.5 | -0.21 | -0.02 | 0.06 | -0.30 |
| <40 km | (0.10) | (20.3) | (0.18) | (0.04) | (0.05) | (2.81) |
| $R^2$ | 0.06 | 0.07 | 0.07 | 0.04 | 0.07 | 0.07 |
| Center | -0.08 | -61.3** | -0.44* | 0.03 | -0.03 | -1.32 |
| <25 km | (0.12) | (29.5) | (0.26) | (0.08) | (0.06) | (4.03) |
| $R^2$ | 0.14 | 0.11 | 0.06 | 0.05 | 0.11 | 0.11 |
| Panel III. Fully Interacted Cubic Polynomial in Algebraic Distance to the New Border | | | | | | |
| Center | -0.06 | -45.7 | -0.34 | 0.02 | -0.03 | -0.44 |
| <40 km | (0.13) | (27.9) | (0.26) | (0.06) | (0.07) | (3.95) |
| $R^2$ | 0.07 | 0.08 | 0.08 | 0.05 | 0.09 | 0.07 |
| Center | -0.09 | -12.9 | -0.33 | 0.16 | -0.10 | -3.08 |
| <25 km | (0.16) | (34.2) | (0.35) | (0.11) | (0.07) | (4.36) |
| $R^2$ | 0.14 | 0.14 | 0.08 | 0.08 | 0.15 | 0.11 |
| Observ.<40km | 126 | 128 | 128 | 128 | 130 | 130 |
| <25km | 69 | 71 | 71 | 71 | 72 | 72 |

*Part b*: Judicial district data within 40 km from the border

| | crimes | | | litigation | crim. court efficiency | civ. court efficiency |
|---|---|---|---|---|---|---|
| | total | *violent* | *property* | | | |
| Panel I. Fully Interacted Linear Polynomial in Algebraic Distance to the New Border | | | | | | |
| Center | 4.0 | 0.4 | 1.0 | -4.9 | 0.02 | -0.02 |
| | (3.0) | (1.7) | (1.0) | (3.2) | (0.02) | (0.11) |
| $R^2$ | 0.11 | 0.05 | 0.08 | 0.14 | 0.26 | 0.07 |
| Panel II.b Interacted Quadratic Polynomial in Algebraic Distance to the New Border | | | | | | |
| Center | 4.0 | 0.3 | 1.0 | -4.9 | 0.02 | -0.02 |
| | (3.0) | (1.7) | (1.0) | (3.2) | (0.02) | (0.11) |
| $R^2$ | 0.12 | 0.08 | 0.08 | 0.14 | 0.26 | 0.08 |

*Note:* Estimated coefficients on "Center" dummy from regressions on polynomials in Euclidean distance from the municipality's townhall to the border, including interaction with the area dummy. "Center" equals 1 if the observation is in *Center*, 0 if it is in *South*. The degree of the polynomial and of the interaction is linear in Panels I, quadratic in Panel II.a, cubic in Panel III, while Panel II.b has quadratic polynomial with linear interaction. Constant and distance coefficients not shown. In Part *b*, trial courts strictly included in the symmetric 40-km bands from each border (25 observations). Robust standard errors in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

tigated criminal trial cases over new plus pending ones, and the same happens for the duration measure of the number of civil lawsuits ruled within 15 days of their incoming.

Hence, starting from homogeneity, culture in *South* (Campania) got worse than that in *Center* (now belonging to Latium) not earlier than a few decades ago, i.e. it became relatively "bad" much more recently than alleged, or assumed, in previous contributions to the literature on persistence and on economic growth. The two contiguous areas, which had long been sharing institutions, cultural legacy and behaviors, bifurcated in culture over the 20th century, with a sharp deterioration in the southern area. Over the last few decades, while *Center* evolved much more in line with previous moderate common patterns, *South* experienced a dramatic, exponential increase in crime rate, and in general an adverse cultural evolution.

## 2.5 At the upper border, cultural convergence: Was it all institutions?

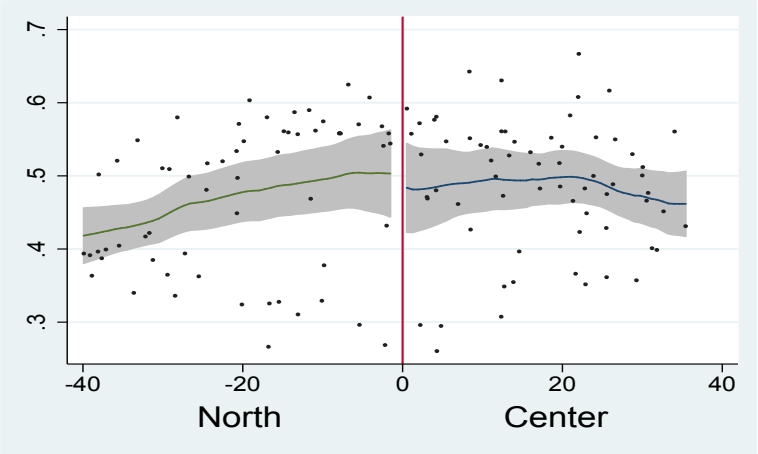
What has been driving the emergence of a cultural discontinuity between areas with common institutions, history and behavior until recently? When bifurcating from *South* with which it was sharing a questioned background, did *Center* culture get assimilated to *North* one? If so, did it do "from above" or "from below", i.e. how did *North* culture fare before the reform, relative to the cultural twins couple *Center* and *South*? Hints to answer this set of questions will be given by looking at the upper, older border, the one between *North* and *Center* (yellow-dashed in Figure 2.1), endowed with strong historical bearings but abolished at first politically in 1870, and then administratively since 1927.

### 2.5.1 Today, *Center* looks like *North*

Let's start from checking whether in recent times there exists cultural homogeneity across this border, which since the reform is just an imaginary line entirely internal to Latium, cutting transversally in halves the provinces of Latina and Frosinone. Graphical evidence (Figure 2.5) as well as a targeted RD exercise (Table 2.5) shows that individual and institutional cultural behaviors do not present discontinuities between *Center* and *North*, administratively assembled by the 1926 reform.[15] Both on average and at border, all contemporary cultural indicators in *Center* take on values similar to those in *North*, while over the century they did not deteriorate as those in *South*.

15. Table A.2 in Appendix A shows that covariates balancing holds across the old border too.

FIGURE 2.5: Non-linear fit of contemporary referenda turnout data around the Old Border




### 2.5.2 *North* used to be worse than *Center* (and *South*)

If, following the separation marked by the reform, *Center* differentiated from *South* while showing cultural assimilation to *North*, downplaying the weight of historical legacies in its cultural evolution, was it because of an institutional channel? Did *Center* people get administered by more civically virtuous northern institutions and become more virtuous, themselves as well as their local institutions? Or was it by contagion? Did people's culture in *Center* benefit from greater interaction with northern people, endowed with higher social capital? But, did *North* have a higher civic and social capital? Was there any other main driver in action? To shed some light on these cultural dynamics questions, it is crucial to check what the situation was like at the upper border before the reform, verifying whether individual and institutional behaviors in *North* were different, and in which direction, from those in *Center* (which Section 2.4 has shown was sharing behaviors with *South* at those times).

The border between these two areas had always been a strong border. Coinciding also with geographical impediments, it had been effective as a political and institutional boundary for more than one millennium, marking a divide epitomized in a linguistic differentiation between its two sides. It was only upon Italian unification, in 1870, that this border had been downgraded to a mere (and very limited) administrative profile, for later being totally erased by the fascist reform. In the decades before such reform, cultural differences might be expected between the two sides, due to the persistence effect of a millennial orographic and institutional separation, but much less informed expectations can be formulated about their sign. It is possibile to check both existence and sign of cultural differences with the same tools used in the previous sections.

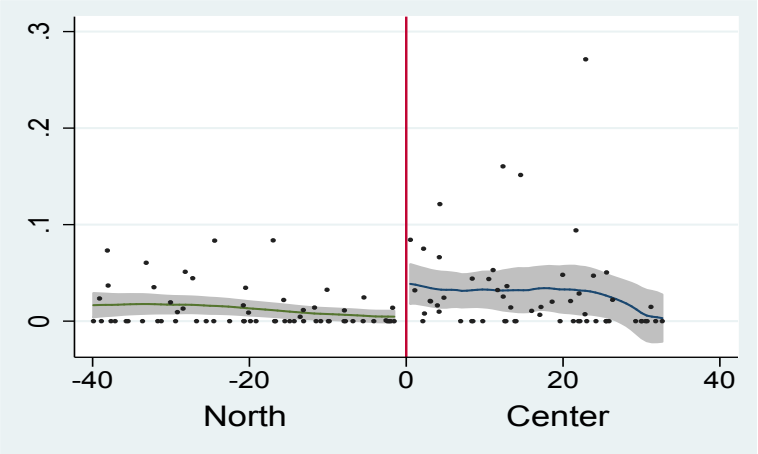
Figure 2.6 and Table 2.6 provide evidence of behavioral and institutional discontinuities across the old border in the decades in which it was in place as an administrative

TABLE 2.5: RD on contemporary outcomes as function of location across the Old Border

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | crimes | | | | refer. | Eur. el. | nonprof. | admin. |
| | total | *violent* | *other* | *"mafia"* | turnout | turnout | empl. | ineffic. |
| Panel I. Fully Interacted Linear Polynomial in Algebraic Distance to the Old Border | | | | | | | | |
| North | -10.7 | 1.3 | -12.0 | 0.1 | 0.04 | 0.01 | -0.3 | -0.4 |
| <40 km | (19.8) | (3.6) | (17.9) | (0.3) | (0.04) | (0.03) | (1.4) | (0.4) |
| $R^2$ | 0.08 | 0.02 | 0.10 | 0.04 | 0.15 | 0.15 | 0.00 | 0.05 |
| North | -10.2 | -1.0 | -9.2 | -0.1 | 0.03 | 0.01 | -0.9 | -0.4 |
| <25 km | (23.8) | (4.0) | (21.8) | (0.3) | (0.05) | (0.03) | (1.7) | (0.6) |
| $R^2$ | 0.25 | 0.06 | 0.27 | 0.14 | 0.11 | 0.24 | 0.04 | 0.04 |
| Panel II. Fully Interacted Quadratic Polynomial in Algebraic Distance to the Old Border | | | | | | | | |
| North | 8.4 | -3.0 | 11.4 | 0.2 | 0.03 | -0.05* | -0.2 | -0.2 |
| <40 km | (28.1) | (4.7) | (25.4) | (0.4) | (0.06) | (0.03) | (1.9) | (0.7) |
| $R^2$ | 0.14 | 0.07 | 0.15 | 0.04 | 0.16 | 0.21 | 0.03 | 0.05 |
| North | -15.2 | -5.6 | -9.6 | 0.5 | 0.03 | -0.06 | -1.3 | -0.2 |
| <25 km | (36.6) | (5.2) | (34.0) | (0.5) | (0.07) | (0.03) | (2.5) | (0.9) |
| $R^2$ | 0.25 | 0.07 | 0.27 | 0.17 | 0.11 | 0.27 | 0.04 | 0.05 |
| Panel III. Fully Interacted Cubic Polynomial in Algebraic Distance to the Old Border | | | | | | | | |
| North | -30.4 | -7.3 | -23.1 | 0.1 | 0.02 | -0.03 | -2.9 | -0.5 |
| <40 km | (40.7) | (6.2) | (37.4) | (0.5) | (0.08) | (0.04) | (2.9) | (1.0) |
| $R^2$ | 0.15 | 0.08 | 0.16 | 0.05 | 0.16 | 0.23 | 0.04 | 0.06 |
| North | -5.3 | -12.0 | 6.6 | 0.2 | -0.05 | -0.05 | -4.3 | -0.1 |
| <25 km | (54.7) | (8.8) | (49.9) | (0.7) | (0.10) | (0.06) | (4.6) | (1.3) |
| $R^2$ | 0.25 | 0.08 | 0.27 | 0.17 | 0.13 | 0.29 | 0.05 | 0.08 |

*Note:* Estimated coefficients on "North" dummy from regressions on polynomials in Euclidean distance from the municipality's townhall to the border, including interaction with the area dummy. "North" equals 1 if the municipality is in *North*, 0 if it is in *Center*. The degree of the polynomial and of the interaction is linear in Panel I, quadratic in Panel II, cubic in Panel III. Constant and distance coefficients not shown. First lines in each panel consider observations within 40 km of the border (127 municipalities); second lines reduce this distance to 25 km (88 municipalities). The North sample excludes Latina, Sabaudia and Pontinia (see Appendix A). Robust standard errors in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

FIGURE 2.6: Non-linear fit of Pre-Reform members of cooperatives and mutual aid societies data around the Old Border




boundary.[16] These discontinuities concern all measures of mutual aid and cooperative activity, litigation, and judicial efficiency, while they are absent in administrative efficiency and crimes. Interestingly, all statistically significant discontinuities but one (one of the court efficiencies) had *North* performing worse than *Center*. Therefore, according to this empirical evidence, in post-unification pre-reform Italy individual and institutional behaviors in *North* were distinct and, more importantly, *no better* than those in the *Center-South* block, and relative cultural disadvantage was more a fact in *North* than in the other areas.

### 2.5.3 Not much of a case for institutions and contagion

Let's take stock. Overall across the two borders in the region that we analyze, comparing results on past data with those on current ones reveals paths of cultural impersistence, quite penalizing southwards. In the first decades of the Kingdom of Italy, individual and institutional behaviors in *Center* were similar to those in *South*, and different from those in *North*. At some point in the 20th century, after the reform, *Center* culturally bifurcated from *South*, and got assimilated to *North*. This assimilation is hardly explained with contagion through enhanced interaction with more civically virtuous northern people: in the decades before the reform, individual behaviors in *North* were not better, and if anything they were worse, than those in the *Center-South* block. This argument is strengthened by looking at the literacy rate, held by the literature as positively correlated with cultural behavior: Table 2.7 shows that in 1921 the literacy rate was lower in *North* with respect to *Center*, where in turn it was similar to *South* (while the situation appears reversed nowadays). A usual institutional channel, by which people and

16. Turnout data at municipal level are not available for the North area in the post-unification, pre-reform years.

TABLE 2.6: RD on Pre-Reform outcomes as function of location across the Old Border

*Part a*: Municipal data within 40 and 25 km from the border

| | Dependent Variable | | | | |
|---|---|---|---|---|---|
| | mutual/coop. members | mutual aid societies | coop eratives | munic. self -admin. | mun. fin. auton. |
| Panel I. Fully Interacted Linear Polynomial in Algebraic Distance to the Old Border | | | | | |
| North | -40.7*** | -0.20*** | -0.07** | -0.03 | 0.36 |
| <40 km | (10.0) | (0.07) | (0.03) | (0.03) | (3.86) |
| $R^2$ | 0.08 | 0.05 | 0.11 | 0.01 | 0.03 |
| North | -33.8** | -0.19** | -0.10** | -0.03 | 0.41 |
| <25 km | (13.8) | (0.10) | (0.04) | (0.04) | (4.12) |
| $R^2$ | 0.10 | 0.07 | 0.11 | 0.02 | 0.02 |
| Panel II.a Fully Interacted Quadratic Polynomial in Algebraic Distance to the Old Border | | | | | |
| North | -32.7** | -0.19* | -0.12** | -0.06 | -4.30 |
| <40 km | (15.3) | (0.10) | (0.06) | (0.05) | (5.28) |
| $R^2$ | 0.10 | 0.07 | 0.13 | 0.02 | 0.04 |
| North | -39.4** | -0.09 | -0.12* | -0.11* | -10.51 |
| <25 km | (19.2) | (0.14) | (0.07) | (0.06) | (6.53) |
| $R^2$ | 0.11 | 0.08 | 0.13 | 0.05 | 0.10 |
| Panel III. Fully Interacted Cubic Polynomial in Algebraic Distance to the Old Border | | | | | |
| North | -40.2** | -0.08 | -0.14* | -0.07 | -8.44 |
| <40 km | (19.6) | (0.14) | (0.08) | (0.07) | (7.11) |
| $R^2$ | 0.10 | 0.09 | 0.15 | 0.02 | 0.05 |
| North | -49.0* | -0.31 | -0.17 | -0.01 | -1.29 |
| <25 km | (25.2) | (0.20) | (0.11) | (0.09) | (9.59) |
| $R^2$ | 0.11 | 0.11 | 0.17 | 0.09 | 0.12 |

*Part b*: Judicial district data within 40 km from the border

| | crimes | | | litigation | crim. court efficiency | civ. court efficiency |
|---|---|---|---|---|---|---|
| | total | *violent* | *property* | | | |
| Panel I. Fully Interacted Linear Polynomial in Algebraic Distance to the Old Border | | | | | | |
| North | -1.4 | -0.1 | -0.1 | 7.3** | -0.09** | 0.25*** |
| | (2.7) | (1.2) | (1.0) | (3.1) | (0.04) | (0.06) |
| $R^2$ | 0.06 | 0.10 | 0.03 | 0.19 | 0.34 | 0.46 |
| Panel II.b Interacted Quadratic Polynomial in Algebraic Distance to the Old Border | | | | | | |
| North | -1.1 | 0.2 | -0.3 | 5.3* | -0.08** | 0.24*** |
| | (2.9) | (1.4) | (1.0) | (2.7) | (0.04) | (0.06) |
| $R^2$ | 0.07 | 0.13 | 0.06 | 0.27 | 0.37 | 0.46 |

*Note:* Estimated coefficients on "North" dummy from regressions on polynomials in Euclidean distance from the municipality's townhall to the border, including interaction with the area dummy. "North" equals 1 if the observation is in *North*, 0 if it is in *Center*. The degree of the polynomial and of the interaction is linear in Panels I, quadratic in Panel II.a, cubic in Panel III, while Panel II.b has quadratic polynomial with linear interaction. Constant and distance coefficients not shown. In Part *a*, first lines in each panel consider observations within 40 km of the border (124 municipalities); second lines reduce this distance to 25 km (85 municipalities). In Part *b*, observations are trial courts strictly included in the symmetric 40-km bands from each border (30 observations). Robust standard errors in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

TABLE 2.7: RD on past and contemporary literacy rates as function of geographic location across the New and the Old Borders

| Dependent Variable: Literacy rate | | | | | |
|---|---|---|---|---|---|
| | New Border | | | Old Border | |
| | 1921 | 2001 | | 1921 | 2001 |
| Center | -0.04 | 11.95* | North | -0.18*** | -8.95 |
| <40 km | (0.06) | (6.50) | <40 km | (0.05) | (6.32) |
| $R^2$ | 0.07 | 0.10 | $R^2$ | 0.27 | 0.16 |
| Center | -0.01 | 17.36** | North | -0.16*** | -11.44 |
| <25 km | (0.08) | (8.67) | <25 km | (0.05) | (7.13) |
| $R^2$ | 0.11 | 0.09 | $R^2$ | 0.39 | 0.11 |

*Note:* The first/last two columns report estimated coefficients on Center/North dummy from regressions on quadratic polynomials in Euclidean distance from the municipality's townhall to the border, including interaction with the area dummy. "Center"/"North" equals 1 if the municipality is in *Center/North*, 0 if it is in *South/Center*. Constant and algebraic distance coefficients not shown. First lines in each panel consider observations within 40 km of the border (128 municipalities in the first column, 136 in the second, 122 in the third, 127 in the fourth); second lines reduce this distance to 25 km (70 municipalities in the first column, 74 in the second, 85 in the third, 88 in the fourth). Rob. st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

institutions in *Center* would have benefited from being exposed to better institutions when administratively joined to *North*, has even more problems, as objections apply. First, and again, institutional behaviors in *North* used to be not better than those in *Center-South*. In addition, as the Kingdom of Italy was a highly centralized State, its main sub-governmental entities (Provinces) were endowed for more than a century (i.e., well into the republican era) with very limited tasks and powers, so that being affiliated to one or the other would not make much of a difference: moving the administrative border could have had limited effect, and we use it mainly as an instrument which divided cultural twins while leaving them in their place. It is true that later on decentralization took place, with the introduction of Regions in the 1970s, but it is also true that most of the behaviors under review (e.g., crimes, referenda turnouts, judicial efficiency) are unrelated to domains that could have been affected by any Italian sub-governmental entity: they are instead ruled by equal national laws and governed by centralized entities. Furthermore, in the first decades of the Kingdom of Italy administrative and judicial efficiency culture was not relatively worse southwards, so that a role for a bad regional administration in the last decades could only stem from recent developments and from an adverse endogenous evolution of individual and institutional culture, having little to do with persistence of a bad culture. Reasons for this endogenous cultural deterioration are an important object to be investigated, as well as those that led a piece of territory with southern cultural legacy to become assimilated to North in presence of a change in administrative borders.

## 2.6 Extending to controls

This section explores a number of extensions of the simple contemporary RD model presented in Section 2.3, to control for variables which might have played a role in the dynamics. The main results in terms of the impact on the contemporary discontinuity at the new border are reported in Table 2.8, just for major outcomes. All regressions columns build on our baseline fully interacted quadratic polynomial in algebraic distance from the border. A single type of control per column is considered.

Appendix A shows balancing of covariates across the border. Therefore, even if estimation has that geographical factors like elevation and distance from the coast (but not territory slope) do appear to have explanatory power for virtually all outcomes, they can only leave unaffected the discontinuity estimated at the border, as is shown in Table 2.8 under the cumulative column for geocontrols.

A nice explanation for referendum participation emerges when controlling for time distance from main cities. This addition narrows the interval for the estimated advantage in participation of *Center* over *South* to 11-13 percentage points (from 10-15 in the baseline, pooling estimates from the larger and the smaller samples). It does so with a strongly statistical significance in the negative coefficient for time to Rome: the less it takes to/from Rome, the more people participate. On the contrary, time from Naples does not appear to matter. Many people commute at medium to low frequency with Rome where many job opportunities are. Italian laws give the chance to vote at referenda away from one's official residence: therefore, it might be possible that people officially resident in municipalities in our sample which are farer from Rome, but who live in that city for work reasons, find it easier to vote where they live, so to avoid the (multi-dimensional) cost of homecoming. A shorter time to go back home over a week-end might affect positively the decision to participate in a (Sunday) referendum. Also, given that referenda usually concern national issues, as opposed to local ones, the higher the possibility of contact with the capital city, the higher the personal involvement in the topic that might result.

In fact, openness - approximated by the number of people that any person can meet - can be a relevant factor for cultural change: nevertheless, controlling for towns with more than 15,000 inhabitants (a number that marks a natural partition in the distribution of population in our sample) does not affect at all our results.

Interestingly, controlling for the number of graduates diminishes the statistical significance and the value of the difference in the crime rate.

Cultural behaviors are mainly influenced by the people we are in relation with. The workplace or the broad work environment is one of the main places of contamination of behavioral models, and one in which we spend several hours a day. When the job implies mobility or commuting, cultural cross-fertilization can result amplified. Therefore, we control for fixed effects of local labor markets (LLM) - as defined by the 2001

ISTAT classification - to give account of behavioral influences, originating in the economic domain, which might be determining local outcomes. First, we notice that some LLM's lay across our border. Commuting or work interactions across the border might help blurring local cultural differences: if this is the case, controlling for them should let discontinuities emerge with more strength. Indeed, this is what there seems to be in the results: net of the component related to be part of a common LLM, effects are generally strengthened in the 25 km sample (more affected by the overlap between border and LLM). It seems that the LLM control manages to isolate the "contamination" effect coming from commuting and interacting for work (and possibly for schooling) across the border: absent this dampening component, the estimated effect of laying on one side or the other would be even higher. This effect is instead tempered by economic driving forces which bring people to be mutually exposed to different local cultural models and which by this means induce cultural convergence.

We would like to control for the effect of economic factors on cultural behaviors more deeply than what we did with LLM's. The 1926 reform was followed by a number of economic shocks of which at least one hit the whole region that we study. It is the infrastructural and firms financing programme carried out in the whole (extended) Southern Italy in post-WWII by the Cassa per il Mezzogiorno, a dedicated public institution. We do our best with available data and control for per capita public and private investment subsidies granted by the Cassa in our municipalities.[17] Our finding is that the significance of the effect across the border is again confirmed. Interestingly, in the case of crimes the effect even increases, although only within 25 km: holding equal investment subsidies per municipality, the difference in the crime rate would have been even higher. We delay discussion of migration to the next dedicated subsection.

### 2.6.1 Migration flows

So far, we have ignored the possibility of migration. In what follows we try to relax this assumption, extending the analysis in two dimensions: first, we try to answer the question of whether estimated cultural changes are, actually, the result of a physical local replacement of people, i.e. the result of their inflows and outflows, carrying different cultural backgrounds with them; secondly, we deal with the problem of selective migration, that is whether people sorted themselves according to their behavioral characteristics.

To answer the first question, we resort to municipal level data on residents migration flows. Unfortunately, these data do not specify at the same time municipality of origin and of destination of migrants, as it is instead the case for interprovincial flows. Nevertheless, they allow us to separately control for the effect of immigration on the one

17. Available data are an elaboration by Letizia Borgomeo and cumulate by municipality public investment expenditure and granted subsidies to private firms, excluding multi-municipality projects. Not shown, we have separately verified their balancing across the new border.

TABLE 2.8: Specification extensions

| | Control Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Base line | Geo contr. | Time from Rom/Nap | Town | Grad uates | LLM | Cassa Mezz. | All |
| Panel *a*. Dependent variable: Total crimes | | | | | | | | |
| Center | -196.0** | -199.1** | -174.8* | -197.0** | -187.2* | -122.4* | -195.3** | -152.6** |
| <40km | (98.2) | (95.6) | (98.2) | (97.8) | (95.6) | (65.8) | (98.3) | (69.2) |
| $R^2$ | 0.13 | 0.34 | 0.18 | 0.25 | 0.23 | 0.39 | 0.13 | 0.65 |
| Center | -248.5* | -274.9** | -257.7* | -246.6* | -218.0 | -275.7** | -257.9* | -247.6** |
| <25km | (129.5) | (136.3) | (130.8) | (129.6) | (131.8) | (104.6) | (132.4) | (95.9) |
| $R^2$ | 0.18 | 0.35 | 0.23 | 0.26 | 0.36 | 0.49 | 0.19 | 0.74 |
| Panel *b*. Dependent variable: Referendum turnout | | | | | | | | |
| Center | 0.15*** | 0.14*** | 0.13*** | 0.15*** | 0.15*** | 0.13*** | 0.15*** | 0.13*** |
| <40 km | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| $R^2$ | 0.58 | 0.70 | 0.66 | 0.61 | 0.59 | 0.82 | 0.58 | 0.85 |
| Center | 0.10*** | 0.08** | 0.11*** | 0.10*** | 0.09** | 0.16*** | 0.09** | 0.15*** |
| <25 km | (0.04) | (0.03) | (0.04) | (0.04) | (0.04) | (0.03) | (0.04) | (0.04) |
| $R^2$ | 0.64 | 0.69 | 0.69 | 0.70 | 0.66 | 0.83 | 0.64 | 0.88 |
| Panel *c*. Dependent variable: Administrative efficiency | | | | | | | | |
| Center | -2.7*** | -2.7*** | -2.5*** | -2.7*** | -2.6*** | -1.9*** | -2.7*** | -1.9*** |
| <40 km | (0.6) | (0.7) | (0.7) | (0.7) | (0.6) | (0.5) | (0.6) | (0.6) |
| $R^2$ | 0.22 | 0.25 | 0.23 | 0.26 | 0.24 | 0.39 | 0.22 | 0.43 |
| Center | -3.0*** | -2.9*** | -3.1*** | -3.0*** | -2.8*** | -2.2*** | -3.0*** | -1.8** |
| <25 km | (0.9) | (0.9) | (0.8) | (0.9) | (0.9) | (0.8) | (0.9) | (0.8) |
| $R^2$ | 0.17 | 0.20 | 0.21 | 0.18 | 0.21 | 0.36 | 0.17 | 0.51 |

*Note:* Estimates from regressions on fully interacted quadratic polynomials in Euclidean distance from the municipality's townhall to the new border. "Center" equals 1 if the municipality is in *Center*, 0 if it is in *South*. Other coefficients not shown. First lines of coefficients in each panel consider municipalities within 40 km of the border (136 observations); second lines reduce this distance to 25 km (74 observations). The "All" column contains all previous controls plus a control for overall migration flows which is better specified in the next subsection. Robust st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

hand, and emigration on the other, thus emphasizing the magnitude of the migration phenomenon (the physical replacement of local people), as opposed to just looking at potentially misleading net flows.

We compute inflow and outflow rates (as equal to the number of immigrants and that of emigrants, each divided by the number of local residents) and add their averages per municipality as direct controls in our regressions. As a first approximation, averages are computed over the longest series available, that is from 1931 to 2012 for municipalities in the Caserta province, and from 1958 to 2012 for all other municipalities. Table 2.9 shows that the effects of being on one side of the new border are robust to the inclusion of these flows, sometimes reinforced in significance or magnitude.

As a refinement, we break up the migration series first according to a chronological criterion, and then according to whether flows were national or international. In order to capture periods of different migration behavior, for each municipality and flow we compute a distinct average for each of three periods: one from 1931 to 1970, when Italy was still a net sender of workforce abroad and inflows mainly consisted of homecomings of previous emigrants; a second period from 1971 to 1986, which we consider a neutral time in which Italy was neither an absorber nor a releaser of workforce; and the last one from 1987 to 2012, when immigration of foreigners became a sizeable phenomenon.[18] As a second refinement, municipal inflows were split according to whether they were from other Italian municipalities or from elsewhere, and the same was correspondingly done for outflows. For the major outcomes of total crime rate and referenda turnout in the 40 km bands aside the new border, Table 2.9 compares baseline estimates to results for the full set of regressions in which migration is controlled for. Again, one can immediately notice that controlling for averages in migration flows throughout all variables cuts does not alter the pattern of discontinuities found earlier.[19] Next we turn to selective migration, or sorting, which could have favored our finding of cultural divergence between areas given that it is based on people's preference to move in places where they can share similar tastes/habits with local residents: brutally speaking, "good" people could have moved in places (inside or outside our region of study) where they were expecting to find other good people, and similarly for bad people.

Had intermunicipality migration flows been available, we could have derived a precise map of the migration flows involving municipalities in our Center and South areas. Intermunicipal migrations just across the border, or to any other Italian province, could have been directly controlled for in the regressions, providing border effect estimates for the limit case in which all migration per area had been selective. However, migration flows only start being available at the interprovincial level, allowing a loose assessment, i.e.

18. The starting date of this phenomenon is controversial and we set it interpolating across different sources and assessments (including charity organizations' ones).

19. It is also noticeable that in general inflows are positively correlated with crimes, while outflows are negatively correlated, in particular for overall flows, for the 1931-1970 period and for domestic flows.

TABLE 2.9: Controlling for migration

| | Baseline | | Overall flows | | Period flows | | Origin/Destination | |
|---|---|---|---|---|---|---|---|---|
| | Crime | Refer. | Crime | Refer. | Crime | Refer. | Crime | Refer. |
| Center | -196.0** | 0.15*** | -216.7*** | 0.15*** | -188.0** | 0.13*** | -214.4*** | 0.15*** |
| | (98.2) | (0.03) | (78.1) | (0.03) | (89.0) | (0.03) | (77.9) | (0.03) |
| Inflow | | | 11,400*** | -2.88** | | | | |
| | | | (1,858) | (1.24) | | | | |
| Outflow | | | -8,626*** | 2.47 | | | | |
| | | | (2,447) | (1.83) | | | | |
| Inflow 1931-1970 | | | | | 4,414*** | -1.18 | | |
| | | | | | (1,601) | (1.04) | | |
| Outflow 1931-1970 | | | | | -4,097*** | 2.15** | | |
| | | | | | (843) | (0.89) | | |
| Inflow 1971-1986 | | | | | 2,026 | 1.54 | | |
| | | | | | (1,604) | (0.97) | | |
| Outflow 1971-1986 | | | | | -1,626 | 1.55 | | |
| | | | | | (1,723) | (1.28) | | |
| Inflow 1987-2012 | | | | | 2,978** | -1.96 | | |
| | | | | | (1,409) | (1.65) | | |
| Outflow 1987-2012 | | | | | 43 | -3.99** | | |
| | | | | | (2,267) | (1.99) | | |
| Domestic Inflow | | | | | | | 12,621*** | -1.19 |
| | | | | | | | (2,210) | (1.64) |
| Domestic Outflow | | | | | | | -9,231*** | 0.62 |
| | | | | | | | (2,885) | (2.12) |
| Inflow from abr. | | | | | | | 1,567 | -9.50** |
| | | | | | | | (3,054) | (3.92) |
| Outflow abroad | | | | | | | -8,372** | 7.57* |
| | | | | | | | (3,481) | (3.97) |
| $R^2$ | 0.13 | 0.58 | 0.46 | 0.60 | 0.46 | 0.65 | 0.50 | 0.61 |

*Note:* Estimated coefficients from regressions on fully interacted quadratic polynomials in Euclidean distance from the municipality's townhall to the new border (40 km bands; 136 obs.). "Center" equals 1 if the municipality is in *Center*, 0 if it is in *South*. Coefficients on algebraic distance and constant not shown. The flow variables are average rates over the period 1931-2012, unless a different period is indicated. Robust st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

TABLE 2.10: Interprovincial migration flows in 1955-2005

| | Provinces of destination | | | |
|---|---|---|---|---|
| Provinces of origin | Caserta | Frosinone + Latina | Northern Italy provinces | Italian provinces |
| Caserta | - | 1 | 7 | 14 |
| Frosinone + Latina | 1 | - | 8 | 12 |

*Note:* Emigration per 1,000 residents of the province of origin, average over years 1955-2005. Northern Italy provinces exclude Frosinone and Latina (Caserta is not in Northern Italy). Italian provinces include all provinces in Italy but the one(s) of origin.

just for the whole provinces interested, of the potential magnitude of selective migration.

In particular, interprovincial flows occurred between our Center and South areas and between each of these areas and other provinces identify a broad upper bound for the selective migration effect. We found two series for such flows: the first one spans years from 1928 to 1938. The earliest series covers the period in which many workers from other provinces immigrated to reclaim and colonize lands, creating Latina and other municipalities in its surroundings, all of them either excluded from our sample or in our North area. Therefore, the bulk of these early years large inflows did not concern municipalities in the Center and South areas (i.e. in the southern part of today's provinces of Latina and Frosinone, and in the northern part of Caserta, respectively), which were little affected. How little? The facts that Latina territories belonged to Rome until 1935, and that Caserta had been merged into Naples with the reform, are another obstacle to a neat number, but we calculate as very limited the impact on people replacement and therefore on possible sorting, and in any case not higher than the one that we are about to see for post-WWII data.

A second series spans years 1955 to 2005, when the relevant phenomenon was emigration towards, rather than immigration from, Northern Italy. What we find is that, in the period average, 8 persons every 1,000 inhabitants of the province of Caserta migrated yearly to provinces more in the north, 1 of which to Frosinone and Latina. Looking at outflows from these latter two provinces, 8 persons every 1,000 migrated to northern provinces, and 4 persons to southern ones, 1 of which to Caserta (see Table 2.10). Even though these figures - and the smaller ones from the 1928-1938 period - are not negligible once cumulated over decades, they can hardly make a difference in explaining cultural heterogeneity across the new border.

TABLE 2.11: RD across placebo borders

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | crimes | | | | refer. | Eur. el. | nonprof. | admin. |
| | total | *violent* | *other* | *"mafia"* | turnout | turnout | empl. | ineffic. |
| South-placebo | -41.6 | -16.4 | -25.2 | 0.3 | -0.04 | 0.10 | 0.8 | -0.4 |
| | (62.0) | (10.4) | (53.9) | (1.0) | (0.02) | (0.12) | (0.8) | (1.1) |
| $R^2$ | 0.15 | 0.31 | 0.12 | 0.15 | 0.10 | 0.14 | 0.02 | 0.12 |
| Center-placebo | -7.9 | 0.3 | -8.2 | 0.7 | 0.03 | -0.07 | -8.6 | -0.6 |
| | (48.6) | (6.9) | (45.9) | (0.8) | (0.06) | (0.07) | (6.6) | (1.0) |
| $R^2$ | 0.07 | 0.03 | 0.08 | 0.08 | 0.05 | 0.06 | 0.24 | 0.06 |
| North-placebo | 14.3 | 7.9 | 6.4 | -0.2 | -0.03 | -0.10*** | -9.0 | -0.0 |
| | (50.0) | (10.1) | (46.3) | (0.8) | (0.07) | (0.03) | (8.6) | (0.8) |
| $R^2$ | 0.23 | 0.08 | 0.26 | 0.11 | 0.27 | 0.21 | 0.13 | 0.04 |

*Note:* Coefficients on placebo area dummies from regressions on fully interacted quadratic polynomials in Euclidean distance from the municipality's townhall to each placebo border (20 km bands; 70, 66 and 61 obs. respectively for South, Center and North placebo areas). Each placebo dummy equals 1 if the municipality is above the placebo border, 0 if it is below. Constant and distance coefficients not shown. The North-placebo sample excludes Latina, Sabaudia and Pontinia, and includes a dummy for each of the three pre-existing municipalities interested by marsh reclamations. Robust st. err. are in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

## 2.7 Robustness: Placebos treatments

In this section, we briefly test whether, in our sample, a discontinuity occurs only on the new administrative border created with the reform, and not elsewhere too. First, recall that no discontinuity on contemporary outcomes is found on the old border (differently from the pre-reform period). Adding to that, we make up a number of placebo borders across which we evaluate border salience. We consider three imaginary borders drawn in the middle of each of the three areas that we identified, i.e. 20 km from either the new or the old borders. Results for observations in each area, i.e. included in 20 km bands, are shown in Table 2.11: they make evident that moving away from the new border washes away any discontinuity, supporting the validity of our findings.

## 2.8 Determinants of cultural dynamics: some hints

The quest for the determinants of culture and social capital dynamics is still an open field. Some contributions have looked in various forms at institutional ones (e.g., Jacob and Tyrell 2010; Nunn and Wantchekon 2011; Barone and De Blasio 2013; Becker et al. 2015; the survey by Alesina and Giuliano 2015). Another group of papers (see, in particular, Glaeser et al. 2002, but also DiPasquale and Glaeser 1999, and Glaeser and Redlick 2009) have focused on the economic side, seeing social capital as any other form of capital, subject to accumulation laws determined by individual purposeful actions. It may be objected that this kind of capital has its own peculiarities, and that culture

may change also as an unintentional externality effect. At any rate, wherever they come from,[20] economic determinants of social capital open the way to a circularity often overlooked in the literature dedicated to the impact of culture on economic variables. As Tabellini (2010, 678) stresses, "the key difficulty in estimating a causal effect of culture is that it is endogenous to economic development".[21] Some empirical contributions have singled effects out by resorting to large unanticipated economic shocks or fully exogenous circumstances, like discoveries of mineral resources (e.g., the Gold Rush exercise by Couttenier and Sangnier 2015),[22] or climatological differences (e.g., Buggle and Durante 2017).

Having these previous analyses in mind, we ask ourselves what could have triggered or be key to the (heterogeneous) cultural transformation that our experiment has demonstrated. In particular, we wonder whether there was any role for other institutional or economic factors in shaping cultural evolution. Section 2.6 has already highlighted how economic driving forces, embedded in the job and network relations of a local labor market, might have favored cultural homogenization across the border. Differentiation at the border must have derived from other, prevailing forces, possibly in the same domain. To try to figure them out, it seems worth to explore the role played by the major institutional and economic innovations which impinged on our areas, and which possibly made a difference.

A sequence of shocks with institutional and economic content did hit the areas with the reform and after. One shock was concomitant, consisting of local innovations in public expenditures associated to the creation of two provincial administrations and the abolition of one: we postpone details of its economic dimensions to Appendix A.

We have already tried, again in Section 2.6, to tackle a second main shock that hit our whole region of study, namely the local funding intervention by the Cassa per il Mezzogiorno, the public institution operative in the whole Southern Italy (slightly extended upward) between the 1950's and the 1980's. At first, it pursued only infrastructuring; later on financial support to entrepreneurial activity too. Results based on the data available to us showed that the Cassa expenditure, balanced across the two sides of the border, only had a limited impact on cultural divergence. Better data might however be more informative and improve this result.

A third, relevant shock was the institution of regional administrations in Italy, starting in 1970. With them, political decentralization brought to a local level the government of many functions that used to be a State prerogative. Regions' weight in public services functions has been ever-increasing since then, up to the new century, which makes it

20. See Miguel et al. (2006), Barone and Mocetti (2016), and Gould and Hijzen (2016), for alternative channels from economics to social capital dynamics.

21. Doepke and Zilibotti (2014) outline a theoretical model of the two-way effect between economic growth and culture.

22. Along these same lines, mines are used as instruments in Buonanno et al. (2015), while rainfall shocks in Barone and Narciso (2013).

the most recent shock among those listed. Previous sections have shown that, before the reform, administrative efficiency was not worse in *South* than in the rest of the area under scrutiny, but the contemporary cultural discontinuity occurs exactly at the Latium-Campania regional jurisdiction limit, i.e. at the *Center-South* border. There follow hints to a sizeable, asymmetric impact of the newly-created institution of Regions for the detected cultural dynamics, but, given evidence on past data, the heterogeneity of such impact between Latium and Campania occurred for reasons unrelated to previous individual or institutional culture. Also, behaviors under scrutiny are not in the domain of those which the administrative activity of a Region may directly affect. One possible source of this differential impact of Regions might have been an indirect one, namely the adverse evolution of the levels of local services provided in Campania, and the effects on social cohesion that this might have entailed. This is definitely a direction worth exploring further.[23] Incidentally, but importantly, notice that under this hypothesis such a role would have been exerted *for any extension* that Latium and Campania had had at the time of the regional decentralization, with no perturbation impact of the border shift by itself, confirming its usefulness *only* as a device for identifying the full pattern of cultural change.

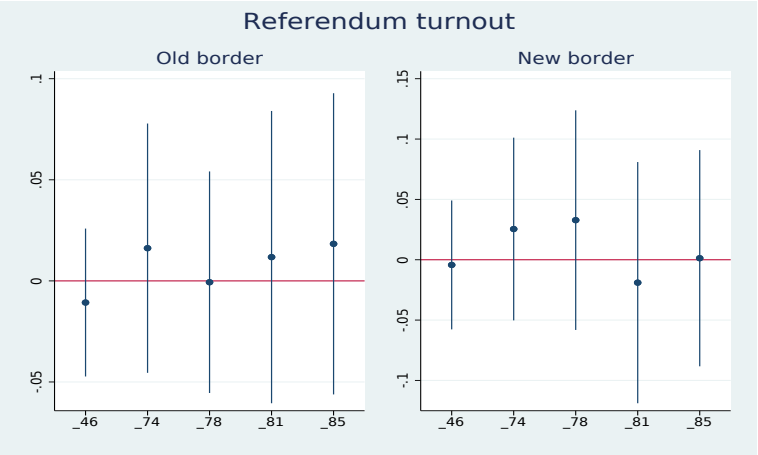
This list does not exhaust all possible events which might have exerted an impact on local cultures. Moreover even if one were able to tell that one shock had an impact in moving one culture, it would be hard to tell the lag at which it did. Some additional data could give some hint, in particular those covering the intermediate period between the reform and the last part of the century. Unfortunately, data spanning that period are very hard to find at the disaggregated level needed for our analysis: we actually found just one variable, referenda turnout, available for years 1946, 1974, 1978, 1981 and 1985. For each border, Figure 2.7 shows the time sequence of estimates and 95% confidence intervals of baseline RD coefficients of border dummies for this variable. It shows that, at least in this dimension, until 1985 the discontinuity between Latium and Campania had not emerged yet (while areas joined in the enlarged Latium had a similar behavior). We interpret this timing and the fact that the cultural discontinuity today occurs on the regional border as supporting the hypothesis of greater (and increasing) relevance of the Regions inception and phasing-in shock for the observed cultural dynamics.

## 2.9 Conclusions

By exploiting an extensive data mining on modern and old local data in a repeated spatial discontinuity exercise on individual and institutional behaviors, this paper has documented a relative change in culture occurred - over a quite short and recent span

23. For contributions on how local administrative behaviors are related to social capital dynamics, see Barone and Mocetti (2011), and Camussi and Mancini (2016). Ongoing work by Grompone et al. (2018) looks at the correlation of local public services provision and culture across the Latium-Campania border.

FIGURE 2.7: Baseline RD estimates of border dummies for referenda turnout




of time - in Southern Italy, a region usually taken as a paradigmatic example of persistence of (bad) culture. In particular, it has documented a sort of "reversal of cultural fortunes", both at an individual and at an institutional level, between Campania (the upper region in Southern Italy) and Latium (its bordering region northwards), unfavorable to Campania. This reversal would by itself imply the need to reconsider the role of historical legacy for current cultural performance, as its low level in Campania, if not in Southern Italy, can no longer be easily dismissed as just dependent on previous institutions or culture.

In addition, using the "highlighting perturbation" of an exogenous downward shift of the (quite neutral) administrative border between the two regions, the paper has shown that territories sharing the same legacy of today's Campania, but joined to upper provincial and regional administrations in the first half of the 20th century, became culturally assimilated to those other territories, at the relatively higher performance level that these reached in the following decades. This cultural transition would seem to reflect standard contagion or institutional channels at work, if it were not for the fact that in the eve of the administrative reform both individual and institutional behaviors in the adopting provinces were not better (mostly, they were worse) than in the adopted areas or in the part of Campania that remained Campania. Moreover, the direct impact of the administrative border shift should be minimal if not zero, given that, even taking into account Italy's decentralization with the introduction of Regions in the 1970s, most of the behaviors that we scrutinized (e.g., crimes, referenda turnouts, judicial efficiency) are immune from subgovernmental influence, being instead ruled by equal national laws and governed by centralized entities regarding all individuals living in the national territory. Altogether, we are prompted with the need to search for new determinants of cultural change, given the difficulty in explaining the described decoupling and reconfiguration of cultural traits with the consensus channels of historical legacy, contagion, or institution.

Nevertheless, in modern data a cultural discontinuity emerges (as an absolute novelty)

*exactly at* the new regional border, unfavorable to Campania both at an individual and at an administrative level. This would hint to an alternative channel involving institutions: in particular, the newly created Regions might have exerted a differential effect across the border, despite their equal and apparently limited role for the kind of behaviors for which the discontinuity is found. These heterogenous dynamics across the border could *not* stem from a corresponding heterogeneity in ancient cultural and institutional legacies as claimed from Putnam (1993) on, given that our results show that locally no such heterogeneity pre-existed; neither they could stem from a difference in previous local environments as in the possibilities described by Bisin and Verdier (2017), also in light of covariates balancing across our border. It seems instead that the different cultural evolution in Campania, detected also at an individual level, must be connected with a differential effect of the Region institution itself.

Given our prevailing results of continuity and southwards advantage in previous institutional efficiency across the current regional border, a hindering role of a bad Campania administration in the last decades might have only stemmed from a recent adverse endogenous evolution of culture, more than from persistence of bad governance and administrative shortfalls. As the discontinuity did not show up until the turn of the century, it could reflect endogenous dynamics related to the creation of Regions itself, in turn possibly connected with some other underlying factor, giving rise to a differential change in the quality of local governance.

Our findings imply the need for a quest for determinants of the described changes in culture, with a reconsideration both of the weight of history in shaping cultural processes and of the impact and ensuing developments of the creation of the subgovernmental level of Regions in Italy. More in general, the current paper implies the need for more work on the determinants of cultural dynamics, and this sets the direction for future research.

# Chapter 3

# Long-term unemployment and subsidies for permanent employment

## 3.1 Introduction

Long-term unemployment (LTU) is one of the main legacies of the Great Recession. Since 2008, many developed countries have experienced a steep rise in LTU rate, which was particularly striking in European countries (Figure 3.1).[1] In 2018, even after the rate fell by two percentage points from its peak, more than 7 million individuals living in the European Union were long-term unemployed, two fifths of the total number of unemployed workers. Even in the US almost one million individuals were still in this condition.

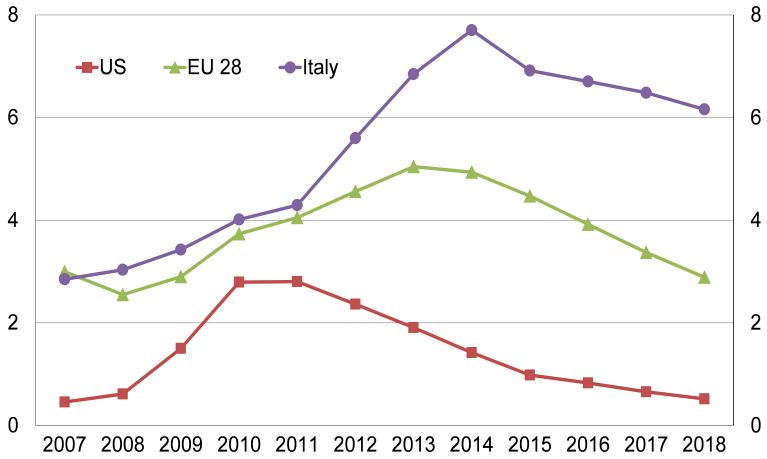
The reasons why LTU is a policy concern are multiple. Firstly, unemployment is one of the most significant causes of households' poverty. The probability to find a job tends to decrease with time spent in unemployment, because both workers' human capital and the intensity of job search may decline over time. LTU might therefore increase poverty persistence. Secondly, since workers detached from the labour market do not compete for jobs, long-term unemployed jobseekers play only a reduced role in compressing wages and, thus, in decreasing the total amount of unemployment (Machin and Manning 1999).[2]

These concerns about the consequences of LTU motivated a number of different policies across developed countries. In particular, many countries addressed the problem by using active labour market policies (ALMPs), whose effects are typically more positive

1. In the European Union the LTU rate reached 5.0 percent in 2013, almost doubling compared to 2008. The LTU rate is defined as the share of active individuals who are out of work and have been actively seeking employment for at least one year over the labour force.

2. More generally, Austin et al. (2018) discuss "three types of externalities associated with non-employment: pure fiscal losses from reduced taxes and increased social spending; social spillovers born by family and friends; and not working spillovers where one individual not working increases the chance that other individuals don't work", because of decreased demand for local products, which reduces local labor demand, reduced stigma of not working, or if the not working enjoy being with each other.

FIGURE 3.1: Long-term unemployment rate




*Source:* OECD. Unemployed for more than one year as percentage of labor force.

in the attempt to combat LTU than short-term unemployment (Bentolila and Jansen 2016).

At the same time, the number of scientific evaluations of these programs has exploded. In particular, Card et al. (2018) undertake a review of the recent literature, by assembling a sample of 207 evaluation studies that provide 857 separate estimates of program effectiveness, and Card et al. (2016) repeat the analysis from the particular viewpoint of long-term unemployed jobseekers. Among ALMPs, the authors show that larger gains have been observed for programs that emphasize human capital accumulation. They include training provision and promotion of search effort (through job search requirements, sanctions and even direct help).[3] Other policies tried instead to encourage firms to hire long-term unemployed through subsidies. The evidence on their effectiveness is mixed: the share of programs that had a significant positive effect (less than 60 percent) is lower than for other ALMPs. One reason why subsidies are less effective is the risk that hiring would have taken place even without any public intervention. In this case subsidies may lead only to a substitution among workers and over time. Thus, they may have detrimental effects on people who are not targeted, as they face stronger job competition from those who are (Crépon et al. 2013).

This paper aims at studying the effectiveness of a subsidy introduced in Italy by law no. 407 of Dec. 29th, 1990 and in force until the end of 2014. The program targeted only firms that hired with a permanent contract a worker who had been unemployed for

3. As far as training programs are concerned, even if in the short term they are poorly effective (Heckman et al. 1999), in the long term their effects seem to be positive and significant. Job search assistance and search requirements typically have an even greater beneficial impact, especially in the short term: these programs often lead to a reduction in the unemployment spell even if only some workers are being employed in a new job (Card et al. 2016, Manning 2009 and Petrongolo 2009 on UK).

at least 24 months or covered by the national short-time work compensation scheme.[4] Its amount was greater for firms in Southern Italy (100 percent of social security contributions for 3 years; only 50 percent for firms in other Italian regions). This differential treatment in favor of Southern regions and long-term unemployed was abrogated in 2015 by the Financial Stability Law, which introduced exemption from social security contributions without distinctions between areas and duration of unemployment (although individuals with a permanent contract in the previous six months were excluded).[5]

This is an interesting case study for a number of reasons. First, Italy is one of the European countries where the LTU rate increased the most during the recession (5 percentage points from 2007 to 2014), and even more in Southern regions (8 percentage points), where LTU rate has steadily doubled the national one. Second, participation rate is particularly low, especially in the South, where the gap with the national average increased from 6 percentage points in the '90s to 11 percentage points during the last decade; thus, the long-term unemployed may be more at risk of leaving the labour force.[6] Third, this program was a relatively big one. Italy's public expenditure for recruitment incentives has been equal to 0.2 per cent of GDP in the period 2004-2015, twice the OECD average, representing 36 per cent of total public expenditure for ALMPs.[7] In 2014 the program involved 260,000 hires in the South, 37,000 in Center-North. Finally, this policy was particularly ambitious for its focus on permanent contracts. Being these contracts generally more expensive for firms, not only in terms of social security contributions, but also because of the stronger employment protection legislation (Grassi 2009), employers may find it riskier to hire people detached from the labour market with a permanent contract.

We use a sample of administrative micro-data about job flows (*Campione Integrato delle Comunicazioni Obbligatorie*, CICO) and select unemployed individuals that lost their job between 2009 and 2013, for whom we can observe the labour market history until the end of 2015. To achieve identification, instead of using simple diff-in-diff estimators (for instance comparing eligible vs non eligible individuals across areas), we employ a triple difference estimator (DDD) that exploits variation in the relative cost of hiring

4. Cassa integrazione guadagni (CIG).

5. On the effects of the 2015 hiring subsidy look at Sestito and Viviano (2018)).

6. Austin et al. (2018) identify spatially targeted employment subsidy as the most effective place-based policy if target areas are those with the highest elasticity of employment to wages. They provide evidence that such areas are those where non-employment is higher. Moreover, distressed areas are characterized by lower prices, which provide additional support to spending more in places where costs are lower, and lower macroeconomic costs of supporting not working, since inflationary pressure due to reduced unemployment is more limited compared to full employment areas. Finally, they find that redistribution across space is more likely to enhance welfare when migration is lower. However, they admit that "high not working rate areas might have social problems that lead even fewer people to be on the margin of working", displaying "extremely inelastic labor demand, so that few new jobs will be created because of a subsidy". Therefore, the effectiveness of this policy is, ultimately, an empirical question.

7. The corresponding figure for the OECD countries is 14 per cent.

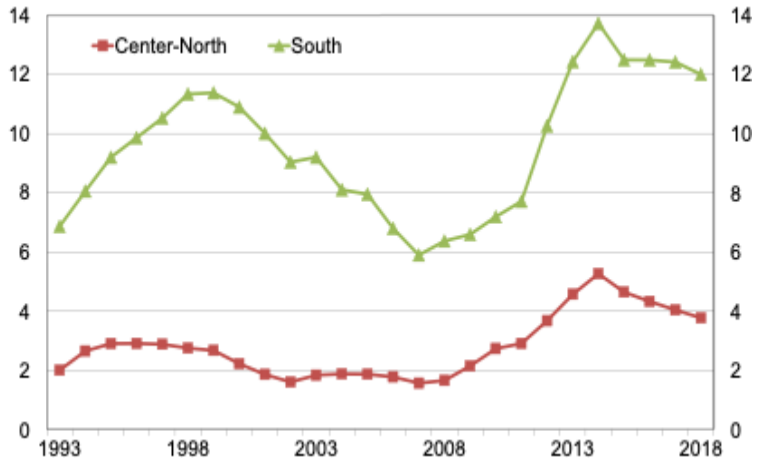
with a permanent contract across time, regions and worker's unemployment length. This choice suits the design of the subsidy and its recent history: law 407/90 granted a preferential treatment to unemployed for at least two years in the South compared to those living in the North, until this preferential treatment ended abruptly with the abrogation of the law at the end of 2014. Therefore, it seems natural to compare how this advantage along two dimensions (eligible vs. non eligible and South vs. North) evolved over the third dimension (time). Intuitively, the DDD approach exploits these three dimensions to remove (i) underlying differences between eligible and non-eligible unemployed; (ii) area-specific time trends and (iii) differential time-trends for the eligible. We implicitly assume that, without any targeted subsidy, the choice to hire with a permanent contract an eligible rather than non-eligible unemployed would have changed in 2015 in the same way in Northern and Southern regions.

A challenge that we face is that our DDD strategy is still only able to capture the differential effect on the eligible versus non eligible unemployed. The positive effect on the former might come at the expense of the latter, for instance if the unemployed will be pushed to wait until reaching the eligibility status in order to benefit from the policy. The estimate is also affected by possible issues of substitution over time (where firms anticipate the end of subsidies), across different types of contracts (where individuals have an advantage in avoiding short-term contracts that would end their eligibility status) and across areas (where individuals move to exploit the preferential treatment). Through a series of robustness checks we provide evidence that our results are not driven by these issues.

Pasquini et al. (2018) also provide an evaluation of law 407/90 subsidies using CICO data and find a positive effect on the probability of getting a job. They use a Regression Discontinuity Design (RDD), looking at unemployed workers in a bandwidth of two weeks around the 24 months threshold to become eligible for the subsidy. Since individuals close to the threshold have a strong incentive to wait, we believe this approach may be flawed by the effect of intertemporal substitution, inducing an upward bias in the estimates. Moreover, their result is not comparable with our own, since their outcome variable is the share of unemployed jobseekers who find any kind of job, while we look at the probability of being employed with a permanent contract, as the subsidy only benefitted this type of contract.

We find that, after the abrogation of law 407/90, eligible individuals in Southern Italy experienced a fall in their probability of finding a permanent job relative to non-eligible unemployed in Center-North. This implies that the targeted subsidy, in place until the end of 2014, was effective in rising their chances in the labor market. We estimate a 41 per cent higher probability of finding a permanent job in the subsequent week and argue that the effect is not driven by substitution over time or across areas, type of contract or category of jobseekers. Moreover, we find that the benefits deriving from jobs created thanks to the policy, measured by tax revenue and social security contribution paid by

FIGURE 3.2: Long-term unemployment rate across areas




*Source:* Istat. Unemployed for more than one year as percentage of labor force.

the employees, outweigh the costs of the policy, given by the amount of the subsidy.

The rest of the paper is organized as follows. In Sections 3.2 and 3.3 we describe the subsidy and our dataset, respectively. Section 3.4 describes aggregate trends to highlight differences across regions, time and eligibility status. In Section 3.5 we describe the empirical strategy adopted, define our treatment and control groups, and present the results. Section 3.6 provides evidence that our results are not driven by substitution effects over time and across workers by performing robustness checks and reporting results on additional outcomes. In Section 3.7, we perform a cost-benefit analysis. Section 3.8 concludes.

## 3.2 The subsidy granted by law 407/90

Until 2014, permanent hires of individuals unemployed or covered by the national short-time work compensation scheme for at least 24 months benefited from a subsidy granted by law 407/90. The subsidy was equal to 100 per cent of social security contributions for three years in the South, 50 per cent in the rest of Italy. The favorable treatment of Southern regions aimed at increasing LTU employability especially where this was more needed. Indeed, the incidence of LTU over the labour force is strongly higher in the South than in Centre-North (see Figure 3.2). From 2008 to 2014 both areas experienced a sharp increase in LTU rate, but the former has been stably three times the latter.

Only firms that had not experienced any firings, workers' suspensions, voluntary resignation or the end of a temporary contract in the previous six months were eligible for the policy. Despite the fact that in June 2012 the rules defining firm's eligibility were relaxed, the usability of this subsidy remained quite low. Accordingly, the take-up was especially low in Centre-North, where the gain from the subsidy was smaller. On

average, almost 300,000 hires per year benefited from the subsidy from 2012 to 2014; almost 90 percent of them in Southern regions.[8]

The Financial stability law for 2015 abolished law 407/90 and introduced a new non-targeted and non-conditional subsidy (with a cap to 8,060 euro per year for three years) to all permanent hires signed from January to December 2015. With respect to previous policies, a more extensive audience could take advantage of the subsidy: the only constraint to workers' eligibility was not having been employed with a permanent contract in the six months before the new hire and not having worked with a permanent contract for the same firm asking the subsidy in the three months before the law was passed (October-December 2014). In 2015, 630,000 hires benefited from the subsidy; about 30 per cent of them in Southern regions.

Because of the abolition of law 407/90, the relative cost of hiring with a permanent contract an individual unemployed for at least 24 months increased both in Southern and in Northern regions. Moreover, individuals who were not long-term unemployed and did not have a permanent contract in the previous six months experienced a strong cut in social security contributions, as they were previously not eligible for similar allowance. Furthermore, the new 2015 subsidy was introduced along with a broader labour market reform package, the Jobs Act, which was passed at the end of 2014. In particular, this reform introduced a cut in firing costs for all new permanent contracts signed from March 7th, 2015 onward, and a new insurance scheme against the risk of unemployment covering a broader set of workers (Nuova Assicurazione Sociale per l'Impiego, NASpI[9]). Since these policies didn't affect contemporaneously the relative outcomes of eligible and non-eligible in the same area and year as the treatment, we believe our identifying assumption still holds.

In Table 3.1 we compute the amount of the three-year exemption for a gross annual wage of 26,000 euro in the two policy regimes. Hiring trends between 2014 and 2015 were strongly affected by these interventions. Sestito and Viviano (2018) provide evidence that both the new subsidy and the reduction in firing costs had a significant positive impact on gross permanent hires. However, the novelties introduced in 2015 applied to both our treatment and control groups (in the way we defined them in Section 3.3) homogeneously across the country. Therefore, if law 407/90 had never been present, the two groups would have been affected similarly by these job market interventions across different geographic areas, and this justifies the assumption of common relative trends that is behind our estimator (see Subsection 3.5.1).

8. Source: INPS, Statistiche in breve, Politiche Occupazionali e del Lavoro.

9. Moreover, the duration of NASpI, in force since May 1st, 2015, is equal to half the period for which the worker paid social security contributions in the previous four years. Instead, previous unemployment insurance scheme related duration with age, with a minimum of eight months for under 50 and a maximum of 16 months for over 54.

TABLE 3.1: Cumulated three-year exemption from social security contributions for permanent hires

| Area | Unemployment duration | 2014 (1) | 2015 (2) | Difference (2015-2014) |
|---|---|---|---|---|
| South | Short | 0 | 18.72 | 18.72 |
| | Long | 20.274 | 18.72 | -1.554 |
| | Difference (L-S) | 20.274 | 0 | -20.274 |
| Centre-North | Short | 0 | 18.72 | 18.72 |
| | Long | 10.088 | 18.72 | 8.632 |
| | Difference (L-S) | 10.088 | 0 | -10.088 |

*Note:* We refer to a gross yearly wage of 26,000 euro, a social security contribution rate for the employer equal to 24 per cent of the gross wage, an INAIL premium equal to 485 euro for Centre-North and 518 euro for the South. – (1) The exemption granted by law 407/90 concerned permanent hires of unemployed individuals and those covered by the short-time work compensation scheme for at least 24 months. – (2) The exemption granted by the Financial Stability Law for 2015 concerned all permanent hires, from January to December 2015, of individuals who did not have a permanent contract in the previous 6 months.

## 3.3 Data

We use a sample of administrative micro-data about the so-called Comunicazioni Obbligatorie, which contains information concerning job positions. Starting from 2009, whenever an employment contract is signed, terminated or changed, employers must electronically submit this information to the Regional agency in charge of active labor market policies, which forwards it to the Ministry of Labor. The administrative archive built on these communications contains, therefore, information on all contracts that were signed, terminated or changed starting from 2009.[10] The Ministry releases a sample of micro-data relative to all workers born on 24 dates (the 1st and 15th day of each month).[11] In this work we use the December 2015 release.

Starting from this dataset we build a weekly panel, by recording job status (unemployed, employed with a fixed-term contract or a permanent contract) for each worker in every Monday between January 2009 and December 2015.[12] Even if our empirical

10. For contracts that were signed before then but were changed or terminated after January 1st, 2009, employers had to submit the entire job history and therefore they are fully included in the archive. On the contrary, the archive does not contain any information on contracts signed before 2009 that were neither changed nor terminated thereafter.

11. Every record contains the following information: employer and employee anonymized identifiers, dates in which the position is created and destroyed, employee's year of birth, gender, region of birth, nationality, schooling, region of residence, region of work, sector of activity, job contract type, full- or part-time status, role, any hiring subsidy granted, reason of job destruction and, for a subsample, wage.

12. Setting up a panel at daily frequency would lead to a hardly manageable large dataset, without bringing significant gains. In fact, we are interested in identifying transitions from unemployment to permanent employment in 2014 and 2015. Therefore, observations at weekly frequency are a sufficiently

analysis focuses on years 2014-2015, we use workers' job history since 2009 in order to determine, for each individual, a starting point of her unemployment spell (see Appendix B for more details). We follow each worker during her entire job history until age 64. We focus only on the effect of the measure on the non-employed, and not also on beneficiaries of the short-time work compensation scheme, since we are not able to identify this latter group.

The definition of unemployment status relevant for law 407/90 differs from the one of the International Labour Organization (ILO), commonly adopted in Labour Force Surveys, where people aged 15 and over are classified as unemployed if they are without work, are available to start working within two weeks and sought employment at some time during the previous four weeks. Furthermore, ILO calculates unemployment duration since the loss of the last job.

On the contrary, according to law 407/90 definition, the unemployment status does not require any frequent job search action. Looking at the rules applying in 2013-14, individuals need to be registered as unemployed in a Job Centre (*Centri per l'impiego*), formally declare to be willing to work and, in principle, accept adequate job offers. Furthermore, the unemployment duration is not set back to zero, but just suspended, during short periods of employment. The time limit necessary to consider a period as "short" changed repeatedly during time and across areas. We chose the one prevalent in our period of analysis, which was six months for the Centre-North, 4 or 8 months for the South according to whether individuals were younger or older than 25 years. The policy also had an additional rule, according to which individuals were still considered unemployed if they got jobs earning less than the no tax area limit (8,000 euro per year). Since for a large fraction of our observations the information for wage is missing, we prefer not to employ it to avoid using a potentially strongly selected sample. In the Appendix B we show that results would be similar if we consider this income-threshold rule or if we use a simplified rule homogeneous across areas.

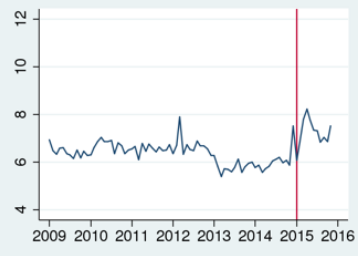
## 3.4 Trends in permanent hires

Figure 3.3 shows the time series of hires with a permanent contract in the two geographic areas. After 2015 hires went up both in the South and the Centre-North, as expected with a new legislation that introduced more generous benefits for most of the new open-ended contracts. Figure 3.3c shows that the rise is relatively larger in the Centre-North. In December 2014, slightly before the change in the system, firms in the South increased the relative number of permanent hires, as shown also by the peak in Figure 3.3a. This

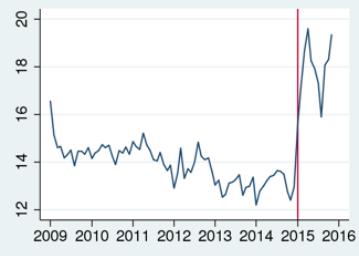
good approximation, since the maximum measurement error is six days. The only moment in which this approximation is problematic is in the week across the two years, because we risk to wrongly attribute to year 2015 transitions occurred in the last days of 2014. To obviate to this measurement error issue, we eliminate observations concerning both the last Monday of 2014 and the first Monday of 2015.

FIGURE 3.3: Permanent hires

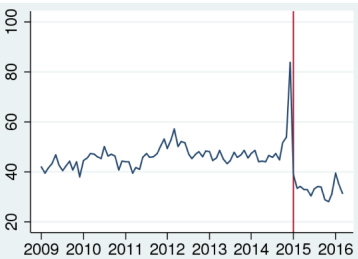
(a) South




(b) Center-North




(c) South over Center-North




*Source:* our elaboration on Ministry of Labor data, *Campione Integrato delle Comunicazioni Obbligatorie* (CICO). Monthly frequency. Thousands in (a) and (b), percentage in (c). Seasonally adjusted series, obtained by subtracting from the raw data the OLS estimate of hires on a set of separate monthly dummies until September 2014, when the Financial Stability Law had not been announced yet.

could be explained by some firms taking advantage of the older benefits for the long term unemployed granted by law 407/90, which were more generous in that area.

To understand whether the change in the relative number of hires between the South and the rest of the country might be due to the disappearance of the preferential treatment for the eligible in the former area, we show two additional pieces of evidence. Figure 3.4a removes from the time series those contracts that actually received the subsidy of law 407/90. Without considering them, there is no strong change in the ratio between the two series in 2015 and the pick of December 2014 disappears. Since it is difficult to draw conclusions from the actual receipt of the subsidy, which strongly reflects the endogenous choice of firms, Figure 3.4b provides the same South/Centre-North comparison but looks only at non employed individuals and splits them according to their non employment duration. Before 2015, relative hires of the eligible in the South, with respect to the rest of the country, were higher than those of non-eligible. This difference disappeared in 2015.

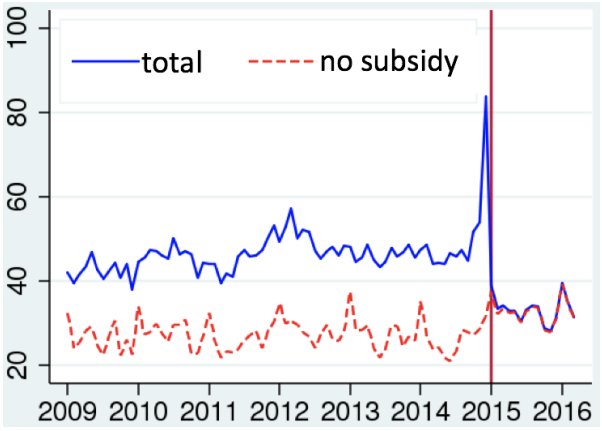
## 3.5 Micro analysis

### 3.5.1 Estimation strategy

As shown by the aggregate trends, law 407/90 seems to have had a positive impact on permanent hires of eligible individuals in Southern regions. In order to provide further evidence, we switch to a micro-level analysis, which also allows us to perform several additional robustness checks to assess the sensitivity of our conclusions. We focus on a panel composed of all unemployed individuals over the weeks between January 2014 and December 2015 and estimate a discrete-time hazard model which predicts the probability to find a permanent job in the subsequent week, conditional on the logarithm

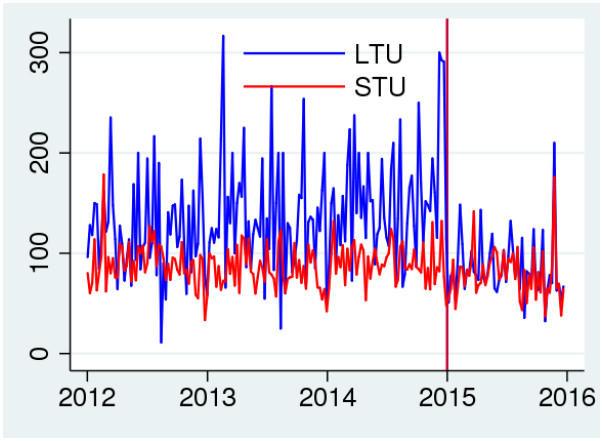
FIGURE 3.4: Permanent hires: South over Center-North

(a) Contracts that did not receive the subsidy




Monthly frequency. Seasonally adjusted series.

(b) Hires of previously non employed




Weekly frequency. LTU (STU) are individuals with non-employment duration of at least (less than) two years.

*Source:* our elaboration on Ministry of Labor data, *Campione Integrato delle Comunicazioni Obbligatorie* (CICO). Percentages.

of unemployment duration.[13] As standard in the literature, we use a logit specification, assuming therefore proportional odds of exiting unemployment in each week. All the comparisons between groups are therefore expressed in odds ratios, although we will also use the predictions to make comparison about the probability of getting a permanent job. In the period of analysis, we are able to exploit three different sources of variation: (i) law 407/90 targeted only individuals with unemployment duration of at least two years; (ii) the subsidy was more generous in Southern regions; (iii) the preferential treatment for long-term unemployed in Southern regions disappeared in 2015, when the new system of generalized hiring subsidies and firing rules was introduced.

The outcome of interest is a dummy $y_{iw}$ equal to 1 if individual $i = 1, \ldots, N$ in week $w = 1, \ldots, W$ finds a permanent job in the subsequent week, 0 if she doesn't. Defining

$$\lambda(y_{iw}) = Pr(y_{iw} = 1 \mid y_{iw-s} = 0, \forall s < w),$$

one could start from a DD that compares the eligible and the non eligible across the two areas when law 407/90 was still in place (year 2014):[14]

$$logit[\lambda(y_{iw}) \mid g, LTU_{iw}] = \gamma_0 + \gamma_1 LTU_{iw} + \gamma_2 \mathbb{1}[g = South] + \gamma_3 LTU_{iw} \mathbb{1}[g = South] + \epsilon_{iw} \tag{3.1}$$

where $g \in \{Centre\text{-}North, South\}$ is the area, $LTU_{iw}$ is a dummy equal to 1 if unemployment duration is longer than two years. The reference group is made of non-eligible individuals[15] in Centre-North regions.

In 3.1 the double comparison of interest is $\exp(\gamma_3)$, which captures the across-area difference in the LTU vs. STU performances. We expect it to be positive because the subsidy was much larger in Southern regions. If we assume that, absent the policy, the difference between the eligible and the non-eligible had been the same in the two areas, then $\exp(\gamma_3)$ would capture the causal effect of interest, i.e. the impact of the more generous subsidy granted to permanent hires in the South.

This assumption is rather strong and might be violated if structural differences between the South and the Centre North affect differently LTU and STU probabilities of being hired with a permanent contract. We therefore opt for a triple difference estimator, exploiting the fact that the differential treatment for Southern LTU workers was abol-

13. We also estimate models where the unemployment duration enters in a more flexible way, like a third order polynomial function and a piecewise constant function, and results are unchanged, as we report in the Appendix B (Tables B.5 and B.6).

14. An alternative would be to focus only on the eligible and compare them across the two areas before and after the abrogation of law 407/90. This strategy would require that changes in macroeconomic conditions and the overall reform of the labor market introduced in 2015 would have affected the two areas in the same way. This assumption is hard to believe, given the strong structural differences between the less developed Southern regions and the rest of the country.

15. From now on we use the abbreviation LTU (STU) to refer to the group of individuals eligible (non-eligible) for the subsidy, whose non-employment duration is at least two years (six months or one year, according to the specification).

ished in 2015. This allows us to use three control groups (STU in each area and LTU in the Centre-North) to control for underlying differences in the LTU vs. STU comparison across areas. With respect to the DD strategy outlined above, the DDD estimator is based on a weaker assumption: it requires that no contemporaneous shock affects the relative outcomes of the treatment group (the eligible) compared to the control group (the non-eligible) in the same area and year as the treatment (Gruber 1994).

The DDD specification is the following:

$$\begin{aligned} logit[\lambda(y_{iw}) \mid t, g, LTU_{iw}] = \beta_0 + \beta_1 LTU_{iw} + \beta_2 \mathbb{1}[t = 2015] + \beta_3 \mathbb{1}[g = South] + \\ \beta_4 LTU_{iw} \mathbb{1}[t = 2015] + \beta_5 LTU_{iw} \mathbb{1}[g = South] + \beta_6 \mathbb{1}[t = 2015] \mathbb{1}[g = South] + \\ \beta_7 LTU_{iw} \mathbb{1}[t = 2015] \mathbb{1}[g = South] + \epsilon_{iw} \end{aligned} \tag{3.2}$$

where $t \in \{2014, 2015\}$ is the year. The coefficient in the triple interaction captures the difference across areas in the trend of relative performance of LTU vs. STU, which is given by A-B, where

$$\begin{aligned} A = \{logit[\lambda(y_{iw}) \mid t = 2015, g = South, LTU_{iw} = 1] - \\ logit[\lambda(y_{iw}) \mid t = 2014, g = South, LTU_{iw} = 1]\} - \\ \{logit[\lambda(y_{iw}) \mid t = 2015, g = South, LTU_{iw} = 0] - \\ logit[\lambda(y_{iw}) \mid t = 2014, g = South, LTU_{iw} = 0]\} \end{aligned} \tag{3.3}$$

and

$$\begin{aligned} B = \{logit[\lambda(y_{iw}) \mid t = 2015, g = CentreNorth, LTU_{iw} = 1] - \\ logit[\lambda(y_{iw}) \mid t = 2014, g = CentreNorth, LTU_{iw} = 1]\} - \\ \{logit[\lambda(y_{iw}) \mid t = 2015, g = CentreNorth, LTU_{iw} = 0] - \\ logit[\lambda(y_{iw}) \mid t = 2014, g = CentreNorth, LTU_{iw} = 0]\} \end{aligned} \tag{3.4}$$

The causal interpretation of this coefficient lies on the assumption that, in the absence of a preferential treatment for the South (i.e. in the absence of law 407/90), the trend in relative performance of LTU vs. STU would have been the same in the two areas (common *relative* trends).[16] Under this assumption, $\beta_7$ is different from zero only if the preferential treatment of LTU in Southern Italy granted by law 407/90 had an impact on the chances of finding a permanent job. If the impact was positive, then $\beta_7$ should turn out to be negative (and $\exp(\beta_7) < 1$), because in 2015 the advantage for LTU workers disappeared.

One crucial issue in the interpretation of the DDD result is that there were two contemporaneous policy changes: the targeted subsidy of law 407/90 was abolished, but

16. Another way to interpret this *relative* trend assumption is that the difference in trends between areas should have been the same for LTU and STU.

a generalized subsidy was introduced, and the employment protection legislation was weakened through the Jobs Act. In the DDD estimates we do not separately identify the effect of the two latter policies (generalized hiring subsidy and Jobs Act), which are captured by $\mathbb{1}[t = 2015]$ and $\mathbb{1}[t = 2015]\mathbb{1}[g = South]$. Nevertheless, given that between 2014 and 2015 the incentives for LTU in the South basically were unchanged (with a small decrease), one might be concerned that $\beta_7$ might actually capture the impact of the generalized subsidy on the STU in the South. This can be seen by reversing the logic followed up to now: STU in the South, relative to LTU living in the same area, experienced an increase in the subsidy equal to 20.274 euro in our simulation reported in the last column of Table 3.1, while the relative increase for STU in the Centre-North was only 10.088 euro. Hence, we might actually be capturing the impact of the new generalized subsidy. The simple DD across areas is helpful to disentangle the two possible interpretations: the results, discussed below, show that there was a relative premium before 2015 for LTU in the South, and this premium disappeared when the old targeted subsidy was abolished.[17]

It is important to stress some other issues related to the interpretation of our results and to which specific population they refer to. Firstly, we focus only on the impact of the policy on eligible individuals, even if they do not actually benefit from the subsidy. Therefore, our parameter of interest can be interpreted as an Intention-To-Treat and not a treatment effect. We believe this is the effect of interest, as the take-up of the subsidy was left to the decision of firms and workers. Since strict firm's eligibility criteria limited the usability of the subsidy, especially in Centre-North, where its amount was smaller, a fraction of eligible individuals did not benefit from the policy. Then, we may underestimate the size of the effect on treated individuals.

Secondly, our estimates do not recover the impact of law 407/90 subsidy on all the eligible, but rather the effect of the more generous subsidy in part of the country. These results are nevertheless interesting to understand whether these policies might have an impact on the more disadvantaged areas, as it is the case of the Italian South.

Thirdly, as discussed in Section 3.3, the eligibility criterion is difficult to measure precisely. This induces measurement error in our estimates, because the control group, i.e. the non-eligible, also includes individuals that, nevertheless, benefited from the policy (see Subsection 3.5.2 for a more elaborate discussion). This measurement error affects also the comparison of trends and not only levels. As we document in Table B.1 of the Appendix B, despite misclassification, there is still a larger fraction of beneficiaries among those that we identify as LTU. Therefore, our estimates should be interpreted as a lower bound.

Finally, the dataset we use allows us to observe only unemployed individuals that have

17. Obviously, this might also imply that the generalized subsidy had an impact on all types of workers, but, as discussed above, we cannot separately identify its effect from other concurrent changes, in particular, the employment protection legislation weakening.

lost a previous job. Those who are searching a first job cannot be observed, as they have not entered any record yet in the administrative system of the *Comunicazioni Obbligatorie*. Our results have, therefore, nothing to say about the impact on the individuals who have never worked before.

In the empirical specification the outcome is an odds ratio. This specification allows to interpret the estimates as the percentage change in the predicted odds ratio due to a unit change in the independent variable regardless of the value of the other variables. From a survival analysis point of view, we treat time in discrete units (weeks), as an approximation of the true daily frequency, and therefore we use a discrete model (Jenkins 2005). We use a logit model, which is more standard in the survival analysis literature, but the main results carry through by using a linear probability model.[18] Although identification does not require other covariates, as standard in the survival analysis literature we also include the logarithm of non-employment duration ($UD_{iw}$) as a control (in the Appendix B we also show that the results are unaffected if we include a polynomial of $UD_{iw}$ to account for non-linearities). This is important because our sample is unbalanced and therefore in different weeks and areas the average unemployment duration might differ. We also include demographic controls, like gender, nationality, education, age, age squared, controls related to the specific labor market, like dummies for 19 major industries of previous employment and incidence of irregular work in each region by macro-sector cell, and monthly dummies, together with their simple and double interactions with 2015 and *South* dummies. These interaction terms are important because in repeated cross-section studies, as our own, one needs to examine if the samples are selected over time in the same way from comparable populations (Meyer 1995). Moreover, in all our analysis we compute the cluster-robust standard errors where clusters are made of classes in which individuals don't change their eligibility status. However, all results carry through by clustering at the individual level.

### 3.5.2 Defining the eligible and the non-eligible

The group of individuals which we define as eligible for the subsidy granted by law 407/90 includes those whose non-employment[19] duration is longer than two years. In order to avoid including observations with extremely long non-employment span, we disregard durations longer than 3 years.

The control group should be made of individuals for which we expect a similar time trend in the absence of the policy. At first sight, it would seem reasonable to select

18. In this case, however, the falsification test on 2013-14 is less neat, which might indicate that the parallel trend assumption holds when it is expressed in terms of proportional odds (the logit model) but not as a difference in hazard rates (the linear probability model).

19. We talk about non-employment instead of unemployment because our dataset only allows us to know periods in which individuals are not engaged in any labor contract, but not if they are actively looking for a job.

TABLE 3.2: Means and standard deviations computed over individuals in each category

| | Eligible | | | | Non-eligible | | | |
|---|---|---|---|---|---|---|---|---|
| | South | | Centre-North | | South | | Centre-North | |
| | 2014 | 2015 | 2014 | 2015 | 2014 | 2015 | 2014 | 2015 |
| Female | 41.6 | 42.5 | 47.7 | 48.5 | 40.5 | 40.6 | 48.5 | 48.8 |
| | [49.3] | [49.4] | [50.0] | [50.0] | [49.1] | [49.1] | [50.0] | [50.0] |
| High sc. | 63.8 | 65.0 | 65.0 | 65.9 | 65.9 | 66.6 | 65.8 | 66.1 |
| dropout | [48.1] | [47.7] | [47.7] | [47.4] | [47.4] | [47.2] | [47.5] | [47.4] |
| Foreign | 12.2 | 13.9 | 32.9 | 33.8 | 15.0 | 15.3 | 36.1 | 36.4 |
| born | [32.8] | [34.6] | [47.0] | [47.3] | [35.7] | [36.0] | [48.0] | [48.1] |
| Age | 39.5 | 39.9 | 40.3 | 40.7 | 38.5 | 39.4 | 39.1 | 40.0 |
| | [11.3] | [11.4] | [11.3] | [11.3] | [11.4] | [11.3] | [11.3] | [11.1] |
| Agric- | 12.0 | 13.4 | 5.6 | 5.9 | 12.5 | 14.6 | 4.9 | 5.6 |
| ulture | [32.5] | [34.0] | [23.0] | [23.6] | [33.1] | [35.3] | [21.6] | [22.9] |
| Manufa- | 13.1 | 12.4 | 15.4 | 14.7 | 12.7 | 11.8 | 15.4 | 13.9 |
| cturing | [33.7] | [33.0] | [36.1] | [35.4] | [33.3] | [32.2] | [36.1] | [34.6] |
| Constr- | 16.6 | 15.2 | 10.8 | 10.0 | 17.1 | 16.5 | 10.3 | 9.7 |
| uction | [37.2] | [35.9] | [31.1] | [30.3] | [37.7] | [37.1] | [30.4] | [29.7] |
| Services | 58.4 | 59.1 | 68.2 | 69.4 | 57.7 | 57.1 | 69.4 | 70.8 |
| | [49.3] | [49.2] | [46.6] | [46.1] | [49.4] | [49.5] | [46.1] | [45.5] |
| Non-empl. | 138.3 | 139.4 | 138.6 | 140.3 | 61.5 | 61.4 | 61.6 | 61.6 |
| duration | [17.8] | [17.5] | [17.7] | [17.3] | [17.0] | [17.6] | [17.0] | [17.5] |
| Perm. job | 0.4 | 0.5 | 0.1 | 0.3 | 0.4 | 0.6 | 0.3 | 0.4 |
| find. rate | [6.1] | [6.7] | [3.8] | [5.4] | [6.5] | [7.9] | [5.0] | [6.7] |
| Individ. | 20,643 | 20,844 | 36,843 | 38,009 | 36,246 | 27,142 | 66,612 | 49,407 |
| Obs. | 483,097 | 487,736 | 887,017 | 926,931 | 810,543 | 554,902 | 1,517,302 | 1,055,532 |

*Note:* Standard deviations are in square brackets. All values are in percentages apart from age (years), non-employment duration (weeks), individuals (units).

individuals with non-employment duration just below the two-year threshold. However, this does not lead to the selection of a good control group for two reasons. First, since the computation of non-employment duration is complex, as we described above, we do not have the true non-employment duration, but only an approximation affected by measurement error. Hence, a sharp cutoff in non-employment duration able to separate the eligible from the non-eligible can lead us to wrongly attribute eligible individuals to the control group and vice-versa. Second, a sharp cutoff has another disadvantage due to strategic behavior by firms, who would prefer to hire individuals just above the threshold, compared to those just below, in order to get the subsidy until it was in place. Therefore, defining as control group individuals just below the threshold would violate the Stable Unit Treatment Value Assumption (SUTVA), because the treatment, i.e. being eligible for the subsidy, would affect also individuals in the control group. For these reasons, we define as control group those individuals with non-employment duration between 6 and 18 months. In the Appendix B we also discuss a robustness check restricting the definition of non-eligible to a 12-18-month window, in order to select individuals resembling the eligible group more closely. Symmetrically, we restrict also the window defining the treatment group to 24-30 months, instead of 24-36. Results are qualitatively similar and suggest an even stronger effect of the policy.

Our final dataset is made of 174,843 individuals, observed at weekly frequency from January 2014 to December 2015, until they find a job, reach age 65, or exceed the thresholds of 36 or 18 months of non-employment duration, for eligible and non-eligible individuals, respectively. Notice that the same individual can be classified as both eligible and non-eligible at different points in time if, starting as STU and not finding a job, she then is classified as LTU, or, on the contrary, she appears first as LTU, then exits the non-employment status and eventually re-enters as STU. Therefore, we deal with an unbalanced panel of 6.7 million observations. Table 3.2 provides summary statistics for the subpopulations of interest, which are mainly made of low-skilled prime aged Italian man, who previously worked in the service sector.

### 3.5.3 Main results

In what follows we focus on the probability that a non-employed find a permanent job, conditional on not having found one in the previous six months at least. Table 3.3 reports results of the (*logit*) across-areas DD performed on 2014. The dependent variable is a dummy equal to one if the individual finds a permanent job in the subsequent week, zero otherwise. In the first column there are no other controls apart from the dummies needed for the triple difference and non-employment duration (in logarithm). The unit of analysis is individual-week. The first dummy ($LTU$) is equal to one if the individual $i$ in week $w$ has a non-employment duration between two and three years (being therefore eligible, until 2014, for the hiring subsidy granted by law 407/90), and it is equal to zero if the individual has a non-employment duration between 6 and 18 months. The second

TABLE 3.3: Logit model — Odds ratios. Across areas comparison, year 2014

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6942*** | 0.7038*** |
| | (0.0251) | (0.0255) |
| LTU | 0.8056*** | 0.8269*** |
| | (0.0390) | (0.0402) |
| South | 1.6771*** | 1.1794 |
| | (0.0395) | (0.4194) |
| **LTUxSouth** | **1.5354*** ** | **1.5195*** ** |
| | **(0.0672)** | **(0.0667)** |
| Constant | 0.0105*** | 0.0005*** |
| | (0.0015) | (0.0001) |
| Observations | 3,697,959 | 3,697,959 |
| Pseudo R-squared | 0.0106 | 0.0389 |

*Note:* Cluster-robust standard errors, where clusters are made of 159,444 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *South*.

dummy (*South*) is equal to one if the individual lives in Southern Italy, zero if she lives in the North or Centre.

Surprisingly, the odds of finding a permanent job in Southern regions is greater than one, even if we control for demographic characteristics; this suggests that non-employed individuals are more likely to find a permanent job in the South than in the Centre-North. This result may reflect geographical differences in job search, which in Southern regions may imply longer non-employment spells.[20] If this is the case, the composition of non-employed individuals may be different in the two areas and in Southern regions individuals with the same non-employment duration may be relatively less detached from the labor market. Another possible explanation may be related to the fact that Southern regions are characterized by higher levels of irregular work: on average 19 per cent of workers in the period 2009-2015 vs. 10 percent in Center-North. This may imply a higher attachment to the labor market for individuals in the South compared to those in the North with the same time span since last regular job loss. In fact, when we control for the regional rate of irregular work, differentiated by sector of previous job, the odds

20. In Southern regions workers tend to look for a job less actively and relying on slower channels. Indeed, according to the Italian labor force survey, in 2014-2015 people out of employment did something to look for a job on average 6 months before the interview in Centre-North and 7 in the South. The share of people who looked for a job on the web was 55 percent in Centre North and 45 in the South. The share of people who turned either to relatives, friends, acquaintances or unions was respectively 69 and 73.

ratio on the South dummy becomes statistically non-significant.

As expected, the odds ratio on $log(duration)$ is smaller than one, implying that the odds of finding a permanent job decrease the longer the individual has been non-employed. Also, the odds ratio on the $LTU$ dummy ($\exp(\beta_1)$) is smaller than one, indicating the deterioration rate is more than linear with respect to non-employment duration. In other words, being non-employed for at least two years (LTU) makes it more difficult to find a permanent job. However, this difference was smaller for the LTU living in the South, as the dummy $LTU$x$South$ has a positive impact on the chances of finding a permanent job, being its odds ratio larger than one. This suggests that the greater subsidy granted to eligible individuals in the South was effective in rising their chances of finding a job with a permanent contract. The coefficient on the double interaction is hardly affected by the introduction of other control variables (interacted with the $South$ dummy).

As already argued, the DD exercise is far from being conclusive, given that the eligible versus non eligible comparison in the South might differ from the one in the Centre-North for reasons other than law 407/90. In Table 3.4 we therefore exploit the change over time in this double comparison, by looking at the triple difference. Overall, in 2015 the odds of finding a permanent job for the individuals in the sample is higher compared to 2014. This captures both the effect of labor market reforms introduced in 2015 (see Sestito and Viviano 2018) and other changes that occurred over time. The interaction 2015x$South$ shows an odds ratio smaller than one, which implies that the improvement occurred in 2015 was less strong in this area. However, the improvement seems to have been larger for the LTU, as the interaction $LTU$x2015 is larger than one. These three coefficients (on year 2015, 2015x$South$ and $LTU$x2015) use the different control groups to capture the underlying trends by area and LTU status.

The triple interaction is therefore the trend for the treated group (the LTU in the South) net of these common trends. The associated odds ratio is smaller than one and statistically significant. This implies that the relative trend for the eligible with respect to non-eligible between 2014 and 2015 was worse in the South. This is in line with the dynamic of the stronger subsidy granted by law 407/90 to permanent hires of LTU in the South, which was abolished and replaced by the new (almost) universal subsidy in 2015. As explained above, individuals in treatment and control groups change over time. For the triple difference estimator not to pick up spurious correlations, observable characteristics across these groups should be similar in the pre-treatment and post-treatment periods (and between areas). To check whether compositional changes affect our results, in the second column we add a set of demographic controls, sector of previous job, incidence of irregular work and monthly dummies, together with their simple and double interaction with 2015 and $South$ dummies, in order to be sure that the two groups can be considered identical in all observable characteristics. Our coefficient of interest, the triple interaction, is basically unchanged.

TABLE 3.4: Logit model — Odds ratios. DDD, years 2014-15

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6618*** | 0.6710*** |
| | (0.0166) | (0.0168) |
| LTU | 0.8443*** | 0.8665*** |
| | (0.0345) | (0.0356) |
| 2015 | 1.7587*** | 1.1229 |
| | (0.0379) | (0.3426) |
| South | 1.6772*** | 1.1804 |
| | (0.0395) | (0.4196) |
| LTUx2015 | 1.1817*** | 1.1430*** |
| | (0.0475) | (0.0462) |
| LTUxSouth | 1.5353*** | 1.5194*** |
| | (0.0672) | (0.0667) |
| 2015xSouth | 0.8513*** | 0.4641 |
| | (0.0273) | (0.2261) |
| **LTUx2015xSouth** | **0.6883*** ** | **0.7058*** ** |
| | **(0.039)** | **(0.0402)** |
| Constant | 0.0126*** | 0.0006*** |
| | (0.0012) | (0.0002) |
| Observations | 6,723,060 | 6,723,060 |
| Pseudo R-squared | 0.0118 | 0.0382 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE 3.5: Logit model — Odds ratios. Across areas comparison, year 2015

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6359*** | 0.6448*** |
| | (0.0219) | (0.0222) |
| LTU | 1.0381 | 1.0303 |
| | (0.0435) | (0.0431) |
| South | 1.4283*** | 0.5474* |
| | (0.0321) | (0.1836) |
| **LTUxSouth** | **1.0565** | **1.0720*** |
| | **(0.0385)** | **(0.0392)** |
| Constant | 0.0258*** | 0.0015*** |
| | (0.0035) | (0.0004) |
| Observations | 3,025,101 | 3,025,101 |
| Pseudo R-squared | 0.0063 | 0.0313 |

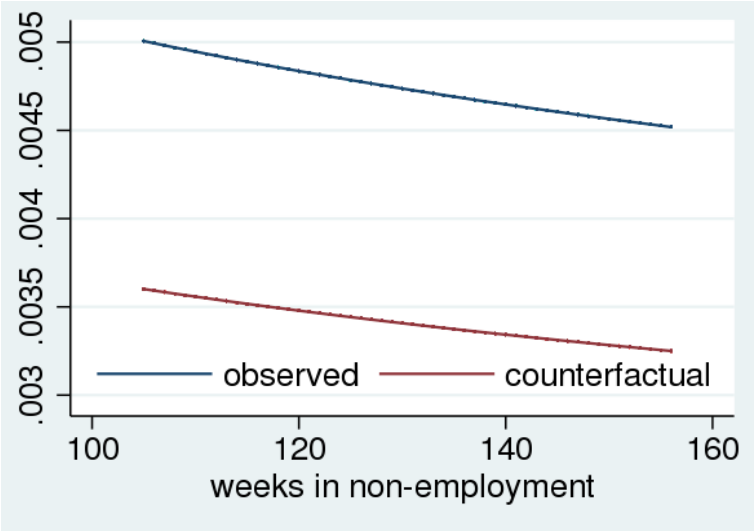
*Note:* Cluster-robust standard errors, where clusters are made of 134,657 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with *South*.

Importantly, the odds ratio on the triple interaction reinforces the result from the simple double comparison: it is the change in the double difference across areas and LTU status, essentially the opposite of the within-2014 across-areas diff-in-diff. The relative advantage of eligible individuals living in the South, for which we find evidence in 2014 (as the odds ratio on $LTU$x$South$ was larger than one), has disappeared in 2015. Another algebraically equivalent way to see this result is to separate the second DD that composes the DDD exercise. In Table 3.5 we show the DD that compares STU and LTU across areas in 2015: in the presence of similar subsidies for both LTU and STU across areas, the odds ratio on the double interaction is close to 1. The fact that the relative advantage for the LTU in the South disappears when the preferential treatment is removed suggests that our estimates can be attributed to the subsidy we are studying, rather than to other contemporary changes.

Law 407/90 seems, therefore, to have had a positive effect on the chances of accessing a permanent employment. Using 3.1 we can simulate the counterfactual conditional probability of finding a permanent job for the eligible in 2014 in the South if the policy was not present, i.e.

$$logit[\lambda(y_{iw}) \mid t = 2014, \widehat{g = South}, LTU_{iw} = 1, UD_{iw}] = \hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_5 + \hat{\beta}_7 + \hat{\beta}_{UD} UD_{iw}$$

FIGURE 3.5: Counterfactual analysis




*Note:* Conditional probability of finding a permanent job in the subsequent week in 2014 for the eligible in the South, observed (as estimated by the model) and counterfactual (in the absence of the targeted subsidy).

and compare it with the observed one, as estimated by the model, i.e.

$$logit[\lambda(y_{iw}) \mid t = 2014, \widehat{g = South}, LTU_{iw} = 1, UD_{iw}] = \hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_3 + \hat{\beta}_5 + \hat{\beta}_{UD} UD_{iw}$$

We plot the results in Figure 3.5. The counterfactual refers to the situation without the targeted subsidy. The effect is non-negligible, since the subsidy was raising the weekly chances of finding a permanent job by 41 per cent. To get an idea of how big the effect is we compare it with that found by Sestito and Viviano (2018) for the generalized hiring subsidy of 2015. The authors find an increase of 100 per cent in the monthly probability of finding a permanent job for those who were not working in the previous period. They show that most part of the effect is due to the hiring subsidy, and only a small part to firing cost reduction granted by the Jobs Act which was passed in the same year. Being of the same order of magnitude, we are reassured about the plausibility of our result.

One issue we do not consider directly is by what extent the subsidy might have shaped the entire distribution of non-employment duration, as it might have given an incentive to wait longer in non-employment. However, from our results we can draw some indirect evidence this is not the case, as the likelihood of transitioning to a permanent contract for the non-eligible in the South vs. their counterpart in the Centre-North deteriorated after the removal of law 407/90 (2015x$South$ in Table 3.4), and it becomes non-significant once controls are included, while we would have expected the opposite if the law had given them a strong incentive to wait to reach the LTU status.

TABLE 3.6: Logit model — Odds ratios. Falsification test: years 2013-14

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6980*** | 0.7123*** |
| | (0.0179) | (0.0183) |
| LTU | 0.7753*** | 0.7991*** |
| | (0.0340) | (0.0352) |
| 2014 | 1.0223 | 0.3509*** |
| | (0.0227) | (0.1130) |
| South | 1.5822*** | 0.4525** |
| | (0.0369) | (0.1567) |
| LTUx2014 | 1.0385 | 1.0236 |
| | (0.0496) | (0.0491) |
| LTUxSouth | 1.4540*** | 1.4201*** |
| | (0.0702) | (0.0688) |
| 2014xSouth | 1.0621* | 2.0851 |
| | (0.0346) | (1.0192) |
| **LTUx2014xSouth** | **1.0643** | **1.0823** |
| | **(0.0690)** | **(0.0705)** |
| Constant | 0.0100*** | 0.0006*** |
| | (0.0010) | (0.0002) |
| Observations | 7,335,744 | 7,335,744 |
| Pseudo R-squared | 0.0098 | 0.0369 |

*Note:* Cluster-robust standard errors, where clusters are made of 236,129 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. These regressions identify treated and controls using the same criteria as in Tables 3.3-3.5. Since years of interest differ, also the sample of individuals may differ. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2014 and *South* dummies.

The meta-analysis by Card et al. (2018) suggests that active labor market policies are more effective for females. We also analyzed the split sample by gender (Tables B.7 and B.8 in the Appendix B), but the odds ratio on the triple interaction is quite similar across the two groups. If nothing, the effect is actually a bit smaller among females.

Our analysis assumes that, absent the change in law 407/90, the double comparison across LTU status and areas would not have changed between 2014 and 2015, so that the odds ratio on the triple interaction would have been one. As an indirect test for the plausibility of this assumption, we run a placebo regression for the years 2013-2014. Reassuringly, we do not find a statistically significant coefficient for the triple interaction term (Table 3.6).

### 3.5.4 Effect on wages

Apart from increasing their chances of obtaining a permanent contract, the subsidy could also potentially rise the entry wage of the beneficiaries. Given that the subsidy consisted of a strong deduction of social security contributions paid by the employer, the effect on wage depends on how much of this gain is shared with the employee. An increase in wage might also be observed if the eligible, knowing that the subsidy raises their likelihood of finding a job, wait longer before accepting a job offer.

In Table 3.7 we select from our dataset only those individuals for which we have the information on wages along the whole job history and perform a regression of the logarithm of wage on the same regressors used in previous tables. Apart from the negative coefficient for the *South* dummy (when we include all the controls), all other coefficients are not statistically significant. This means that the only effect of the policy for the eligible was to increase their chances of getting a permanent job, while the reduction of hiring costs did not trickle down to the employees. This is reasonable considering that the policy target includes disadvantaged individuals, whose bargaining power is most likely very low.[21]

## 3.6 Robustness checks

As common in this kind of studies, our results are affected by possible issues of substitution. Firstly, the subsidy of law 407/90, which applies only to permanent contracts, might have displaced temporary contracts, leading to a null net employment creation. In Subsection 3.6.1 we look at exit towards these contracts.[22]

Secondly, any benefit for LTU creates an incentive to wait longer in non-employment. We partially addressed this issue by excluding individuals just below the 24-month threshold. However, one may want to fully evaluate how the entire non-employment duration distribution changes because of the policy. We are unable to properly perform this full evaluation using our natural experiment. In the previous section we provided some indirect evidence that our conclusions are not biased by this issue, given that the change in outcomes for non-eligible in the South between 2014 and 2015 was worse than for their counterpart in the Centre-North, while we would have expected the opposite if —when the law 407/90 was in place— they had an incentive to wait longer in non-employment. In Subsection 3.6.1 we provide additional evidence analyzing whether the change of policy affected the take-up of very short-term contracts that did not reset the

21. The effect of the subsidy could have been even negative. Adamopoulou and Viviano (2018) find that the more general subsidy introduced in 2015 had a negative effect on wages.

22. In the case of interval-censoring, one can prove that it is possible to estimate separate *logit* models for each possible type of exit from the unemployment status, under some assumptions. In particular, we need to assume that events only happen at the boundaries of the interval, which seems appropriate in our case (as contracts usually start on Monday). See Section 9.3 of Jenkins (2005) for further reference.

TABLE 3.7: Ordinary least squares regression

| Dependent variable: Wage in logarithm | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.0218 | 0.0202 |
| | (0.0181) | (0.0171) |
| LTU | 0.0741** | 0.0003 |
| | (0.0354) | (0.0341) |
| 2015 | 0.0876*** | 0.2377 |
| | (0.0159) | (0.2247) |
| South | 0.1004*** | -0.4903** |
| | (0.0170) | (0.2384) |
| LTUx2015 | -0.0462 | -0.0139 |
| | (0.0367) | (0.0360) |
| LTUxSouth | -0.0531 | -0.006 |
| | (0.0381) | (0.0369) |
| 2015xSouth | -0.0526** | -0.0218 |
| | (0.0239) | (0.3230) |
| **LTUx2015xSouth** | **0.0093** | **0.0055** |
| | **(0.0469)** | **(0.0458)** |
| Constant | 6.6215*** | 6.4569*** |
| | (0.0716) | (0.1899) |
| Observations | 9,791 | 9,791 |
| Adjusted R-squared | 0.01 | 0.135 |

*Note:* Cluster-robust standard errors, where clusters are made of 9,496 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

unemployment duration (according to the legal definition).

Thirdly, employers, learning about the incoming end of the subsidy, might have anticipated at the end of 2014 some contracts that they would have signed in the following year. In Subsection 3.6.2 we assess whether our results are driven by those contracts signed after the law was announced.

Finally, individuals might move across the country to exploit the difference in the intensity of the subsidy. In Subsection 3.6.3 we run a robustness check excluding those non employed that found a job in a different area with respect to where their last job was located.

### 3.6.1 Are results driven by substitution with other types of contract?

The analysis carried out so far considered only the outcome "exit to permanent jobs" as dependent variable, disregarding other possible exits, namely getting another type of contract, reaching retirement age, or being right censored because the non-employment spell exceeds the three-year threshold. In what follows we explore the outcome "exit to fixed-term contracts" in order to see if the positive impact on permanent contracts came at the expenses of temporary ones, reducing the net employment gains. This might also happen if the policy has been raising the reservation wage of the LTU, because they knew the subsidy was increasing their likelihood to receive, at some point in the future, an offer for a permanent job.

We look first at exits from non-employment to fixed-term contracts that last more than 6 months (4/8 months for the South), because, as discussed in Section 3.3, according to law 407/90 individuals were still considered unemployed if they got shorter term contracts. Table 3.8 reports the estimated odds ratios. The triple interaction ($LTU$x2015x$South$) does not highlight significant differences for the eligible living in the South. We can interpret this result as evidence that law 407/90 did not imply a simple substitution of fixed-term contracts with permanent jobs, but rather a net employment gain.

Notice also that, when the subsidy was available, both eligible and non-eligible had an incentive to avoid taking "long" fixed-term contracts, because such contracts reset unemployment duration, losing possible gains associated with LTU. The fact that the odds ratio on the triple interaction term is close to one suggests this concern is not stronger for the eligible compared to non-eligible. Our identification strategy does not provide a full assessment for the non-eligible alone. Nevertheless, the interaction 2015xSouth indicates the differential trend for non-eligible in the South after law 407/90 was abolished (compared to non-eligible in the North). If individuals at the beginning of their non-employment spell were avoiding fixed-term contracts in order to become eligible and benefit from law 407/90, then we expect them to become relatively more likely to take

TABLE 3.8: Logit model — Odds ratios. Transitions to "long" fixed-term contracts

| Dep. variable: Dummy equal to one if individual finds a "long" fixed-term job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.7045*** | 0.7187*** |
| | (0.0160) | (0.0164) |
| LTU | 0.6558*** | 0.6800*** |
| | (0.0208) | (0.0216) |
| 2015 | 0.7662*** | 0.2304*** |
| | (0.0136) | (0.0558) |
| South | 0.6526*** | 5.5836*** |
| | (0.0133) | (1.6296) |
| LTUx2015 | 1.3125*** | 1.2901*** |
| | (0.0431) | (0.0423) |
| LTUxSouth | 0.9080** | 0.9027** |
| | (0.0407) | (0.0405) |
| 2015xSouth | 1.0763** | 0.4938 |
| | (0.0364) | (0.2395) |
| **LTUx2015xSouth** | **0.9856** | **0.9816** |
| | **(0.0641)** | **(0.0638)** |
| Constant | 0.0245*** | 0.0574*** |
| | (0.0022) | (0.0096) |
| Observations | 6,723,060 | 6,723,060 |
| Pseudo R-squared | 0.0119 | 0.0311 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

these contracts in 2015. Without demographic and time controls there is some evidence that this could be the case, but the odds ratio become smaller than one and not significant once we include them, thus suggesting that no substitution in favor of these contracts was in place even for the short-term unemployed.

A similar issue concerns "short" fixed-term contracts. As opposed to before, we expect a higher likelihood to use these contracts until law 407/90 was in place, because they did not reset unemployment duration. In Table 3.9 we consider the conditional probability of being employed with such a short-term contract, which technically does not constitute an exit from the "legal" definition of unemployment. The odds ratio on the triple interaction is again not significant, indicating that the eligible in the South did not disproportionately use these contracts. A similar concern, however, applies also to the non-eligible in the South. Again, our identification strategy cannot provide clean evidence about this group alone, but it is useful to highlight that the odds ratio on the interaction 2015x*South* —once we include demographic and time controls— does not indicate a differential trend with respect to the Centre-North after the law was abrogated, providing evidence that no substitution was in place in favor of this contract.

### 3.6.2 Are results driven by substitution over time?

In Table 3.10 we repeat the same analysis of Table 3.4, but with a restricted sample, that excludes the fourth quarter of 2014 and the first quarter of 2015. We do so because our result may be entirely driven by an anticipation effect. Indeed, the Financial Stability Law for 2015 was announced at the end of October 2014, and employers in the South could have anticipated to the last quarter of 2014 those hires they had in program for the beginning of 2015. In so doing, they could have got the larger benefit granted by law 407/90, which would have expired at the end of 2014. Specularly, employers in the Center and North found it more convenient to wait until 2015 in order to benefit from the more generous hiring subsidy granted by the Financial Stability Law for 2015, by postponing to the new year those hires they had in program for the end of 2014. The results are robust to the exclusion of these two periods. Although the odds ratio on the triple interaction (*LTU*x2015x*South*) gets closer to one, meaning a milder effect, the change is relatively small.

### 3.6.3 Are results driven by substitution across areas?

In Table 3.11 we report a further robustness check, where we exclude from the sample individuals that moved from South to Centre-North or vice versa. We do so in order to check whether our result is influenced by people moving where the most profitable subsidies apply. The results are robust to this sample restriction. Specifically, the odds ratio on the triple interaction term does not change, being statistically significant. This

TABLE 3.9: Logit model — Odds ratios. Transitions to "short" fixed-term contracts

| Dep. variable: Dummy equal to one if individual finds a "short" fixed-term job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.7786*** | 0.7755*** |
| | (0.0140) | (0.0138) |
| LTU | 0.7968*** | 0.7837*** |
| | (0.0220) | (0.0216) |
| 2015 | 0.8875*** | 0.3254*** |
| | (0.0137) | (0.0695) |
| South | 1.3933*** | 0.4548*** |
| | (0.0220) | (0.0997) |
| LTUx2015 | 1.1426*** | 1.1602*** |
| | (0.0309) | (0.0316) |
| LTUxSouth | 1.008 | 0.9833 |
| | (0.0315) | (0.0306) |
| 2015xSouth | 1.0737*** | 0.9482 |
| | (0.0243) | (0.3054) |
| **LTUx2015xSouth** | **0.9815** | **1.0058** |
| | **(0.0391)** | **(0.0404)** |
| Constant | 0.0257*** | 0.0265*** |
| | (0.0018) | (0.0039) |
| Observations | 6,723,060 | 6,723,060 |
| Pseudo R-squared | 0.0066 | 0.0323 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE 3.10: Logit model — Odds ratios. Excluding the fourth quarter of 2014 and the first of 2015

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6388*** | 0.6416*** |
| | (0.0182) | (0.0183) |
| LTU | 0.9070** | 0.9236* |
| | (0.0416) | (0.0425) |
| 2015 | 1.6610*** | 1.7528 |
| | (0.0405) | (0.6053) |
| South | 1.6774*** | 0.9991 |
| | (0.0432) | (0.3825) |
| LTUx2015 | 1.1821*** | 1.1642*** |
| | (0.0534) | (0.0529) |
| LTUxSouth | 1.4116*** | 1.4064*** |
| | (0.0689) | (0.0690) |
| 2015xSouth | 0.8622*** | 0.4854 |
| | (0.0314) | (0.2646) |
| **LTUx2015xSouth** | **0.7302*** ** | **0.7526*** ** |
| | **(0.0469)** | **(0.0487)** |
| Constant | 0.0151*** | 0.0005*** |
| | (0.0017) | (0.0002) |
| Observations | 5,105,412 | 5,105,412 |
| Pseudo R-squared | 0.0109 | 0.0369 |

*Note:* Cluster-robust standard errors, where clusters are made of 217,774 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE 3.11: Logit model — Odds ratios. Excluding individuals moving across areas

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6687*** | 0.6759*** |
| | (0.0171) | (0.0173) |
| LTU | 0.8245*** | 0.8497*** |
| | (0.0345) | (0.0358) |
| 2015 | 1.7729*** | 1.1673 |
| | (0.0390) | (0.3636) |
| South | 1.6923*** | 1.2649 |
| | (0.0408) | (0.46 00) |
| LTUx2015 | 1.2065*** | 1.1641*** |
| | (0.0495) | (0.0480) |
| LTUxSouth | 1.5821*** | 1.5555*** |
| | (0.0707) | (0.0698) |
| 2015xSouth | 0.8456*** | 0.4386* |
| | (0.0277) | (0.2179) |
| **LTUx2015xSouth** | **0.6675*** ** | **0.6854*** ** |
| | **(0.0385)** | **(0.0398)** |
| Constant | 0.0118*** | 0.0005*** |
| | (0.0012) | (0.0001) |
| Observations | 6,575,184 | 6,575,184 |
| Pseudo R-squared | 0.0121 | 0.0385 |

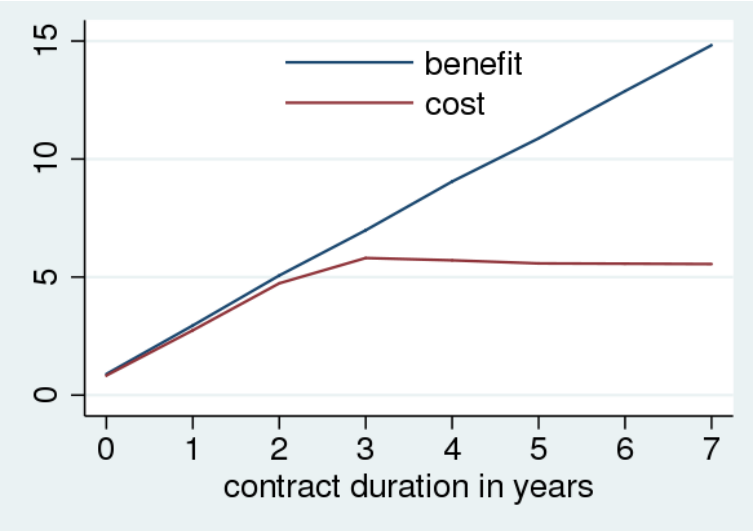
*Note:* Cluster-robust standard errors, where clusters are made of 215,997 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

is not surprising since labor mobility is quite low: only about 6,000 individuals were excluded from the sample over a total of 175,000 in the full specification.

## 3.7 Cost-benefit analysis

We finally perform a cost-benefit analysis, where we compare the cost of the subsidy, i.e. the three-year exemption from social security contribution paid by the employer, with the benefits, measured by tax revenue and social security contribution paid by the employee generated by job creation as in Cahuc et al. (2019). We do not consider savings on unemployment benefit since the Italian unemployment insurance scheme does not cover the unemployed for more than two years. Moreover, we are not able to quantify other social costs associated with long-term non-employment, like costs suffered by family and friends and possibly spillovers that encourage more non-employment, as discussed in the

FIGURE 3.6: Cost-benefit analysis




*Note:* Average cost and benefit of the subsidy by contract duration. Thousands of euro.

introduction. We made this computation just for contracts signed in the South, since our identification makes sense for this subgroup only, which is also the most expensive in terms of cost of the policy.

More specifically, on the cost side we just considered the part of the subsidy exceeding the amount granted to firms in Center-North, since what we estimate is the effect of the higher subsidy for firms in the South. While we compute these costs for all subsidized contracts, the benefits are instead computed just for those additional contracts that would have not been signed absent the policy. This fraction is recovered from our estimated change in conditional probability of finding a permanent job: if we use the estimate taken from our main specification in Table 3.4 (i.e. increased probability of finding a permanent job for the eligible individuals in the South equal to 41 percent), the share of contracts signed because of the policy is equal to $41/(100 + 41)$, i.e. 29 percent of all eligible contracts. Of course, as outlined in the introduction and justified by indirect evidence, our estimates rest on the assumption that no substitution occurs between long- and short-term non employed, types of contracts, geographic areas or time periods.

As we report in Table B.1 in the Appendix B, only 66 percent of eligible long-term non-employed in the South did actually benefit from the subsidy in 2014. Therefore, we divide the share of eligible contracts due to the policy by the fraction of eligible contracts that actually got the subsidy, i.e. 29 divided by 66 equals 44 percent of eligible contracts that got the subsidy and would have not been signed without the policy. As shown in Figure 3.6 we find that benefits outweigh costs even for short contract duration.

## 3.8 Discussion and conclusion

In recent years the Italian labor market has undergone several reforms, the Jobs Act being a last example, which received a lot of attention. A different subsidy, which was abolished at the same time, was instead almost entirely neglected, despite having been in place for 25 years.

By exploiting the timing of the abolition, eligibility criteria, and geographical variation of the subsidy generosity, we perform a policy evaluation exercise which leads us to conclude that, in fact, the policy measure had been effective in promoting the employability of long-term non-employed. In particular, we find that the subsidy granted by law 407/90 was able to counteract the deterioration in employability associated with long-term non-employment. The disadvantage in accessing permanent jobs for long-term non-employed (vs. short-term ones) was smaller in Southern regions, were the subsidy was larger. When, in 2015, the preferential regime granted by law 407/90 was removed, this difference disappeared. This positive effect on permanent employment does not seem to be due to an anticipation effect, substitution across areas, with fixed-term contracts or among jobseekers, and there is no evidence that it led to an increase in wages. Moreover, a cost-benefit analysis shows that revenues from subsidized jobs outweighed their costs.

One issue that we do not discuss in the paper is by what extent the subsidy might shape the entire distribution of the non-employment spells. For instance, the subsidy might give an incentive to wait longer in non-employment —also by repeatedly taking very-short term contracts that were not changing the "legal" status of unemployed. Our results do not seem to indicate that this is the case, as (i) the subsidy does not seem to impact the chances of taking "long" fixed-term contracts, that reset the legal unemployment duration to zero, nor "short" ones, and (ii) the likelihood of transitioning to a permanent contract for the non-eligible in the South vs. their counterpart in the Centre-North deteriorated after the removal of law 407/90, while we would have expected the opposite if the law had given them a strong incentive to wait to reach the LTU status. Nevertheless, our main analysis recovers only the relative impact on LTU vs. STU workers and does not identify the heterogeneous effect of the policy across geographic areas. Further analysis is needed to shed light on this issue, possibly by means of a structural model that considers changes in the entire non-employment duration distribution.

# Appendix A

## A.1 The data

We set up an innovative data-set, assembling cultural and economic variables from a large number of sources, mostly at subprovincial level, running throughout the history of unified Italy. Non-contemporary entries (the earliest, from 1874) were mainly inputted from books stored in the archives of three libraries, whose welcome and help were much appreciated: Baffi (Banca d'Italia), Istat, and National of Naples. Data were collected for the areas of interest (provinces of Caserta, Latina, Frosinone, Rome plus two Isernia municipalities) at the level of provinces, judicial districts and municipalities.

### A.1.1 Provincial data

- Province Administration Balance Sheets, 1936-1939 and 1946-1948. Source: Ministry of Treasury.
- Interprovincial migration flows, 1928-1938 and 1955-2012. Source: ISTAT.
- Marsh reclamation expenditures per province at 1938. Source: Tassinari (1939).

### A.1.2 Judicial district data

We collect data on incoming and settled first-degree lawsuits per judicial district of the pre-reform period. We use the judicial district capital to pinpoint geographic location of the district. We consider only districts strictly included in each of our areas, exploiting the fact that their territories did not cross any of the two borders in our focus. For the years for which we present data (1874-1875 for crime rate and criminal court efficiency, 1874-1876 for civil court efficiency and 1885-1891 for litigation rate), we have a total of 40 judicial districts observations, covering 176 out of the 197 municipalities registered in our full sample.

The collection of useful data is constrained by the evolution of recording strategies and of judicial geography. First, in the years immediately after those just listed, civil and criminal lawsuits data started to be provided with no distinction between new and pending lawsuits. Also, they quite soon started to be published in more aggregate forms, i.e. for higher-level judicial areas. Together with the continuous process of judicial districts merging (leaving few available observations, also for entrance-level *Giudici di*

*Pace*), such modifications have been limiting up to our days the use of judicial sub-provincial data.

A descriptive list of the variables used, in order of appearance, follows:

*Crimes*: Number of convictions per 1,000 inhabitants. Average over years 1874-1875. Source: Ministero di Grazia e Giustizia e dei Culti (henceforth, MGGC). "Statistica degli affari civili e commerciali e degli affari penali per l'anno ..."

*Litigation rate*: Number of new civil lawsuits per 1,000 inhabitants. Average over years 1885-1891. Source: MGGC. "Statistica degli affari civili e commerciali e degli affari penali per l'anno ..."

*Criminal court efficiency*: Number of first-degree defined cases divided by the sum of new and pending criminal lawsuits. Average over years 1874-1875. Source: MGGC. "Statistica degli affari civili e commerciali e degli affari penali per l'anno ..."

*Civil court efficiency*: Number of first-degree settled cases divided by the sum of new and pending civil lawsuits. Average over years 1874-1876. Source: MGGC. "Statistica degli affari civili e commerciali e degli affari penali per l'anno ..."

### A.1.3 Municipal data

As explained in Section 2.2, given the distance between the two borders identifying our Center area, our total sample includes municipalities lying within 40 km from each border; as a result, our three areas host 200 equally-distributed municipalities (Table A.1). These exhaust municipalities in Frosinone province, about so in Latina (28 out of 33), two thirds of them in Caserta (68 out of 104); for geographical continuity, 13 municipalities in Rome province and 2 in Isernia are also included. As visible in Figure 2.1, three observations (Latina, Sabaudia and Pontinia) were excluded from the North area sample as they were founded after 1927 almost exclusively by families coming from elsewhere in Italy for the marsh reclamation and city building campaigns, according to the policies of internal migration of the Fascist regime.[23] Results on the local cultural transformation would be biased by their inclusion. In the RD analysis for the border between North and Center areas (Section 2.5) we use a dummy for Sermoneta, Terracina and San Felice Circeo, pre-existing villages affected by the campaign and by the specific internal migration in the decade after the administrative reform.

A descriptive list of the variables used, in order of appearance, follows, together with a table sorting municipalities in our sample per area:

*Elevation*: Elevation (m) of the main square of each municipality. Source: ISTAT.

*Maximum difference in elevation*: Distance (m) between the highest and the lowest point in the municipal territory. Source: ISTAT.

23. The pre-existing municipality of Cisterna di Latina, which experienced triple population in ten years for being the seat of the campaign headquarters, was excluded from the outstart for just exceeding our distance criterion.

*Distance from coast*: Euclidean distance (km) from the municipality's townhall to the coast. Source: elaborations on GIS.

*Time from Rome/Naples*: Hours needed to reach either Rome or Naples (decimal scale). Source: www.viamichelin.it

*Size*: Size of the municipal territory (square km). Source: ISTAT.

*Population*: Number of inhabitants. Year: 2012. When used as denominator to compute rates, the year matches with that of the respective numerator. Source: ISTAT.

*Graduates*: Number of graduates per 1,000 inhabitants more than six years old. Average over years 1981, 1991 and 2001. Source: ISTAT.

*Income tax base*: Income tax base over population. Euros. Average over years 2003-2006. Source: Ministry of Treasury.

*Residential property wealth*: Residential property tax base over population. Euros. Year: 2006. Source: Ministry of Interior.

*Houses served by aqueduct*: Percentage of houses served by aqueduct. Average over years 1981, 1991 and 2001. Source: ISTAT.

*Municipal personnel expenditure*: Per-capita municipal personnel expenditure. Euros. Average over years 1998-2011. Source: Ministry of Interior.

*Crimes*: Number of criminal events per 1,000 inhabitants. Sum over years 2004-2009. Source: Ministry of Interior.

*Violent crimes*: Number of murders, assaults, batteries, threats, insults and sexual crimes per 1,000 inhabitants. Sum over years 2004-2009. Source: Ministry of Interior.

*Other crimes*: Number of episodes of theft, robbery, extortion, usury, kidnapping, conspiracy, organized crime, money laundering, fraud, arson, damage, smuggling, drug, exploitation of prostitution, forgery, intellectual property right infringement, others, per 1,000 inhabitants. Sum over years 2004-2009. Source: Ministry of Interior.

*Mafia crimes*: Number of episodes of murders, extortion, organized crime, money laundering, arson, per 1,000 inhabitants. Sum over years 2004-2009. Source: Ministry of Interior.

*Referenda turnout*: Turnout rate at Italian referenda. Years: 1946 (institutional), 1974 (divorce), 1978 (public order and public funding of parties), 1981 (public order, life sentence, firearms license, abortion), 1985 (wage escalator) and average of 2009 (electoral law) and 2011 (local public services provision, water service tariffs, nuclear energy and criminal procedure). Source: Ministry of Interior.

*European elections turnout*: Turnout rate at 2004 and 2009 European elections. Average. Source: Ministry of Interior.

*Non-profit organizations*: Number of local branches of non-profit organizations (excluding Church-based voluntary organizations) per 1,000 inhabitants. Year: 2001. Source: ISTAT.

*Non-profit organizations' employees*: Number of employees in non profit organizations (excluding Church-based voluntary organizations) per 1,000 inhabitants. Year: 2001.

Source: ISTAT.

*Administrative inefficiency*: Number of municipal balance sheet parameters in the "structural deficit" range. Year: 2009-2011. Source: Ministry of the Interior.

*Literacy rate*: Percentage of residents, at least 6 years old, that could read. Years: 1921, 2001. Source: Istituto Centrale di Statistica and ISTAT.

*Distance to highway*: Euclidean distance in km from each municipality townhall to the closest Rome-Naples highway entry. Source: elaborations on GIS.

*Population density*: Number of inhabitants divided by the size of municipal territory. Year: 2012. Source: ISTAT.

*Town*: Dummy for municipalities with more than 15,000 inhabitants in 2012.

*Local labor markets (LLM)*: Categorical variable indicating the local labor market in which each municipality is included. Year: 2001. Source: ISTAT.

*Inflow/Outflow*: Number of immigrants/emigrants divided by the number of local residents. *Domestic* is for people coming from/going to other Italian municipalities. *Inflow from abroad / Outflow abroad* is for people coming from/going abroad plus other incoming/outgoing. Average over years 1931-2012 for municipalities in the province of Caserta, and over years 1958-2012 for the other municipalities. Source: ISTAT - Municipal Offices of Vital Statistics data.

*Municipal election turnout*: Turnout rate at municipal election. Years: 1865, 1993-2016. Source: Ministry of Interior. In particular, for 1865 elections we referred to "Statistica del Regno d'Italia. Elezioni politiche e amministrative. Anni 1865-1866"

*Mutual aid societies and cooperative organizations (members)*: Number of mutual aid societies and cooperative organizations (members), per 1,000 inhabitants present at the 1901 census. Year: 1904, substituting missing values with available data taken from the closest previous publication (1894, 1895, 1896, 1897, 1902, 1903). Source: Ministry of agriculture. "Statistica delle società di mutuo soccorso"

*Municipal self-administration expenditure*: Municipal self-administration expenditure over total expenditure. Year: 1912. Source: Municipal administration balance sheets. "Bilanci di previsione dei singoli comuni del Regno esposti nelle voci principali di entrata e di spesa distinte per categorie"

*Municipal financial autonomy*: Municipal financial autonomy over population present at the 1911 census. Year: 1912. Source: Municipal administration balance sheets. "Bilanci di previsione dei singoli comuni del Regno esposti nelle voci principali di entrata e di spesa distinte per categorie"

*Cassa del Mezzogiorno*: per capita private investment subsidies and public investment expenditure granted per municipality by the "Cassa del Mezzogiorno" in the period 1951-1992, excluding multi-municipality projects. Source: SVIMEZ.

TABLE A.1: List of municipalities in our sample by area

| North | (Prov.) | Center | (Prov.) | South | (Prov.) |
|---|---|---|---|---|---|
| Acuto | FR | Acquafondata | FR | Ailano | CE |
| Affile | Roma | Alvito (<25km) | FR | Alife | CE |
| Alatri | FR | Aquino | FR | Alvignano | CE |
| Amaseno | FR | Arce | FR | Baia e Latina | CE |
| Anagni | FR | Arpino | FR | Bellona | CE |
| Arcinazzo Romano (<25km) | Roma | Atina | FR | Caianello (<25km) | CE |
| Arnara | FR | Ausonia | FR | Calvi Risorta | CE |
| Bassiano | LT | Belmonte Castello (<25km) | FR | Camigliano | CE |
| Boville Ernica | FR | Broccostella (<25km) | FR | Cancello ed Arnone (<25km) | CE |
| Carpineto Romano | Roma | Campodimele | LT | Capriati a Volturno | CE |
| Castro dei Volsci | FR | Campoli Appennino | FR | Capua (<25km) | CE |
| Ceccano | FR | Casalattico | FR | Carinola | CE |
| Ceprano | FR | Casalvieri | FR | Casal di Principe | CE |
| Collepardo | FR | Cassino | FR | Casapesenna | CE |
| Cori | LT | Castelforte | LT | Castel di Sasso | CE |
| Falvaterra (<25km) | FR | Castelliri | FR | Castel Volturno | CE |
| Ferentino (<25km) | FR | Castelnuovo Parano (<25km) | FR | Castello del Matese | CE |
| Filettino (<25km) | FR | Castrocielo (<25km) | FR | Cellole | CE |
| Fiuggi | FR | Cervaro (<25km) | FR | Ciorlano | CE |
| Frosinone (<25km) | FR | Colfelice | FR | Conca della Campania | CE |
| Fumone | FR | Colle S. Magno (<25km) | FR | Dragoni | CE |
| Gavignano | Roma | Coreno Ausonio | FR | Falciano del Massico | CE |
| Giuliano di Roma | FR | Esperia (<25km) | FR | Fontegreca | CE |
| Gorga | Roma | Fondi | LT | Formicola (<25km) | CE |

TABLE A.1

Continued

| North | (Prov.) | Center | (Prov.) | South | (Prov.) |
|---|---|---|---|---|---|
| Guarcino | FR | Fontana Liri | FR | Francolise | CE |
| Jenne | Roma | Fontechiari (<25km) | FR | Gallo Matese (<25km) | CE |
| Maenza | LT | Formia | LT | Galluccio | CE |
| Monte S. Giovanni Campano | FR | Gaeta | LT | Giano Vetusto | CE |
| Montelanico | Roma | Gallinaro (<25km) | FR | Grazzanise | CE |
| Morolo | FR | Isola del Liri | FR | Letino | CE |
| Norma | LT | Itri | LT | Liberi | CE |
| Paliano | FR | Lenola (<25km) | LT | Marzano Appio (<25km) | CE |
| Patrica | FR | Minturno (<25km) | LT | Mignano Monte Lungo | CE |
| Piglio | FR | Monte S. Biagio (<25km) | LT | Mondragone | CE |
| Pofi | FR | Pastena | FR | Pastorano | CE |
| Priverno (<25km) | LT | Pescosolido | FR | Piana di Monte Verna | CE |
| Prossedi | LT | Picinisco | FR | Piedimonte Matese | CE |
| Ripi | FR | Pico | FR | Pietramelara (<25km) | CE |
| Roccagorga | LT | Piedimonte S. Germano (<25km) | FR | Pietravairano | CE |
| Roccasecca dei Volsci | LT | Pignataro Interamna | FR | Pignataro Maggiore | CE |
| Roiate | Roma | Pontecorvo | FR | Pontelatone | CE |
| S. Felice Circeo | LT | Posta Fibreno (<25km) | FR | Prata Sannita (<25km) | CE |
| Segni | Roma | Rocca d'Arce (<25km) | FR | Pratella (<25km) | CE |
| Sermoneta | LT | Roccasecca (<25km) | FR | Presenzano | CE |
| Serrone | FR | S. Biagio Saracinisco | FR | Raviscanina (<25km) | CE |
| Sezze | LT | S. Donato Val di Comino (<25km) | FR | Riardo | CE |
| Sgurgola (<25km) | FR | S. Giorgio a Liri | FR | Rocca d'Evandro | CE |
| Sonnino | LT | S. Giovanni Incarico | FR | Roccamonfina | CE |

TABLE A.1

Continued

| North | (Prov.) | Center | (Prov.) | South | (Prov.) |
|---|---|---|---|---|---|
| Strangolagalli | FR | S. Vittore del Lazio | FR | Roccaromana | CE |
| Subiaco (<25km) | Roma | S. Ambrogio sul Garigliano | FR | Rocchetta e Croce | CE |
| Supino | FR | S. Andrea del Garigliano | FR | S. Cipriano d'Aversa | CE |
| Terracina | LT | S. Apollinare | FR | S. Gregorio Matese (<25km) | CE |
| Torre Cajetani (<25km) | FR | S. Elia Fiumerapido | FR | S. Pietro Infine | CE |
| Torrice (<25km) | FR | S.ti Cosma e Damiano | LT | S. Potito S.nitico | CE |
| Trevi nel Lazio (<25km) | FR | Santopadre | FR | S. Prisco | CE |
| Trivigliano | FR | Settefrati (<25km) | FR | S. Tammaro | CE |
| Vallecorsa | FR | Sora (<25km) | FR | S. Maria Capua Vetere | CE |
| Vallepietra | Roma | Sperlonga | LT | S. Maria la Fossa | CE |
| Veroli | FR | Spigno Saturnia (<25km) | LT | S. Angelo d'Alife | CE |
| Vico nel Lazio | FR | Terelle (<25km) | FR | Sessa Aurunca (<25km) | CE |
| Villa S. Stefano | FR | Vallemaio | FR | Sesto Campano | IS |
| | | Vallerotonda (<25km) | FR | Sparanise | CE |
| | | Vicalvi (<25km) | FR | Teano | CE |
| | | Villa Latina | FR | Tora e Piccilli | CE |
| | | Villa S. Lucia (<25km) | FR | Vairano Patenora (<25km) | CE |
| | | Viticuso | FR | Valle Agricola | CE |
| | | | | Venafro | IS |
| | | | | Villa di Briano | CE |
| | | | | Villa Literno | CE |
| | | | | Vitulazio | CE |

*Note:* FR stands for province of Frosinone, LT stands for province of Latina, CE stands for province of Caserta and IS stands for province of Isernia. Municipalities within 25 km from the corresponding border are indicated in parenthesis.

## A.2 Specification robustness and covariates balancing across borders

We briefly comment on robustness and comparative exercises that we have conducted on our main RD analysis on contemporary outcomes as function of location across the new border. Results presented in Table 2.3 turn out to be robust to specifications without interaction (constant-effect polynomial assumption) or in coordinates. For all polynomial degrees, comparative estimates confirm that the interaction terms (which are generally statistically significant, at least up to quadratic forms) generally improve the fit. The interacted quadratic specification seems to be preferred by the data. Test statistics like the Akaike information criterion concentrate favor on the fully interacted quadratic specification: moving up one degree from quadratic does not generally improve these statistics and yields cubic coefficients which are rarely statistically significant (never in their dummy interaction). Therefore, it has been quite natural to adopt the fully interacted specification $d_i + d_i^2 + d_i T_i + d_i^2 T_i$ as baseline of $f(\text{geographic location}_i, T_i)$.

We also tackle spatial dependence of observations, as it may affect detected statistical significance. Actually, ignoring spatial autocorrelation should go, if anything, against our result of a significant border effect, as this would inflate the variance of estimated coefficients. Nevertheless, the bias from geographic proximity turns out to be quite negligible, as the Moran's Index on our RD residuals out of Table 2.3 estimates lies between 0.04 and 0.08, far from the [-1, 1] extremes of perfect autocorrelation.

Finally, we check the continuity assumption necessary to validate results out of each RD exercise, i.e. that all relevant variables (covariates) vary smoothly at each border. Denoting by $y_1$ and $y_0$ potential outcomes for treated and non-treated units, respectively, the requirement amounts to continuity of $E[y_1|d]$ and $E[y_0|d]$ at the border discontinuity. This would make sure that on each threshold or just around it we are comparing a treated observation to its appropriate untreated counterpart. In order to assess the plausibility of this assumption, Table A.2 and A.3 explore the balancing properties, around each of our thresholds, of a number of covariates. They report results from RD regressions (of the type in equation (1)) run using as dependent variables some dimensions of municipalities heterogeneity, from geographic to socio-economic characteristics, which might be driving the evolution of $y_1$ and $y_0$ and hence results in each RD. We use the baseline specification, a quadratic polynomial where the interaction makes sure that our evaluation takes place exactly at the border. According to Tables A.2 and A.3, no jump occurs for any of the variables considered for either distance bands, the RD's assumption of continuity in the covariates at each border is satisfied, and their results are reliable.

TABLE A.2: Balancing properties for the baseline covariates across the New Border

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Elevation | Dist. coast | Pop. | Time Rome | Time Naples | Dist. highway | Grad uates | Pop. density |
| Panel I. Municipalities within 40 km from the border (136 obs.) | | | | | | | | |
| Center | -12.75 | -0.79 | -2,137 | -0.12 | 0.12 | 0.43 | -0.003 | -202.7 |
| | (96.99) | (6.39) | (3,922) | (0.13) | (0.09) | (3.63) | (0.007) | (138.7) |
| $R^2$ | 0.16 | 0.18 | 0.03 | 0.49 | 0.61 | 0.13 | 0.03 | 0.28 |
| Panel II. Municipalities within 25 km from the border (74 obs.) | | | | | | | | |
| Center | -36.51 | -5.12 | -1,812 | 0.17 | 0.03 | 3.77 | -0.007 | 11.1 |
| | (120.74) | (7.70) | (3,934) | (0.16) | (0.12) | (4.76) | (0.009) | (65.3) |
| $R^2$ | 0.11 | 0.23 | 0.05 | 0.34 | 0.33 | 0.07 | 0.05 | 0.04 |

*Note:* Estimated coefficients on "Center" dummy from regressions on fully interacted quadratic polynomials in Euclidean distance from the municipality's townhall to the border. "Center" equals 1 if the municipality is in *Center*, 0 if it is in *South*. Constant and algebraic distance coefficients not shown. Robust st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

TABLE A.3: Balancing properties for the baseline covariates across the Old Border

| | Dependent Variable | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Elevation | Dist. coast | Popu lation | Time Rome | Time Naples | Dist. highway | Grad uates | Pop. density |
| Panel I. Municipalities within 40 km of the border (127 obs.) | | | | | | | | |
| North | -117.0 | -8.71 | 3,100 | -0.12 | -0.14 | -1.61 | -0.01 | 5.0 |
| | (90.2) | (6.26) | (3,454) | (0.13) | (0.16) | (3.26) | (0.01) | (65.4) |
| $R^2$ | 0.23 | 0.25 | 0.07 | 0.40 | 0.57 | 0.22 | 0.13 | 0.03 |
| Panel II. Municipalities within 25 km of the border (88 obs.) | | | | | | | | |
| North | 124.9 | -0.23 | -3,045 | -0.10 | -0.08 | -2.60 | -0.01 | -99.7 |
| | (93.2) | (7.56) | (4,278) | (0.15) | (0.19) | (4.25) | (0.01) | (87.3) |
| $R^2$ | 0.24 | 0.23 | 0.09 | 0.40 | 0.51 | 0.29 | 0.13 | 0.04 |

*Note:* Estimated coefficients on "North" dummy from regressions on fully interacted quadratic polynomials in Euclidean distance from the municipality's townhall to the border. "North" equals 1 if the municipality is in *North*, 0 if in *Center*. Constant and algebraic distance coefficients not shown. The North sample excludes Latina, Pontinia and Sabaudia (see App. A). Robust st. err. in parentheses. *Significant at 10%; **Significant at 5%; ***Significant at 1%.

## A.3 The economic implications of the reform

We provide some detail on two main exogenous shocks with economic content which came along with the administrative reform: the creation of two new provinces (Frosinone and Latina) and the abolition of a third one (Caserta), and the huge campaign of public works (especially, marsh reclamation) associated to the colonization of the province of Latina.

At the end of 1926, Frosinone, with its 13,380 inhabitants, was only the sixth largest town of the province that it was going to head. It was lacking not only all kinds of buildings, facilities and services to serve its new role, but also basic premises like a secondary school (see Jadecola 2003). The news of the upgrade caused a great local movement, aimed at hosting all the new kind of institutions and organizations, from the largest to the smallest, that by law or default the new status of capital of province entailed. Just to mention a few, these included premises (and employees' housing) for: the Province Administration itself, the local representation of the Government (*Prefettura*), the provincial headquarters for a number of public and private institutions and services (Bank of Italy, Fascist Party, postal office, trade unions, tax collection, public health service).[24]

As for Latina, not only those institutions, but the city itself had to be created anew. In view of obvious large start-up needs, Mussolini himself - reporting to the Parliament on the law for the creation of the Province at the end of 1934 - highlighted the provision according to which the government would have taken charge of the financial unbalances of the new administration and its municipalities (often newly founded, as in the case of the province capital itself).[25] Data below will show that actually it did.

24. In an interview of March 1927, i.e., on the immediate aftermath of the reform, the new Prefetto gave an idea of these movements: "La prefettura è stata allocata nella sede della sottoprefettura. L'alloggio prefettizio è già in ordine. Mentre gli uffici amministrativi funzionano provvisoriamente nei locali del Commissariato di P.S. la Questura ha trovato conveniente sede, riordinata ed adattata allo scopo, nell'antica pretura, che è passata al vecchio municipio, il quale si è trasferito nella nuova degnissima sede recentemente completata (...) Sono anche ormai a posto la Federazione Provinciale Fascista, i Sindacati, il Patronato Nazionale, la Federazione degli Enti Autarchici, la Milizia, la Cattedra Ambulante di Agricoltura, la Divisione dei Regi Carabinieri, l'Amministrazione Provinciale. (...) Sono in corso le pratiche per il miglioramento degli uffici postali e per la costituzione della Direzione Provinciale delle Poste e Telegrafi. È già pronta la sede per l'Intendenza di Finanza. Gli altri uffici, a mano a mano che saranno qui costituiti, potranno avere conveniente sistemazione nei locali dove sono ora le scuole, le quali passeranno tra pochi mesi nel nuovo edificio scolastico, che potrà essere degnissima sede anche di istituti medii. Sono già innanzi le pratiche per la edificazione di case per impiegati, e si prepara il progetto del Palazzo di Giustizia che renderà disponibili gli attuali locali destinati a sede del Tribunale e della corte di Assise. La Giunta Provinciale Amministrativa è già in funzione; la commissione Provinciale annonaria già attende al suo compito. Il Comitato forestale è già nominato, la Commissione del Tiro a Segno è costituita, quella per gli orfani di guerra lo sarà tra giorni, e cosí tra poco funzioneranno il Consiglio Provinciale Sanitario e tutte le altre Commissioni stabilite dalle varie leggi" (Giuliani 1928). He was not mentioning a key organism, which he was chair of, the Consiglio Provinciale dell'Economia, a promoter of local economic development.

25. "L'articolo 4 pone sotto la speciale tutela del Ministero dell'Interno anche la provincia di Littoria

TABLE A.4: Real per capita Provinces expenditures and government transfers

| | Total expenditure | Gov.t transfers | Total expenditure | Gov.t transfers |
|---|---|---|---|---|
| | 1936 to 1939 | | 1946 to 1948 | |
| Latina | 28.3 | 7.2 | 34.1 | 3.4 |
| Frosinone | 24.4 | 2.8 | 26.6 | 2.1 |
| Caserta | 21.9 | 1.4 | 22.6 | 0.3 |

*Note:* Average over years. Values are expressed in liras per capita at 1938 constant prices. For years 1936-1939, unitary values for Caserta are those for the Provincial administration of Naples.

According to an historian, the creation of the two new provinces constituted "an attempt to give impulse to a stagnating local life by introducing the political and bureaucratic structures of the State and of the Fascist Party (prefetture, federazioni dei fasci, consigli provinciali dell'economia, sindacati, ispettorati e uffici di settore, opere assistenziali) to later assign them public investment".[26]

The investment flows to set and start up the new provincial institutions and organisms, corresponded by parallel expenditure for their ordinary business (including personnel expenditure), overall generated an increase in all branches of local economy. By contrast, the economy of the Caserta area received the same kind of shock but with a negative sign, until its reinstitution on a smaller scale at the end of 1945.

In order to try to grasp a quantitative flavor of this asymmetric economic shock, we look at provincial administration balance sheets checking the territorial distribution of per capita local public expenditure and government transfers in the years after the reform. However, available data are affected by three problems: first, in those immediate years the comparison of Latina and Frosinone can only be made with the wider province of Naples (in which the remainder of that of Caserta was melted), highly conditioned by its capital city; secondly, after the reform, 1936 is the first year for which balance sheet data were published (budgeting is instead available already for 1928 and 1935); last, the war soon interrupted the series. With these constraints in mind, in the first two columns of Table A.4 we can see that, in the 1936-1939 average, real per capita expenditure in Latina and Frosinone was equal to 28 and 24 liras, respectively; in Naples was 22. Real

e i Comuni assegnati alla sua circoscrizione sprovvisti dei mezzi necessari per raggiungere il pareggio dei proprio bilanci. (...) Per assicurare il funzionamento dei detti Enti, lo stesso art. 4 dispone che venga ad essi corrisposto dall'Opera Nazionale Combattenti un contributo integrativo da determinarsi con poteri discrezionali dal Ministero dell'Interno." *Atti Parlam. Legisl. XXIX, Camera dei Deputati, doc. 254.*

26. Our translation from Musci (1996, 129-130), who later concludes that "Gli anni Trenta e Sessanta sono quelli in cui il rafforzamento dell'iniziativa statale nei flussi di investimento permette un aumento sensibile degli interventi di trasformazione infrastrutturale e produttiva del territorio".

TABLE A.5: Works for marsh reclamation realized in concession in 1938 per province

| | Total Amount (million liras) | Per-capita Amount (liras) |
|---|---|---|
| Latina | 599.4 | 2,244 |
| Frosinone | 5.6 | 12 |
| Caserta | 81.4 | 149 |

*Note:* Per-capita amounts are based on population at 1943, as this is the first year for which a population referred to the province of Caserta is available after its abolition.

government transfers were more unbalanced, being 7 liras per capita in Latina, 3 in Frosinone, and only 1 in Naples. The table uses the Bank of Italy public administration value added deflator (Baffigi 2013) and ISTAT inter-census population per year, but this kind of ranking is robust to a number of deflationary and population measures. As the right-hand side couple of columns in the table show, the ranking applies also when looking at immediate post-war data, when the Province of Caserta was re-established.

Another big source of public works was the one that accompanied, and followed, the reform, namely the huge campaign of marsh reclamation that made living and farming possible where earlier it was not. This involved also the province of Caserta, but it happened with a unique intensity in that of Latina, whose very existence is due to that campaign. Works were either realized directly by the State, or —as it was most the case— given in concession to other entities. Based on Tassinari (1939) we can have a detailed picture of the distribution of the provincial per-capita expenditure for reclamation work realized in concession as of July 1938. Table A.5 shows that reclamation expenditures were much more intense in the province of Latina, where the ensuing creation of land value was therefore extraordinary.

# Appendix B

In what follows we describe in more detail how we built the dataset. First, computing non-employment duration was not straightforward, since very short-term contracts do not reset, just suspend, the non-employment duration counter. The time limit necessary to consider a period as "short" changed repeatedly during time and across areas. Therefore, the calculation of unemployment duration had been subject to several changes between 1990 and 2012. Between 2002 and 2012 the suspension was granted for temporary contracts shorter than 8 months (lowered to 4 months for individuals aged 25 or younger, 30 years if college graduate). The rule was simplified to contracts shorter than 6 months by law 92/2012, and slightly modified to include contracts of exactly 6 months by law 76/2013. For some Southern regions the rule remained 4 or 8 months (for younger than 25, 30 if college graduate, or older, respectively) during the whole period, and therefore we prefer it.

Moreover, since our goal is to identify, as precisely as possible, long-term non-employed who are eligible for the subsidy, we exclude from the sample workers who had self-employment events, as these workers follow other rules concerning the computation of unemployment duration. We also exclude from the definition of permanent job contracts those relative to domestic workers hired by households, those in the agricultural sector, agency workers and work-sharing agreements, as they are not subject to the policy measures.

As we do not know workers' job history before 2009, we need, for each individual, a starting point in which non-employment duration is equal to zero, in order to avoid the problem of left censoring. For this reason, we select only workers that, between 2009 and 2013, experienced the termination of a job lasting more than 6 months (4 or 8 months in the South).[27] Starting from the first job loss that satisfies these requirements, we track the individual over the following years. We increase non-employment duration by one week in every following Monday, as long as she does not find a new job.[28] Non-

27. The contracts may have started at any point in time before 2009, because the sample includes any contract that was subject to a change (firing and termination included) since 2009, independently from its starting date.

28. One issue is that individuals may have other part-time jobs that we do not observe because they started before 2009 and they were neither changed nor terminated thereafter. We cannot address this problem with the available data. According to the Italian Labour Force Survey, in 2009-2015 only 0.6 per cent of employees had more than one employment contract.

TABLE B.1: Percentage of permanent contracts receiving the subsidy in 2014, by eligibility status defined according to three different rules for computing non-employment duration

| | Eligible (1) | | Non-eligible (1) | |
|---|---|---|---|---|
| Rules (2) | South | Center-North | South | Center-North |
| Baseline | 66.31 | 16.97 | 21.93 | 2.31 |
| Simplified | 65.92 | 15.60 | 23.06 | 2.42 |
| Income | 72.05 | 16.16 | 20.87 | 1.34 |

*Note:* (1) We define as eligible those individuals with non-employment duration between two and three years, and as non-eligible those with non-employment duration between 6 and 18 months. (2) The baseline rule defines as short-term contracts, that suspend the non-employment duration counter, those shorter than 6 months in Centre-North and 4/8 months in the South. The simplified rule defines as short-term contracts those shorter than six months everywhere. The income rule uses the same definition as the baseline rule, plus an additional condition on income (the income earned by the worker must not exceed 8.000 euro).

employment duration is kept constant if the individual finds a job lasting less than the time limit described above. If, instead, the contract exceeds these limits, non-employment duration is set back to zero until the individual loses her job again.

As explained in the main text, we define as control group those individuals with non-employment duration between 6 and 18 months. In this way we aim at minimizing the classification error, i.e. the percentage of hires that, according to our calculation of non-employment duration, should not be eligible for the subsidy but were nevertheless subsidized. We can use the information about the actual receipt of the subsidy in 2014 to understand how large this classification error is. In Table B.1 we focus on permanent contracts and look at the percentage of them which benefited from the subsidy granted by law 407/90. As expected, the fraction is larger in the South. More importantly, it is more than three times higher among those that we define as eligible, although it is still not negligible among those that we assign to the control group. Hence, despite the classification error, the distinction by predicted eligibility status is still informative. A simplified rule, where we consider as short periods of employment those below 6 months for everyone irrespective of geographic area of work, leads to a higher classification error. Using instead a more complex rule —where we also account for the low-income limit— would improve precision, but we would lose a sizeable amount of observations for which the information about wage is not available. Hence, we prefer to focus on the baseline rule in the main text, but we also show that our results are robust to changing the definition (see Tables B.2-B.3).

We also used a more restrictive definition for the non-eligible group, reducing the non-employment duration window to 12-18 months, in order to select individuals resembling the eligible group more closely (symmetrically, we restrict also the window defining the treatment group to 24-30 months, instead of 24-36). In this case the percentage of

wrongly attributed hires to the control group is higher. Nevertheless, results (Table B.2) are qualitatively equal to those reported in Table 3.4. The odds ratio of the triple interaction is even lower, suggesting therefore an even stronger effect of law 407/90.

In Tables B.5 and B.6 we perform a further robustness check by estimating the same model as in Table 3.4 with the difference that the non-employment duration enters with a more flexible specification: instead of a logarithmic function, we first estimate a third order polynomial function (Table B.5), then a piecewise constant function (Table B.6), where we partitioned non-employment duration into ten intervals using deciles of the distribution as cut-points (which happened to fall at week 35, 43, 52, 61, 70, 106, 117, 129, 142), and defined a dummy variable for each of them, assuming the hazard rate to be constant within intervals, and allowing it to differ between them. Our main results are not affected by the choice of the functional form.

Finally, in Tables B.7 and B.8 we report results for the split sample by gender. The effect of the policy seems milder for females.

TABLE B.2: Logit model — Odds ratios. Income rule

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.7361*** | 0.7373*** |
| | (0.0367) | (0.0369) |
| LTU | 0.6575*** | 0.7028*** |
| | (0.0578) | (0.0621) |
| 2015 | 1.7675*** | 4.1009** |
| | (0.0749) | (2.4038) |
| South | 1.5622*** | 5.1591** |
| | (0.0704) | (3.3998) |
| LTUx2015 | 1.3875*** | 1.3285*** |
| | (0.1213) | (0.1173) |
| LTUxSouth | 2.0137*** | 1.9277*** |
| | (0.1883) | (0.1814) |
| 2015xSouth | 0.8648** | 0.2444 |
| | (0.0547) | (0.2197) |
| **LTUx2015xSouth** | **0.6127*** ** | **0.5977*** ** |
| | **(0.0716)** | **(0.0704)** |
| Constant | 0.0086*** | 0.0001*** |
| | (0.0017) | (0.0001) |
| Observations | 1,646,053 | 1,644,079 |
| Pseudo R-squared | 0.0113 | 0.0549 |

*Note:* Cluster-robust standard errors, where clusters are made of 54,396 (first column) and 54,353 (second column) classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and South dummies. 1974 observations are dropped because, among jobseekers in "International organizations and Public administration" in 2015 in Center-North and "energy and extraction" sectors in 2015 in South, nobody gets a permanent job.

TABLE B.3: Logit model — Odds ratios. Simplified rule

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6351*** | 0.6449*** |
| | (0.0159) | (0.0162) |
| LTU | 0.8461*** | 0.8710*** |
| | (0.0349) | (0.0361) |
| 2015 | 1.7727*** | 1.2661 |
| | (0.0382) | (0.3886) |
| South | 1.5754*** | 1.326 |
| | (0.0372) | (0.4758) |
| LTUx2015 | 1.2084*** | 1.1685*** |
| | (0.0491) | (0.0477) |
| LTUxSouth | 1.5875*** | 1.5649*** |
| | (0.0705) | (0.0698) |
| 2015xSouth | 0.8573*** | 0.6483 |
| | (0.0274) | (0.3174) |
| **LTUx2015xSouth** | **0.6652*** ** | **0.6742*** ** |
| | (0.0380) | (0.0388) |
| Constant | 0.0150*** | 0.0007*** |
| | (0.0015) | (0.0002) |
| Observations | 6,725,038 | 6,725,038 |
| Pseudo R-squared | 0.0116 | 0.0388 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,081 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE B.4: Logit model — Odds ratios. Restricted non-employment duration intervals

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6132*** | 0.5968*** |
| | (0.0593) | (0.0579) |
| LTU | 0.8540** | 0.8903 |
| | (0.0629) | (0.0657) |
| 2015 | 1.7258*** | 1.1881 |
| | (0.0571) | (0.5173) |
| South | 1.5837*** | 1.0379 |
| | (0.0576) | (0.5197) |
| LTUx2015 | 1.2691*** | 1.2419*** |
| | (0.0709) | (0.0696) |
| LTUxSouth | 1.7388*** | 1.7344*** |
| | (0.1050) | (0.1050) |
| 2015xSouth | 0.9252 | 0.3354 |
| | (0.0457) | (0.2323) |
| **LTUx2015xSouth** | **0.5994*** ** | **0.6007*** ** |
| | **(0.0472)** | **(0.0474)** |
| Constant | 0.0178*** | 0.0011*** |
| | (0.0072) | (0.0006) |
| Observations | 3,385,610 | 3,385,610 |
| Pseudo R-squared | 0.0107 | 0.035 |

*Note:* Cluster-robust standard errors, where clusters are made of 176,319 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE B.5: Logit model — Odds ratios. Duration dependence: third order polynomial

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Unemployment duration | 0.9906* | 0.9924 |
| | (0.0055) | (0.0055) |
| Unemployment duration squared | 1.0000 | 1.0000 |
| | (0.0001) | (0.0001) |
| Unemployment duration cubed | 1.0000 | 1.0000 |
| | (0.0000) | (0.0000) |
| LTU | 0.9916 | 1.0272 |
| | (0.0769) | (0.0798) |
| 2015 | 1.7594*** | 1.1219 |
| | (0.0380) | (0.3423) |
| South | 1.6772*** | 1.1826 |
| | (0.0395) | (0.4204) |
| LTUx2015 | 1.1854*** | 1.1463*** |
| | (0.0477) | (0.0463) |
| LTUxSouth | 1.5357*** | 1.5196*** |
| | (0.0671) | (0.0666) |
| 2015xSouth | 0.8513*** | 0.4642 |
| | (0.0273) | (0.2261) |
| **LTUx2015xSouth** | **0.6875*** ** | **0.7046*** ** |
| | **(0.0389)** | **(0.0401)** |
| Constant | 0.0039*** | 0.0002*** |
| | (0.0005) | (0.0001) |
| Observations | 6,723,060 | 6,723,060 |
| Pseudo R-squared | 0.0119 | 0.0383 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE B.6: Logit model — Odds ratios. Duration dependence: piecewise constant function

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Unemployment duration 1 | 1.8120*** | 1.7741*** |
| | (0.1121) | (0.1100) |
| Unemployment duration 2 | 1.7493*** | 1.7046*** |
| | (0.1092) | (0.1065) |
| Unemployment duration 3 | 1.5107*** | 1.4872*** |
| | (0.0945) | (0.0931) |
| Unemployment duration 4 | 1.4759*** | 1.4689*** |
| | (0.0925) | (0.0922) |
| Unemployment duration 5 | 1.3160*** | 1.2938*** |
| | (0.0831) | (0.0818) |
| Unemployment duration 6 | 1.3700*** | 1.3479*** |
| | (0.0753) | (0.0742) |
| Unemployment duration 7 | 1.3380*** | 1.3235*** |
| | (0.0434) | (0.0430) |
| Unemployment duration 8 | 1.1296*** | 1.1164*** |
| | (0.0379) | (0.0375) |
| Unemployment duration 9 | 1.0640* | 1.0631* |
| | (0.0361) | (0.0361) |
| LTU | 0.7647*** | 0.7873*** |
| | (0.0470) | (0.0485) |
| 2015 | 1.7582*** | 1.1190 |
| | (0.0379) | (0.3414) |
| South | 1.6771*** | 1.1818 |
| | (0.0395) | (0.4201) |
| LTUx2015 | 1.1869*** | 1.1473*** |
| | (0.0477) | (0.0464) |
| LTUxSouth | 1.5359*** | 1.5197*** |
| | (0.0671) | (0.0666) |
| 2015xSouth | 0.8513*** | 0.4635 |
| | (0.0273) | (0.2257) |
| **LTUx2015xSouth** | **0.6873*** ** | **0.7046*** ** |
| | **(0.0389)** | **(0.0401)** |
| Constant | 0.0016*** | 0.0001*** |
| | (0.0001) | (0.0000) |
| Observations | 6,723,060 | 6,723,060 |
| Pseudo R-squared | 0.012 | 0.0384 |

*Note:* Cluster-robust standard errors, where clusters are made of 221,176 classes in which individuals don't change their eligibility status, are in parentheses. The dummy "Unemployment duration 10" has been excluded to avoid perfect collinearity. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, gender, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE B.7: Logit model — Odds ratios. Male sub-population

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.6176*** | 0.6339*** |
| | (0.0188) | (0.0193) |
| LTU | 0.8444*** | 0.8784** |
| | (0.0427) | (0.0446) |
| 2015 | 1.6889*** | 1.1157 |
| | (0.0452) | (0.4365) |
| South | 1.5131*** | 0.8762 |
| | (0.0432) | (0.3998) |
| LTUx2015 | 1.1899*** | 1.1471*** |
| | (0.06 00) | (0.0582) |
| LTUxSouth | 1.6405*** | 1.5913*** |
| | (0.0881) | (0.0857) |
| 2015xSouth | 0.9029*** | 0.4273 |
| | (0.0353) | (0.2714) |
| **LTUx2015xSouth** | **0.6684*** ** | **0.6866*** ** |
| | **(0.0466)** | **(0.0481)** |
| Constant | 0.0216*** | 0.0010*** |
| | (0.0026) | (0.0003) |
| Observations | 3,609,022 | 3,609,022 |
| Pseudo R-squared | 0.0119 | 0.0318 |

*Note:* Cluster-robust standard errors, where clusters are made of 119,277 classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies.

TABLE B.8: Logit model — Odds ratios. Female sub-population

| Dep. variable: Dummy equal to one if the individual finds a permanent job in $t+1$ | | |
|---|---|---|
| | No controls | With controls (1) |
| Log (duration) | 0.7678*** | 0.7536*** |
| | (0.0337) | (0.0332) |
| LTU | 0.8150*** | 0.8399** |
| | (0.0568) | (0.0587) |
| 2015 | 1.9151*** | 1.0027 |
| | (0.0704) | (0.5247) |
| South | 1.8337*** | 1.0611 |
| | (0.0766) | (0.6764) |
| LTUx2015 | 1.1567** | 1.1223* |
| | (0.0773) | (0.0752) |
| LTUxSouth | 1.3993*** | 1.3833*** |
| | (0.1059) | (0.1049) |
| 2015xSouth | 0.7532*** | 0.9575 |
| | (0.0429) | (0.8263) |
| **LTUx2015xSouth** | **0.7445*** ** | **0.7572*** ** |
| | **(0.0727)** | **(0.0744)** |
| Constant | 0.0048*** | 0.0002*** |
| | (0.0008) | (0.0001) |
| Observations | 3,114,038 | 3,113,168 |
| Pseudo R-squared | 0.011 | 0.0416 |

*Note:* Cluster-robust standard errors, where clusters are made of 101,899 (first column) and 101,876 (second column) classes in which individuals don't change their eligibility status, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. (1) We control for nationality, education, age, age squared, sector (19 industries) of previous occupation, regional irregular work rate by macro sector in the previous year and monthly dummies, together with their simple and double interaction with 2015 and *South* dummies. 870 observations are dropped because, among jobseekers in "energy and extraction" sector in 2014 in Center-North, nobody gets a permanent job.

# Bibliography


Adamopoulou, E., and E. Viviano. 2018. "More Stable and Better Paid? The Effect of Hiring Subsidies on Wages."

Alesina, Alberto, and Paola Giuliano. 2015. "Culture and Institutions." *Journal of Economic Literature* 53:898–944.

Alesina, Alberto, Paola Giuliano, and Nathan Nunn. 2013. "On the Origins of Gender Roles: Women and the Plough." *The Quarterly Journal of Economics* 128:469–530.

Austin, Benjamin A., Edward L. Glaeser, and Lawrence H. Summers. 2018. *Jobs for the Heartland: Place-Based Policies in 21st Century America.* NBER Working Papers 24548. National Bureau of Economic Research, Inc.

Baffigi, Alberto. 2013. "Italian National Accounts, 1861-2011." In *The Oxford Handbook of the Italian Economy since Unification,* edited by Gianni Toniolo. Oxford: Oxford University Press.

Barone, Guglielmo, and Guido De Blasio. 2013. "Electoral Rules and Voter Turnout." *International Review of Law and Economics* 36:25–35.

Barone, Guglielmo, and Sauro Mocetti. 2011. "Tax Morale and Public Spending Inefficiency." *International Tax and Public Finance* 18:724–749.

———. 2016. "Inequality and Trust: New Evidence from Panel Data." *Economic Inquiry* 54:794–809.

Barone, Guglielmo, and Gaia Narciso. 2013. *The effect of organized crime on public funds.* Temi di discussione (Economic working papers) 916. Bank of Italy, Economic Research and International Relations Area.

Becker, Sascha O., Katrin Boeckh, Christa Hainz, and Ludger Woessmann. 2015. "The Empire is Dead, Long Live the Empire! Long-Run Persistence of Trust and Corruption in the Bureaucracy." *The Economic Journal* 126:40–74.

Bentolila, Samuel, and Marcel Jansen. 2016. *Long-term unemployment after the Great Recession: causes and remedies.* Edited by CEPR Press. London.

Bertarelli, Luigi V. 1940. *Campania.* Edited by Consociazione Turistica Italiana. Guida d'Italia. Milano.

Bisin, Alberto, and Thierry Verdier. 2017. *On the Joint Evolution of Culture and Institutions.* NBER Working Papers 23375. National Bureau of Economic Research, Inc.

Bruni, Francesco. 1984. *L'italiano. Elementi di storia della lingua e della cultura.* Edited by UTET. Torino.

Buggle, Johannes, and Ruben Durante. 2017. *Climate Risk, Cooperation, and the Co-Evolution of Culture and Institutions.* CEPR Discussion Papers 12380.

Buonanno, Paolo, Ruben Durante, Giovanni Prarolo, and Paolo Vanin. 2015. "Poor Institutions, Rich Mines: Resource Curse and the Origins of the Sicilian Mafia." *The Economic Journal* 125:F175–F202.

Cahuc, Pierre, Stéphane Carcillo, and Thomas Le Barbanchon. 2019. "The Effectiveness of Hiring Credits." *The Review of Economic Studies* 86 (2): 593–626.

Camussi, Silvia, and Anna Laura Mancini. 2016. *Individual trust: does quality of public services matter?* Temi di discussione (Economic working papers) 1069. Bank of Italy, Economic Research and International Relations Area.

Caputo, Eugenio, and Federico Romero. 1931. *Il Lazio.* 2nd edition. Edited by UTET. Torino.

Card, David, Jochen Kluve, and Andrea Weber. 2016. "Active labour market policies and long-term unemployment." In *Long-term unemployment after the Great Recession: causes and remedies,* edited by Samuel Bentolila and Marcel Jansen. London: CEPR Press.

———. 2018. "What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations." *Journal of the European Economic Association* 16 (3): 894–931.

Couttenier, Mathieu, and Marc Sangnier. 2015. "Living in the Garden of Eden: Mineral Resources and Preferences for Redistribution." *Journal of Comparative Economics* 43:243–256.

Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. "Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment." *The Quarterly Journal of Economics* 128 (2): 531–580.

D'Adda, Giovanna, and Guido De Blasio. 2017. "Historical Legacy and Policy Effectiveness: The Long-Term Influence of Preunification Borders in Italy." *Journal of Regional Science* 57:319–341.

De Blasio, Guido, and Giorgio Nuzzo. 2009. "Historical Traditions of Civicness and Local Economic Development." *Journal of Regional Science* 50:833–857.

Dell, Melissa. 2010. "The Persistent Effect of Peru's Mining *Mita*." *Econometrica* 78:1863–1903.

Di Liberto, Adriana, and Marco Sideri. 2015. "Past Dominations, Current Institutions and the Italian Regional Economic Performance." *European Journal of Political Economy* 38:12–41.

DiPasquale, Denise, and Edward L. Glaeser. 1999. "Incentives and Social Capital: Are Homeowners Better Citizens?" *Journal of Urban Economics* 45:354–384.

Doepke, Matthias, and Fabrizio Zilibotti. 2014. "Culture, Entrepreneurship, and Growth." Chap. 1 in *Handbook of Economic Growth,* edited by Philippe Aghion and Steven Durlauf, 2:1–48. Elsevier.

Epifanio, Vincenzo. 1931. *Campania.* 2nd edition. Edited by UTET. Torino.

Galasso, Giuseppe. 1996. "Dalla Terra di Lavoro alla Provincia di Caserta: Travaglio e Durata di un'Antica Circoscrizione Provinciale." In *Caserta e la sua Diocesi - Territorio, Istituzioni, Politica, Economia,* edited by Giuseppe De Nitto and Giuseppe Tescione. Napoli: ESI.

Giuliani, Sandro. 1928. *Le 19 Province create dal Duce.* Edited by Tipografia de Il Popolo d'Italia. Milano.

Giuliano, Paola, and Nathan Nunn. 2017. *Understanding Cultural Persistence and Change.* NBER Working Papers 23617. National Bureau of Economic Research, Inc.

Glaeser, Edward L., David Laibson, and Bruce Sacerdote. 2002. "An Economic Approach to Social Capital." *The Economic Journal* 112:F437–F458.

Glaeser, Edward L., and Charles Redlick. 2009. "Social Capital and Urban Growth." *International Regional Science Review* 32:264–299.

Glaeser, Edward L., Bruce Sacerdote, and Josè A. Scheinkman. 1996. "Crime and Social Interactions." *The Quarterly Journal of Economics* 111:507–548.

Gould, Eric D, and Alexander Hijzen. 2016. *Growing Apart, Losing Trust? The Impact of Inequality on Social Capital.* IMF Working Papers 16/176. International Monetary Fund.

Grassi, Emanuele. 2009. "The Effect of EPL on the Conversion Rate of Temporary Contracts into Permanent contracts." *Giornale degli Economisti* 68 (2): 211–231.

Grompone, Adele, Giuseppe Perna, and Luca Sessa. 2018. "Does Local Public Services Provision affect Culture?"

Gruber, Jonathan. 1994. "The Incidence of Mandated Maternity Benefits." *American Economic Review* 84 (3): 622–41.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2004. "The Role of Social Capital in Financial Development." *American Economic Review* 94 (3): 526–556.

———. 2016. "Long-Term Persistence." *Journal of the European Economic Association* 20:1–36.

Heckman, James J., Robert J. Lalonde, and Jeffrey A. Smith. 1999. "The economics and econometrics of active labor market programs." Chap. 31 in *Handbook of Labor Economics,* edited by O. Ashenfelter and D. Card, 3:1865–2097. Elsevier.

Jacob, Marcus, and Marcel Tyrell. 2010. "The Legacy of Surveillance: An Explanation for Social Capital Erosion and the Persistent Economic Disparity between East and West Germany." *SSRN Electronic Journal.*

Jadecola, Costantino. 2003. *Nascita di una provincia.* Edited by Le Tre Torri. Roccasecca.

Jenkins, Stephen P. 2005. "Survival analysis." Institute for Social and Economic Research, University of Essex, Colchester, UK.

Machin, Stephen, and Alan Manning. 1999. "The causes and consequences of longterm unemployment in Europe." Chap. 47 in *Handbook of Labor Economics,* edited by O. Ashenfelter and D. Card, 3:3085–3139. Elsevier.

Manning, Alan. 2009. "You can't always get what you want: The impact of the UK Jobseeker's Allowance." *Labour Economics* 16 (3): 239–250.

Meyer, Bruce. 1995. "Natural and Quasi-experiments in Economics." *Journal of Business & Economic Statistics* 13 (2): 151–61.

Miguel, Edward, Paul Gertler, and David I. Levine. 2006. "Does Industrialization Build or Destroy Social Networks?" *Economic Development and Cultural Change* 54 (2): 287–317.

Musci, Leonardo. 1996. "Il Lazio contemporaneo: regione indefinita, regione indefinibile." In *Atlante Storico-Politico del Lazio,* edited by Regione Lazio. Roma-Bari: Laterza.

Nunn, Nathan, and Leonard Wantchekon. 2011. "The Slave Trade and the Origins of Mistrust in Africa." *American Economic Review* 101 (7): 3221–52.

Pasquini, Alessandra, Marco Centra, and Guido Pellegrini. 2018. *Long-Term Unemployed hirings: Should targeted or untargeted policies be preferred?* Papers 1802.03343. arXiv.org.

Petrongolo, Barbara. 2009. "The long-term effects of job search requirements: Evidence from the UK JSA reform." *Journal of Public Economics* 93 (11-12): 1234–1253.

Putnam, Robert D. 1993. *Making Democracy Work. Civic Traditions in Modern Italy.* Edited by Princeton University Press. Princeton, NJ.

Sestito, Paolo, and Eliana Viviano. 2018. "Firing costs and firm hiring: evidence from an Italian reform." *Economic Policy* 33 (93): 101–130.

Tabellini, Guido. 2010. "Culture and Institutions: Economic Development in the Regions of Europe." *Journal of the European Economic Association* 8 (4): 677–716.

Tassinari, Giuseppe. 1939. *La bonifica integrale nel decennale della legge Mussolini.* Roma.

Voigtländer, Nico, and Hans-Joachim Voth. 2012. "Persecution Perpetuated: The Medieval Origins of Anti-Semitic Violence in Nazi Germany." *The Quarterly Journal of Economics* 127 (3): 1339–1392.