Essays in Applied Economics

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"Something to do, someone to love, and something to hope for. . . "

A.Chalmers

Declaration

I hereby declare that except where specific reference is made to the work of others, the contents of this dissertation are original and have not been submitted in whole or in part for consideration for any other degree or qualification in this, or any other university. This dissertation is my own work, joint with others as specified in the text.

Gianluca Cerruti

Preface

This thesis is the result of the research carried out during the PhD course in Economics and Political Economy at the University of Genoa. The common thread that ties together the three different chapters that compose this thesis is given by the methods that were used. Although different, all the implemented methods are policy evaluation methods. Policy evaluation is an instrument of the economic policy which is often used in order to assess the effects of a public policy and/or program. This kind of analysis is usually conducted ex post. That is, to assess whether or not a program has been useful in achieving a particular objective. However, such a tool is not only useful in the context of public policy, but also for assessing the relationship between events, history and other phenomena. The advantage of using such tools lies in the fact that they are based on identification strategies that capture the causal effect of a specific policy and/or event on a given outcome. In doing so, the policy evaluation employs econometric and statistical strategies using a counterfactual approach, which allows to overcome the problems of the regression method, such as the selection bias. Policy evaluation methods include: randomization, instrumental variables (IV), regression discontinuity design (RDD), Matching and Difference in Differences (DiD). While it is indeed very important in the social sciences to assess the causal impact, it is quite complicated (if not impossible) to conduct randomized controlled experiments. This is due to organizational and logistical reasons, as well as ethical and moral ones. The other methods of policy evaluation mentioned above, on the other hand, allow the use of non-experimental data, such as observational or survey data, to obtain information about causal impact.

In the three chapters that make up this thesis, therefore, I will present three applications of the aforementioned methods relating respectively to: the role of history in the relation between migrant perceptions and votes for far-right parties, the impact of a labor market reform on the fertility and family formation intentions of young Italians, and finally the impact of the largest health care reform in American history (The Affordable Care Act) on Americans' time use.

Three topics that are different from each other but are somehow intertwined with the present and, each in a different way, provide policy suggestions.

More specifically, the first chapter is entitled:

1. Migrant Perceptions and Extreme Right Voting. The Role of Historic Sea Trade.[1]

In this chapter, we examine the connection between political ideologies and migrant perception. We test the hypothesis that a negative perception of migrants influences individuals' far-right political positioning. In order to address likely endogeneity issues, we rely on historical Genoese and Venetian trade routes to Africa between XI and XIV century. Having routes to Africa in the Middle Ages implied hosting slave communities, as well as communities of sailors who met Muslims in Islamic ports. Thus, it meant somehow being in contact with unalike people many years earlier than those who lived elsewhere. On this basis, we construct a set of measures related to the proximity of each individual's municipality of residence to the nearest Medieval port, calculated on the ancient Roman road network. Our models account for personal controls as well as historical, geographical and socio-economic municipal characteristics. Results suggest that historical ports play a significant role by shaping migrant perception affecting political positioning. We also test the persistence of history on electoral outcomes at the municipality level, using data from the 2018 Italian national elections. The outcome supports the main individual-level findings.

The second chapter was written during my visiting period at the European Commission Joint Research Centre, and is entitled:

2. Employment protection legislation and household formation: evidence from Italy.[2]

While many studies have investigated the determinants of household formation and fertility of young adults, only a few focused on the impact of employment protection legislation (EPL) on these outcomes. In this paper, we study the differentiated impact of the EPL reduction associated to the Jobs Act in 2015 in Italy on the household formation and fertility intentions of young Italians in various districts. To do this, we use data from a survey conducted on a sample of 18-34 years old for the years 2012, 2015, 2016 and 2017. The identification strategy exploits local variation in the level of efficiency of courts, measured in terms of average duration of proceedings, to assess the existence of within country and across district heterogeneity of the reform impact. Indeed, firing costs used to be relatively larger in those districts characterized by a larger duration of labor trials. The Jobs Act, by reducing firing costs, and modifying the autonomy of judges, should have had a larger impact in districts with less efficient courts. According to our results, the reform seems to have indirectly levelled out the fertility and household formation intentions of young Italians living

[1] Co-authored with Anna Bottasso, Maurizio Conti and Marta Santagata.

[2] Co-authored with Gianluca Mazzarella and Mauro Migliavacca.

in districts with more and less efficient courts.

The third chapter was written during my visiting period at the Paris School of Economics, and is entitled:

3. The effects of the Affordable Care Act on time use.[3]

In that chapter, through the analysis of the American Time Use Surveys daily diary data, we study the impact of the Affordable Care Act on the time allocation of childless adults focusing on two key pillars of the Affordable Care Act: Medicaid expansion and Tax Premium Subsidies. We take a triple differences-in-differences approach that hinges on income eligibility thresholds and cross states variation in the time of implementation of these two pillars, to conclude that individuals newly eligible to Medicaid reduced their labour supply at the intensive margin, while potential beneficiaries of Tax Credit Premium Subsidies increased their labour supply at the extensive margin. In particular, our estimates suggest that people newly eligible to Medicaid may reduce long working hours and spend lesser time waiting to and receiving care. Moreover, they perform more household chores and management tasks, and also dedicate more time to caring for individuals from other households and volunteering. In contrast, potential beneficiaries of Tax Credit Premium Subsidies reduce their leisure time, on average. The rationales for these findings are discussed and our results are set in perspective of earlier studies.

[3]Co-authored with Anna Bottasso, Maurizio Conti and Elena Stancanelli.

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CHAPTER 1
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Migrant Perception and Extreme Right Voting. The Role of Historic Sea Trade*

Abstract

This study examines the connection between political ideologies and migrant perception. We test the hypothesis that a negative perception of migrants influences individuals' far-right political positioning. In order to address likely endogeneity issues, we rely on historical Genoese and Venetian trade routes to Africa between XI and XIV century. Having routes to Africa in the Middle Ages implied hosting slave communities, as well as communities of sailors who met Muslims in Islamic ports. Thus, it meant somehow being in contact with unalike people many years earlier than those who lived elsewhere. On this basis, we construct a set of measures related to the proximity of each individual's municipality of residence to the nearest Medieval port, calculated on the ancient Roman road network. Our models account for personal controls as well as historical, geographical and socio-economic municipal characteristics. Results suggest that historical ports play a significant role by shaping migrant perception affecting political positioning. We also test the persistence of history on electoral outcomes at the municipality level, using data from the 2018 Italian national elections. The outcome supports the main individual-level findings.

Keywords: Political Ideology, Immigration, Cultural Transmission, Medieval Trade Sea Routes, Roman Road Network, Instrumental Variable.

JEL Classification: C26; D72; J13; N70; N90; O10; O12; P48.

*Joint with Anna Bottasso[a], Maurizio Conti[a], and Marta Santagata[a].
([a] University of Genoa, Department of Economics).

1 Introduction

The rise of the far-right parties strongly influenced European politics, particularly in the 2019 European Parliament elections but also in national and local polls.[1] In recent Italian national parliamentary elections, it is evident how far-right parties have risen on the Italian political scene. In this regard, consider that the first party expression of the extreme right, the *Lega* party, has grown from 4.09% of votes in the 2013 general election to 17.35% in the 2018 election. Furthermore, the other main Italian far-right party, *Fratelli d'Italia*, experienced the same growth, collecting only 1.96% in 2013 and then reaching 4.35% in 2018.[2] On top of that the subsequent electoral appointment, i.e. the European elections in 2019, the two parties obtained a total of more than 40% of votes.[3] This phenomenon has often been analyzed in the literature by relating the presence of migrants and the votes taken by extreme right-wing parties (see among others Harmon, 2018; Dustmann et al., 2019; Halla et al., 2017; Dinas et al., 2019). In this scenario, the Italian case is of great interest given the large influx of migrants in the last years. Consider that in the years between 2014 and 2017 more than 600,000 people reached Italian coasts. Given the scale of this phenomenon, the issue of immigration has become central to the Italian political debate. Extreme right-wing parties have used the anti-migrant rhetoric to create political opposition and, above all, gain consensus. The starting point of this study is the belief that the rise of extreme right-wing parties in Italy has been enabled by their ability to interpret and amplify (see Bove et al., 2019) individuals' fears towards immigrants.[4]

While the literature has mainly focused on the relation between the presence of migrants and votes for extreme right-wing parties, in this paper we aim at testing the hypothesis that, at the individual level, a negative migrant perception leads individuals to vote fot extreme right-wing parties.

To conduct our analysis we use an individual-level survey containing, among others, responses related to political positioning and perception of migrants, in the year 2017.[5] Investigating this relation, nevertheless, implies a difficult identification issue, since it might happen that the perception of migrants shapes political ideology, or viceversa. Indeed, it could also be argued that it is the political propaganda perpe-

[1]Among others, mention should be made of the performance in the respective national elections of the *Alternative for Deutschland* (AfD) in Germany, the *Austrian Freedom Party* (FPO) in Austria, and the *Rassemblement National* (called *Front National* until 2018) in France.

[2]The data refer to the percentages of votes obtained in the Chamber of Deputies and are available on the website of the Ministry of the Interior at: https://elezionistorico.interno.gov.it/. Notice that in 2013 the *Lega* party was named *Lega* party.

[3]More specifically, *Lega* party obtained 34.26% of the votes, while Fratelli d'Italia obtained 6.44%. Data source: https://elezionistorico.interno.gov.it/.

[4]In this context, another central point of their rhetoric has also been the attack on the European Union.

[5]The survey used is "Osservatorio Giovani" and is conducted by the Giuseppe Toniolo Institute for Advanced Studies.

trated by right-wing parties that has a negative impact on the perception of migrants. To overcome this endogeneity problem and to find the impact of the perception of migrants on the political positioning of each individual, we use an Instrumental Variable (IV) approach. In the spirit of Allport et al. (1954)'s contact theory, we use, as an instrument of individual perception of migrants, the distance of each individual's municipality of residence from the nearest Medieval port. Specifically, we consider the ports that in the Medieval Era had a trade route to Africa.[6] In the Middle Ages, the Mediterranean was a melting pot of contrasting cultures (Abulafia, 2011; Braudel, 1995). As a consequence, port cities had large communities of sailors, who usually met Muslims in the main Islamic ports, as well as "fondaci" (warehouses), which often served as lodgings for foreign merchants, by forming districts. Over and above that, having routes to Africa implied the existence in the port cities of slave communities that could represent up to 10% of the population. More generally, at that time, living in a city that had a port with a connection to Africa meant being in contact with different people many years earlier than those who lived elsewhere. In most cases, it was the first time they had contact with a non-white. Forbye, it is worth noting that this was happening at a time when society was much more closed and unaccustomed to differences.

We argue that our instrument is plausibly exogenous, conditional on controls, since the trade routes to Africa, largely Genoese and Venetian, were established in pursuit of purely commercial interests, without any ideological motivation. It is therefore unlikely that the ports were selected along unobserved dimensions correlated with today trust on islamic and african migrants. Nevertheless, we make the exogeneity statement conditionally on a rich set of geographic controls. Withal, our measure of distance from the Medieval ports is calculated on the ancient Roman road networks dating back to 117 A.D..[7] Roman roads are strongly predetermined (Dalgaard et al., 2018) and the literature has identified military reasons as the main purposes of Roman road construction (e.g. Garcia-López et al., 2015; De Benedictis et al., 2018). We then argue that the distance from a Medieval port, calculated on the ancient Roman road network, conditionally on a set of controls, can be considered exogenous.

Our main analysis is conducted using as dependent variable a dummy related to the far-right political positioning of the interviewed individuals. As the main independent variable we avail ourselves of individuals' self-reported perception of migrants.

[6]Specifically we refer to several sources to retrieve the most important sea routes (among other Shepard, 1926; Musarra, 2020; Lampman, 2018). For further details see Section 2.3. Note that in the rest of the paper, for the sake of brevity, we will also use the more generic term "Medieval port" to refer to Medieval ports that had trade routes to Africa.

[7]Roman roads represent one of the largest infrastructure investments in history. Given that no such large investments are documented in the Medieval Era and that the network remained basically unchanged, we use data on the Roman network provided by Talbert (2000) in the Barrington Atlas of the Greek and Roman world and digitized by McCormick et al. (2013) to calculate the distance of each municipality to the nearest Medieval port.

The instrument is constructed from three dissimilar configurations of Medieval ports and using different distance thresholds to define whether an individual lives far or near one of the aforementioned port. Notably, we mainly consider two separate cut-offs, i.e. 10 and 15 kilometers, to construct for each threshold a dummy variable. The rationale of this choice comes from the fact that during the Middle Ages the vast majority of people used to cover both shorter and longer distances by foot (Fonseca, 2000). Each model is always estimated taking into account personal and geographical controls. We also present estimates where we consider a full set of covariates, including among others historical and socio-economic variables.

Overall, our results suggest that the perception of migrants, as instrumented by proximity to a Medieval port, has a significant impact on today's political positioning. Principally, in the most robust specifications a negative perception of migrants would lead to at least twice the probability of having far-right positions. In some cases, this probability triples or even almost quadruples. What is more, returns suggest that probably the influence of a Medieval port on migrant perception wears off at the 15 km threshold.

Furthermore, to validate our outcome, we perform a comprehensive set of robustness tests. First, we construct two alternative sets of instruments, based on the distance to the coast and the distance to today's ports. We observe that the estimates, as expected, do not report any significant results. In this sense, we support the hypothesis that it is rather the presence of a Medieval port that impacts, through the perception of migrants, on the political positioning of individuals, and that being close to the sea or to a port city is not a sufficient condition to recover this relation. Then, we avail ourselves of alternative dependent variables. That is, we consider both a separate metric to construct the measure of political positioning and new variables related to voting intentions. In general, our results are confirmed. Finally, we create a "placebo" indicator of proximity to a Medieval port to avoid that our instrument can be spuriously correlated with the perception of migrants, and results confirm that the relation is not mechanical and automatic.

We then devote a section to the study of possible existence of heterogeneous effects, dividing the sample according to certain characteristics of the individuals and their municipality of residence. Chiefly, we employ information on educational attainment, social network use and volunteering activities. It turns out that having a university degree, or using social networks assiduously, as well as doing voluntary work, lead to interesting heterogeneous effects. For example, using cultural toponymy we test our main model using as a control group a sub-sample of municipalities with a strong cultural heritage of tolerance and openness to diversity. The rationale behind the choice of these characteristics as major heterogeneity factors lies in the conviction that they, albeit in contrasting ways, may modify the relation between proximity to a Medieval port and the perception of migrants today, as well as the impact of the

latter on political ideology.
The last part of this study proposes an analysis at the municipal level to investigate the relation between the proximity to a Medieval port and results in the 2018 national elections. We make use of the share of votes obtained by the right-wing coalition in the Italian national election in 2018 as dependent variable, and the proximity of the municipality to a Medieval port as the main explanatory variable. Controlling for geographic, socio-economic and historical characteristics of the municipality, as well as for a measure of social capital and for the presence of migrants, we find a positive and significant relation, supporting our main findings at individual-level.
To conclude, our study fits several strands of literature. First, it contributes to recent literature on the relation between the presence of migrants and votes for extreme right-wing parties. Our understanding of literature is that the evidence on migrants/refugees and voting is still mixed. Indeed, on the one hand, a first group of researchers find a positive impact of the presence of migrants/refugees on the number of votes obtained by anti-migrant, right-wing populist parties in various European countries (see among others Dustmann et al., 2019; Harmon, 2018; Dinas et al., 2019; Vasilakis, 2018; Halla et al., 2017; Barone et al., 2016; Bratti et al., 2020). In addition, in literature some studies find a heterogeneous effect of immigration on native support for anti-immigration parties based on the ethnic origins of immigrants (Coffé et al., 2007; Shvets, 2004; Mendez and Cutillas, 2014). In general, these studies suggest that it is mainly the presence of migrants from Africa that favours the success of extreme right-wing parties, while a lower or even zero effect is associated with the presence of migrants from other ethnic groups. On the other hand, another group of researchers find a negative or zero impact of the presence of migrants on the amount of votes obtained by anti-migrant and right-wing populist parties (see among others Altındağ and Kaushal, 2020; Fisunoğlu and Sert, 2019; Gehrsitz and Ungerer, 2017; Steinmayr, 2020). In any case, it is worth noting that the latter group of studies refers to non-European countries, while all the aforementioned analyses focused on EU countries support the hypothesis of a positive effect of the presence of migrants on the rise of extreme right-wing parties.
Moreover, the belief that our instrument is relevant and as a consequence a correlation between positive (negative) attitudes toward migrants and proximity (distance) from a Medieval port may still exist is in the path of the literature that has sought to establish a link between long term persistence of culture and disparate outcomes (see among others Acemoglu et al., 2001*a*; Tabellini, 2008; Durante, 2009; Algan and Cahuc, 2010; Nunn, 2012; Alesina et al., 2013; Guiso et al., 2016; Becker et al., 2016; Giuliano and Nunn, 2021). More specifically, our work fits in the literature on the long run persistence of (in)tolerance behaviours towards minorities (Voigtländer and Voth, 2012; Fielding, 2018; Schindler and Westcott, 2020).
Finally, this study is related to seminal work by Jha (2013), where the relation be-

tween Medieval ports and ethnic conflicts in assessed. Author finds that places that had ports operating in the Medieval period in South Asia exhibit a lower level of Indo-Muslin conflict in the 20^{th} century.[8]

The main novelty of this work is to investigate the relationship between the perception of migrants and political positioning, thus using a subjective aspect rather than the actual presence of migrants in a given territory to explain the rise of far-right parties. Moreover, we utilize a historical instrument that allows us to relate the presence of sea routes to Africa in the Middle Ages with the current perception of migrants.

The rest of the paper is organized as follows. Data are presented in Section 2. We show the preliminary Ordinary Least Squares (OLS) results in Section 3, while the empirical strategy and related IV outcomes are described in Section 4. Our robustness checks are illustrated in Section 5, while heterogeneous effects are shown in Section 6. Finally, Section 7 is devoted to our municipality level analysis. Conclusions are in Section 8.[9]

2 Data and Descriptive Statistics

This analysis mainly relies on data from the survey "Osservatorio Giovani", carried out by IPSOS for the "Giuseppe Toniolo Institute of Higher Education",[10] a compilation of national individual-level survey on a wide variety of topics. The objective of the database is to provide a comprehensive and detailed source of information on the new Italian generations and their connections with the transformations taking place in society in which they live. For the purposes of our research, we make use of the surveys for the year 2017. Indeed, although the questionnaire was also conducted in previous years, 2017 is the first year in which questions were added regarding trust, perceptions of migrants, voting intentions, which are the subject of this research. The survey also entails information on standard demographic characteristics, e.g. education, gender, age and marital status. In 2017, 3,034 young people, representative of the universe of reference (individuals between 20 and 35 years), participated in

[8] A more extensive and complete review of the literature is presented in Appendix A, with a specific focus on three separate strands. Section A.1 is devoted to the literature on the relation between the presence of migrants and votes to (extreme) right-wing parties. Section A.2 presents the relevant studies on the long run persistence of culture, institutions, and attitudes towards minorities. In Section A.3 we present some studies belonging to a more recent strand dedicated to the persistence of infrastructures, with particular attention to the ancient Roman road network and ports.

[9] Moreover, Appendix A report an extensive review of the literature, while Appendix B includes additional robustness and heterogeneity analysis, as well as additional historical maps.

[10] All the waves of the survey data form the "Rapporto Giovani" database. The "Rapporto Giovani" database contains all the data of the survey conducted on a sample of young people aged 18 to 34 years. Promoted by the Istituto di Studi Superiori Giuseppe Toniolo (in collaboration with the Università Cattolica del Sacro Cuore and with the support of Fondazione Cariplo and Intesa San Paolo) and carried out by Ipsos, the "Rapporto Giovani" is the most in-depth and extensive research on the world of youth in the last decade.

the survey. In carrying out our analysis, we take into account the sampling weights attributed to individuals.
The choice of this database is motivated by the uniqueness of the individual-level information contained within it. To the best of our knowledge, there is no other data source that provides information on both perception of migrants and political positioning for the Italian case. As far as the issue of external validity is concerned, we think that the validity of the results can also be extended to the entire population. In fact, young people are the segment of the population that is more social network addicted. By definition, social networks represent a channel to enter into contact with cultural contexts that are contrasting from those of their territory of origin. Over and above, the historical context and the opportunities for mobility peculiar to this generation also contribute to this process. In light of these considerations, we believe that if the relation put under scrutiny in this analysis does exist in the target sample, it is reasonable to think that it exists throughout the population.
Alongside individual data provided by IPSOS, we avail ourselves of information on geographical, socio-economic and historical characteristics of the municipality of residence of the individuals in our sample.
Finally, we use three separate cartographic sources to define the ports that had trade routes with Africa during the Middle Ages. The distance of each individual from one of these ports is calculated using the ancient Roman road network.
The next paragraphs are dedicated to explaining in detail the sources and variables involved in this research.

2.1 Political Positioning and Voting Intention

Using the survey "Osservatorio Giovani", we construct several dummy variables aimed at capturing the political positioning and voting intention of individuals.
To construct our main dependent variable, we consider the following question in the survey: "In politics, we often talk about "left" and "right". Considering your political beliefs, where would you place yourself?". The respondents are asked to choose among a 1 (left) to 10 (right) scale. We omit observations for which the respondents answered "I don't place myself anywhere, I don't care", and code values from 1 to 7 as 0, and from 8 to 10 as 1.[11] This variable is called *FarRightPositioning*.
We also consider two measures of the far right-voting intention, relying on a specific question that ask individuals how much they are likely to vote each party in the next parliamentary election in a scale from 1 ("I would definitively NOT VOTE for it") to 10 ("I would definitively VOTE for it"). First, we assign 1 to each individual that an-

[11]This choice is made in line with the fact that our analysis is focused on the relation of the far right-wings political positioning to the perception of migrants. We also construct an alternative dummy variable that takes value 1 when the respondents answered 9 or 10, and 0 elsewhere. This variable is called *FarRightPositioning2*.

swered at least 8 to one of the following political parties: *Lega*, *Fratelli d'Italia*, *Forza Nuova* (or other extreme right parties).[12] We code as 0 the answers from 1 to 7 or "I don't know". This variable is called *FarRightPartiesVotingInt*. Second, we assign 1 to each individual that answered at least 8 only to *Lega* party, the major far right political party in the Italian scenario. This variable is called *LegaPartyVotingInt*. We code as 0 the answers from 1 to 7 or "I don't know".
In some specifications, we make use of measures of progressive voting intention as the dependent variable. Specifically, to construct the variable called *LeftPositioning*, we use again the following question: "In politics, we often talk about "left" and "right". Considering your political beliefs, where would you place yourself?". The variable takes value 1 when an individual answered from 1 to 4, among a 1 (left) to 10 (right) scale, and 0 elsewhere. We omit observations for which the respondents answered "I don't place myself anywhere, I don't care". Using the same logic with which the *LegaPartyVotingInt* variable is constructed, we construct a variable called *DemocraticPartyVotingInt*, considering the answers individuals gave to the question: "How much they are likely to vote for *Democratic Party* in the next parliamentary election in a scale from 1 ("I would definitively NOT VOTE for it") to 10 ("I would definitively VOTE for it"). This variable takes on a value of 1 if the individual responded from 6 to 10 and 0 otherwise.

2.2 Migrant Perception

Also with regard to the perception of migrants, our source is the "Osservatorio Giovani" survey. Mainly, to construct our variable we utilize the following question: "Immigrants make Italy an unsafe place. How much do you agree, in reference to this statement?". Respondents could choose a response ranging from "Not at all agree" to "Very much agree". We code values as 1 if the answer to the question is "Very much agree", and 0 otherwise. In this way, we isolate only those who have a strongly negative migrant perception. This variable is called *MigrantPerception*. Among the questions related to the perception of immigrants we believe that, for the purpose of our research, this is the question that better proxies individuals' perception of migrants. Indeed, this choice is made in the light of the rhetoric that the political opposition to the Italian government in 2017 (the year of the survey) and 2018 has carried forward. Immigration has been a key topic in Italy's electoral campaign, and several candidates claimed that the flow of people into the country during

[12] These three parties can be classified as far-right parties in the Italian political scenario in 2017. In constructing this measure, we exclude the two center-right parties, *Forza Italia* and *Alternativa Popolare*, as they are conservative parties but not expressions of the extreme right. Other parties involved in the political scenario (and whose preference were requested by the survey) are the following: *Movimento 5 Stelle*, as an expression of a populist party with no collocation in the traditional right-left ideology; *Partito Democratico*, progressive center-left party; *Sinistra Italiana* and *Articolo 1*, expression of the far left.

that period had increased the risk of crime. In particular, the 2018 Eurobarometer on immigration depicts Italy as one of the European countries with the most negative opinions towards non-European immigrants, i.e. mainly those who immigrate from the coasts of North Africa.[13] In addition, according to data from the Pew research centre, Italy is the second European country most hostile towards Muslims after Hungary.[14] Equally important, the negative attitude towards migrants in the years 2017-2018 is clearly described by the association Lunaria, which on its website "Cronache di ordinario razzismo" holds a dataset that is fed by the reports of racist and xenophobic attacks that appeared in the press. The number of aggressions reported in 2017 was 557, while in 2018 grew again to 628. These numbers are much higher than in previous years.[15]

All the same, both the crime rate across Italian regions and the number of crimes committed by foreigners has dramatically decreased over the past decade (Di Carlo et al., 2018). In addition, if it is true that in the years between 2014 and 2017 more than 600,000 individuals crossed the Mediterranean to land in Italy, the year 2017 ended with the lowest number of migrants arriving by sea on the Italian coasts since the beginning of the massive flow of entries to Europe (119,000 landings in 2017 against 181,000 in the previous year). Moreover, Bove et al. (2019) find that immigration has led to an increase in public spending on security, but this is not due to an increase in crime rates but to the deterioration of social capital and an unjustified fear of crime. The subject of migration is one where perception and reality do not talk to each other. The 17^{th} annual National Institute of Social Security report[16] highlights how Italians overestimate the population of immigrants: in fact, Italy is the country with the greatest deviation between perception and reality on this issue. For this set of reasons, the variable related to whether immigrants make Italy an unsafe place appears the most interesting to us.

2.3 Ports and Routes in the Medieval Era

In the field of historical studies related to the Mediterranean Sea, one has to mention Ferdinand Braudel. His two-volume masterpiece (Braudel, 1995) (Braudel, 2017) is still a point of reference for anyone wishing to investigate any aspect of the history of

[13]For further details, see https://ec.europa.eu/commfrontoffice/publicopinion/index.cfm/Survey/.

[14]For further details, see https://www.pewresearch.org/fact-tank/2018/12/10/many-worldwide-oppose-more-migration-both-into-and-out-of-their-countries/.

[15]In 2011, for example, racist and xenophobic attacks reported by the website were 'only' 156. For more details, see http://www.cronachediordinariorazzismo.org/il-razzismo-quotidiano/. A further set of data is collected by the Office for Democratic Institutions and Human Rights (ODIHR) of the OSCE, the Organisation for Security and Cooperation in Europe.The data processed by ODIHR comes from OSCAD and the Ministry of Interior. These data also show an increasing trend of violent episodes against immigrants in Italy in 2016-2017-2018. For more information, see https://hatecrime.osce.org/italy?year=2018.

[16]For further details, see https://www.inps.it/nuovoportaleinps/default.aspx?itemdir=51978 (*17^{th} annual National Institute of Social Security report*, n.d.).

the Mediterranean. And it was Braudel himself who inaugurated the debate on the conceptual unity of the Mediterranean, to which we also refer in some way. Basically, what historians are asking themselves is whether the tendency of societies living in contact with the Mediterranean is unity or fragmentation. Certainly there was a time when traveling between one end of the Mediterranean and the other meant discovering worlds immensely unalike from one's own, and this can also be seen in the words of Abulafia (2011). By way of example, Abulafia (2011) tells of Benjamin of Tudela, a rabbi of the city of Navarre, who undertook his travels around 1160, wrote of Genoa, *"It is surrounded by walls and the inhabitants are not governed by a king, but by magistrates whom (the Genoese) appoint as they please"* and *"They have dominion over the sea."* In another passage of the book, he tells instead of another journey of Ibn Jubayr, who during a pilgrimage to Mecca, arrived for a stop on the coast of Sardinia and was impressed to see eighty people of his own faith, men and women, for sale in the market as slaves. From these few fragments (and many others could be added), three main points emerge: the enormous cultural differences, the importance of Genoese trade (and, as we shall see, that of Venice) and the presence of communities of African slaves in the major commercial and port areas of the peninsula. Of course, the basic question about the union or fragmentation of the Mediterranean remains open and we do not presume to give a definitive answer through this study. However, what we are trying to do is to evaluate whether the openness that trade routes with Africa brought, and, as a result, the presence of both slave communities and, more generally, black people, left a positive influence in terms of openness, tolerance and less fear of the other person, which still lasts today. Moreover, sailors who had relations with ports in North Africa transmitted what they experienced during their journeys, once they returned home.
In the light of the above, we have explored Genoese and Venetian trade relations in a period from the 11th to the 14th century. In addition, we have investigated the question of the presence of slaves in the various port cities covered by the analysis. Principally, in this historical period (XIII-XIV century) there was a situation where the second Venetian-Genoese war did not yet have a winner. With the treaty of Milan, both had renounced to the request of reparations for the losses suffered and had made an agreement of non-belligerence in their respective areas of influence: the Tyrrhenian Sea for Genoa (which contended the hegemony with Pisa) and the Adriatic Sea for Venice (which contended the hegemony with Zara). Conflicts that had characterized the period and that found their own reason in the necessity to preserve the commercial routes. Musarra (2020) recalls how the first half of 1300 was also dismayed by a strong demographic contraction as a direct result of famine and especially the great plague that halved the population in Genoa and Venice.[17] This demographic and

[17] As a result of the "Great Plague" of the first half of the fourteenth century, Genoa lost about 30,000 inhabitants of the 70,000 who had a few years earlier and Venice went from 120,000 inhabitants of

pestilence situation also changed the approach to maritime trade, especially through the construction of larger ships, the reduction of men on board and the tendency to shorten journey times and thus skip ports considered secondary. On the top of that, in the eleventh century during the voyages to the East the merchants stopped only the bare minimum to conclude business and then return. Whereas, following the first crusade and the conquest of the Syrian-Palestinian coast, Genoese and Venetians (and later also Pisans) had obtained privileges and even neighborhoods where they settled in a more stable manner, and where they created small communities that replicated the characteristics of their community of origin.[18] Accordingly, at the beginning of the fourteenth century the Venetian and Genoese colonial world was really variegated, and there were by now long and varied ways of traffic and scattered settlements for all the coasts of the Mediterranean. The trades were about the most varied goods, among the others cereals (essential for the subsistence), wine, oil and many others. Just these trades often drew further commercial routes. Genoa, for example, in addition to exporting oil from its own hills, brought oil from Seville, Gaeta, Naples and Gerba to the East. Obviously, there were also various materials that were purchased in the East and brought to the ports of the modern peninsula. As an example, think of the lead that Genoa imported from Constantinople, or cotton that the same Genoese bought in Egypt and the Venetians in Syria. But trade was not only about raw materials or finished products, but also about the slave trade. Olgiati and Zappia (2018) and Pistarino (1964) centre on the slave trade in Genoa, and find essentially three periods in which large numbers of slaves flowed into Genoa. The first is the period we are discussing and is the period between the 11^{th} and 13^{th} centuries. Genoa made many ships available to transport crusaders to the Near East, who then returned home laden with booty, goods trafficked on eastern markets and slaves taken from the Ottomans. In the same period the Genoese merchants arrived in the Black Sea, and even here gave life to the trade of slaves, which were used both in port and on galleys as rowers. The second period begins at the end of the thirteenth century and extends into the fourteenth. The third period of Genoese slavery, one of the most prosperous, occurred in the fifteenth century. At this point Genoa became a true maritime and financial power, thanks to its alliance with Spain during the Reconquista. According to Pistarino (1964), Genoa at that time was the main slave market in the Mediterranean. At that time not only aristocrats but also merchants, artisans and those who carried out liberal professions were used to keep one or more slaves at home. Some historians, including Pistarino (1964), through the analysis of documents of sale-purchase of slaves, estimate that the slaves reached

1338 to about 65,000 in 1351.

[18]In every port on the southern side of the Mediterranean that had commercial ties with the Genoese and Venetian powers, there was basically a public building, the seat of civil and judicial administration, where consuls and podestà resided. Furthermore, there were at least one church and a "loggia" where notaries carried out their services. Gradually in these communities developed a real civil life made of artisans, bankers, peasants, greengrocers, etc.

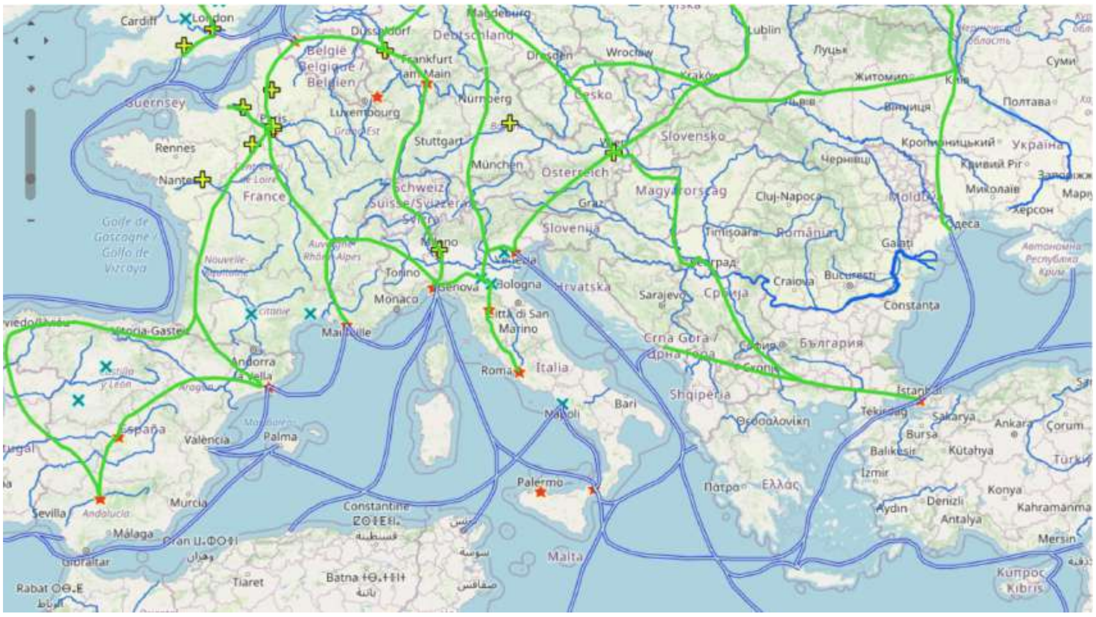
about 10% of the total population in Genoa, most of them blacks.
Although in smaller numbers, traces of human trafficking have also been found in Venice. In the documents of the notary Marco dei Raffanelli, active in Venice at the end of the 14^{th} century, 292 deeds of sale of slaves have been found.[19] We have focused mainly on Genoa and Venice given their pivotal role in the Middle Ages in the Mediterranean. On the other hand, the presence of trade routes with the East at that time also involved other cities[20] (as we will see in the following maps), and often, where there was a route with Africa, there was also a slave trade. There are also some examples of this phenomenon in other port cities of the time. In this regard, Campagna (2019) explains how even in Messina the activity of buying slaves covered a non-negligible share of trade. According to Pispisa and Tramontana (1987) Messina had a dominant role due to its geographic position and consequently its port was vastly significant. After the conquest of Palestine by the crusaders, the city constituted indeed the shortest and safest way to reach the Holy Land for travelers. Bresc (1986) defined Sicily, and especially Messina, as "a relay of commerce and a point of conjunction between two great supplies, two areas of slavery, the Eastern, Romaniote and Tartar worlds, and North African and African slavery".
In order to conduct our analysis, we study several historical maps which we found mainly in history books. From their consultation we selected ports that had a route to Africa. All the maps relate to trade and routes in the Mediterranean between the 11^{th} and 15^{th} centuries. Three separate configurations of ports emerge from our study and it appears reasonable to consider them for the purposes of our analysis. In Figure 1, we present the first port configuration: Genoa, Venice, Messina, Reggio Calabria, Naples and Amalfi.[21] In Figure 2 our second configuration is presented: Venice, Messina, Reggio Calabria, Amalfi, Naples, Palermo, Genoa, Pisa, Cagliari and Syracuse. Finally Figure 3 shows the last port configuration: Venice, Messina, Palermo, Reggio Calabria, Naples, Genoa, Gaeta, Livorno, Syracuse, Otranto and Porto Torres. From now on, we will call Map 1, 2 and 3 respectively the three maps presented in Figures 1, 2, and 3.

[19] This information was consulted at Enciclopedia Treccani, under the rubric "I meccanismi dei traffici" by Jean Claude Hocquet - Storia di Venezia (1997). Given the interest of Venice in the western Mediterranean and the Adriatic, the slaves were not only of African origin, but also Tatars, Caucasians and Mongols.

[20] Among other cities, see Varriale (2013) concerning the port of Naples and Loi (2015) concerning the port of Cagliari.

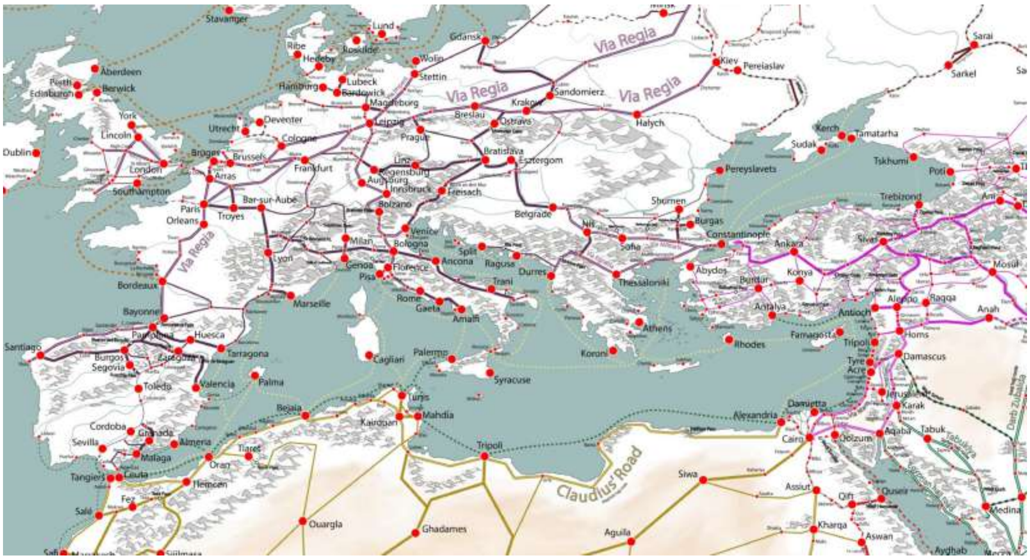
[21] We present in Appendix B two other maps that confirm this ports configuration.

Figure 1: Medieval Ports Configuration Map 1



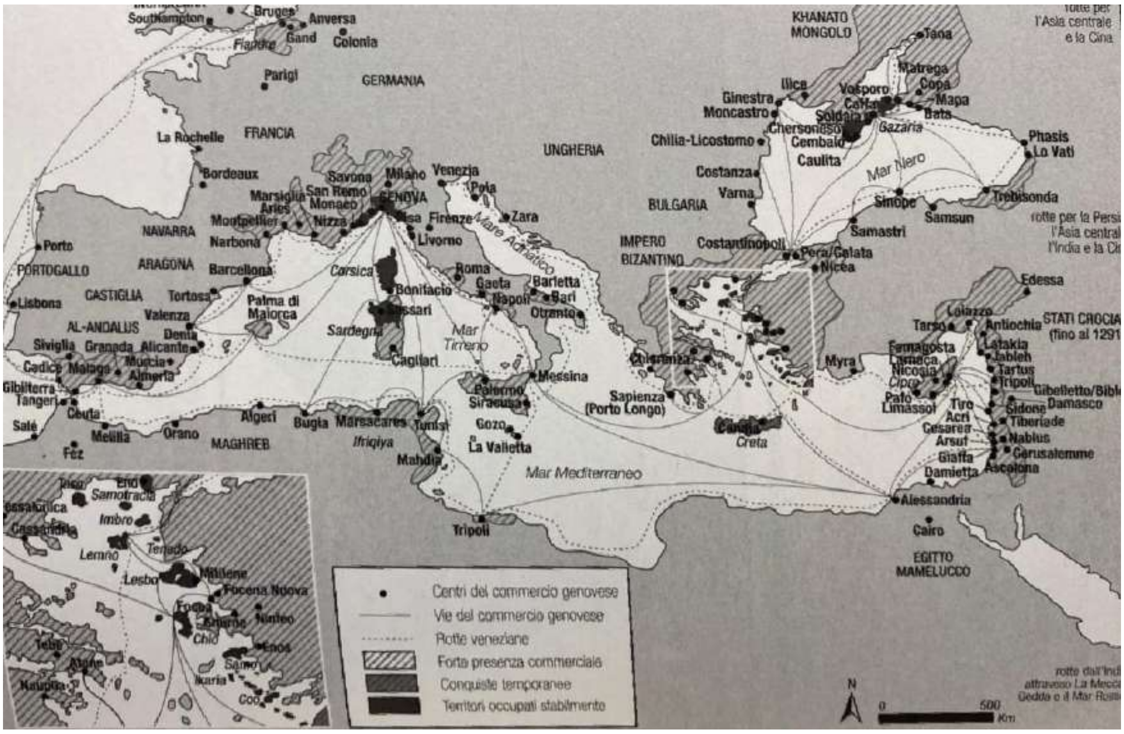
Notes: The source of the map is the Harvard WorldMap, available at http://worldmap.harvard.edu/maps/5080. The map provides a comprehensive summary of the Europe's trade networks through the Medieval Europe. The blue lines indicate the sea trade routes, while the green lines indicate the major land trade routes (both of the Medieval Europe). The red stars indicate the notable locations.

Figure 2: Medieval Ports Configuration Map 2



Notes: The author of the map is Martin Jan Mansson, and the map can be found at: https://kottke.org/. The map provides a comprehensive summary of the world's trade networks through the 11th and 12th centuries. Routes that helped connect kingdoms and traders in Asia, Africa and Europe. The first routes were traced by the great movements moved by events such as the first crusade in 1096. The dot lines indicate the sea routes, the red dots indicate the notable locations while the density of the lines indicate the status and volume of traffic.

Figure 3: Medieval Ports Configuration Map 3



Notes: The source of the map is Musarra (2020), re-elaboration of a map already appeared in Beneš et al. (2018). The map provides a comprehensive summary of the commercial expansion of Genoa and Venice between the thirteenth and fourteenth centuries. The black lines indicate the Genoese sea trade routes, while the black dotted lines indicate the Venetians sea trade routes. The black dots indicate the Genoese trade centers.

2.4 Distances in the Medieval Era

In this paper, we use the proximity to a Medieval port that had routes to Africa as an instrument of each individual's perception of migrants. More specifically, each individual in our sample is considered close to (far from) an ancient port if the distance from the centroid of his or her municipality of residence to the centroid of the nearest port is below (above) a given threshold. We first construct a matrix of distances, according to the Roman road network dating back to 117 A.D. between each individual place of residence and each Medieval port. Indeed, Roman roads represent one of the largest infrastructure investments in history. Given that no such large investments are documented in the Medieval Era and that the network has remained basically unchanged, we decide to make use of the distance on the Roman road network as a good proxy for the Medieval distances. Data on the Roman road network are provided by McCormick et al. (2013) and available in the Digital Atlas of Roman and Medieval Civilization (DARMC), i.e. the digitized version of the Barrington Atlas of the Greek and Roman world (Talbert, 2000).

We employ the digitized information on the Roman network and integrate it with shapefiles on municipal administrative limits provided by the Italian National Institute of Statistics (ISTAT). Then we combine it with information on the location of Medieval ports of interest. In Figure 4 we show an example of the resulting outcome, using the configuration of ports contained in Map 1. To calculate the distance matrix we compute the centroid for each municipality of residence and for each municipality where a Medieval port is located. Then, we are able to determine which is the closest port with respect to each individual place of residence. In this regard, the left panel of Figure 5 shows how each municipality of residence is associated with just one Medieval port, that is the nearest one.[22] It is worth noting that even though in the left hand panel of Figure 5 the lines connecting the centroid of the municipality of residence to the centroid of the municipality where the historical port was located are straight-lines, the nearest Medieval port is found using the distance along the Roman network as shown in the right panel of Figure 5. Following Flueckiger et al. (2019), when the centroid of a municipality is out of the network, we connect it to the Roman road network by creating an artificial straight-line road segment between the centroid and the closest point on the network.

Once the nearest port is calculated, we construct a dummy variable that takes value 1 if the individual is close to a Medieval port and 0 if the individual is far away, according to distinct distance thresholds, i.e. 10 and 15.[23] The rationale behind this choice is in line with the fact that during the Middle Ages the vast majority of people

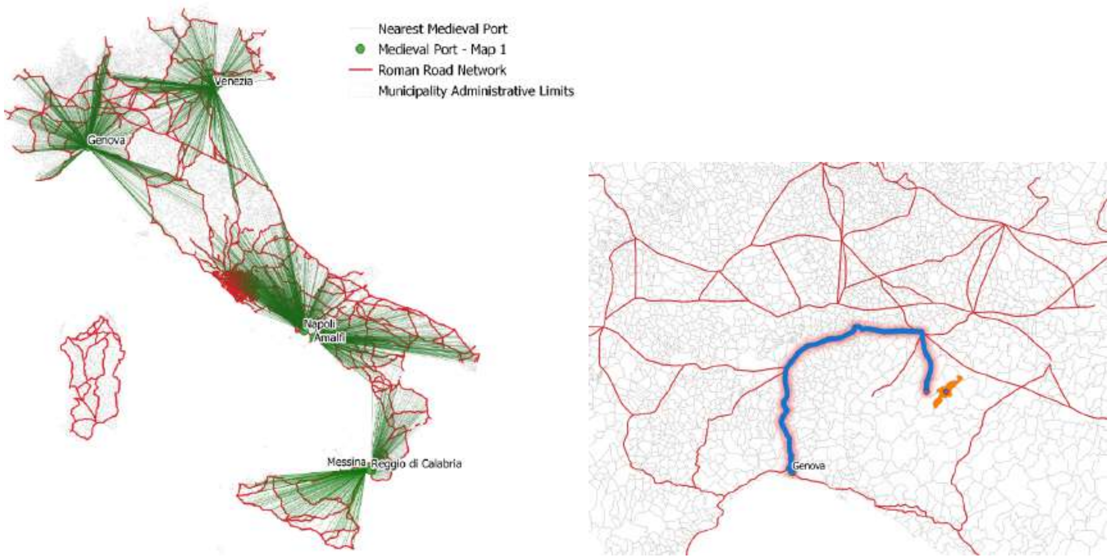
[22]For illustrative purposes, this figure includes Medieval ports as identified in Map 1 , but to construct our proximity measure, the distance matrix is recalculated using the three separate port configurations as explained in Section 2.3.

[23]In some specification we use 5 and 20 kilometers thresholds.

Figure 4: Medieval Ports Configuration

Source: Authors' elaboration based on McCormick et al. (2013) and ISTAT Administrative Limits at the Municipal Level

Figure 5: Nearest Medieval Port on the Actual Path on Roman road Network



Source: Authors' elaboration based on McCormick et al. (2013) and ISTAT Administrative Limits

was used to cover both shorter and longer distances by foot (Fonseca, 2000). Then the procedure is repeated assuming three different configurations of Medieval ports, according to the maps presented in Section 2.3.

To sum up, in this paper we construct the instrument based on the distance to a Medieval port according to different metrics. That is, the nearest ports are associated to each individual, following each of the three maps. On the basis of this, each individual is then considered to be either near or far from the nearest port by considering separate distance thresholds.

2.5 Geographic Variables

In our analysis, we employ a comprehensive set of geographical controls to take into account the fact that geography both influenced the location of ports in the Middle Ages and there may still be a correlation between place of residence and political ideologies. First of all, we construct a measure of geodetic distance from the sea and one of distance from Tunis. Indeed, following Accetturo et al. (2019), Tunis was the main port of departure for the raids and the latter variable can proxy the probability of being attacked by pirates. This, in turn could negatively affect the attitudes towards migrants.

In addition we take into account certain characteristics of the territory. Specifically, we construct an index of terrain ruggedness[24]. What is more, we build an index of accessibility starting from data by Beria et al. (2017). Authors provide detailed maps of accessibility into 371 zones. This measure allow us to account for a control variable that is significantly more realistic than simple infrastructure indicators. We retrieve data on whether the area of residence of individuals is urban or rural from Schaub and Morisi (2020).

Finally, we account for the resident population in 2017 and for the population density in 2001.[25]

2.6 Other Controls

In this section we discuss different sets of control variables included in the analysis. The first set of variables belongs to the personal controls category. Specifically, directly from the aforementioned survey, we use individual variables related to age, educational qualifications, marital status, and gender. In addition, we collect information on the occupation of the parents, as a proxy for family income. We also entail some variables related to the socioeconomic context of the municipality of residence of individuals. Chiefly, from the "Atlante Statistico dei Comuni" provided by ISTAT we make use of data on the average municipal income in 2017.[26] From the same source, we also use municipal data on the number of local units of manufacturing firms operating in the year 2017.

From Schaub and Morisi (2020), we collect municipal data regarding the number of people without internet connection (adsl).[27]

[24]Authors' elaboration from Nunn and Puga (2012).

[25]Data source is "Atlante Statistico dei Comuni" provided by ISTAT.

[26]This variable is constructed on the base of tax declarations.

[27]Data available in Schaub and Morisi (2020)'s Online Appendix. The data refer to the years ranging from 2012 to 2015, and we use those for 2015.

Regarding controls related to immigrants, we employ the percentage of foreign residents on the total population for the year 2017, at the municipal level (ISTAT data). As a measure of social capital, we collect data on the number of non-profit associations at the municipal level (2001), weighted by resident population.[28]

Regarding historical information, we refer to data provided by Guiso et al. (2016) (Online Appendix). Specifically, we collect information on city-level dummies created according to the size of cities in the 1300s. The *Large* variable is a dummy that has value 1 if the population in 1300 exceeded 10,000 people. The variable *Medium* is instead a dummy that has value 1 if the population is between 1,000 and 10,000 people in that same year.[29] In addition, we also use a dummy that identifies the cities that was a seat of a Bishop before 1000 C.E. [30] Additionally, authors provide important information on Communes (or free city states). Nevertheless, we decide not to include it because this information is only available for Northern Italy. Replicating our analysis for Northern Italy only, would reduce our sample, and would also lead to the exclusion of most of the ports in our configurations.

2.7 Descriptive Statistics

In the previous subsections, we have introduced many variables. Table 1 below shows descriptive statistics. We consider individuals living in a municipality within 15 km (calculated using the Roman Roads network) from a port that had routes to Africa in the Middle Ages on the one hand, and all other individuals on the other. We used Map 2 as the reference model. Anyhow, if we consider Map 1 and Map 3, the descriptive statistics are stable. The same applies if we use 10 km as a threshold. As can be seen from the combined reading of the data reported in Table 1, the two samples are very similar to each other. Anyway, we control for all these aspects in all estimates reported in the study.

[28]Data relative to different Measures of social capital for Italian provinces and municipalities are available at Tommaso Nannicini's personal website:
https://www.tommasonannicini.eu/it/works/measures-social-capital-italian-provinces-and-muni/.

[29]Authors obtained information on city size from Bairoch et al. (1988), who report the population of European cities between 800 and 1850, approximately every 100 years. Although there are population data referring to earlier periods, 1300 is the first year in which there are only few missing data.

[30]Guiso et al. (2016) obtained this information from the map "Italia alto Medievale: sedi vescovili" from Treccani (2007), which shows the Bishop cities in the late Middle Ages. As recalled by the same authors, the Bishop cities were mostly formed between the first and third century A.D., years in which the Christian movement spread out.

Table 1: Descriptive Statistics (Map 2)

Variable	RRdist >15 Km from a Medieval Port					RRdist <15 Km from a Medieval Port				
	Mean	**Std. Dev.**	**Min**	**Max**	**Obs.**	**Mean**	**Std. Dev.**	**Min**	**Max**	**Obs.**
Far Right Political Positioning	0.26	0.44	0	1	1,704	0.23	0.42	0	1	155
Migrant Perception	0.20	0.40	0	1	1,704	0.13	0.33	0	1	155
Age	27.55	4.66	20	35	1,704	27.66	3.93	20	35	155
Man	0.46	0.50	0	1	1,704	0.48	0.50	0	1	155
Father unemployed	0.26	0.44	0	1	1,704	0.20	0.40	0	1	155
Mother unemployed	0.54	0.50	0	1	1,704	0.66	0.47	0	1	155
Unmarried	0.80	0.40	0	1	1,704	0.87	0.34	0	1	155
Education	2.13	0.66	0	4	1,704	2.17	0.74	0	4	155
North	0.46	0.50	0	1	1,704	0.10	0.30	0	1	155
Center	0.20	0.40	0	1	1,704	0.06	0.25	0	1	155
South / Islands	0.34	0.48	0	1	1,704	0.84	0.37	0	1	155
Municipal Income	1,332.696	374,777.50	457,040.6	2,557.213	858	1,115.317	352,346.50	631.729	1,852.105	25
Share of population without broadband (Municipality) (%)	0.011	0.016	0	0.128	858	0.004	0.009			25
Ruggedness (Municipality)	1.30	1.58	0	8.67	858	1.13	1.20	0.014	4.18	25
Accessibility (Municipality)	107.05	41.16	16.09	180.74	858	89.35	41.58	32.87	131.98	25
Population (Municipality)	31,805.05	117,614.40	320	2,873.494	858	153,173.20	239,824.10	7,356	970,185	25
Population Density (Municipality)	698.04	962.38	6.78	6,914.69	858	4,110.54	4,081.85	47.04	13,157.14	25
Resident Foreigners (standardized)	7.07	4.20	0.32	24.32	858	4.28	3.67	0.79	13.47	25
Non Profit Association (per capita)	0.004	0.002	0.00017	0.02213	858	0.003	0.002	0.00060	0.00923	25
Bishop city	0.19	0.39	0	1	858	0.40	0.50	0	1	25

Dataset: Osservatorio Giovani 2017, IPSOS, Giuseppe Toniolo Institute of Higher Education.
Sample: young adults with at least 20 years of age. In the computation of the descriptive statistics, sample weights are applied. The descriptive statistics refers to the Map 3. However, the descriptive statistics are very similar if we consider the ports configuration reported in Map 1 and 3.

3 Migrant Perception and Far-Right Political Positioning: OLS estimates

First of all, we analyze the relation between the migrant perception and far-right political positioning. We estimate the following equation by using ordinary least squares:

$$FarRightPositioning_{i,m,r} = \alpha_r + \beta MigrantPerception_{i,m,r} + X_m\pi + W_i\varphi + \epsilon_{i,m,r}, \tag{1}$$

where i denotes an individual, m a municipality, and r a NUTS-3 region. The dependent variable, $FarRightPositioning_{i,m,r}$, measures the far-right political positioning and the main independent variable is $MigrantPerception_{i,m,r}$.[31] W_i denotes individual-level controls: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. In addition, the set of control variables, X_m, intends to capture geographic, economics, social and historical characteristics of the municipality, m, where each individual resides. We include: geography controls (terrain asperity index, the geodetic distance both from Tunis and from the sea, accessibility index, resident population in 2017, population density in 2001 and a rural or urban area dummy), socio-economic controls (average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage), the percentage of foreigners in the total population, the number of non-profit associations per capita as a proxy for social capital, and historical controls (presence of a Bishop and Medieval city size).[32] Finally, α_r denotes fixed effects (FE) at the NUTS-3 region level and $\epsilon_{i,m,r}$ is the error term.

In Table 2 we present estimates of Equation (1). In column (1) we report the baseline specification where we incorporate regional FE as well as personal and geographic controls. From columns (2) to (5) we then encompass further additional regressors to the set of basic controls reported in column (1). Respectively, in column (2) we include socio-economic controls, in column (3) we introduce our measure of social capital, in column (4) we account for the the share of immigrants, while in column (5) we employ historical controls. Finally, in column (6) we present the most comprehensive specification, which accounts for all the aforementioned controls. The coefficient of $MigrantPerception$ is always positive and significant at the 1% level, and it remains stable across all the different specifications. This result indicates that the relation between the negative perception of migrants and the far-right political positioning is positive. Expressly, the magnitude of the coefficient indicates that having a negative perception of migrants increases the probability of far-right political positioning of about 32%. Indeed the coefficient remains stable in all models, even when

[31] For further details on the metrics used to calculate our main measures of political positioning and migrant perceptions see Sections 2.1 and 2.2.

[32] All of the variables listed above are presented in Sections 2.5 and 2.6.

we account for the full set of controls. Considering that the average probability of far-right positioning in our sample is 25%, this result implicates that in the case where all individuals have a negative perception of migrants this percentage could increase up to 33%. At any rate, there might be simultaneity between individual voting behaviour and attitudes towards migrants. To overcome this endogeneity problem and to find the impact of the perception of migrants on the political positioning of each individual, we introduce in the next section an Instrumental Variable approach.

Table 2: Relation between Far-Right Political Positioning and Migrant Perception

Dependent Variable: *Far-Right Political Positioning*						
	(1)	(2)	(3)	(4)	(5)	(6)
Migrant Perception	0.323***	0.321***	0.323***	0.324***	0.321***	0.322***
	(0.0427)	(0.0430)	(0.0426)	(0.0424)	(0.0427)	(0.0425)
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic		√				√
Social Capital			√			√
Migrants				√		√
History					√	√
R-squared	0.240	0.242	0.241	0.241	0.244	0.247
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Ordinary Least Squares. The dependent variable is the dummy related to far-right political positioning, $FarRightPositioning_{i,m,r}$, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is the variable $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. Personal controls include: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) include: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) entail: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls encompass: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

4 Does History Matter? Medieval Routes and Far-Right Political Positioning: IV Estimates

As mentioned in the previous section, from OLS results we cannot deduce the direction of the relation between migrant perception and far-right political positioning of individuals. Indeed, it could be that far-right belief leads people to have a negative perception of migrants, or, reversely, the latter can impact on political positioning. Moreover, since in 2017 in Italy the anti-government parties have widely relied on anti-immigrant arguments during the political campaign, we are even more confident that reverse causality is a central issue in this analysis.

Furthermore, there might be omitted factors that drive both migrant perception and voting behaviour. Possible correlation between unobservables, $\epsilon_{i,m,r}$, and the perception of migrants of each individual in Equation 1, would bring biased and inconsistent OLS estimates. We use Two-Stage Least Squares (TSLS) method to estimate the following equation:

2^{nd} stage:

$$FarRightPositioning_{i,m,r} = \alpha_r + \beta MigrantPerception_{i,m,r} + X_m \pi + W_i \varphi + \epsilon_{i,m,r}, \tag{2}$$

where the 1^{st} stage is:

$$MigrantPerception_{i,m,r} = \alpha_r + \beta ProximityMedPort_{m,r} + X_m \pi + W_i \varphi + \upsilon_{i,m,r}, \tag{3}$$

where $ProximityMedPort_{m,r}$ is a dummy variable indicating whether a municipality, m, belonging to a NUTS-3 region r, is near to a Medieval port that had a route to Africa,[33] and X_m and W_i include the usual additional control variables. The proximity to a Medieval port may have influenced the perception of migrants, and affects far-right political positioning through that. The choice of our instrument hinges on the fact that in the Middle Ages the Mediterranean sea was a melting pot of different cultures (Abulafia, 2011; Braudel, 1995). Accordingly, living in a port city that had routes to Africa meant being in contact with blacks, and more generally with different people, hundred years earlier than those who lived elsewhere. Real communities of slaves were formed in these cities and in some cases, they could represent as much as ten percent of the resident population. It was also common that one or more slaves belonged to families of aristocrats, merchants and artisans (see among others Pistarino, 1964; Olgiati and Zappia, 2018; Loi, 2015; Campagna,

[33] We calculate the distance from the centroid of each Medieval port to the centroid of each individual municipality of residence according to the ancient Roman road network. For further details see Section 2.4.

2019). In this context we argue that the *contact theory* of Allport et al. (1954), according to which interpersonal contact is an effective way to reduce prejudice between a majority and a minority, might find an application. Furthermore, we suppose this cultural *humus* persists through centuries. According to Boyd and Richerson (2005); Cavalli-Sforza and Feldman (1981); Benhabib et al. (2010) cultural elements such as attitudes, values, beliefs, are transmitted from generation to generation. More specifically, seminal works by Fielding (2018); Voigtländer and Voth (2012); Durante (2009); Schindler and Westcott (2020) apply to our context.[34] Indeed, in these studies, the long run persistence of trust and (in)tolerance behaviours towards minorities is put under scrutiny and the historical origin of modern attitudes is demonstrated. For this reason, we argue that our chosen instrument might be relevant, i.e. it might explain today's attitudes towards migrants.

Moreover, the validity of our instrument requires that it should affect far-right positioning only through its effect on the perception of migrants and it should be not correlated to the error term. We argue that, conditional on controls, our instrument can be considered exogenous for separate reasons. First, the trade routes with Africa, largely Genoese and Venetian, were established in pursuit of purely commercial interests, without any ideological motivation. Furthermore, our measure of distance from the Medieval ports is calculated on the ancient Roman road networks dating back to 117 A.D. Roman roads are strongly predetermined (Dalgaard et al., 2018) and the literature has identified military reasons as the main purposes of Roman road construction, thus excluding a direct economic reason for their location (e.g. Garcia-López et al., 2015; De Benedictis et al., 2018), and as a consequence we can exclude any reason related to political ideology today.

However, since geography may have influenced the configuration of ports that had routes to Africa, we always control for a set of geographic characteristics in order to make our exclusion restriction more likely to hold. What is more, by controlling for the importance of city in 1300 as well as for today's municipality level of development, we argue that other channels through which the presence of medieval ports might affect political positioning should have been controlled for.

We present estimates of Equation (3) in Table 3. We make use of three different dummy variables that are based on three different thresholds. More specifically, in panel A, the proximity threshold is 10 km, and in panel B and panel C we use a distance of 15 and 20 km respectively[35] Conjointly, we employ three different port configurations, according to the maps reported in Section 2.3: map 1 in columns (1) and (2), map 2 in columns (3) and (4), and map 3 in columns (5) and (6). We report results for both the model that includes the usual set of basic controls (column (1), (3) and (5)), and the model with the full set of covariates (column (2), (4) and (6)).[36] In

[34]For an in-depth analysis of these studies see Section A.2.

[35]For the rationale behind this, see Section 2.4.

[36]Our baseline specifications always take into consideration personal and geography controls, as

panel A and in panel B, the coefficient of $ProximityMedPort_{m,r}$ ranges from -0.13 to -0.19 when we take into account the basic set of controls (columns (1), (3) and (5), while it slightly decreases when we introduce the full set of covariates (columns (2), (4) and (6), ranging from -0.09 to -0.15. It is always negative and statistically significant at 1% level, thus confirming the relevance of the chosen instrument.

Whatever is the chosen specification, all coefficients of $ProximityMedPort_{m,r}$ reported in Panel A are higher in magnitude than those in panel B, indicating that while always negative, the relation between distance to the port and the perception of migrants declines as distance increases. Indeed, in panel C we show that at the 20 km threshold the relation ceases to be significant, with the only exception of the coefficient presented in column (3).

well as NUTS-3 region FE. When we refer to the full set of covariates we also entail: socio-economic, social capital, migrants, and history controls. We list all the control variables in Section 3. For an in-depth explanation of each variable see Section 2.

Table 3: First Stage Results: relation between Migrant Perception and Proximity to a Medieval Port

Dependent Variable: *Migrant Perception*	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 10 km						
Proximity to Medieval Port	-0.191***	-0.146***	-0.176***	-0.140***	-0.163***	-0.115***
	(0.0265)	(0.0413)	(0.0295)	(0.0425)	(0.0275)	(0.0387)
R-squared	0.198	0.209	0.198	0.209	0.197	0.208
Panel B: 15 km						
Proximity to Medieval Port	-0.148***	-0.114***	-0.151***	-0.120***	-0.127***	-0.088***
	(0.0375)	(0.0379)	(0.0349)	(0.0410)	(0.0321)	(0.0341)
R-squared	0.197	0.208	0.197	0.209	0.196	0.208
Panel C: 20 km						
Proximity to Medieval Port	-0.0663	-0.0304	-0.0997**	-0.0698	-0.0589	-0.0218
	(0.0438)	(0.0435)	(0.0453)	(0.0483)	(0.0478)	(0.0470)
R-squared	0.195	0.207	0.196	0.208	0.195	0.207
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic		√		√		√
Social Capital		√		√		√
Migrants		√		√		√
History		√		√		√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Ordinary Least Squares. The dependent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to three different maps (For further details see Section 2.3). In panel A, the proximity threshold is 10 km on the ancient Roman road network. In panel B and panel C, we employ a distance of 15 and 20 km, respectively. Personal controls encompass: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) include: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) incorporate: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) take into account: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

In Table 4 we present results for the reduced form estimates. In particular we regress the right-wing voting intention, as captured by the variable $FarRightPos-itioning_{i,m,r}$, on the proximity to a Medieval port. We replicate the structure of Table 3 by presenting various panels, according to the three distance thresholds, as well as the three different port configurations. In panels A and B, the coefficient of $ProximityMedPort_{m,r}$ is always positive and tipically statistically significant at the 1% level. The magnitude of the coefficient is similar with regard to map 1 and 3, while it decreases when considering map 2 configuration. As in Table 3, the relation cease to exist when we employ the 20 km threshold. This result is reasonable in line with the fact that during the Middle Ages (and up to the XIX century when the advent of the railroad revolutionized transportation) the vast majority of people used to cover both shorter and longer distances by foot (Fonseca, 2000). Covering a distance of 20 kilometres corresponds to an average time of 5 hours of uninterrupted walking. This probably means that people living more than 15 kilometres from the port had much less chance of frequent contact with slave communities and sailors. In light of the fact that there is no relation in the first stage and reduced form of proximity from the medieval port with migrant perception and political positioning respectively when considering the 20 kilometres proximity threshold, we will only utilize the distance thresholds of 10 and 15 kilometres in the IV estimates. All in all, the results reported in Table 3 reveals *Proximity to Medieval Port* coefficient values highly stable across specification. This stability suggests that the full set of controls are accounting for potential selection effects. Following Altonji et al. (2005), we have also checked for variations in R^2 between specifications. In fact, as the number of controls increases, R^2 increases but the coefficient of interest does not change significantly. Furthermore, Oster (2019) test confirm the stability of the aforementioned coefficient.[37] Thus, our results are robust, and therefore unobservables should not be more important than observables in explaining Y. This, again, confirms the validity of our instrument.

[37]Under the assumption that the maximum R^2 obtainable, when the coefficient of interest β is equal to zero, is 30% greater than the R^2 reported in the specification that takes into account the full set of controls, we tipically find a δ value greater than 1.

Table 4: Reduced Form: relation between Far-Right Political Positioning and Proximity to a Medieval Port

Dependent Variable: *Far-Right Political Positioning*	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 10 km						
Proximity to Medieval Port	-0.218***	-0.249***	-0.151***	-0.149**	-0.210***	-0.212***
	(0.0642)	(0.0619)	(0.0495)	(0.0621)	(0.0707)	(0.0691)
R-squared	0.173	0.182	0.172	0.180	0.174	0.182
Panel B: 15 km						
Proximity to Medieval Port	-0.259***	-0.293***	-0.173***	-0.177**	-0.238***	-0.247***
	(0.0546)	(0.0558)	(0.0634)	(0.0764)	(0.0639)	(0.0682)
R-squared	0.175	0.184	0.173	0.181	0.176	0.184
Panel C: 20 km						
Proximity to Medieval Port	-0.107	-0.108	-0.0820	-0.0747	-0.133	-0.127
	(0.0860)	(0.0910)	(0.0644)	(0.0677)	(0.0913)	(0.0881)
R-squared	0.171	0.179	0.171	0.178	0.172	0.179
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic		√		√		√
Social Capital		√		√		√
Migrants		√		√		√
History		√		√		√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Ordinary Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to far-right political positioning, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is, $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to three separate maps (For further details see Section 2.3). In panel A, the proximity threshold is 10 km on the ancient Roman road network. In panel B and panel C, we utilize a distance of 15 and 20 km, respectively. Personal controls take into account: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) entail: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) involve: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) take into consideration: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

In Table 5 we show our IV estimator results obtained using port configuration of map 1. In column (1) we report the baseline model, with the usual set of controls, while from columns (2) to (5) we add one set of controls at time, and in column (6) the full set of covariates are included. For each specification, we report the first-stage F-statistics. In both panels A and B, the robust F-statistic ("Kleibergen Paap Wald F statistic") generally satisfies the rule of thumb, showing values greater than ten. We use weak-instrument robust inference for specifications in columns (4) and (6) of panel B, and we reject the null hypothesis of Wald test at 1% level. At the same time, F-statistics reported in columns (1) to (4) in panel A are also greater than Olea and Pflueger (2013)'s critical values at 5% of worst-case bias, which are the appropriate critical values to consider in the non-homoschedastic case (see Andrews et al. 2019).[38]

All reported coefficients of $MigrantPerception_{i,m,r}$ are statistically significant at 1% level, except for that in column (6) in panel B. The magnitude of the coefficients slightly differs in the two panels, ranging from 1.14 to 1.70 in panel A and from 1.75 to 2.57 in panel B.

Overall, results in Table 5 suggest a positive and significant impact of the migrant perception on the far-right political positioning of individuals. In particular, it is worth recalling that we assign 1 to the individual that declare a far-right political positioning (and 0 elsewhere), and 1 to individuals denoted by a negative perception of migrants (and 0 elsewhere). In the consequence of this, the magnitude of the coefficient of $MigrantPerception_{i,m,r}$ indicates that the probability of supporting a far-right party more than double when an individual has a negative perception of migrants, as instrumented by the proximity to a Medieval port (1=close and 0 elsewhere).

[38]Whenever the Olea and Pflueger (2013)'s critical values are not achieved, we use weak-instrument robust inference and we always reject the null hypothesis of Wald test at 1% level.

Table 5: Main Results - Map 1 : IV Estimates

Dependent Variable: ***Far-Right Political Positioning***						
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: 10 km Distance						
Migrant Perception	1.142***	1.162***	1.146***	1.185***	1.338***	1.699***
	(0.334)	(0.350)	(0.329)	(0.344)	(0.424)	(0.616)
F-statistic	51.70	44.49	54.22	42.34	21.07	12.57
PANEL B: 15 km Distance						
Migrant Perception	1.746***	1.869***	1.748***	1.938***	1.866***	2.567**
	(0.568)	(0.670)	(0.561)	(0.740)	(0.539)	(1.007)
F-statistic	15.66	14.05	17.69	9.796	21.17	9.043
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Eco		√				√
Social Capital			√			√
Migrants				√		√
History					√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to port configuration as reported in Map 1 (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls encompass: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) include: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) count: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) involve: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

In Table 6, we show results for IV estimates with regard to port configuration as reported in Map 2. Following the structure of Table 5, we present the baseline model, with the usual set of controls, in column (1), we then add one set of controls at time (from columns (2) to (5)), and in column (6) we include the full set of covariates. For each specification, we report the first-stage F-statistics. In both panels A and B, the robust F-statistic ("Kleibergen Paap Wald F statistic") satisfies the rule of thumb, showing values greater than ten, with the only exception of column (6) of panel B. Nevertheless, using weak-instrument robust inference for this latter specification, we reject the null hypothesis of Wald test at 10% level. In addition to that, F-statistics reported in columns (1) to (4) in panel A are also greater than Olea and Pflueger (2013)'s critical values at 10% of worst-case bias.[39]

In panel A all reported coefficients of $MigrantPerception_{i,m,r}$ are statistically significant at 1% level, except for that in column (6). While in panel B the significance level is 5%, with the exception of columns (2) and (6). The magnitude ranges from 0.79 to 1.07 in panel A and from 1.12 to 1.48 in panel B. As in Table 5, the magnitude of the coefficients is higher in panel B than in panel A.

Overall, results in Table 6 confirm our previous findings (Table 5), highlighting a positive and significant impact of the migrant perception on the right-wing voting intention of individuals. On average, the magnitude of the coefficient of $MigrantPerception_{i,m,r}$ indicates that the probability of supporting a right-wing party double when an individual has a negative perception of migrants, as instrumented by the proximity to a Medieval port (1=close and 0 elsewhere).

[39]Whenever the Olea and Pflueger (2013)'s critical values are not achieved, we employ weak-instrument robust inference and we generally reject the null hypothesis of Wald test at 5% level.

Table 6: Main Results - Map 2 : IV Estimates

Dependent Variable: ***Right-wing Voting Intention***						
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: 10 km Distance						
Migrant Perception	0.860***	0.791***	0.840***	0.840***	1.005***	1.065**
	(0.265)	(0.274)	(0.270)	(0.267)	(0.364)	(0.451)
F-statistic	35.56	33.21	34.91	27.41	14.31	10.80
PANEL B: 15 km Distance						
Migrant Perception	1.144**	1.124*	1.138**	1.173**	1.282**	1.478*
	(0.496)	(0.579)	(0.509)	(0.565)	(0.539)	(0.790)
F-statistic	18.81	15.30	20.39	11.91	18.06	8.549
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Eco		√				√
Social Capital			√			√
Migrants				√		√
History					√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to port configuration as reported in Map 2 (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls involve: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) include: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) take into consideration: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) encompass: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

In Table 7, we show results for IV estimates with regard to the third and last port configuration (map 3). We show results as follows: a baseline model, with the usual set of controls (column (1)), models adding one set of covariates at time (from columns (2) to (5)), and a model with the full set of control variables (column (6)). In panel A all reported coefficients of $MigrantPerception_{i,m,r}$ are statistically significant at 1% level, except for that in columns (2) and (6). In panel B it is significant at 1% in columns (1), (3), and (5), while at 5% level elsewhere. The magnitude ranges from 1.28 to 1.83 in panel A and from 1.86 to 2.80 in panel B. The first-stage F-statistics ("Kleibergen Paap Wald F statistic"), in both panels A and B, are generally greater than ten, satisfying the rule of thumb. Model presented in column (6) of panel A and in columns (4) and (6) of panel B shows robust F-statistic lower than 10. Using weak-instrument robust inference for these three specifications, we always reject the null hypothesis of Wald test at 5% level. Moreover, F-statistics reported in columns (1) to (4) in panel A are also greater than Olea and Pflueger (2013)'s critical values at 10% of worst-case bias.[40]

All previous findings are confirmed; the impact of the migrant perception on the far-right political positioning of individuals remains positive and significant, using 10 and 15 kilometers distance thresholds.[41] A negative perception of migrants leads to a probability of far-right political positioning that is doubled (panel A) or three time higher (panel B) than that of the reference group.

In the light of the results shown, using an IV approach, coefficients of $Migrant$ $Perception$ are higher than those estimated by OLS. The relation between the variables of interest is confirmed regardless of the port configuration used. This means that by using three separate historical sources, we are able to demonstrate a significant impact of the current perception of migrants on individuals' political beliefs. Nevertheless, we are aware of the fact that some robustness analysis are needed in order to validate and enhance our findings. We devote Section 5 to this issue.

[40] Whenever the Olea and Pflueger (2013)'s critical values are not achieved, we make use of weak-instrument robust inference and we generally reject the null hypothesis of Wald test at 1% or at least 5% level.

[41] It is worth noting that we also check if our results in Tables 5, 6, and 7 hold when using a 5 km distance thresholds. Coefficients of $MigrantPerception$ are tipycally positive and significant.

Table 7: Main Results - Map 3 : IV Estimates

Dependent Variable: *Far-Right Political Positioning*						
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: 10 km Distance						
Migrant Perception	1.283***	1.304**	1.284***	1.368***	1.364***	1.834**
	(0.486)	(0.515)	(0.482)	(0.531)	(0.510)	(0.797)
F-statistic	35.35	29.86	34.17	28.25	15.48	8.895
PANEL B: 15 km Distance						
Migrant Perception	1.873***	2.021**	1.873***	2.166**	1.861***	2.801**
	(0.698)	(0.829)	(0.694)	(0.927)	(0.627)	(1.284)
F-statistic	15.68	12.51	16.86	9.140	17.38	6.677
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Eco		√				√
Social Capital			√			√
Migrants				√		√
History					√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to far-right political positioning, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to port configuration as reported in Map 3 (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls cover: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) entail: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) involve: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) take into account: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

5 Robustness Checks

To validate our empirical approach, we run a battery of robustness checks. One of the first concerns might derive from the fact that the relation between the perception of migrants and the distance from a Medieval port might capture a kind of "openness" that is common to all port cities today, regardless of their Medieval routes. In this spirit, we construct the usual instrument based on the distance in Roman roads according to various configurations of today's ports. Principally, we refer to two configurations related to the 2017 Assoporti ranking where the most important commercial Italian ports are listed.[42] We investigate the relation between the proximity to a today's commercial port and the perception of migrants. We present in Table 8 the results of the OLS estimates, using the 10 and 15 most important today's ports and considering the usual distance cut-offs, as reported in panels A and B. We present both baseline specifications (columns (1) and (3)) and models with the full set of covariates (columns (2) and (4)). Interestingly, none of the relevant coefficients is statistically significant, highlighting the absence of correlation between the instrument and the endogenous regressor.

[42] We consider the following Assoporti ranking (2017) of commercial ports: Trieste, Genova, Cagliari, Livorno, Gioia Tauro, Augusta, Messina, Ravenna, Venezia, Napoli, Taranto, La Spezia Salerno, Savona, and Civitavecchia. Source: http://www.assoporti.it/media/3854/movimenti_portuali_2017_17gen19.pdf

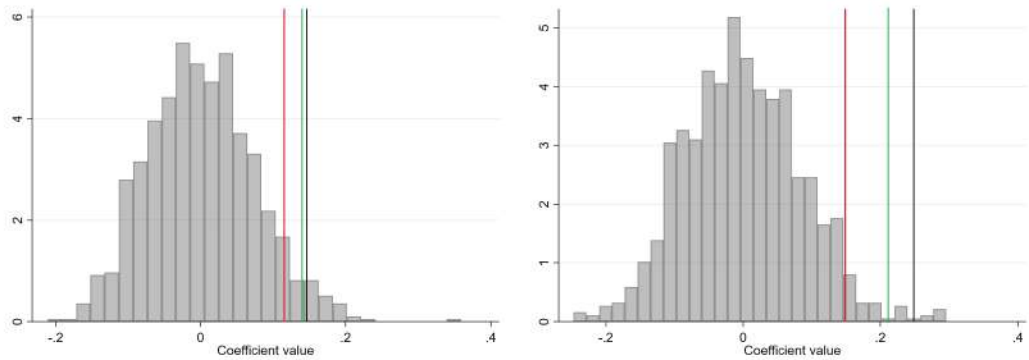
Table 8: Robustness to the use of today's most important ports

Dependent Variable: *Migrant Perception*	10 Major Ports		15 Major Ports	
	(1)	(2)	(3)	(4)
Panel A: 10 km				
Proximity to Medieval Port	-0.0755	-0.0265	-0.00389	-0.0652
	(0.0639)	(0.0743)	(0.0716)	(0.0764)
R-squared	0.195	0.207	0.194	0.208
Panel B: 15 km				
Proximity to Medieval Port	-0.0402	-0.00164	0.0300	0.0920
	(0.0557)	(0.0662)	(0.0631)	(0.0682)
R-squared	0.195	0.207	0.195	0.208
NUTS-3 Region FE	√	√	√	√
Personal	√	√	√	√
Geography	√	√	√	√
Socio-Economic		√		√
Social Capital		√		√
Migrants		√		√
History		√		√
Observations	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Ordinary Least Squares. Estimates refer to the following equation: $MigrantPerception_{i,m,r} = \alpha_r + \beta ProximityTodayPort_{m,r} + X_m\pi + W_i\varphi + v_{i,m,r}$. The dependent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $ProximityTodayPort_{m,r}$, the dummy variable indicating the (historical) proximity to one of the today's most important ports. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) respectively from one of the 10 (Columns (1) and (2)) and 15 (Columns (3) and (4)) today's most important commercial ports. In panel B we make use of a distance of 15 km. Personal controls take into consideration: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) encompass: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) take into account: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls incorporate: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

We have argued that, conditional on controls, our instrument is exogenous. To further corroborate this assumption, we have created a "placebo" indicator of proximity to a Medieval port.[43] Accordingly, we have randomly assigned our "placebo" indicator to the various municipalities, and thus to the individuals. We replicated this random allocation for 1000 times. Left graph in Figure 6 shows the results of the various random allocations for the first stage model presented in Table 3, while the right one shows the same results for the reduced form model shown in Table 4. For both models we use a specification that account for the full set of control variables In both figures the average of the estimated coefficients is centered at zero, and this shows that the relation is not mechanical and automatic. All the same, the "treated" municipalities are only few and, as a result, in the various simulations it can happen that by chance some combinations are overly similar to the true one, making to fall back the true effect (reported in correspondence of the coloured lines) inside the range. Principally, in the left graph the lines refer to the coefficients of *Proximity to Medieval Port* reported in panel A of Table 3; the red line corresponds to estimated coefficient in column (6), the green line to that in column (4), and the black one to the coefficient reported in column (2). In the right graph, the lines indicate the value of the coefficients of *Proximity to Medieval port* showed in columns (4) (red line), (6) (green line), and (2) (black line) of Table 4 panel A.

Figure 6: Random allocation of the indicator of Proximity to a Medieval Port



Notes: In both graphs, the x-axis shows all the various values that the coefficient of interest (*Proximity to a Medieval Port*) takes on in the various models, in which the true indicators have been randomly shuffled and reallocated among municipalities one thousand times. The y-axis shows the probability density function of the estimated coefficients. The vertical lines are placed in correspondence of the "true" estimated value of the coefficients. Each estimate shown in left graph of Figure 6 is made by taking Equation 3 as the base equation. Each estimate shown in right graph of Figure 6 is made by taking the reduced form equation as the base equation: $Far-RightPoliticalPositioning_{i,m,r} = \alpha_r + \beta ProximityMedPort_{m,r} + X_m\pi + W_i\varphi + \epsilon_{i,m,r}$. In the left graph the dependent variable is $MigrantPerception_{i,m,r}$, the dummy related to perception of migrants, while in the right graph the dependent variable is the aforementioned dummy related to far-right political positioning. In both specifications the main independent variable is $ProximityMedPort_{m,r}$. We incorporate the full set of covariates: personal controls, geography controls at the municipal level, socio-economic controls at the municipal level, the % of foreigners in the total population in 2017 in each municipality, the number of non-profit associations per capita in each municipality in 2011, and historical controls. The sample weights are applied. All specifications take into consideration regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level.

[43]Since the ratio of treated municipalities varies according to the map and the threshold used in each specific case, we avail ourselves of an average percentage of treated equal to 5%, i.e. the 95% of the municipalities takes value 0.

We now turn to present a set of robustness of our findings to the choice of the dependent variable. In particular we use three different measures that capture alternatively the far right ideology or voting intention, i.e. *FarRightPositioning2*, *FarRightVotingInt*, and *LegaPartyVotingInt*. Notably, *FarRightPositioning2* dummy variable is constructed considering the same question we employ for our main dependent variable, but using a different metric. Indeed, we consider again the following question: "In politics, we often talk about "left" and "right". Considering your political beliefs, where would you place yourself?", and we code 1 when the respondents answered 9 or 10, and 0 elsewhere.[44]. To construct the other two measures, i.e. *FarRightPartiesInt* and *LegaPartyVotingInt* of the far-right voting intention, focusing on the question in the survey that ask individuals how much they are likely to vote each party in the next parliamentary election in a scale from 1 ("I would definitively NOT VOTE for it") to 10 ("I would definitively VOTE for it"). The dummy variable *FarRightPartiesVotingInt* takes value 1 if the individual answered at least 8 with respect to one of the following political parties: *Lega*, *Fratelli d'Italia*, *Forza Nuova* (or other extreme right parties). We code as 0 the answers from 1 to 7 or "I don't know". To construct the dummy variable *LegaPartyVotingInt*, we assign value 1 to each individual that answered at least 8 only to *Lega* party, the major far right political party in the Italian scenario. We code as 0 the answers from 1 to 7 or "I don't know".[45]
Results from IV estimates are show in Table 9. We present estimates of our baseline specification using the aforementioned measures, relying to the three separate maps (Map 1 in columns (1), (4), and (7); Map 2 in columns (2), (5), and (8); Map 3 in columns (3), (6), and (9)) and using the usual distance thresholds (Panel A 10 km and Panel B 15 km). Using *FarRightPartiesVotingInt* as dependent variable in models in columns (1) to (3), the estimated coefficients of *MigrantPerception* remain substantially unchanged if compared with those reported in columns (1) of Tables 5, 6, and 7, respectively.
Moreover, turning to the other models presented in Table 9, results of the estimated coefficients of *MigrantPerception* seems to confirm and strengthen our main results. Indeed, although the dependent variables are constructed with a totally different question of the survey, the sign of the coefficients remain positive and significant, confirming the positive impact of the perception of migrants on far-right beliefs and political ideology.
Finally, in all specifications, the reported F-Statistics are always greater than 10, satisfying the traditional *"rule-of-thumb"*. Furthermore, for models estimated in Panel A using port configuration of Map 1, F-statistics are also greater than Olea and Pflueger (2013)'s critical values at 5% of worst-case bias, and at 10% when using

[44]We remind that the respondents were asked to choose an answer on a scale from 1 (left) to 10 (right). We omit observations for which the respondents answered "I don't place myself anywhere, I don't care", and code values from 1 to 7 as 0, and from 8 to 10 as 1.
[45]For further details see Section 2.1.

Maps 2 and 3. In addition, all F-statistics reported in Panel B are greater than Olea and Pflueger (2013)'s critical values at 20% or 30% of worst-case bias.

Table 9: Robustness to the choice of the dependent variable: alternative far-right metrics for political positioning and voting intention

Dependent Variable:	***FarRightPositioning2***			***FarRightPartiesVotingInt***			***LegaPartyVotingInt***		
	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 10 km									
Migrant Perception	1.022***	0.596*	1.208**	0.811*	0.726**	0.806*	0.796	0.725*	0.773
	(0.346)	(0.333)	(0.500)	(0.487)	(0.354)	(0.475)	(0.525)	(0.375)	(0.508)
F-Statistic	51.70	35.56	35.35	51.70	35.56	35.35	51.70	35.56	35.35
Panel B: 15 km									
Migrant Perception	1.500***	0.815	1.672**	1.675**	1.127*	1.618**	1.456**	0.984*	1.405**
	(0.574)	(0.563)	(0.714)	(0.665)	(0.591)	(0.691)	(0.584)	(0.530)	(0.609)
F-statistic	15.66	18.81	15.68	15.66	18.81	15.68	15.66	18.81	15.68
NUTS-3 Region FE	√	√	√	√	√	√	√	√	√
Personal	√	√	√	√	√	√	√	√	√
Geography	√	√	√	√	√	√	√	√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variables are: $FarRightPositioning2_{i,m,r}$, the dummy related to far-right political positioning constructed with the same variable used in our main specification, but with a different metric (Columns (1) to (3)), $FarRightPartiesVotingInt_{i,m,r}$, the dummy related to far right parties voting intention (Columns (4) to (6)), and $LegaPartyVotingInt_{i,m,r}$, the dummy related to *Lega* party voting intention (Columns (7) to (9)). The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to one of the three maps considered in this study. (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Each column refers to Personal controls involve: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) take into account: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

Following Giuliano and Tabellini (2020), we present in Table 10 models where we employ as dependent variables some measures intended to capture individual ideology other then political preferences. Indeed, the authors assess the relation between the historical immigration and political preferences in the US, using both a measure of liberal ideology and a measure of support for the Democratic Party. In the same spirit, we want to test if our results are robust to the use of dependent variables such as personal beliefs on trust, public security, and religious pluralism. Mainly, in order to construct a measure of trust, we rely on a question where respondents are asked how much they agree with a person that believes most people are trustworthy. We give 1 to the individuals who answered "Quite a lot" and "Very much", and 0 to the remaining individuals who answered "A little" or "Not at all". What is more, to construct our second measure, we consider the following question: "One person thinks it is very significant that his country is safe and believes that the state should be on guard against threats from both inside and outside. How similar do you consider this person to be to you?". Principally, we create a dummy variable that takes the value

1 when the individual's response was "Quite similar to me", "Similar to me", or "Very similar to me". In the remaining cases, i.e. "Not at all like me", "Not like me", "A little like me", we assign value 0. Finally, we make use of a measure of individual openness to other religions. We refer to a question where individuals are asked how much they agree with a person that believes that religious pluralism is an ordinary phenomenon of democratic societies. The dummy takes value 1 when the respondents chose "Quite a lot" and "Very much", and 0 to the remaining individuals who answered "A little" or "Not at all". We present results from IV estimates in Table 10, where we report our baseline specifications according to the three separate port configurations and the three different distance thresholds. We always instrument the endogenous regressors with the traditional instrument based on the proximity to a Medieval port. Estimated coefficients of *MigrantPerception* in columns (1) to (3) of both Panel A and B have a negative sign, indicating that individuals with a negative perception of migrants exhibit a lower degree of trust in other people. Anyhow, the coefficient is significant only using 10 km threshold and port configurations of maps 1 and 3. Apart from this, F-statistics in columns (1) and (3) of Panel A are also greater than Olea and Pflueger (2013)'s critical values at 5% and at 10% of worst-case bias, respectively.

Turning to estimates in columns (4) to (6), the coefficient of *MigrantPerception* indicates that having a negative perception of migrants lead to a higher degree of concern about issues related to state security. We find a significant coefficient in columns (4) and (6) of Panel A. Even in this case, F-statistics related to this two specifications are higher than Olea and Pflueger (2013)'s critical values at 5% and 10% of worst-case bias, respectively. Finally, as far as our measure of openness to distinct religions is concerned, we find evidence that a negative perception of migrants brings a more negative view of religious pluralism. Indeed, in all specifications in columns (7) to (9) of panels A and B, the coefficient of *MigrantPerception* has a negative sign. Nevertheless, we find a significant coefficient only in specifications in Panel B, except for the ports configuration reported in map 2. The F-statistics are greater than 10 and satisfy the Olea and Pflueger (2013)'s critical values at 30% of worst-case bias.

Table 10: Robustness to the choice of the dependent variable: trust, public security and religious pluralism

Dependent Variable:	*Most of the people are trustworthy*			*The State must be on alert to protect itself*			*Religious pluralism is a value of democracy*		
	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 10 km									
Migrants Perception	-0.760*	-0.666	-0.772*	0.910*	0.694	0.907*	-0.677	-0.265	-1.031
	(0.436)	(0.943)	(0.413)	(0.513)	(0.465)	(0.470)	(0.506)	(0.396)	(0.664)
F-Statistic	51.70	35.56	35.35	51.70	35.56	35.35	51.70	35.56	35.35
Panel B: 15 km									
Migrants Perception	-0.803	-0.412	-0.776	0.254	0.147	0.332	-1.417***	-0.724	-1.593***
	(0.545)	(0.912)	(0.536)	(0.492)	(0.485)	(0.484)	(0.347)	(0.475)	(0.531)
F-statistic	15.66	18.81	15.68	15.66	18.81	15.68	15.66	18.81	15.68
NUTS-3 Region FE	√	√	√	√	√	√	√	√	√
Personal	√	√	√	√	√	√	√	√	√
Geography	√	√	√	√	√	√	√	√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variables are: a dummy related to the degree of trust in other people (Columns (1) to (3)), a dummy related to the belief that the State must be on alert to protect itself (Columns (4) to (6)), and a dummy related to the belief that religious pluralism is a value of democracy (Columns (7) to (9)). The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to one of the three maps considered in this study. (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we make use of a distance of 15 km. Each column refers to Personal controls involve: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) encompass: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. The sample weights are applied. All specifications take into account regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

To conclude our set of robustness checks, we leverage the survey's informative richness by exploiting whether the aforementioned relation between the perception of migrants and political positioning (or voting intention) is still valid, with opposite sign, even using left-hand positioning (or left voting intention) as our dependent variables. Chiefly we employ a measure of left political positioning, *LeftPositioning*, and a measure of the intention of voting the *Partito Democratico*, namely the *DemocraticPartyVotingInt*. The latter is constructed following the same logic of the *LegaPartyVotingInt* variable, and consider the following question: "How much are you likely to vote for Democratic Party in the next parliamentary election in a scale from 1 ("I would definitively NOT VOTE for it") to 10 ("I would definitively VOTE for it")?". It takes a value of 1 if the individual responded from 6 to 10 and 0 otherwise. The variable *LeftPositioning* is built using again the following question: "In politics, we often talk about "left" and "right". Considering your political beliefs, where would you place yourself?". The variable takes value 1 when an individual answered from 1 to 4, among a 1 (left) to 10 (right) scale, and 0 elsewhere. We omit observations for which the respondents answered "I don't place myself anywhere, I don't care". Estimated model using both the dependent variables are reported in Table 11, using TSLS estimation method. All the reported coefficients of *MigrantPerception* exhibit the expected negative sign, indicating that having a negative perception of migrants leads people to be less supportive of left-wing parties. Nevertheless, the coefficients are significant only in columns (1) to (3) of Panel B, and in columns (1) and (3) of Panel A.

Table 11: Robustness to the choice of the dependent variable: Left-wing Voting Intention

Dependent Variable:						
	LeftPositioning			***DemocraticPartyVotingInt***		
	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 10 km						
Migrant Perception	-0.323	-0.322	-0.534	-1.017*	-0.546	-1.103*
	(0.297)	(0.322)	(0.398)	(0.561)	(0.575)	(0.595)
F-Statistic	51.70	35.56	35.35	51.70	35.56	35.35
Panel B: 15 km						
Migrant Perception	-1.267**	-0.963**	-1.242***	-0.320	0.0916	-0.539
	(0.561)	(0.447)	(0.471)	(0.366)	(0.356)	(0.562)
R-squared	-0.633	-0.289	-0.600	0.011	-0.029	-0.063
F-statistic	15.66	18.81	15.68	15.66	18.81	15.68
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Observations	1,859	1,859	1,859	1,859	1,859	1,859

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variables are: $DemocraticPartyVotingInt_{i,m,r}$, the dummy related to left-wing political positioning (Columns (1) to (3)), and $DemocraticPartyVotingInt_{i,m,r}$, the dummy related to *Partito Democratico* voting intention (Columns (4) to (6)). The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to one of the three maps considered in this study. (For further details see Section 2.3). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls encompass: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) include: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

6 Heterogeneous Effects

We extend our analysis by investigating if the impact of migrant perception on far-right political positioning differs according to individuals characteristics or environmental aspects.

First, we split the sample according to the level of education, i.e. we divide individuals between those who have at least a university degree and those who do not. Indeed, it is well established in the literature that greater open-mindedness is associated with a higher level of education, thus reducing individuals' prejudices and tolerant attitudes towards immigrants.[46] As a matter of fact, students learn about disparate aspects of the world, which reduces fear of the unknown and of strangers (Vogt, 1997; Pascarella et al., 1996). We present results in Table 12, where we re-estimate our main model (Equations 3 and 2) in the two sub-samples. In odd columns the sample refers to not graduated individuals, while in even columns we consider the graduated ones. Results highlight that the correlation between the migrant perception and the far-right political positioning holds when considering not graduated individuals, while it ceases to exist for those who are (at least) graduated. It appears, as a result, that education is a powerful means of reducing prejudices arising from both historical and socio-economic characteristics of the place where one resides.[47] On the one hand, these findings support the positive impact of education that is unquestioned in the literature. On the other hand, the underlying mechanisms have been much discussed by researchers (Lancee and Sarrasin, 2015). One strand of literature fixates on the fact that educational institutions transmit norms of tolerance and equality, fostering tolerant and egalitarian attitudes towards immigrants. Other researchers, even so, support the ethnic competition hypothesis, whereby individuals with higher levels of education are less likely to compete with immigrants for the same job. Consequently, more educated individuals feel less threatened and are less likely to oppose immigrants.[48] Nevertheless, although an interesting topic, the investigation of the mechanism is outside the main purpose of our work.

[46] For a review see Ceobanu and Escandell (2010).

[47] As a further refinement of results shown in Table 12, we present in Appendix B additional estimates with regard to parental educational attainment. Indeed, parents' education indirectly relates to children's academic achievement Davis-Kean (2005).

[48] See among others Jenssen and Engesbak (1994); Hello et al. (2004, 2006); Hainmueller and Hiscox (2007); Meeusen et al. (2013).

Table 12: Heterogeneous Effect: Educational Attainment

Dependent Variable: *Far-Right Political Positioning*	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
	Not Grad	Graduated	Not Grad	Graduated	Not Grad	Graduated
Panel A: 10 km						
Migrant Perception	1.934***	7.186	1.412***	3.103	2.738*	5.892
	(0.724)	(18.02)	(0.515)	(2.925)	(1.430)	(7.967)
F-statistic	10.08	0.165	10.96	1.145	3.995	0.539
Panel B: 15 km						
Migrant Perception	2.732**	1.682	1.805**	2.174	3.769*	2.221
	(1.130)	(3.990)	(0.888)	(2.583)	(2.018)	(2.712)
F-statistic	6.294	0.563	6.885	1.293	3.437	1.357
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic	√	√	√	√	√	√
Social Capital	√	√	√	√	√	√
Migrants	√	√	√	√	√	√
History	√	√	√	√	√	√
Observations	814	1,045	814	1,045	814	1,045

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1) and (2)), Map 2 (columns (3) and (4), and Map 3 (columns (5) and (6), respectively. (For further details see Section 2.3). The sample is split in two separate sub-samples. In odd-numbered columns we consider individuals who do not have a bachelor's degree, while in even-numbered columns the model is estimated for individuals with at least a bachelor's degree. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls entail: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) encompass: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) involve: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) include: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

We further split the sample according to individuals' attitude towards social networks. More specifically, in the table below we analyze separately the group of individuals who spend more and less time on social networks respectively. We consider "Very Active Users" those who spend at least 3 hours a day on social networks, while "Normal Users" the others.[49] Our starting hypothesis is that young people who spend more time on social networks are somehow less driven by the context in which they live. According to Eurispes data, young people mainly utilize online channels (especially social networks) as their preferred information channel. It could be argued that on social networks there is a representation deficit, i.e. a self-selection mechanism of people.[50] According to *Social Network Theory* (Barabasi, 2016), furthermore, people are influenced by the social networks they belong to and the positions they hold within them. On the other hand, for the purposes of our analysis we believe that investigating the heterogeneous effect based on the use of social networks leads to major considerations. Indeed, with all its limitations, the assiduous presence on social networks can be a proxy for openness outside one's own context. Social networks make it possible to establish relationships that, until a few years ago, were constrained by territorial proximity.[51] Results shown in Table 13 confirm our hypothesis. In columns (1), (3), and (5) of panels A and B, the coefficient of $MigrantPerception_{i,m,r}$ is always statistically significant and positive. It is worth noting that the magnitude of the coefficient is generally greater than those in our main specifications (Tables 5, 6, and 7). This confirms that for those who do not utilize in an assiduous way social network the territorial context is even more important. In columns (2), (4), and (6), as expected, none of the reported coefficients is significant.

[49]According to data from the Censis Report (2019) on the social situation of the country, 78.4% of the population use the Internet, and 72.5% are present on social networks. Furthermore, it should be considered that our sample is made up of young people, and it is precisely for this group that the percentage of presence on social networks is close to 100% (Source: "52° Rapporto Censis 2019. *Gli italiani e l'informazione sul Web*"). Reading the audiweb data we discover that young people, on average, spend more than 2 hours a day on Social Networks. The decision to consider Active Users those who spend at least 3 hours on social networks derives from the above considerations (Source: https://www.audiweb.it/).

[50]Problems related to social networks include: confirmation bias, bandwagon effect, availability heuristic, overconfidence bias, and identity protection cognition. For more details see: Riva (2016).

[51]Moreover, Hermann et al. (2020) state that on social networks individuals have the opportunity to come into contact with groups of users of distinct nationalities and ethnicities. This may cultivate perceptions of ethnic diversity through which users infer and develop attitudes about ethnic diversity. The latter could then precisely shape cultural openness.

Table 13: Heterogeneous Effect: Social Networks

Dependent Variable: *Far-Right Political Positioning*						
	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
	Normal User	Very Active User	Normal User	Very Active User	Normal User	Very Active User
Panel A: 10 km						
Migrant Perception	2.895**	0.0350	1.192**	-1.663	2.157**	-0.375
	(1.306)	(0.397)	(0.577)	(3.732)	(1.007)	(0.470)
F-statistic	5.206	2.828	6.843	0.172	6.167	2.244
Panel B: 15 km						
Migrant Perception	2.884**	3.725	1.373**	5.047	2.389**	2.941
	(1.153)	(4.384)	(0.653)	(23.11)	(1.051)	(4.270)
F-statistic	7.514	0.455	6.213	0.0268	7.072	0.346
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic	√	√	√	√	√	√
Social Capital	√	√	√	√	√	√
Migrants	√	√	√	√	√	√
History	√	√	√	√	√	√
Observations	1,533	326	1,533	326	1,533	326

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the distinct specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1) and (2)), Map 2 (columns (3) and (4), and Map 3 (columns (5) and (6), respectively. (For further details see Section 2.3). The sample is split in two separate sub-samples. In even-numbered columns the model is estimated for individuals that spend more then 3 hours on social networks, while in odd-numbered columns we consider individuals who spend less then 3 hours on the social networks. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls take into account: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) involve: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) encompass: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) involve: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

Furthermore, we investigated a heterogeneous effect regarding whether or not people do (or did) volunteer activities. The basic idea is that individuals who are doing, or have done, volunteer activity are, by definition, more open to difference, tolerant, and less prejudiced. We show the results in Table 14. We estimate our model in the two sub-samples, showing in even-numbered columns only those individuals who are doing or have done volunteer work. The odd-numbered columns show the sub-sample consisting of those who answered "never" to the question about having volunteered at least once. In panel A and B, in columns (1), (3), and (5), the coefficient of migrant perception is almost always significant at the 1% level, confirming our hypothesis. Moreover, the relation disappears when considering individual who are doing, or have done, volunteer activities. This indicates to us that doing voluntary work increases tolerance in itself, making the impact of geographical proximity to a medieval port disappear.

Table 14: Heterogeneous Effect: Volunteer Activity

Dependent Variable: *Far-Right Political Positioning*	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	No	Yes	No	Yes
Panel A: 10 km						
Migrant Perception	0.798***	4.295	0.701***	2.221	1.077***	2.262
	(0.265)	(5.180)	(0.188)	(2.562)	(0.384)	(2.523)
F-statistic	8.236	0.756	11.37	1.051	8.254	1.179
Panel B: 15 km						
Migrant Perception	1.166***	6.519	0.641**	3.323	1.270***	4.259
	(0.351)	(8.908)	(0.285)	(4.253)	(0.397)	(6.248)
F-statistic	7.093	0.573	11.07	0.808	8.636	0.571
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic	√	√	√	√	√	√
Social Capital	√	√	√	√	√	√
Migrants	√	√	√	√	√	√
History	√	√	√	√	√	√
Observations	876	983	876	983	876	983

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1) and (2)), Map 2 (columns (3) and (4), and Map 3 (columns (5) and (6), respectively. (For further details see Section 2.3). The sample is split in two separate sub-samples. In odd-numbered columns the model is estimated for individuals that have never done a volunteer activities, while in even-numbered columns we consider individuals who are doing, or have done, volunteer activities. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls include: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) encompass: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) count: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) include: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications take into consideration regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

As a final heterogeneous effect, we make use of information about the toponymy of municipalities to test the validity of our results in certain cultural contexts. Indeed, according to Augustins (2004); Conedera et al. (2007); Weaver and Holtkamp (2016); Oto-Peralías (2018), street name can be used as proxies for the social and cultural characteristics of municipalities. We use toponymic data contained in the 2011 Istat population and housing census. The database contain information on 22 million names of streets, roads and squares related to all Italian municipalities. Following Oto-Peralías (2018), the naming decision process must meet certain conditions in order for the information on street names to be valid for the study of social and cultural phenomena. In our opinion, Italy fulfils these conditions since the streets are labelled with names and are the result of decisions that correspond to the commemorative priorities of the local community, reflecting people's social and cultural values.[52] Naming streets is a way of commemorating martyrs, heroes and glorious events to promote particular notions of national identity and history. With this in mind, we select streets dedicated to the phenomenon of resistance, to politicians opposed to fascism, to prominent post-war left-wing politicians, but also to artists who inspired tolerance and inclusion.[53] We look at the average number of such streets, standardising it for the population, in order to observe their average value in the municipalities contained in our complete sample. In the consequence of this, we create a sub-sample containing only individuals residing in municipalities with a higher than average value of streets with the above-mentioned toponymic characteristics. We expect these to be municipalities with a cultural "humus" more favorable to openness towards the different. What we want to test is whether the relationship between the perception of migrants, instrumented by the presence of a Medieval port, and the right political positioning continues to be valid if we use a control sample composed of municipalities with the aforementioned cultural traits.[54] With reference to this subsample, results in Table 15 show the estimates of the models in our main analysis (Table 3,4,5) using the full set of controls. Using the port configuration relative to map one, the coefficient of migration perception loses significance, while in columns 2 and 3 the coefficients are significant, but smaller in magnitude than those reported in table 4 and 5, respectively. This leads us to hypothesise that, in some way, living in a municipality with a collective culture of tolerance and openness played a positive role for those living far from a port.

[52]In Italy, the body delegated to decide on the naming of streets, roads and squares is the municipal council.

[53]By way of example, we have considered the following politicians: Giacomo Matteotti, Antonio Gramsci and Palmiro Togliatti. With regard to the phenomenon of resistance, we have included among others: streets containing the word 25 April (Italian day of Liberation from Nazism) and Partisans. With regard to artists, we have selected: Fabrizio De Andrè and Giorgio Gaber.

[54]It is important to note that the criterion for inclusion in the sub-sample applies only to the municipalities of residence of individuals living far from a Medieval port. In fact, the sub-sample contains all individuals living near a medieval port, regardless of whether their municipality of residence meets the above-mentioned requirements.

Table 15: Heterogeneous Effect: toponymy

Dependent Variable: *Far-Right Political Positioning*	Map 1	Map 2	Map 3
	(1)	(2)	(3)
Panel A: 10 km			
Migrant Perception	1.060	0.673**	1.082**
	(0.859)	(0.333)	(0.533)
F-statistic	2.792	22.26	7.390
Observations	475	536	519
Panel B: 15 km			
Migrant Perception	1.405	1.025***	1.300*
	(1.188)	(0.379)	(0.669)
F-statistic	1.191	4.769	3.076
Observations	494	555	537
NUTS-3 Region FE	√	√	√
Personal	√	√	√
Geography	√	√	√
Socio-Economic	√	√	√
Social Capital	√	√	√
Migrants	√	√	√
History	√	√	√

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1)), Map 2 (columns (2)), and Map 3 (columns (3)), respectively. (For further details see Section 2.3). The sample include only individuals who reside in municipalities with number of streets named after left-wing political and popular culture figures, and dedicated to the resistance against Nazi-fascism above the average value in the full sample. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls involve: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) entail: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) encompass: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) include: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications cover regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

Finally, we restrict our sample to those municipalities that are located within a radius of 15 kilometres (expressed in geodetic distance) from the sea. Although all our specifications include, among others, geographical controls that take into account important spatial features, we check whether results hold when we employ a subsample nearly comparable according to both observable and unobservable characteristics. We present in Table 16 the results of the IV estimates. The *MigrantPerception* coefficient, is significant and positive in columns (1) and (3), suggesting that the effect of the proximity to a Medieval port typically holds when considering only the coastal municipalities. It is worth noting that the coefficient of $ProximityMedPort_{m,r}$, in the First stage model as reported in Table 16, is always negative and statistically significant, thus confirming an important difference in migrant perception between individuals who live 15 km from a Medieval port and individuals who live 15 km from the sea. We employ weak-instrument robust inference for specifications in columns (1) and (3), and we reject the null hypothesis of Wald test at 10% and 5% level respectively.

Table 16: Heterogeneous Effect: distance from the sea

Dependent Variable: *Far-Right Political Positioning*	Map 1	Map 2	Map 3
	(1)	(2)	(3)
IV Estimates			
Migrant Perception	1.020*	0.476	1.095**
	(0.859)	(0.333)	(0.533)
F-statistic	4.750	3.959	6.428
First Stage Results			
Proximity to Medieval Port	-0.179**	-0.144**	-0.186***
	(0.082)	(0.072)	(0.073)
NUTS-3 Region FE	√	√	√
Personal	√	√	√
Geography	√	√	√
Socio-Economic	√	√	√
Social Capital	√	√	√
Migrants	√	√	√
History	√	√	√
Observations	672	672	672

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1)), Map 2 (columns (2)), and Map 3 (columns (3)), respectively. (For further details see Section 2.3). The sample include only individuals who reside in municipalities within a 15 km radius (in geodetic distance) from the sea. In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we make use of a distance of 15 km. Personal controls encompass: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) involve: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) take into account: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) encompass: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

7 Municipality Level Analysis: Hard Outcomes

So far, the analysis has focused on studying the relation between perceptions of migrants and two main outcomes, namely each individual's political ideology and voting intentions. Thus, both variables are self-reported and by definition cannot be supported by hard evidence. In this section we present a municipal-level analysis exploiting the relation between the proximity to a Medieval port and results in the 2018 National Elections. In particular we estimate the following reduced form equation:

$$RightVotesShare_{m,r} = \alpha_r + \beta ProximityMedPort_{m,r} + X_m \pi + \epsilon_{m,r}, \quad (4)$$

where $RightVotesShare_{m,r}$ is the share of votes obtained by the right-wing coalition in the Italian national election in 2018, in municipality m, in NUTS-3 region, r.[55] $ProximityMedPort_{m,r}$ represents our usual measure of proximity to a Medieval port, $X_m \pi$ include the usual set of controls at the municipal level, and α_r account for NUTS-3 regional fixed effect. Alongside, in estimating Equation 4 we account for average weights based on the resident population in each municipality in 2017. In the light of the fact that we deal with a continuous variable of vote share, we do not need to use the usual "dummy variable approach".

We present OLS estimates of the model in Table 17. In estimating our model the three different maps are considered, and for each of them we report both results for the baseline specifications (odd columns) and models that account for the full set of covariates (even columns). Our traditional cut-offs are applied in the two different panels A and B. Reported coefficients are always positive and significant, tipically at 1% level. This conclusions support our findings based on individuals' perception as presented in Table 4.

Finally, in the spirit of Acemoglu et al. (2020), in the Appendix B we provide a falsification test. We concentrate on past national election, especially on both 1963 and 1968 elections. Results show that the relation between electoral outcomes and proximity to Medieval port is never significant, thus implying that in those times openness towards the different, and expressly the Muslim, was not a salient issue for voting to right-wing parties.

[55] The Italian Parliament is composed of the Chamber of Deputies and Senate of the Republic. The national elections for the renewal of members of the Senate of the Republic and Chamber of Deputies are held on the same day, with two separate votes. The Senate members are elected by universal suffrage and by citizens who are 25 or older, while the Chamber comprises members elected by universal suffrage and by citizens who are 18 or older. Given the age target of our respondents (18 - 35 years old), in this analysis we consider the votes share obtained by the coalition at the Chamber of Deputies. The right-wing coalition in the Italian national election in 2018 was composed by: *Lega*, *Casapound Italia*, *Fratelli d'Italia*, *Italia agli Italiani*, *Grande Nord*, *Forza Italia*, *Noi con l'Italia*, and *Il Popolo della Famiglia*.

Table 17: Hard Outcomes Results

Dependent Variable: ***Right-wing Parties Coalition, % votes in National Election 2018***						
	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 10 km						
Proximity to Medieval Port	0.0434***	0.0230**	0.0407***	0.0181*	0.0447***	0.0256**
	(0.00957)	(0.0116)	(0.00806)	(0.0108)	(0.00790)	(0.00978)
R-squared	0.728	0.756	0.729	0.756	0.730	0.757
Panel B: 15 km						
Proximity to Medieval Port	0.0509***	0.0358***	0.0469***	0.0296***	0.0492***	0.0349***
	(0.00604)	(0.00630)	(0.00520)	(0.00795)	(0.00517)	(0.00603)
R-squared	0.730	0.758	0.731	0.758	0.732	0.759
NUTS-3 Region FE	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic		√		√		√
Social Capital		√		√		√
Migrants		√		√		√
History		√		√		√
Observations	7,960	7,960	7,960	7,960	7,960	7,960

Notes: All specifications are estimated by Ordinary Least Squares. The dependent variable is $RightVotesShare_{m,r}$, the % of votes received by the Right-wing coalition in the National Election in 2018, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to three different maps (For further details see Section 2.3). In panel A, the proximity threshold is 10 km on the ancient Roman road network. In panel B we use a distance of 15 km. Geography controls (at the municipal level) take into consideration: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) encompass: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) include: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The 2017 municipal population weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

8 Conclusions

This article investigates whether migrant perceptions of individuals have a significant impact on their political positioning. While the literature has addressed the issue of the relation between presence of migrants and votes, to the best of our knowledge, no one has addressed the issue using individual perceptions and ideologies. One of the main challenges concerns the likely endogeneity of migrant perception. Indeed, it might happen that the perception of migrants shapes political ideology, or vice versa. To overcome this issue, we rely on a Instrumental Variable approach.
The way this work contributes to the literature is two-folds. First, we attend to data of perception and ideology at the individual level, rather than on the relationship between actual immigration and electoral outcomes. Indeed, the main variables in our models are based on individuals' responses to questions related to political positioning, party voting intention, and the belief that migrants do or do not make Italy an unsafe place. Second, to overcome the problem of endogeneity we propose a set of instruments that have never been used. That is, we instrument the individual perception of migrants with the distance of each individual's municipality of residence from the nearest Medieval port. Specifically, we consider the ports that in the Medieval Era, according to several historical maps and sources, had a trade route to Africa. Having routes to Africa in the Middle Ages implied hosting slave communities, as well as communities of sailors who met Muslims in Islamic ports. Thus it meant somehow being in contact with different people many years earlier than those who lived elsewhere. All the ports configurations considered are related to trade and routes in the Mediterranean between the 11^{th} and 15^{th} centuries. Chiefly, we identify new instruments sets, according to three separate ports configurations. In addition, we employ two different distance thresholds, i.e. 10 and 15 kilometers, alternatively. We argue that our instrument, conditional on controls, is exogenous, and entails different political positioning across individuals through its influence on current migrant perception. Furthermore, our first-stage results confirm the relevance of our instrument. We always find a significant correlation between positive (negative) attitudes toward migrants and proximity (distance) from a Medieval port, in line with the literature of long term persistence of (in)tolerance behaviours towards minorities.
Overall, our two stage least squares estimates clearly suggest that personal beliefs on migrants play an authoritative role in shaping political ideology of individuals.
In our main analysis, we make use of a measure of far-right political positioning as the dependent variable and a measure of negative perceptions of migrants, instrumented by proximity to a medieval port, as the main explanatory variable. Using a dummy approach, we observe that the coefficient of the negative perception of migrants is always positive and significant. Thus, residing in a municipality distant

from a port is positively correlated with a negative perception of migrants, which in turn increases the probability of having extreme right-wing ideologies. The relation is always valid when using the 10 or 15 kilometers thresholds, and ceases to exist beyond 20km. On top of that, this relation holds even when we include the full set of controls in the models. Thus, ports play a role that is robust to the inclusion of variables that take into account personal, geographical, socio-economic and even historical characteristics.

In our robustness checks we first control if the relation between the perception of migrants and the distance from a Medieval port capture a kind of "openness" that is common to all port cities today, regardless of their Medieval routes. In the same spirit, we check whether the main results are derived from mere proximity to the sea, or not. In both cases, the coefficient of the migrant perception is not significant, thus excluding these two potential explanations. Moreover, in order to test whether our instrument can be spuriously correlated with the perception of migrants, we have created a "placebo" indicator of proximity to a Medieval port. Even in this respect we do not detect any effect of the "placebo" instrument, confirming that the relation under scrutiny is not mechanical and automatic. We then investigate a set of robustness to the choice of the dependent variable, using both different measures that capture alternatively the far right ideology or voting intention, and variables related to personal beliefs on trust, public security, and religious pluralism. Overall, our main findings still valid. Finally, we verify whether the relation between the perception of migrants and political positioning (or voting intention) is still valid, with opposite sign, even using left-hand positioning (or left voting intention) as our dependent variable. Overall, our results confirm this hypothesis.

Turning to the heterogeneous effects, we first split the sample according to the level of education of individuals and we find that the effect of migrant perception on political ideology disappears when we consider the sub-sample of graduated individuals. What is more, when investigating the parental educational attainment results go in the same direction. In our second heterogeneous effect we split the sample according to the time spent by individuals on social networks. Social networks seem to play a significant role in reducing geographical constrained. Our third heterogeneous effect relates to whether or not people did volunteer activities. Results suggest that doing voluntary work increases tolerance in itself, making the impact of geographical proximity to a Medieval port disappear. Finally, we check whether also the toponymy of municipalities can reduce the gap in terms of openness and tolerance between who lives in a city with a Medieval port, and who does not. Even though not conclusive, results seem to support this hypothesis.

The last part of this study is devoted to a municipal-level analysis, i.e. we analyze the relation between the proximity to a Medieval port and results in the 2018 National Elections. We find a positive and significant relation between the right-wing coalition share of votes and the proximity to a Medieval port.

In sum, our analysis shows that migrant perception matter for political ideology. Our instrumental variable approach based on historical instruments suggests that history can be important in shaping today's attitudes and values. Indeed our analysis shows that a history of tolerance, acceptance and openness can last for centuries. Where such a milieu does not exist, education can create the basis for it.

This study is focused on a very crucial aspect such as the relation between politics and the perception of migrants. However, we are aware that individual political choices can be determined by other socio-cultural characteristics that have their roots in the distant past. We aim to investigate this issues in the next future.

9 Appendix A: Related Literature

We present in this Section a complete review of the literature, with a specific attention to three separate strands. Section A.1 is devoted to the literature on the relation between the presence of migrants and votes to (extreme) right-wing parties. Section A.2 presents the relevant studies on the long run persistence of culture, institutions, and attitudes towards minorities. In Section A.3 we present some studies belonging to a more recent strand dedicated to the persistence of infrastructures, with particular attention to the ancient Roman road network and ports.

A.1 Presence of migrants and voting behaviour

In recent years, many researchers in the social sciences have investigated the impact of the presence of migrants in a given territory on voting behaviour and on other electoral or similar outcomes. More specifically, the current literature on migration is very active on these issues, especially considering the causal impact of the presence of refugees on right-wing party voting and anti-government sentiment. Considering refugees and not migrants means considering only those who have been forced to leave their homes. Refugees, according to the Office of the United Nations High Commissioner for Refugees (UNHCR), are people who are "fleeing armed conflict or persecution" and "for whom denial of asylum has potentially deadly consequences." Refugees leave their home countries because it is dangerous for them to stay.[56]

Anyway, in this work we analyze a mixed literature which takes into account both migrants and refugees. The anecdotal evidence gives us good reason to assume that the increase in the presence of migrants/refugees, and thus in ethnic diversity, has a causal impact on political outcomes. All the same, our understanding of literature is that the evidence on migrants/refugees and voting is still mixed. A first group of researchers found a positive impact of the presence of migrants/refugees on the number of votes obtained by anti-migrant, right-wing populist parties.

Dustmann et al. (2019) studying the specific case of Denmark, exploit a policy that assigns refugees to municipalities on a quasi-random basis to assess the causal effect of refugees migration on voting outcomes. They find that in all municipalities, except the big cities, the allocation of a greater share of migrants between electoral cycles leads to an increase in the share of votes to the right wing anti-migrant parties.

Harmon (2018) also studies the Danish case, though focusing on migration and electoral outcomes of the last 20 years of the 20^{th} century. To address endogeneity problem, he use an instrumental variable strategy. Since by law in Denmark the ability of foreigners to buy a house is limited, he exploits an instrumental variable strategy using the number of rented accommodation as instrument for the probability of the place to be chosen by an immigrant to live. The author, like Dustmann et al. (2019),

[56] Source:https://www.britannica.com/story/whats-the-difference-between-a-migrant-and-a-refugee.

finds a positive impact of ethnic diversity associated to the increased presence of migrants on votes in right-wing anti-migrant parties.

Dinas et al. (2019) exploit a natural experiment in the Greek islands on the border of Turkey, and report evidence of an increase in votes for the far right *Golden Dawn* party in the islands most exposed to the arrival of refugees compared to those less exposed (but with similar institutional and socioeconomic characteristics).

Vasilakis (2018) studies the impact of the presence of refugees in the Greek islands on votes in the Golden Dawn party and found results in line with those of Dinas et al. (2019), robust to new and disparate methodologies of estimation and placebo regressions.

Halla et al. (2017) study the Austrian case by analyzing the impact of the presence of migrants on votes in the *Freedom Party*, the anti-migrant party. They find that a large share of the votes to the far-right can be attributed to cross-community variation in the inflow of immigrants.

Barone et al. (2016) study the Italian case that is of great interest given the large influx of migrants in the last years. In order to assess the impact of immigration on the political preferences of the natives, they make use of municipal data and an instrumental variable strategy in the tradition of Card's shift-share approach, in the same way described by Card (2001) as modified by Cortés and Pan (2015). Like all the other contributions so far presented here, they find a positive impact of the presence of migrants on right-wing party voting.

Bratti et al. (2020) using a unique dataset on the location of refugee reception centres in Italy, i.e. SPRAR[57], analyze the existence of geographical spillover effect of refugee settlements on voting behaviour in Italy. Their results show that for those who live near a SPRAR centre, the turnout increases and so does the likelihood of voting for an anti-migrant party.[58]

In addition to that, there exists a literature which find results substantially in line with those analyzed so far but which, in addition, find a heterogeneous effect of immigration on native support for anti-immigration parties based on the ethnic origin of immigrants. It is find that the presence of Turkish and Maghreb citizens favors the success of the extreme right, while the presence of immigrants from other ethnic groups has no effect (Coffé et al., 2007, in Flanders) or its effect is much lower (Shvets, 2004, in France). Also, Mendez and Cutillas (2014) finds something similar in Spain, a country that welcomed more than 6 million immigrants between 1998 and 2008. Their results show that Latin American migrants have no effect on voting while African migrants have a negative effect. These arguments go in the direction of reducing the weight to be given to economic "competition" in this mechanism, putting

[57] SPRAR is the acronym that stands for "Sistema di protezione per richiedenti asilo e rifugiati", and in Italy SPRAR centres identifies refugee reception centres.

[58] And also the likelihood of an antigovernment choice, i.e. voting "No", in the 2016 constitutional referendum.

more emphasis on cultural, language, religious and ethnic aspects.
Contrary to the literature surveyed so far, another group of researchers found a negative or zero impact of the presence of migrants on the amount of votes obtained by anti-migrant and right-wing populist parties.
Altındağ and Kaushal (2020) study the impact of the influx of more than 3.5 million Syrian refugees on individual political preferences in Turkey in 2012-2016. The Syrian Civil War has been one of the largest movements of people since World War II, causing an unprecedented influx of Syrian refugees into Turkey. To carry out the analysis, they make use of difference-in difference research design and compare areas with high and low refugee intensity, before and after the begin of the Syrian civil war. To overcome the endogeneity problem of the refugee's location choice, they adopt an IV approach using both a historical measure of the presence of arabic speakers in the various Turkish provinces and a measure of road distance between Turkish and Syrian residential areas, both proxies of refugee flows during the study period. Their results suggest that the influx of refugees has only a modest effect on the political affiliations of Turkish voters and a negligible effect on the actual voting results. One possible explanation is that the Turks do not hold Erdogan, head of *AKP* (party in charge), responsible for this huge influx of refugees. Another possible explanation is that the opposition parties have not offered any restrictive migration policy that would induce the electorate to change affiliation.[59]
Fisunoğlu and Sert (2019) also study the Turkish case using a difference-in-difference identification strategy, comparing electoral outcomes in cities hosting few refugees with cities with large refugee populations. What they find is a non-statistically significant effect of refugee presence on voting outcomes, in line with the results of Altındağ and Kaushal (2020).[60]
Gehrsitz and Ungerer (2017) using administrative data on refugee allocation study the influx of more than one million refugees into Germany between 2014 and 2015. Using the existence of an automatic housing allocation mechanism for refugees, they study the short-term impact of migration on votes, as well as other outcome variables. The analysis suggests that counties with a higher influx of refugees see neither more nor less support for the main anti-immigrant party than counties with small influxes. At any rate, this is not the main finding of the paper. As also specified by the authors themselves, these results must be taken with caution for at least three reasons. First, the analysis is based on only a small subset of all German states and counties. Secondly, the far-right *AfD* party did not exist in the previous state elections considered,

[59]The *CHP*, the only political party that has supported more restrictive policies towards refugees, is probably suffering from the idiosyncrasy of voters in the transition from secular to religious political camps. Moreover, it should not be forgotten that the Turkish political environment is very different from that of Western countries.

[60]Among the factors leading to this result, Fisunoğlu and Sert (2019) also highlight the ability of the incumbent party to provide social and political services, avoiding discontent deriving also from socio-economic comparisons between the poorest population and refugees.

and what is more, between 2013 and 2016 it changed its political orientation several times, focusing first on a firm opposition to the euro and then on the migration issue. Finally, researchers still find a heavier loss of votes in counties with a larger influx of migrants than in those with a smaller influx.
Steinmayr (2020), like Halla et al. (2017), studies the Austrian case, but comes to opposite conclusions. In his analysis he distinguishes between exposure to refugees at macro and micro level and suggests that these have contrasting effects in terms of electoral support to extreme right-wing parties in Austria. For eminently practical reasons, most of the refugees were housed in boarding houses, so the researcher use an IV strategy using the existence of group accommodation in a community as instrument. Indeed, communities with group housing are 30 percentage points more likely to host refugees. Macro exposure means exposure to the media, both social and traditional media, as well as election campaigns, while micro exposure means contact with refugees. The study concluded that, on the one hand, macro exposure has led to an increase in votes for the anti-migrant party *FPOE* and, on the other hand, direct contact with refugees at neighbourhood level has led to a decrease in the *FPOE*'s share of votes. Steinmayr (2020) explains this negative effect with the "contact theory" of Allport et al. (1954). Allport et al. (1954) states that interpersonal contact is an effective way to reduce prejudice between a majority and a minority. Nothwithstanding, certain characteristics of the work must be taken into account when reading this data carefully. The time horizon is much shorter than the one considered by the author, and the negative impact is likely to occur more in the medium to long term, when, for example, the negative effects of competition on the labour market unfold. Secondly, only refugees are taken into account and not migrants as a whole, as other studies have shown. Furthermore, the work deals with the extensive margin, i.e. the presence of refugees, while many studies put mind to the intensive margin, i.e. the percentage of refugees in the total population.
Overall, recent research on the electoral consequences of the presence of refugees consists of case studies scattered in various countries at different times. However, most of the works with a focus in European Union countries, and which consider more robust assumptions, comes to a similar conclusion: the influx of refugees tends to increase support for right-wing and anti-immigrant parties and, usually, to decrease support for ruling parties.

A.2 Persistence of Culture and Institutions

An authoritative recent literature has documented the long-term persistence and long-run effects of institutions and culture on various outcomes.
Acemoglu et al. (2001*b*) in their seminal contribution, following North and Thomas (1973) and North (1990) question the fundamental causes of the difference in GDP

per capita between nations. Countries with better institutions, better property rights and less distortionary policies, invest more in physical and human capital and utilize it more efficiently to achieve better income levels. Starting from the relation between institutions and economic performance, they seek a source of exogenous variation in institutions to avoid problems of reverse causality. Using the mortality rate of soldiers, bishops and sailors between 1600 and 1800 in the colonies as an instrument for the quality of institutions today, they estimated a large impact of institutions on economic development.

Tabellini (2008) studies the persistence of institutions by investigating some outcomes related to values and behaviours, such as trust and respect. Following Banfield (1958), he starts from the concept of "amoral familism" and the definition of generalized morality, which plays a crucial role in the good functioning of institutions. Indeed, in this scenario citizens are more law-abiding, bureaucrats are less corrupt, and those who vote demand and monitor that there are higher standards of behaviour among politicians. They are also more inclined to vote for the general good and not just for their own. He also assumes that values are largely transmitted vertically, from one generation to another, in a conservative mechanism which takes place mostly within the family, rather than across unrelated individuals. He finds evidence that past political institutions leave their mark on values and attitudes, in particular respect and trust. Using data from the world value survey on the current values of the second generation of immigrants in the US, he finds that values reflect characteristics of the country of origin of the respondents' ancestors.

Similar evidence is also found with aggregate data on European regions in Guiso et al. (2006). Indeed, authors find that current values close to the concept of generalized morality are more widespread where centuries ago executive powers were limited by the prerogatives of an independent judiciary or a chamber of political representatives. The second finding of the paper is related to the relation between values and economic or institutional outcomes: regions whit more trust and respect nowadays are more developed, and have grown more in the last 30 years.

Putnam and Leonardi (1993) state that social capital can be the result of historical experience, and especially the explanation of the large differences in social capital between North and South to the period of independence that Northern cities had 500 years ago. Guiso et al. (2008), following Putnam and Leonardi (1993), try to explain the persistence of effects of Northern Italy independent cities and thus the long-lasting effects of institutions. To do so they begin from an overlapping-generations model where children absorb the prior from their parents and then, after experiencing the real world, they transmit it (updated) to their own children. In that way, they explain the long-term persistence of social capital, even after 500 years. Their definition of social capital is "the set of beliefs and values that foster cooperation". Their theoretical model is also validated using the German national socio-economic

panel and the world value survey.
Durante (2009) complements the literature on the long-term persistence of cultural norms examining the historical relation between risk, cooperation and the emergence of social trust. He tests high-resolution climate data for the period 1500-2000 with contemporary survey data (European social survey). His main findings are that regions characterized by environmental risk, namely higher year-to-year variability in temperature and precipitation, have higher levels of trust. The result documents that historical patterns of cooperation and norms of trust developed in response to risk continue to influence how individuals relate to each other nowadays. That is, he empirically finds that a culture of trust may have emerged in areas with more variable and heterogeneous weather patterns, and that differences in trust have persisted over time.[61]
In the same spirit, Giuliano and Nunn (2021) studies the determinants of cultural persistence based on a class of models derived from evolutionary anthropology (among others Richardson and Boyd, 1985; Feldman et al., 1996). In the light of numerous examples of cultural persistence and also of cultural change, they wonder when culture persists and when it changes. Their hypothesis is that in states where the environment is more stable across generations, more weight is given to tradition and there is greater cultural persistence (and vice versa). To test this hypothesis, they utilize climate variability across two sources of paleoclimatic data measured at 20-year intervals from 500 to 1900 as an indicator of intergenerational stability. To do this, they make use of various source of data. The first dataset is related to the importance of self-reported tradition from the World Value Survey (WVS), while the second dataset is related to gender role norms, polygamy and consanguineous marriage across countries over long period of time. In addition, they also employ other data related to behaviours of children of immigrants in the same nation and the behaviours of indigenous populations. All the strategies used and all the populations studied lead to the same conclusion. That is, that traditions are less important and culture less persistent in populations that have ancestors living in environments that are less stable across generations.
Instead, Durante and Buggle (2020) study the relation between pre-industrial climatic variability and trust levels today. What they find is that, among European regions, those with greater pre-industrial climatic variability show higher levels of trust today. And this phenomenon gives the impression of being pronounced in agricultural regions. Farmers in a pre-industrial rural economy, in fact, in the absence of well-functioning credit and insurance markets were forced to use strategies to protect themselves from climate risks. And these often involved various members of the community. Among these, various examples of inter-community exchange and simi-

[61] The result is robust to the inclusion of country fixed effects and a variety of others controls (geographical, political and economic).

lar behaviours are well documented in the historical, anthropolitical, and economic literature (Dean et al., 1985; Halstead and O'Shea, 2004; Platteau, 2000), among others. However, these socioeconomic connections often and frequently required high levels of interpersonal trust. Their study uses high resolution climate data for the period 1500-2000 and contemporary survey data regarding self-reported trust for a sample of more than 250,000 individuals living in 25 different countries. Fielding (2018) puts forward the hypothesis that intolerance in a given period towards a minority is at least partly a function of characteristics which, transmitted from generation to generation, will lead to intolerance towards other people in subsequent periods. He studies the relation between attitudes towards immigrants in 21st century England and the distribution of Jews in 12th and 13th century Britain. He finds that tolerance towards immigrants in today's England is much higher in regions that had a Jewish settlement in the Middle Ages. In the same regions, he also finds less support for far-right parties. What Fielding (2018) finds is in line with what Allport et al. (1954) found in his seminal work on the effect on prejudice of personal contact with members of an external group. A fundamental work regarding the intergenerational persistence of regional variation of prejudice is Cavalli-Sforza and Feldman (1973), who develop a mathematical framework to predict the level of persistence of variation between and within communities. This literature suggests that the determining factor in measuring the extent of persistence in regional variation of culture is probably the strength of many-to-one assimilation effects compared to parental inheritance effects. In social psychology, among others McFarland (2010) argues that dislike towards out-groups is linked to other traits such as authoritarianism and social domination. These psychological traits would then have been transmitted to the next generation. These conclusions imply that some cities have an inherent capacity to deal more easily with ethnic diversity.
Alesina et al. (2013) examine the historical origins of intercultural differences related to the role of women in society. If in some societies the dominant belief is that it must be normal for them to have an occupation, in other societies the role of women is still confined within the domestic sphere. Specifically, the authors test the hypothesis put forth by Boserup (2007), that differences in gender roles have their origins in the form of agriculture traditionally practiced in the pre-industrial period. To better understand the causal impact of plough agriculture on cultural norms, they instrument them with the geoclimatic conditions of the various states, a condition that strongly influences the advantage of having plough agriculture. They find a positive and solid relation between the historical plough use and unequal gender roles today. These conclusions hold across countries, districts within countries and ethnicities. In researching the transmission mechanisms, they study both the possibility of persistence of cultural beliefs or the result of the development of different institutions, policies and markets. Examining the children of immigrants living in Europe and

the United States, they found that at least in part culture and values matter. Their findings refer and contribute to a greater understanding of the origins of cultural norms and beliefs.

Guiso et al. (2016) start from the seminal work of Acemoglu and Robinson (2012), who argue that shocks to institutions can affect results over extended periods of time. What they want to probe is whether the culture or formal institutions are the source of long-term persistence. In doing so, they take into consideration the Italian case and more specifically they wonder if the Italian cities that were free city-states during the Middle Ages now show higher levels of civic capital than the others. This case study gives the impression of being particularly suitable as it is easy to disentangle the impact of institutions and the impact of culture as formal institutions are long gone. The conclusions go exactly in the assumed direction, net of the use of various controls and civic capital indicators (non-profit organizations, blood donations, frequency of cheating in a national exam). The degree and the duration of independence of the historical free city-states also positively impact today's civic capital. [62] And this report remains valid even taking into account the fact that city states have not become independent in a random way. Furthermore, the authors question the potential transmission channel. Based on Banfield (1958) studies, they hypothesize that the city-state experience has fostered a sense of self-efficacy. Following the work of Putnam et al. (2000) and Ostrom (1990), they also argue that direct participation in public life empowers people by strengthening their sense of self-efficacy. In this sense, events that impact on attitudes shape culture that, in turn, is transmitted over the centuries through education and socialization. In conclusion, it can be said that such findings are consistent with the idea that distant historical experience can influence individual behaviour many years later.

Nunn (2012) reviews the literature related to the long-term impact of historical shocks. In this context, culture is intended as the "rule of thumb", evolved according to the need to make decisions in complex and uncertain environments. Thus, culture is understood as the set of decision-making heuristics that often manifest themselves as values, beliefs or social norms (Boyd and Richerson, 2005). The author argues that cultural change and persistence are two channels through which culture continues to matter today. Mainly, the articles on which the review is focused describe historical shocks that have had a long-term impact on culture. The work cites various sources that explain the cultural difference between societies, and then goes on to analyze history as a source of historical persistence, explaining the difference between vertical and horizontal transmission. The following works are divided into sections: migrations to the United States, farming practices, Africa's slave trades, episodes in European history and religion. Of interest is the article by Greif (1994), who through a mix of game theory and archival evidence studies the historical origins of the di-

[62] On related issues and focused to Italy, see Di Liberto and Sideri (2015).

vergent evolution of collectivist and individualist cultures among the Genoese and Maghribis. The work explains that from two separate types of commercial relations have derived two different cultural trajectories. Influences that still affect various outcomes today. The last part of the paper explains the relation between institutions and culture, deepening the work of Tabellini (2008) and Guiso et al. (2016)

Nunn and Wantchekon (2011) using contemporary individual survey data from the 2005 Afrobarometer survey and historical data about slave shipments by ethnic group explain how current differences in levels of trust in Africa can be traced back to the transatlantic and Indian Ocean slave trades. What they find is that individuals whose ancestors were heavily raided during the slave trade are less trusting today. Focusing on transmission mechanisms and using different identification strategies, they also find that the impact on trust occurs through factors within the individual such as cultural norms, beliefs and values. And this remains valid by controlling for the various forms of European influence and pre-colonial characteristics of ethnic groups. The authors also report an IV specification in which they employ distinct measures of distance from the coast. This measure works only in Africa and not in Asia and Europe, demonstrating that it is a valid proxy for the likelihood of being the subject of the slave trade in the Middle Ages. There are as a result two transmission mechanisms that have been hypothesised. The first relates to the influence of the slave trade on the cultural norms of ethnic groups, while the second relates to the fact that people in the areas most affected by the slave trade are less confident because legal and political institutions have also deteriorated as a result of this historical phenomenon. The tests reported reveal that both channels are important. One result, perhaps the most significant of the article, is that one of the reasons why history still counts today is through cultural norms.

Also Voigtländer and Voth (2012) study the persistence of cultural traits. They focalize on the German case, studying inter-ethnic hatred through a new set of data from almost 400 cities in which Jewish communities are documented for both the Medieval and inter-war periods in Germany. Here Jews were often blamed, especially in the period between 1348 and 1350 when the Black Plague killed about a third of the population living in Europe. Using plague-era pogroms as an indicator for Medieval anti-Semitism, they find relevance in predicting violence against Jews in the 1920s, over 600 years later. And this is valid for several outcomes: votes for the Nazi Party, attacks on synagogues, deportations after 1933, and letters to Der Sturmer. The authors find heterogeneity on treatment effects associated to the level of trade or immigration of the different cities, which seem to reduce the influence of persistence. And this relation remains valid by checking for economic, geographical and institutional variables. This is largely irrelevant to the anti-Semitism of the 20^{th} century. Here again, the main conclusions of the paper go in the direction of demonstrating the historical origin of modern attitudes.

Becker et al. (2016) study the Habsburg Empire and the long-term persistence of cultural norms and the functioning of institutions many generations after the Habsburg Empire and its institutions ceased to exist.[63] More specifically, they analyze citizens' trust in these state institutions and corruption. To do so, they use a dataset of the Life in Transition Survey (LiTS) that provides measures of trust and corruption in Eastern European countries. Using these data and various historical sources, they analyze data from 17 countries that are the successor states of the Habsburg Empire and neighboring countries, using border specification and two-dimensional geographic regression discontinuity to identify individuals with or without a past under the Habsburg Empire. The analysis is carried out by comparing individuals living in communities within 200 km of each other on both sides of the Habsburg border and using country fixed effects to avoid taking into account unobserved country heterogeneity and using another Regression Discontinuity Design (RDD) specification including latitude and longitude relative to the location indicated by the individuals in the survey. In both specifications the results suggest that, by establishing cultural norms, the Habsburg Empire still influences human interactions with their state institutions. Individuals who lived under the Habsburgs have more confidence in the police and courts than those who lived near them but never lived under the Habsburgs. And the same can be said about a negative view of corruption. Through a robust series of falsification tests the authors validate the causal channel of interpretation and exclude the presence of other historical channels.

Fritsch and Wyrwich (2014) study Germany focusing on the long-lasting effects of the regional entrepreneurship culture. Their results suggest the existence of a regional culture of entrepreneurship. This culture is reflected in informal institutions, i.e. in the norms, values and codes of conduct of a society that are in favor of entrepreneurship. In doing so, they analyze three separate scenarios over a period of time of about 80 years. Furthermore, the validity of the results for East Germany, a region that has been under a socialist regime for forty years, together with other robustness checks, supports the thesis that the change is due more to cultural than socio-economic aspects. This is in line with other research showing a high stability of informal institutions over time (among others North, 1990; Williamson, 2000). History matters.

Algan and Cahuc (2010) are part of the debate on the fundamental causes of differences in GDP per capita between countries. Chiefly, they look at the importance of

[63] On the same topic, see Peisakhin (2013), who aims to analyze the impact of imperial institutional legacies on contemporary mass attitudes and political behaviour. The author's aim is to verify whether historical political identities are able to persist for a long time after the disappearance of the formal institutions that generated them. To do this, he studies the case of Western Ukraine, which in the 17^{th} century found itself divided between the Austrian and Russian empires for a period of about 150 years. The results of the work show that the communities living on either side of the deceased imperial border today differ in foreign policy preferences, interpretation of the recent past and voting behaviour.

the role played by trust. Their thinking is in the spirit of Arrow (1972), one of the first to link the lack of trust to economic backwardness. Their contribution is to use a measure of trust with intertemporal variation, which allows for control by invariant factors. In fact, they use the trust that US citizens inherited from their immigrant ancestors from different countries on different dates to identify changes in the trust inherited in their countries of origin. Once they have obtained a measure of trust, they use it to estimate the GDP per capita in the countries of origin. The analysis is carried out on General Social Survey data using citizens from all over the world as the reference population, and the time span analyzed is the 20^{th} century. What they find is that differences in inherited confidence explain a paramount part of the changes in the economic development of countries during the period under review.
Schindler and Westcott (2020) investigate the sources of negative attitudes and biases toward minority groups, as well as the persistence of these biases. Principally, they show that the presence of African military personnel in the UK around 1940, during World War II, persistently reduced prejudice among the British population. There is historical evidence that these troops of soldiers came into contact with the local population. For many locals, it was the first time they had contact with a non-white. Using a dataset that takes into account the location of these military bases and contemporary measures of anti-minority preferences, they find that English people today are more tolerant in the locations where these soldier located than in the rest of England. And this remains true both using votes for far-right parties as a proxy for intolerance and using measures of negative bias toward immigrants.

A.3 Persistence of Infrastructure

Since in this analysis we employ the port infrastructure of the Middle Ages and the ancient Roman network to construct our instrument, this study also fits within the recent strand of literature on the long-term impact of infrastructure. Indeed, within the overriding strand of long-term persistence, a recent literature has focused on the role played by historical infrastructure on present outcomes.
Of particular interest for our study is the literature on the persistence of ports. In a seminal work, Jha (2013) studies the interaction between Hindus and Muslims in South Asia, two ethnicities engaged in 13 centuries of violent interaction between Hindus and Muslims. What he finds is that the ports operating in the Medieval period in South Asia, more ethnically mixed than other areas, were places with a lower level of Indo-Muslim conflict at that time. More interesting is the fact that the level of conflict remains five times lower than in other cities between 1850 and 1950. According to the author, the institutions that evolved in that context played a great role as a channel of persistence of specific "non-violent" characteristics.
Conjointly, in the study by Jia (2014) the persistent role of the *Chinese Treaty ports*

is put under scrutiny.[64] The author investigates the long-term effect of this series of treaties signed by the Quing government, in China, with Western countries, from the 1840s and 1910s. Thanks to these agreements, some Chinese ports were opened to trade by the so-called "unequal treaties", but in 1943 the port system of the treaties ceased to exist. The author finds that in the short-term, Chinese prefectures that, thanks to *Treaty ports*, had opened up to Western institutions and foreign trade, experienced higher population growth rates. Furthermore, in the long run (after 1980), the author observes higher growth rates of GDP and population in the prefectures that were affected by the *Treaty ports*.

As already anticipated, we decide to employ the distance on the Roman road network as a good proxy for the Medieval distances. In this regard, a huge literature is focused on the persistence of the Roman road network (Wahl, 2017; Dalgaard et al., 2018; De Benedictis et al., 2018).In Wahl (2017) and Dalgaard et al. (2018) nighttime light intensity is used as a good proxy for today economic development and Roman Roads represent a key element in the model used by authors. Both studies advance the hypothesis that Roman roads have a lasting effect on economic activity today. De Benedictis et al. (2018) confirm a strong correlation between the ancient infrastructures, i.e. the Roman road network, and the modern road system, analyzing the Italian provinces.[65]

[64] This expression indicates the port cities of China open to foreign trade during the 19^{th} century.

[65] Conjointly to the Roman network, other past infrastructure investments have also attracted the interest of researchers, especially the ancient railways. The literature on the persistence of ancient railways investments mainly focus on both the short and the long run effect of colonial infrastructure (e.g. Jedwab and Moradi, 2016; Brata, 2017; Jedwab et al., 2017) on urban growth, with the exception of Berger and Enflo (2017).

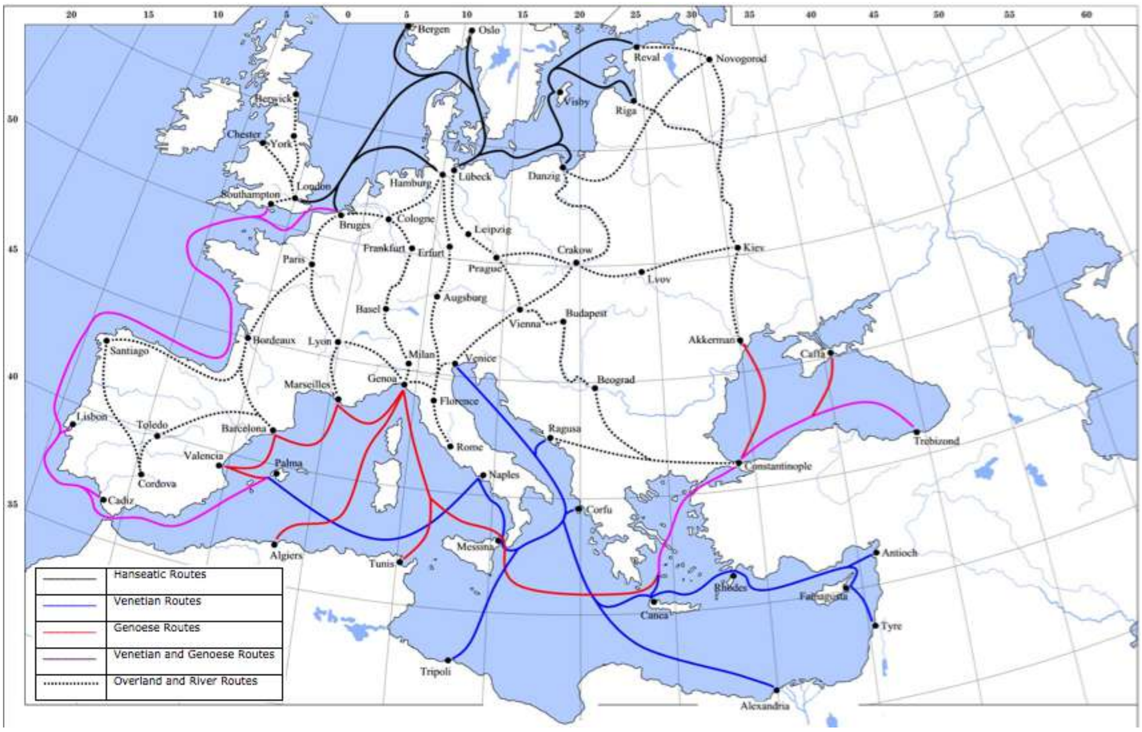
10 Appendix B

B.1 Alternative Maps

In the main text we show the main sources used to create the three port configurations, while we present in this section alternative maps from distinct historical sources. Notably, since map 1 is the one with the fewest ports and maps 2 and 3 can be considered as extensions of the latter, we check evidence to support the common framework of these three maps.

The maps in Figures B1 and B2 fully confirms the configuration of the ports in Figure 1 of the main text. The only difference is that in the map shown in Figure B2 there is also a trade route from Syracuse to the Black Sea. In the first port configuration (map 1) we do not take into account Syracuse. On the other hand, in the second and third configurations (maps 2 and 3) Syracuse is always included. In Figure B3 we present a map showing the Genoese sea routes. With respect to these, all ports included correspond to those we employ.

Figure B1: Medieval Ports Configuration Map 1 - other source

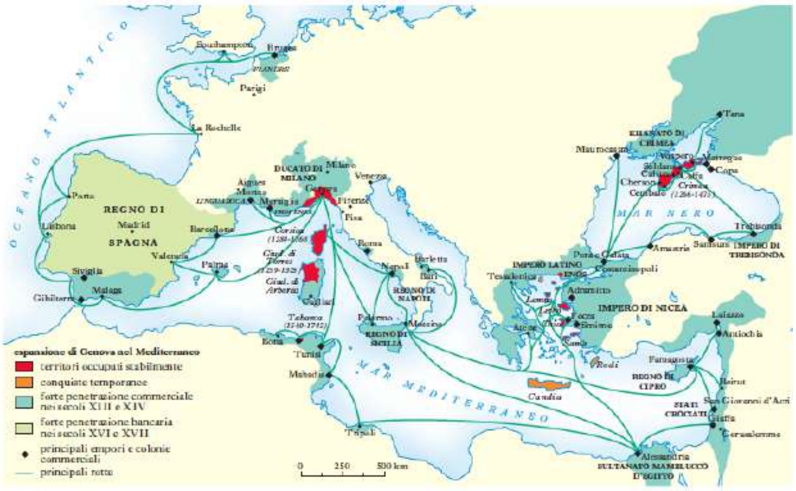


Notes: The source of the map is Lampman (2018), Late Medieval Land Maritime Trade Routes, The Ancient History Encyclopedia. The map provides a comprehensive summary of the Europe's trade networks through the Medieval Europe. The blue lines indicate the Venetians sea trade routes, while the red lines indicate the Genoese sea trade routes. The purple lines indicate the the Venetian and Genoese routes, while the black lines indicate the Hanseatic Routes. The black dotted lines indicate the major land trade routes, and the black dots indicate the notable locations.

Figure B2: Medieval Ports Configuration Map 1 - other source

Notes: The source of the map is the Historical Atlas by Shepard (1926). The map provides a comprehensive summary of the Europe's trade networks through the Medieval Europe. The blue dotted lines that are shaped like stars indicate the venetians sea trade routes, while the blue dotted lines indicate the Genoese sea trade routes. The red lines indicate the major land trade routes.

Figure B3: Medieval Ports Configuration - Genoese commercial sea routes



Notes: The source of the map is the Treccani Encyclopedia, available at https://www.treccani.it/enciclopedia/genova%28Dizionario-di-Storia%29/ The map provides a comprehensive summary of the Genoese's trade networks through the Medieval Europe (XIII-XIV century). The green lines indicate the sea trade routes, while black dots indicate the notable locations. Unlike the other maps, in this map only the Genoese routes are reported, so the Venetian trade sea route is not shown.

B.2 Additional Heterogeneous Effects

We present in this Section results from an alternative heterogeneous effects based on educational qualification. Mainly, we report in Table B1 estimates with regard to parental educational attainment, i.e. we split the sample according to the fact that at least one parent holds a bachelor's degree (or higher attainment). We conduct this analysis as a further refinement of that shown in Table 12 of the main text. Indeed, parents' education indirectly relates to children's academic achievement Davis-Kean (2005). We show results in Table B1. When we consider the 10 km threshold (panel A), we observe that the fact that the parents have a university education makes the coefficient of the perception of migrants non-significant. On the contrary, for the sub-sample of individuals with parents without a university degree the relationship between the perception of migrants and far-right political positioning holds. Nevertheless, in panel B the difference between the two sub-samples disappears.

Table B1: Heterogeneous Effect: Parental Educational Attainment

Dependent Variable: *Far-Right Political Positioning*	Map 1		Map 2		Map 3	
	(1)	(2)	(3)	(4)	(5)	(6)
	Not Grad	Graduated	Not Grad	Graduated	Not Grad	Graduated
Panel A: 10 km						
Migrant Perception	1.092*	0.322	0.875*	2.211	1.339*	2.393
	(0.625)	(5.814)	(0.451)	(2.805)	(0.806)	(4.988)
F-statistic	6.593	0.0431	8.843	0.424	6.385	0.173
Panel B: 15 km						
Migrant Perception	1.711	-14.42	0.904	-1.770	2.094	-1.770
	(1.490)	(107.7)	(0.737)	(6.818)	(1.865)	(6.818)
F-statistic	1.867	0.0148	3.675	0.119	1.881	0.119
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Socio-Economic	√	√	√	√	√	√
Social Capital	√	√	√	√	√	√
Migrants	√	√	√	√	√	√
History	√	√	√	√	√	√
Observations	1,364	495	1,364	495	1,364	495

Notes: All specifications are estimated by Two Stage Least Squares. The dependent variable is $FarRightPositioning_{i,m,r}$, the dummy related to right-wing voting intention, and it remains unchanged in all the different specifications shown in the Table. The main independent variable is $MigrantPerception_{i,m,r}$, the dummy related to the perception that migrants make Italy an unsafe place. We instrument the endogenous independent variable with $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to the three port configuration as reported in Map 1 (columns (1) and (2)), Map 2 (columns (3) and (4), and Map 3 (columns (5) and (6), respectively. (For further details see Section 2.3). The sample is split in two different sub-samples. In odd-numbered columns we consider individuals whose parents do not have a bachelor's degree, while in even-numbered columns the model is estimated for individuals with at least one parent holding a bachelor's degree (or a higher degree). In panel A, individuals whose municipality of residence is within (or beyond) the 10 km distance calculated in Roman roads are considered close (or distant) from one of the Medieval port. In panel B we use a distance of 15 km. Personal controls include: age, educational attainment, marital status, gender, and parental income, as proxied by a measure of the employment status. Geography controls (at the municipal level) incorporate: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, resident population in 2017, population density in 2001 and a variable related to whether a city is in a rural or urban area. Socio-economic controls (at the municipal level) include: average income per capita of the municipality of residence, the number of manufacturing firms in 2017 and the broadband internet coverage. The Migrants measure refers to the % of foreigners in the total population in 2017 in each municipality. Social Capital is measured by number of non-profit associations per capita in each municipality in 2011. Historical controls (at the municipal level) encompass: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E. The sample weights are applied. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-3 level. F is the First stage Kleinbergen-Paap Statistic. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

B.3 Hard Outcomes - Past National Elections

In this section we propose an analysis at municipal level using data from past national elections in order to provide a falsification test in the spirit of Acemoglu et al. (2020). Using data on both 1963 and 1968 elections we investigate whether the relation between electoral outcomes and proximity to Medieval port still holds. In particular, in Table B2 we show the results for the elections of 1963 and 1968. The models are estimated according to Equation 4 in the main text. The dependent variable is given in each year by the % of votes received by right-wing parties. We consider the votes received by the *Movimento Sociale Italiano* (MSI) and by the *Partito Democratico Italiano di Unità Monarchica* (PDIUM). In columns (1) and (2) we report the estimates for the elections in 1963, while in columns (3) and (4) those for the year 1968. We present the results for maps 1, 2, and 3 as described in the main text. We observe that the coefficient of the dummy related to the proximity to a Medieval port is not statistically significant.

These results are in line with the fact that the issue of immigration has become central to the Italian political debate only in recent years and confirm the fact that extreme right-wing parties have used the anti-migrant rhetoric to gain consensus in the last political appointment. It is reasonable to believe that, in the sixties, openness towards the different, and especially the Muslim, was not a salient issue for voting to right-wing parties.

Table B2: Hard Outcomes Results - Past Elections

Dependent Variable: ***Right-wing Parties Coalition, % votes in National Election***						
	1963 National Election			*1968 National Election*		
	Map 1	Map 2	Map 3	Map 1	Map 2	Map 3
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 10 km						
Proximity to Medieval Port	0.254	1.802	0.769	0.663	1.383	0.503
	(1.747)	(1.099)	(0.867)	(1.977)	(1.066)	(0.949)
R-squared	0.270	0.270	0.270	0.247	0.247	0.247
Panel B: 15 km						
Proximity to Medieval Port	-1.425	0.465	0.641	-0.435	0.257	-0.259
	(0.943)	(0.844)	(0.880)	(0.832)	(0.640)	(0.600)
R-squared	0.270	0.270	0.270	0.247	0.247	0.247
NUTS-3 Region FE	√	√	√	√	√	√
Personal	√	√	√	√	√	√
Geography	√	√	√	√	√	√
Observations	7,043	7,043	7,043	7,153	7,153	7,153

Notes: All specifications are estimated by Ordinary Least Squares. The dependent variable is $RightVotesShare_{m,r}$, the the % of votes received by the right-wing coalition alternatively in the National Election in 1958 and in 1963. The main independent variable is $ProximityMedPort_{m,r}$, the dummy variable indicating the (historical) proximity to a Medieval port according to two separate maps (For further details see Section 2.3). In panel A, the proximity threshold is 10 km on the ancient Roman road network. In panel B we use a distance of 15 km. Geography controls (at the municipal level) encompass: an index of terrain asperity, the geodetic distance both from Tunis and from the sea, an index of accessibility, and a variable related to whether a city is in a rural or urban area. Historical controls (at the municipal level) involve: an indicator whether the city was a seat of a Bishop before 1000 C.E. and two dummy variables accounting for the size of city in year 1300 C.E.. All specifications include regional Fixed Effects at NUTS-3 level. Standard errors are clustered at NUTS-2 level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: *** p<0.01, ** p<0.05, * p<0.1.

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CHAPTER 2

Employment protection legislation and household formation: evidence from Italy

Employment protection legislation and household formation: evidence from Italy*

Abstract

While many studies have investigated the determinants of household formation and fertility of young adults, only a few focused on the impact of employment protection legislation (EPL) on these outcomes. In this paper, we study the differentiated impact of the EPL reduction associated to the Jobs Act in 2015 in Italy on the household formation and fertility intentions of young Italians in various districts. To do this, we use data from a survey conducted on a sample of 18-34 years old for the years 2012, 2015, 2016 and 2017. The identification strategy exploits local variation in the level of efficiency of courts, measured in terms of average duration of proceedings, to assess the existence of within country and across district heterogeneity of the reform impact. Indeed, firing costs used to be relatively larger in those districts characterized by a larger duration of labor trials. The Jobs Act, by reducing firing costs, and modifying the autonomy of judges, should have had a larger impact in districts with less efficient courts. According to our results, the reform seems to have indirectly levelled out the fertility and household formation intentions of young Italians living in districts with more and less efficient courts.

Keywords: Household formation intentions, Fertility intentions, Jobs Act, Difference-in-Differences, Employment Protection Legislation, Labor Market Reform, Court Efficiency.

JEL classification: C31, M51, J2, J12, J13, J41.

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1 Introduction

Pursuant to Eurostat's 2018 data, the average age across the European Union at which young people leave their parents' home is increasingly higher. The average age at the European Union level (28 countries) in 2018 is 26 years. Nonetheless, there are significant differences between member states. In Southern European countries, the age is higher than in Northern Europe, and similar across countries: 30.1 years in Italy, 29.5 years in Spain and 29.3 years in Greece.

The situation is very different in Northern Europe: in Denmark, the estimated average age at which young people leave the parental household is 21.1 years, in Finland 22 years and in Sweden 18.5 years (Eurostat, 2018)[1]. The age at which children leave their homes varies considerably both between countries and within country over time. A similar reasoning can be made regarding fertility. The total fertility rate at the European Union level (28 countries) decreased from 1.62 in 2010 to 1.59 in 2017, while in Italy from 1.46 to 1.32 (Eurostat, 2019).[2] These two strands of literature, household and fertility, are linked together, given the implications that household formation choices have on fertility decisions. These choices are also of primary importance in relation to other factors, one of which is the sustainability of social security programs. The literature on household formation and fertility is more empirical than theoretical. As for household formation, in recent years the literature in demography and economics has focused on the determinants of the process of leaving the family by young people, some with a focus in a specific country and others at a comparative level between countries. One strand focused on the importance of income shocks, unemployment, low-income groups and income insecurity (Becker et al.2010, Aparicio-Fenoll and Oppedisano, 2015). Others focused on the role played by housing costs (Haurin et al.1993), and the role played by the credit market (Martins and Villanueva, 2009).

There are also scholars who, while recognizing the importance of economic variables, focused on the role played by culture and social norms (Giuliano, 2007). In terms of fertility, the literature related to the main fertility drivers is quite analogous to those related to household formation. Another strand of literature fixated on the role played by economic uncertainty, both by using objective measures (De La Rica and Iza, 2005) and subjective measures (Bhaumik and Nugent, 2011), others on the role played by housing conditions (Vignoli et al. 2013), and others on the role played by families of origin and cultural factors (Kertzer et al. 2009). Although there are many studies on this issue, Prifti and Vuri (2013) studied more specifically the impact of a reform of the labor market, and therefore a change in terms of employment pro-

[1]Source: `https://ec.europa.eu/eurostat/web/products-eurostat-news/-/EDN-20190514-1`

[2]Source: `https://ec.europa.eu/eurostat/documents/2995521/9648811/3-12032019-AP-EN.pdf`

tection legislation (EPL), on the fertility decisions of young Italians. In their paper, Gianfreda and Vallanti (2017) explained how labor market rigidities are not only the result of regulatory provisions, but are also determined by the institutional framework. In line with this view there would, therefore, be a within-country difference in terms of employment protection legislation in Italy.

In this paper, we exploit the exogenous variation of the "Jobs Act reform" approved by the government lead by Mr. Matteo Renzi in 2015 to estimate the effect of employment protection on household formation intentions and fertility intentions of young Italians. After the introduction of the Jobs Act, in case of dismissal there is no longer a need to bring the dispute to the court.[3] Our starting hypothesis is that the reduction in employment protection provided by the Jobs Act could increase permanent contracts. This is what Bratti et al. (2021) found with regard to the Fornero reform, as well as Boeri and Garibaldi (2019) with respect to the Jobs act reform. Moreover, Kugler and Pica (2008) found that the increase in small firm's dismissal costs associated to the Italian 1990 Reform decreased new hires. It is reasonable to expect that more workers on permanent contracts will also mean an improvement in their household formation and fertility intentions. To account for the reduction in employment protection, we used an indicator of court efficiency related to individual and collective dismissals. Indeed, the efficiency of the courts contributes significantly in defining firing costs. Gianfreda and Vallanti (2017), using a formula proposed by Garibaldi and Violante (2005), reported ex post firing costs equivalent to approximately 36 months of wages in Trento (with an average length of labor trials of 313 days) versus 160 months in Salerno (with an average length of labor trials of 1397 days).[4] We estimated the effects with a difference-in-differences strategy, comparing outcomes for individuals living in areas with more or less efficient courts, before and after the policy implementation.

Using data from the "Osservatorio Giovani", carried out by IPSOS, we found that household formation and fertility intentions have improved more for those who, in the pre-reform period, lived in areas with less efficient courts than those who lived in areas with more efficient courts. This is consistent with the hypothesis that in areas with less efficient courts, before the reform, entrepreneurs hired less workers with

[3]After the Jobs Act reform, the employee of a company with more than 15 employees (5 in the case of an agricultural company) is not entitled to reinstatement in the event of illegitimate dismissal, but only to compensation of an economic nature that increases with length of service. The Jobs Act always provides for full real protection of the worker only in three cases: null dismissals, discriminatory dismissals and oral notice of dismissal. Full real protection entails the reemployment of the worker, the employer's payment of compensation for the damage suffered by the worker and for their social security contributions related to that period. For further details, see the institutional background section.

[4]The aforementioned firing costs are related to a blue-collar worker with 8 years of tenure in a firm above the 15-employee threshold. In addition to the time needed to reach a sentence, the formula by Garibaldi and Violante (2005) also takes into consideration the forgone wage, the health as well as the social security contributions, the penalty rate, the severance payments and the legal fees (for further details, see Garibaldi and Violante, 2005).

permanent contracts, often relying on a series of successive temporary contracts, given the guarantees provided by law and the long times of the courts. After the reform, the combined EPL reduction provision and the definitive overcoming of the obligation to go through the court changed the behaviour, perceptions and intentions of both workers and employers.
In our paper, we want to focalize our investigation on short-term impacts of the reform on both household formation and fertility. Bearing that in mind, the intentions are to be considered instead of their realizations. The main reason is that people who may benefit from the reform, in the post-reform scenario are likely to change their intentions about family formation or fertility. Only afterwards these intentions will become achievements.
This work is also part of the strand that analyzes the effects of the Jobs Act. Among the works that dealt with the effects of the Jobs Act, Sestito and Viviano (2016) as well as Boeri and Garibaldi (2019) and De Paola et al.(2020) are of particular importance. The rest of the paper is organized as follows. Section 2 presents a literature review both for household formation and fertility. Section 3 summarizes the institutional background, which contains both a summary of the latest labor market reforms in Italy and a more detailed explanation of the Jobs Act reform.
In section 4, the data of the survey “Osservatorio Giovani" are presented, as well as the Istat and Ministry of Justice data used to create the indicator of court efficiency. In section 5, we present the identification strategy, while section 6 we reports the results, some robustness checks and heterogeneous effects. Section 7 concludes.[5]

2 Related Literature

In investigating the determinants of household formation, economists study the role played by economic variables such as unemployment, income and income insecurity, contextual variables such as house prices, cultural variables, the role of parental preferences and the position of the credit market. Some studies contemplate a specific country and others analyze the phenomenon in a comparative perspective between different nations, some directing attention to a limited period and others to a longer period.
A first group of papers, key on the effect that some economic variables (income, unemployment, perceived insecurity) have on the household formation of young adults. Starting from theoretical works, an important reference model in terms of household formation is certainly that of Becker et al. (2008). They propose a theoretical model to study the effect of the insecurity of parents and children on residential choice. Becker et al. (2010), in addition to the theoretical work just mentioned, carried out an empirical study both at macroeconomic and microeconomic level to test whether

[5]Moreover, in Appendix A we provide additional placebo and robustness checks.

(and in case how much) the perception of job insecurity of parents and children impacts on the decisions of leaving the family of young adults. They found that the perceived job insecurity is a relevant explanatory variable of the co-residence decisions.

Other works concentrate empirically on the role of economic variables such as unemployment and income insecurity on household formation choices and intentions. In this area, Aparicio-Fenoll and Oppedisano (2015) studied the role of a conditional cash transfer on household formation decisions in Spain, Lee and Painter (2013) assessed the impact of the financial crisis on these issues, and Paciorek (2016) investigated the relationship between labor market conditions and household formation.

Among others, Garcia-Ferreira and Villanueva (2007) studied the link between the different types of contracts and household formation. In turn, Ermisch and Di Salvo (1997) predicted that higher income for the young adults increases the probability to leave the family of origin while higher parental income reduces it. Ermisch (1999) also finds that, among young adults, those who have higher current income are also more likely to leave home to live alone than others.

A second group of papers, including Ermisch (1999), examine the effect of the housing market and the price of houses on the likelihood of forming a family of young adults. Haurin et al. (1993) found that the average probability of leaving the parental home and living alone is lower in the high rental cost areas than in the low rental cost ones. Borsch-Supan (1986) studied a model of housing demand and the parameters that he estimates reveal the considerable importance of house prices (both rental and purchase).

Another strand of literature broods over the role played by the credit market in this process. Fogli (2004) found that in an economy where agents face borrowing constraints, the latter prefer to live longer with their parents in order to smooth consumption. Martins and Villanueva (2009) used the introduction and cancellation of the “Credito Bonificado" program in Portugal to find a relationship between differences in access to credit and different dynamics of household formation. Chiuri and Jappelli (2003) found that in countries with high down payment ratios, the proportion of owner occupation of the young is relatively low, and vice versa.

While recognizing the predominant role of economic conditions, another strand of literature studies the role played by the cultural aspects and the social norms. Manacorda and Moretti (2006) focused on on the impact of parental preferences and intrahousehold transfers on children’s living arrangements. Cooper and Luengo-Prado (2018) addressed the role played by the increased comfort of living at home in the increase of co-residence between young adults and parents. Sevilla-Sanz (2010) spotlit the study of the social norms and find that individuals living in societies with more egalitarian norms are more likely to enter a household. Giuliano (2007), starting from a contribution of Reher (1998), studied the difference in second-generation Eu-

ropean immigrants in the US in terms of household due to the sexual revolution.
The issue of fertility is also much debated. Even if household formation and fertility are different, the choices to leave the family of origin and to make children can be seen as two vital moments in the formation of the family. It is essential, however, to take into account the fact that the two moments are chronologically located in two different periods. The reproductive decision is the last of a series of steps aimed at the gradual construction of stability, like completing education, getting a stable job, go live alone or in any case buying a house. In recent years, a large number of studies on demography, microeconomics, labor economics and macroeconomics considered closely the channels that are likely to contribute to fertility changes.
From 1960 to 2000, the average total fertility rate fell dramatically in the OECD countries. Many economists have studied the relationship between different aspects of economic uncertainty and changes in fertility. On a theoretical level, Sommer (2016) studied the link between labor market risks, childbearing and saving, while Ranjan (1999) implemented a two-period model of fertility decision and notes the relationship between uncertainty about future income and childbearing decision. On an empirical level, many studies have supported the hypothesis of a negative correlation between economic uncertainty and fertility.
A first group of studies is centered on objective measures of economic uncertainty. Ahn and Mira (2001) studied the relationship between high unemployment and the drastic fall in the fertility rate in Spain, De La Rica and Iza (2005) deepened the difference in terms of entry into motherhood among the women who have fixed-term contracts compared to those who have open-ended contracts. Andersson (2000) studied the relationship between economic fluctuations and different levels of childbearing in Sweden. Adsera (2005) exploiting cross-country variation in labor market institutions in various OECD countries (1994-2000), documented the lower fertility rate in countries with higher unemployment rates. Bratti et al. (2005) focused on pre-marital job characteristics. Their results showed that mothers who worked without a contract before becoming pregnant are less likely to be in the workforce after childbearing than those who worked in the public sector or in large firms. Gutierrez-Domenech (2008) gave attention to the Spanish case to show that an increase in instability markers (unemployment and temporary contracts) postpones female marriage, which in turn affects negatively the fertility rate. Anderson and Pontusson (2007) emphasized the relation between employment protection legislation and economic uncertainty. They found that government legislation that increases the cost of employers to fire workers leads to a reduction of workers' cognitive job insecurity. Prifti and Vuri (2013) studied the effect of employment protection legislation (EPL) on fertility decisions of Italian working women, both through the credit channel and through the economic insecurity channel. Thus, they used a 1990 reform that introduced for the first time EPL in small firms (below 15 employees), in particular

firing costs for unmotivated dismissal, leaving the firing costs unchanged for bigger firms. Their outcomes showed that an increase in EPL effectively reduced economic insecurity and had a sizable and positive effect on childbearing decisions. A second group of papers deals instead with the effect that subjective measures of economic uncertainty have on fertility. Kreyenfeld (2005) and Bhaumik and Nugent (2011) using data from Germany addressed the impact on fertility of perceived economic uncertainty of women, measured as the feeling that the personal economic situation is insecure. Vignoli et al. (2020) combined the strand of literature on economic uncertainty and fertility with the literature on subjective well-being and fertility and explained how the impact of fixed-term contracts on fertility intentions is channeled by an individual level of subjective well-being.

Another strand of literature contemplates the relationship between fertility intentions, housing conditions and (albeit indirectly) the structure of the credit market. An example of this literature is given by the paper by Vignoli et al. (2013), which analyzes how people's degree of security about their housing situation influence fertility intentions. Their starting point was a paper written by Mulder and Billari (2010), which presented at a macro level the relationship between countries where access to home ownership is limited and low fertility levels. Others, as already seen in the case of household formation literature, have analyzed the relationship between culture and education on fertility. Kertzer et al. (2009) using Italian data studied the role played by the transition from familism to self-realization and the shift from religious attachments toward secularism. In the same context, Bratti (2003) targeted the impact of education on labor force participation and fertility rate.

When it comes to the impact of the Jobs Act, Boeri and Garibaldi (2019) documented the increase in flexibility in large firms. On the top of that, they showed that in the post-reform period the total number of hires with permanent contracts increased by more than 60% in large firms, while it remained unchanged in small firms. Along with it, they also noticed an increase in transformation of fixed therm contracts into open-ended contracts. Also Sestito and Viviano (2016) looked into the impact of the two different part of the Jobs Act reform: hiring incentives and reduction of firing costs for firms with at least 15 employees. They showed that the two policies were successful in both reducing dualism and stimulating labour demand. In addition, they found that both measures were effective in both shifting employment towards permanent contract and raising overall employment levels. De Paola et al. (2020) instead researched on the impact of the Jobs Act and the consequent reduction of EPL on fertility (measured through maternity leave). Even though the object of study is our own, however, the focus is different. They found that the fertility rate of treated women, i.e. those who have a contract started after 7 March 2015 and therefore those who are directly impacted by the reform, fall by about 1.4 percentage points more than that of women hired in small firms. What we want to study instead, as we will see in the next section, is the indirect impact of EPL reduction. That is, companies are hiring more easily in the light of the new regulations on hiring and firing, and especially where the de facto EPL change is greater, i.e. based on the performance of the courts. What we want to estimate is the effect on potential beneficiaries of this situation.

3 Institutional background

Since the nineties of the last century, there has been an awareness in Europe of the need to make work more flexible. Competition from companies in countries with very low labor costs and globalization have forced governments to find a solution to adapt work more efficiently to market needs.
Following this necessity, in the last two decades, the Italian labor market has undergone a profound change from a legislative point of view, as well as from a structural and social perspective.
After the law no.355/1995 that sanctioned the passage of the pension system from the salary method to the contributory method and the establishment of the separate INPS management, in 1997 there was another major reform. Through the law no.196/1997, better known as "Treu law" (from the name of the Minister of Labor), the government promised to move towards greater flexibility in the labor market (we initially talk about flexibility in hiring) by introducing atypical and precarious contractual forms. The "Treu law" introduced significant innovations, the most important of which is the introduction of the so-called "lavoro interinale". This was a particular form of "contract for the provision of temporary work services" with which companies could benefit temporarily from a work service, without assuming all the burdens that arise from the establishment of a subordinate employment relationship. The possibility of extending the fixed-term contract is also included and part-time work is encouraged. As a consequence, atypical contractual figures are promoted at the expense of permanent contracts.
In 2003 the Berlusconi government approved the law no.30/2003, better known as "Biagi law" (from the name of the professor as well as creator of the labor market reform). This law repealed, modified and introduced many employment contracts. The legislator's intention was based on the assumption that flexibility in hiring was the best way, given the economic downturn, to facilitate the creation of new employment.
Afterwards, in 2012 the Monti cabinet approved the law no.92/2012, better known as "Fornero law". While the previous legislative actions were all aimed at increasing the flexibility in hiring, this intervention instead aims at increasing the flexibility in dismissal, modifying the dismissal discipline. In particular, full effective protection, i.e. reintegration into the workplace with a sentence to pay compensation for the damage caused to the employee by the employer, is limited to cases of null dismissals,

discriminatory dismissals and oral notice of dismissal. Differently, the reduced real protection is introduced, providing reintegration into the workplace without, however, providing for compensation for the damage but merely an indemnity.
To sum up, the new law leaves wide discretion to the judge, even in assessing if the conditions for reintegration are verified.
Finally, in 2015, under the Renzi government, the Jobs Act was approved, a reform that goes in the direction of further reducing the employment protection legislation. The Jobs Act allowed the introduction of the "contratto a tutele crescenti" (CTC) discipline concerning, still, merely employment contracts entered after 07/03/2015. Under the new CTC, the employee of a company with more than 15 employees (5 in the case of an agricultural company) is not entitled to reinstatement in the event of illegitimate dismissal, but only to compensation of an economic nature that increases with length of service. Regarding the permanent contract previously stipulated, law no.92/2012 (Fornero reform) applies, and therefore regulates on an exhaustion basis, unless the employment requirement of the 15 employees (to which the Jobs Act refers when referring to its scope of application) was reached after 07/03/2015 with new CTC hire. In this case, the new discipline also applies to the employees previously hired. Legislative Decree 23/2015 always lays out full real protection of the worker in the three cases indicated above, i.e. null dismissals, discriminatory dismissals and oral notice of dismissal. Full real protection entails the reemployment of the worker, the employer's payment of compensation for the damage suffered by the worker and for their social security contributions related to that period. The reduced real protection always imparts reintegration into the workplace without, however, providing for compensation for damage but an indemnity commensurate with the last reference salary for the calculation of the "Trattamento di fine rapporto (TFR)".[6] That amount may not exceed twelve months' salary, less what the worker received for other work activities and how much he could have received by accepting an appropriate offer of work. This allowance is dispensed only in the case of dismissal for justified subjective reason and right cause, in which it is directly demonstrated in court that there is no material fact alleged against the worker, in respect of which there is no assessment of the disproportionate nature of the dismissal.
In other cases of unjustified dismissal, only an indemnity is granted (not less than 4 and not more than 24 months' salary).
It is therefore clear that the main dissimilarity from the Fornero reform is the role played by the judges. If with the Fornero reform the judges still had an important role in verifying whether the conditions for reintegration were met, now they no longer have that role. As we have seen, even if the employer decides to dismiss the worker, the CTC already provides for months to be paid to the worker without having to go

[6] The TFR, i.e. severance pay, is an element of the employee's remuneration that is paid cumulatively after employment is over.

to court.

4 Data and Descriptive Statistics

The main dataset is the result of the survey "Osservatorio Giovani", carried out by IPSOS for the "Giuseppe Toniolo Institute of Higher Education".[7] The objective of the database is to provide a comprehensive and detailed source of information on the new Italian generations and their connections with the transformations taking place in society in which they live. For the purposes of our research, we used the surveys for 2012, 2015, 2016 and 2017.

From these data, we obtained a lot of information on the household formation intentions and the fertility intentions of young people aged 18 to 34 in Italy in the years already mentioned. To use this data for our analysis, we proceeded to homogenize the data relating to the four years of the survey. The data for the pre-reform period refer to the year 2012 and are the result of a survey of 9,087 adherents. These 9087 individuals are representative of the universe of young people aged 19 to 31 years resident in Italy in 2012. The year 2015 is the year zero when the observations on a new group of young people started again. In particular, 9,358 young people from 18 to 33 years of age -representative of the universe of reference- were followed. In 2016, 6,172 young people completed the interview, with a redemption 2016-2015 of 66%. The 6,172 young people are representative of the reference universe (individuals between 19 and 34 years old). In 2017, 3,034 young people participated in the survey, with a redemption 2017-2016 of 49% and 32% if we compare 2017 to 2015, the year in which it began. As seen in other years, also in 2017 the 3,034 young people are representative of the universe of reference (individuals between 20 and 35 years). In carrying out our analysis, we have taken into account the sampling weights attributed to individuals. In order to not inflate our estimates including in the sample individuals too young, we have decided to take into account for our analysis only young adults over or equal to 25 years of age. Young people under 25 years of age are less expected to leave the family of origin or decide to have a child compared to older children, who have already finished university education.[8]

The estimation sample varies from 11,179 and 27,180 individuals according to the chosen specification. The database consists of repeated cross-sections. The data contain detailed information on age, gender, region and town of residence, educational

[7] All the waves of the survey data form the "Rapporto Giovani" database. The "Rapporto Giovani" database contains all the data of the survey conducted on a sample of 9,000 young people aged 18 to 34 years. Promoted by the Istituto di Studi Superiori Giuseppe Toniolo (in collaboration with the Università Cattolica del Sacro Cuore and with the support of Fondazione Cariplo and Intesa San Paolo) and carried out by Ipsos, the "Rapporto Giovani" is the most in-depth and extensive research on the world of youth in the last decade.

[8] However, some robustness checks on the age threshold are provided in the chapter dedicated to "Robustness checks".

qualifications, parents, parents' qualifications and size of the household. The survey over several years has different questions and different sets of answers. For both household formation intentions and fertility intentions, in some years the survey asked young adults to answer questions about a period of one year only in case of a positive answer in questions about a time frame of three years. For good measure, in some years there were three possible answers, while in others there were four. Before starting the analysis, we then homogenized the questions and answers.

Information about children's cohabitation with their parents, or rather their intention to household formation, derive from a specific question in the initial survey. The question is as follows: "Do you plan to live alone within the next year?". We took the outcome as 1 if the answer is "Yes, it is very likely", and zero otherwise. When it came to the fertility intentions, we used the answers to an explicit question in the survey. The question is as follows: "Over the next three years, do you expect to have (another) child?". As before, we took the outcome as 1 if he/she responded "Certainly yes" or "Probably yes", and zero otherwise.[9] As for the controls, in our main specification we controlled for the title of study of each respondent and the father and mother's qualifications (as a proxy of the economic conditions of the family of origin of the young adults). We likewise checked for the number of brothers and/or sisters (a proxy to the propensity to motherhood), the marital status, the age and the provincial data on youth unemployment in the years of interest (Istat).

[9]In some robustness, we also used another variable that is equal to one if the answer is "Yes, it is very likely" or "Probably not", and zero if the answer is "Certainly not, certainly later" both for household and fertility intention. Results are shown in the Appendix A.

Table 1: Descriptive Statistics

Variables	Mean	Std.Dev.	Min	Max	Obs.
Household formation intentions	0.39	0.49	0	1	12,788
Fertility intentions	0.35	0.48	0	1	18,754
Age	28.99	2.77	25	35	18,754
Man	0.48	0.50	0	1	18,754
Woman	0.52	0.50	0	1	18,754
Presence of brothers/sisters	0.83	0.37	0	1	18,754
Child	0.18	0.38	0	1	18,754
Married	0.22	0.42	0	1	18,754
Education	3.95	1.06	0	6	18,754
Youth unemployment rate	28.51	11.82	6	72	18,754
Mother's educational qualification	2.95	1.35	0	6	18,729
Father's educational qualification	2.91	1.39	0	6	18,673
North	0.44	0.50	0	1	18,754
Centre	0.19	0.39	0	1	18,754
South	0.38	0.48	0	1	18,754
Firms above the 10-employees threshold*	0.04	0.01	0.01	0.07	18,754

Dataset: Osservatorio Giovani (2012, 2015, 2016, 2017), IPSOS, Giuseppe Toniolo Institute of Higher Education. Sample: young adults with at least 25 years of age. Here, as in all other analyses, sample weights are used. * The firms variable refers to the number of firms present in the interviewee's province with more than ten employees out of the total number of enterprises.

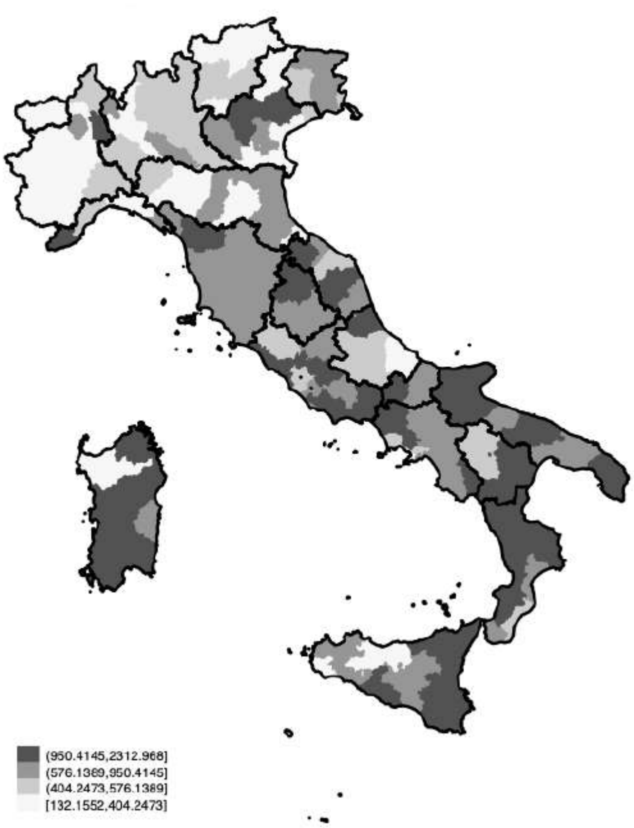
4.1 Courts' data

For the purposes of our identification strategy, we needed data relating to the performance of the courts, data that we did not have available in our reference dataset. We decided, as already done by others in literature, to create a synthetic indicator in order to use a proxy for the performance of Italian courts. Whit the aim of doing that, we used data relating to the operation of Italian courts from the DG-stat portal, portal of the General Directorate of Statistics and Organizational Analysis established by the Ministry of Justice. Here data divided by district (judicial district of an ordinary court), by subject and by periods of interest were available. Regarding the object of interest, the data related to "Work and social security", to the subject "Private sector" were used, with a specific focus on the detail "Dismissal (individual/collective)". We made this choice in light of the fact that the interest in terms of performance of the court is that relating to the single topic of interest and not to all the procedures (ordinary civil, special summary proceedings, voluntary jurisdiction and so on). The data relating to the performance of the courts are those relating to 2014, i.e. the last year before the entry into force of the Jobs Act. The measure is the following:

$$DLT_t = \frac{(P_{t-1} + P_t)}{(F_t + C_t)} 365,$$

where P_{t-1} and P_t are respectively the numbers of cases pending at the beginning and the end of the year. F_t is the number of new cases filed during the year and C_t is the number of cases that ended with a judicial decision or were withdrawn by the parties during the year. This is a measure of efficiency (or inefficiency) of the court for the subjects under investigation. The higher the indicator, the more cases the court accumulates and vice versa. The indicator can be interpreted as the average duration of court proceedings. It is a measure used in the economic literature, both for cross-country and within-country studies. Among others, Giacomelli and Menon (2013) used spatial discontinuities in court jurisdictions to investigate causal relationships between judicial efficiency and firm size. They found that a reduction in the length of civil proceedings could have ceteris paribus a positive and significant effect on the average size of Italian companies.

Figure 1: Average length of judicial proceedings, 2013-2014



Notes: the polygons in the map correspond to the Italian districts. At each Italian municipality is attributed the indicator relating to the judicial district to which it belongs. The average length of judicial proceedings is calculated by an index, as explained in section 4.1.

Source: authors' calculations based on ISTAT and Ministry of Justice data.

5 Identification Strategy

Our identification strategy takes advantage of two sources of variation. The first one is determined by the temporal variability of the law. The second is given by the different characteristics of the districts that make them more exposed to the common nation-wide reform. In this regard, Gianfreda and Vallanti (2017) explained how labor market rigidities are not only the result of regulatory provisions, but are also determined by the institutional framework. Specifically, they argued that the "slowness" of judicial proceedings relating to the labor market increases dismissal costs for employers, who are obliged to bear the cost of longer trials. Gianfreda and Vallanti (2017), using a formula proposed by Garibaldi and Violante (2005), reported ex post firing costs equivalent to approximately 36 months of wages in Trento (with an average length of labor trials of 313 days) versus 160 months in Salerno (with an average length of labor trials of 1,397 days).[10] According to this view, there would therefore be a difference in terms of "de facto" employment protection legislation, and the length of the processes in the matters under discussion would be a cardinal part of the problem.

As specified by this hypothesis, in the districts where there was a greater degree of employment protection legislation due to the worst performance of the courts, the labor market reform should have impacted more, at least in terms of creating new open-ended contracts with respect to the pre-reform period. The Jobs Act, in fact, already provides expressly the possibility of dismissal of a permanent worker. Given this clear legislative provisions, there is no longer a need to bring the dispute to court. Likewise, in districts where there was less pre-reform employment protection legislation due to improved court performance, the labor market reform should have impacted less, at least in terms of creating new open-ended contracts compared to the pre-reform period.

Ultimately, the identification scheme is based on the idea that firms before and after the reform are affected by the trial length in different ways: longer processes in the pre-reform period directly created higher costs for businesses, discouraging employers from hiring with an open ended contract. Correspondingly, longer post-reform processes should not anymore impact on business choices, as judges stopped playing an active role in the dispute. The pre-reform difference in terms of the efficiency of the judicial system is precisely the discriminating factor that explains the likelihood that companies will create more permanent contracts in the post-reform period.

If the reform has a direct impact on firms, and therefore on the employers' choices about hiring and firing, this in turn affects the employment opportunities of youngsters and consequently on their life choices.

The young person to be effectively treated would have to be unemployed the year

[10] For further details, see footnote 4.

before and get an open-ended contract in the current year. As far as we know, young adults in the years under review could have precarious contracts, be unemployed, work in companies with less than 15 employees or be already employed in a large company since the base year of the survey. Be as that it may, it is also true that the young adults' intentions may also change in an indirect way. For instance, the household formation intentions of a young adult may also change after her/his partner gets a permanent job. Unfortunately, our data do not allow us to estimate the effective take-up[11] and our results show the combined effect of the two mechanisms (i.e. reduced form estimates). The objective is to verify whether in the post-reform period there is a difference in family formation and fertility intentions between places where the court efficiency is notoriously low and those where it is high. To do so, we use a difference-in-differences estimation strategy that assumes the following form:

$$Y_{ict} = \alpha + \beta(CourtEff_c \times Post_t) + \delta X_{ict} + \mu_c + \tau_t + \epsilon_{ict} \tag{1}$$

Where the coefficient of interest is β, which is the coefficient of the interaction of the post-treatment indicator and the treatment variable. This coefficient submits the effect on the outcome that results from an increase in the treatment intensity. Hence, looking at β we can see the average effect of the subsidy for those who live in a district of a very efficient court compared to those who live in a district of an inefficient court.

X_{ict} controls for individuals' observable characteristics such as marital status, presence of brothers and sisters, age, educational qualifications and parents' educational qualifications and also on macro-economic characteristics such as youth unemployment or unemployment on a provincial basis, depending on the chosen specification. μ_c and τ_t are district and time fixed effect. Standard errors are clustered at a court level.

We cannot empirically test the fact that different districts were on a common trend before the intervention, because we have only one year available in the pre-reform period. All the same, we provide a battery of placebo that supports our empirical approach.

[11]At the best of our knowledge, no data on 15 employees threshold are available. Indeed, in the survey is not declared the company for which individuals work. In addition to that, we do not know when the contract is started.

6 Empirical results

Table 2 shows the main results for both variables: the household formation intentions and the fertility intentions. More specifically in Panel A, as we have seen in the previous paragraphs, the dependent variable is a dummy equal to zero if the answer to the question "Do you plan to live alone within the next year?" is "Yes, it is very likely ", and zero if the answers are "Probably not" or "Certainly not, certainly later". In all the specifications in the table, we decided to follow the literature on household formation and fertility and focus our analysis on young adults aged 25 years or older.[12] Young people under 25 years of age are less likely to leave the family of origin or decide to have a child compared to older children, who have already finished university education.

The first column shows the estimate of the coefficient of the interaction term between time dummy and court efficiency indicator for young adults over 25 years of age, in a setup without controls and with fixed effects of time and region. In this and in all other specifications reported in Table 2 standard errors are clustered at the district level. The positive sign of the interaction term can be interpreted as the reform has had a positive impact on household formation intentions. Above and beyond, a reduction in the efficiency of the court (and therefore an increase in the indicator of the inefficiency of the court) corresponds to a greater effect of the reform ("Jobs Act") on the intentions of household formation of young adults. Our coefficient in column (1) is significant (5%), and still significant (1%) in column (2), where all specifications previously analyzed are kept unchanged, with the only difference that controls are added. In particular, in this and in all subsequent regressions we used as controls: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualification and youth unemployment rate at a provincial level. Apart from that, each regression takes into account time fixed effects and geographical fixed effects (at different levels, depending on specifications). Column (3) shows the coefficient in a specification equal to that used in column (1), with the difference that fixed effects are at the district level and not at the regional level. In addition, in this case the coefficient is significant, even if at the level of significance of 10%. Column (4) shows the coefficient in a specification equal to that used in column (3), with the addition of controls only. The coefficient is significant at the level of 5%. The coefficient explains how for every 1,000 points of increase in the court inefficiency indicator, the probability of young adults answering yes to the main question increases by 14%.[13] Or, in other words, for every year of delay that one court takes to reach a

[12]For more detailed information regarding the approaches used by "Related Literature" section. Moreover, according to Eurostat data presented in the "Introduction section", in Italy the average age at which young people leave their parents' home is 30.1 years.

[13]In our sample, the court inefficiency indicator ranges from 132.16 (the value related to the most efficient court) to 2,312.97 (the value related to the most inefficient court). The average of our indicator is 785.69.

ruling compared to another, in the post-reform period young adults living in the district are 5.1% more likely to have positive household formation intentions compared to those who already lived in areas with very efficient courts.

Panel B of Table 2 shows the results related to the fertility intentions of young people. More accurately, as we have observed in the previous paragraphs, the dependent variable is a dummy equal to zero if the answer to the question "Over the next three years, do you expect to have (another) child?" are "Certainly yes" or "Probably yes", and zero if the answers are "Probably not" or "Certainly not". The Panel B essentially follows the same logic as the Panel A, but with a different outcome. In the same way that we have decided to use data relating only to young adults who are at least 25 years of age with regard to household formation intention, here as well we have decided to employ the same specification. While it is true that the period for fertility intentions asked in the survey is longer (3 years) than for household formation intentions (1 year), it is also legitimate that both literature and empirical evidence recognize household formation as the first step towards fertility. It therefore seems justified to resort to the same minimum age threshold. All specifications below therefore fixate on young adults over or equal to 25 years of age.

The specification used in column (1) includes fixed region and time effects, in a setup without controls. The value of the coefficient is positive but not significant. Given the positive sign of the coefficient, the impact of the reform on that variable is positive. What is more, an increase in the indicator of the inefficiency of the courts corresponds to a greater effect of the reform ("Jobs Act") on the fertility intentions of youngsters living in those specific districts. Or rather, that the reform in question seems to have had a greater impact on people living in districts with less efficient courts in terms of work-related processes (individual and collective dismissals). This result is detectable more clearly in column (2), in a specification that endows regional and time fixed effects, in addition to the use of controls. The coefficient in this case is positive, significant at the level of significance of 5% and is higher than that reported in column 1. Column (3) shows the coefficient in a specification that provides for district and time fixed effects, but not the use of controls. The diff-in-diff coefficient is positive and significant at a significance level of 10%. Column (4) shows our coefficient of interest in a set-up similar to that seen in column (3), with the only difference that we add the complete set of controls. The coefficient explains how for every 1,000 points of increase in the court inefficiency indicator, the probability of young adults answering yes to the main question in the post reform period increases by approximately 10%. Or, to put it in another way, it means that for every year of delay that one court takes to reach a ruling compared to another, young adults living in that district are 3.7% more likely to have positive fertility intentions (in the post-reform period).

Table 2: Main results

	(1)	(2)	(3)	(4)
Panel A: Household formation intentions				
*CourtEff*Post*	0.1650**	0.1542***	0.1414*	0.1406**
	(0.0835)	(0.0584)	(0.0726)	(0.0562)
Observations	12,728	12,728	12,728	12,728
R-squared	0.0811	0.1343	0.1388	0.1720
Panel B: Fertility intentions				
*CourtEff*Post*	0.0990	0.1032**	0.0884*	0.0998**
	(0.0606)	(0.0481)	(0.0470)	(0.0402)
Observations	18,661	18,661	18,661	18,661
R-squared	0.0133	0.0871	0.0732	0.1337
age	≥25	≥25	≥25	≥25
FE	region,time	region,time	district,time	district,time
Controls		√		√

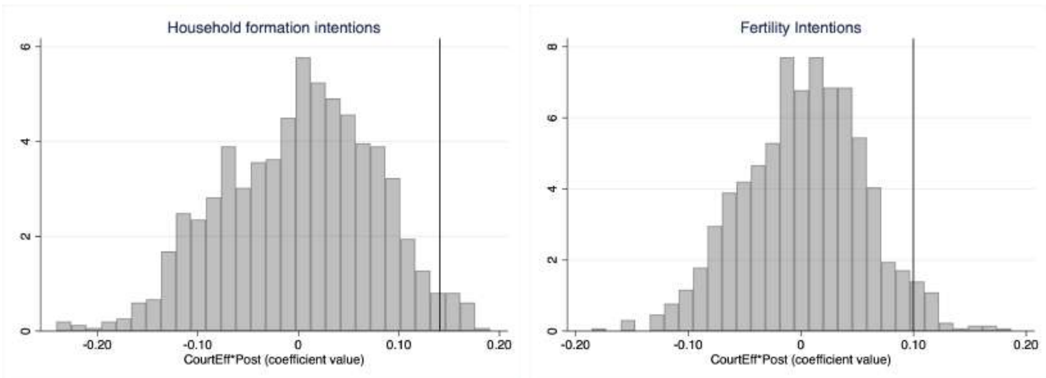
Notes: In Panel A the dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in that Panel. In Panel B the dependent variable is the dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in that Panel. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, different geographical level specifications and time fixed effect. The sample weights are applied. Standard errors are clustered at district level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

6.1 Placebo Tests and Robustness checks

To validate our empirical approach, we ran a battery of placebo and robustness checks. Here we present only the main ones, while the Appendix A contains further robustness checks. The diff-in-diff estimator is valid under the assumption of a common trend between treatment and comparison groups in the absence of the reform. Inasmuch as we do not have two years of survey available in the pre-reform period, as already mentioned above, we do not have the opportunity to investigate the common trend assumption directly. To provide evidence supporting the validity of the design, we performed some placebo tests. The first placebo test is related to the policy in question and targets the identification strategy adopted. Our identification strategy is based on the fact that the Jobs Act was more supposable to have a deeper impact in those areas characterized by a relatively higher courts inefficiency. In order to control that our indicator can be spuriously correlated with the outcome of interest, we therefore created a new efficiency indicator of the courts, created keeping the true values of the indicator and randomly shuffled them across districts.

At this point, we randomly distributed our "placebo" indicator to the various districts, and therefore to the young people living in the various districts. We replicated this random allocation for 1,000 times. The object of the analysis, as in the main specifications, are young adults with at least 25 years of age. Left graph in Figure 2 shows the results of the various random allocations for household formation intentions, while the right one shows the same results for fertility intentions. The independent variable, *CourtEff*Post (placebo)*, is the variable of interaction between the fake treatment indicator and the time dummy. In both figures the average of the estimated coefficients is centered at zero, and this shows that the relationship is not mechanical and automatic. Nonetheless, the districts are few and therefore in the various simulations it can occur that by chance some combinations are very similar to the true one, making to fall back the true effect (reported in correspondence of the black line) inside the range.

Figure 2: Random allocation of the court efficiency indicator



Notes: Each estimate shown in both graphs of Figure 2 is made by taking our main equation as the base equation. In the left graph the dependent variable is the dummy related to household formation intentions, while in the right graph the dependent variable is the dummy related to fertility intentions The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, different geographical level specifications and time fixed effect. Each estimate shown in the figures is made by taking our main equation as the base equation. For both graphs, the x-axis shows all the various values that the coefficient of interest (*CourtEff*Post*) takes on in the various models, in which the true indicators have been randomly shuffled and reallocated among districts one thousand times. The y-axis shows the probability density function of the estimated coefficients. The black vertical line is placed in correspondence of the "true" estimated value of the coefficient, reported respectively in column (4) of Table 2 Panel A/ Household formation intentions (*CourtEff*Post*=0.1406) and Panel B/ Fertility intentions (*CourtEff*Post*=0.0998).

In the same spirit, in Table 3, we followed the approach proposed by Pei et al. (2019) and we progressively excluded an X variable from the right side of the equation (1) and instead we employed it as a placebo outcome. This test should underline potential sources of unobservable bias by capturing the unbalancing of these variables. Reassuringly, our results do not show any significant relation between the variable *CourtEff*Post* and all the observed covariates.

Table 3: Test of covariate balance

OUTCOMES	**age**	**brother/sister**	**marital status**	**education**	**youth unemp**	**father's edu**	**mother's edu**
*CourtEff*Post*	-0.3012	-0.0473	-0.0192	0.1187	-0.0231	-0.1560	0.0315
	(0.2420)	(0.0356)	(0.0154)	(0.0974)	(0.1283)	(0.0999)	(0.1092)
Observations	19,899	19,899	19,899	19,899	19,899	19,899	19,899
R-squared	0.4039	0.1286	0.2427	0.2986	0.8981	0.3994	0.4034
age	overall	overall	overall	overall	overall	overall	overall
FE	district,time	district,time	district,time	district,time	district,time	district,time	district,time
controls	√	√	√	√	√	√	√

Notes: In each of the seven columns the dependent variable is different More specifically, each column has as a dependent variable a variable used in the controls in the main specification. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Since all controls are used as dependent variables in rotation, they are removed from the controls when they are used as dependent variables. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

In addition to these, the results of another robustness test are shown in the Table 4. For this specification we exploited the real efficiency indicator again. More specifically, we canceled every year until 2016 to focus our analysis on the years 2016 and 2017. At this stage, we arbitrarily shifted the introduction of the reform to 2016. Columns (1) and (2) show the results of this test with regard to household formation intentions, using different specifications. The variable *CourtEff*Post* is the interaction term between the efficiency indicator of the courts, which has remained unchanged, and the new time dummy, which contains values 0 for the year 2016 and 1 for the year 2017. Since we deleted three years of the survey, the observations are less than in the previous specifications. The coefficients of our variable of interest, i.e. the household formation intentions, is positive in column (1) and negative in column (2), but highly insignificant. With this test, we thus verified that if we had considered a fictitious reform started in January 2016, we wouldn't have found any results. Columns (3) and (4) report the results of the arbitrary shift of the year of implementation of the reform with respect to fertility intentions. As can be deemed from the magnitude and significance of the coefficients, no results were found here either.

That is why, what emerges from the last two robustness is that if the judicial efficiency indicators are assigned randomly or if the year of introduction of the reform is changed arbitrarily, the coefficients are always statistically not significant. And this applies to both outcomes: Household formation intentions and Fertility intentions.

Table 4: Arbitrary shift in the year of implementation of the reform

	Household formation intentions		**Fertility intentions**	
	(1)	(2)	(3)	(4)
OUTCOMES				
*CourtEff*Post*	0.0005	-0.0207	0.0135	0.0028
	(0.0517)	(0.0520)	(0.0311)	(0.0303)
Observations	4,706	4,706	7,232	7,232
R-squared	0.0993	0.1775	0.0691	0.1280
age	≥25	≥25	≥25	≥25
year	2016-2017	2016-2017	2016-2017	2016-2017
FE	district,time	district,time	district,time	district,time
controls		√		√

Notes: From column (1) to column (2) the dependent variable is the dummy related to household formation intentions, while from column (3) to column (4) the dependent variable is the dummy related to fertility intentions. The variable *CourtEff*Post* in column is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy (arbitrarily shifted, which takes value 0 in 2016 and 1 in 2017), divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications,parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

Besides the validation tests of our design, we also performed robustness checks to verify any incorrect specifications. The first robustness check is related to the age threshold. In the main specifications we chose to direct the analysis to all young people between 25 and 34 years of age. The choice was dictated by both the literature and the formulation of the questions in the survey. On the other hand, to demonstrate that the choice of age is not arbitrary and, above all, that the change in minimum age threshold does not change the results, we decided to replicate the main tables using 24 and 26 years of age as the minimum age. Panel A of Table 5 shows the results relevant to household formation intentions. In the first two columns of the table, the minimum age threshold is 24 years old, while in the other columns the minimum age threshold is raised to 26 years old. As for the previous tables, the specifications used replicate those in the main table. All the coefficients obtained from the various specifications are statistically significant. More in detail, in column (1) the specification accounts for district and time fixed effects, and no controls are used. The coefficient reported in column (1) is significant at the 10% level. In column (2) the specification remains the same, with the only addition of controls. The coefficient increases slightly in magnitude and turns significant at the 5% level. In columns (3) and (4) is reported the same pair of specifications just examined, but with respect to young adults over 26. In both columns the coefficient is significant at the level of 1%. From the comparison of these results with main results reported in Table 2, it is clear that magnitude of the coefficient, compared to the main specification (25 years), is slightly lower in the version with minimum age 24 years, while it is 5% higher if the minimum age is fixed to 26 years old. This is in line with the fact that a higher age "per se" increases the likelihood to form a family.

Panel B of Table 5 shows the results of the robustness check on age threshold concerning fertility intentions. Again, in the first two columns of the table, the minimum age threshold is set to 24 years old, while in the other columns the minimum age threshold is raised to 26 years old. In this case, the coefficients are significant in all specifications that require the use of controls, while they are not significant in the absence of controls when the minimum age threshold is 24 years old. In the specification shown in column (4), which accounts for district and time fixed effects, and for the use of controls, the coefficient is significant at the 1% level. Again, the magnitude of the coefficient, compared to the main specification (25 years), is slightly lower (2%less) in the version with minimum age 24 years, while it is 3% higher if the minimum age is fixed to 26 years old. [14]

Table 5: Robustness test on age threshold

	(1)	(2)	(3)	(4)
Panel A: Household formation intentions				
*CourtEff*Post*	0.1130*	0.1247**	0.2139***	0.1922***
	(0.0680)	(0.0525)	(0.0744)	(0.0596)
Observations	14,232	14,232	11,124	11,124
R-squared	0.1244	0.1659	0.1452	0.1728
Panel B: Fertility intentions				
*CourtEff*Post*	0.0535	0.0740**	0.1280**	0.1279***
	(0.0379)	(0.0338)	(0.0518)	(0.0444)
Observations	20,741	20,741	16,742	16,742
R-squared	0.0670	0.1503	0.0734	0.1253
age	≥24	≥24	≥26	≥26
FE	district,time	district,time	district,time	district,time
Controls		√		√

Notes: In Panel A the dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the Panel. In Panel B the dependent variable is the dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in the Panel. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, different geographical level specifications and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

[14] In the Appendix A, further robustness checks are provided. Among others, robustness checks are reported in order to investigate possible links between the dependent variables and the macroeconomic scenario. In addition, a robustness to polynomial order, a specification where we remove the age restriction and a specification where we use different dependent variables (aggregating differently the variables of the questionnaire) are reported.

6.2 Heterogeneous effects

It is interesting to investigate the possible existence of heterogeneous effects. The lines of heterogeneity that we decided to examine are essentially related to the characteristics of individuals and the environment in which they live.

In Panel A of Table 6 are reported the results of that analysis of heterogeneous effects with regard to the family formation intentions. In Panel A, Columns (1) and (2) show the coefficients for our interaction term calculated respectively on the sub-sample of men (column 1) and women only (column 2). The coefficient is significant in both cases, and also in terms of magnitude there are no particular dissimilarities. In the following three columns the analyses were made by dividing the sample according to the geographical area of residence of the individuals. In particular, respectively, in column (3) the analysis zeroes in on northern Italy, in column (4) on central Italy and column (5) on southern Italy. It is interesting to note that the relationship that we are investigating still valid for the north and the centre, but not for the south. The effect for those living in southern Italy is not statistically significant. In terms of magnitude, the greatest impact of the Job Act on the intention to form a family is in central Italy. Anyway, we found that the positive effect of the Jobs Act in the less efficient districts is mainly driven by the northern and central Italy. However, southern Italy is certainly the geographical area with the least efficient districts, as well as the one with the lowest number of efficient courts. As a consequence, there is not enough variability across districts in the south. Therefore, applying our strategy to this specific geographical area could be problematic by construction. Figure 1 shows this condition at a glance. The last two columns of Table 8 show the results respectively for the sub-population of graduates (column 7) and for those who are not in possession of degree (column 8). The coefficient of our interest is statistically significant only for graduates young Italians. This outcome does not surprise us because those who have not studied, on average, start working and therefore earning before. In the short term (18-24 years of age) they are more likely to have an open-ended contract and they accumulate more savings than their peers who attend university. As a result, it is not surprising that the characteristics of the reform and the indirect channel through which it operates impact more on those who started working later, have fewer resources and have yet to stabilize. For this category of people, therefore, it is more likely that the indirect channel that we assumed impacts stronger than on others.

Panel B of Table 6 shows the same set of heterogeneous effects with regard to fertility intentions. Columns (1) and (2) show the coefficients for our interaction term calculated respectively on the sub-sample of men (column 1) and women (column 2). By contrast with what we noted for the family formation intentions, in this case there is a difference between the coefficient of men and the coefficient of women. In this case only the coefficient of women is statistically significant. The difference probably partly reflects the fact that women may be more influenced by the reform and thus change their intentions more than men as the weaker part of the labour market. Istat 2020 data report a situation according to which women are less than men on the labour market, are paid less and have fewer permanent positions. Columns (3), (4) and (5) show the results for the different geographical areas: north, centre and south. Bearing in mind the coefficients, only in central Italy young adults living in areas with less efficient post-reform courts are more "optimistic" about fertility intentions than their counterparts living in areas with more efficient courts. The last two columns show the results for those who have graduated and those who have not continued their studies. As far as this aspect is concerned, the same thing applies as before: the positive impact in terms of intentions mainly affects university students living in districts that have courts with negative performance. As for those who have not continued their studies, it is worth what was already said: more savings and a higher probability of having a job, make the indirect mechanism we investigate in this work disappear.

Table 6: Heterogeneous effects

	(1) men	(2) women	(3) northern Italy	(4) central Italy	(5) southern Italy	(6) graduate	(7) not graduate
Panel A: Household formation intentions							
*CourtEff*Post*	0.1017**	0.1073*	0.1784**	0.2833***	-0.0028	0.1334**	0.0912
	(0.0485)	(0.0583)	(0.0880)	(0.0873)	(0.0449)	(0.0526)	(0.0592)
Observations	4,693	8,035	5,200	2,028	5,500	6,376	6,352
R-squared	0.1916	0.2314	0.2247	0.2128	0.1489	0.2550	0.1847
Panel B: Fertility intentions							
*CourtEff*Post*	0.0257	0.1094**	0.0531	0.1786**	0.0191	0.0986**	0.0402
	(0.0382)	(0.0483)	(0.0854)	(0.0729)	(0.0395)	(0.0406)	(0.0356)
Observations	6,673	11,988	8,238	3,187	7,236	9,912	8,749
R-squared	0.1873	0.1618	0.1599	0.1465	0.1275	0.2348	0.1413
age	≥25	≥25	≥25	≥25	≥25	≥25	≥25
FE	district,time	district,time	district,time	district,time	district,time	district,time	district,time
controls	√	√	√	√	√	√	√

Notes: In Panel A the dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the Panel. In Panel B the dependent variable is the dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in the Panel. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. In each column we removed from the controls the variable that is used to split the sample. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

In Table 7 others potential heterogeneous effects are shown, according to a battery of characteristics both individual and relative to the territory in which the people reside. In Panel A of Table 7 are reported the results of that analysis of heterogeneous effects with regard to the family formation intentions. In column (1) and (2) we divide the sample in line with the Youth unemployment level at a provincial level, using as threshold the median value. We found that the reform impact only on those

who live in territories with a youth unemployment rate lower than the median value (column 2), with a higher magnitude than the main specification. This fact can be read in relation to the geographical heterogeneity of the previous table. Where unemployment is high and the socio-economic characteristics of the territory are negative, the reform does not appear to have had an impact (column 1). Instead, it seems to have produced an impact where the context conditions, although negative in terms of court performance, were not as negative in terms of labour market opportunities. It is therefore likely that where youth unemployment is very high, the reform is not strong enough to change the outcomes (youth perceptions remain strongly negative). In column (3) and (4) we divide the sample according to the number of firms above the 10 employees threshold out of the total number of firms in the province. Although the ten employee threshold is used as a proxy, as a matter of fact the Jobs Act has only impacted on those who work in a company with more than fifteen employees. Also in this respect what is noticeable is that the reform seems to have impacted especially on the provinces with a higher number of firms above the threshold (column 4). As we would have expected, in provinces with few firms impacted by the reform, the coefficient is not statistically significant (column 3). This result is consistent with our hypothesis regarding the mechanism of indirect transmission between law, courts, employers and youth. Where there is a low presence of firms, the reform does not impact on young people intentions because they do not perceive a positive indication in terms of increased chances of finding a job. No matter how efficient the courts are. Thus, it appears to validate our identification strategy.
If we had previously tried to change the minimum age of the young people to be considered in our analysis, in columns (5) and (6) we then try instead to verify if there are heterogeneous effects between the youngest and the oldest. In column (5) the results are reported only for those who are 25 and 26 years old, while in column (6) for all those who are between 27 and 35 years old. The result is statistically significant only in column (6). This does not surprise us, given the increasingly higher average age at which young Italians manage to leave their family of origin. In columns (7) and (8) we divide the sample between those who are married and those who are not. What we obtained is that the coefficient of our interest is significant only for those who are not married, as those who are already married are very likely to already live together.
In Panel B of Table 7 we look at heterogeneous effects according to the same battery of characteristics seen in Panel A, but with regard to fertility intentions. From the interpretation of columns (1) and (2), it is evident that the reform affected only those who live in territories with a youth unemployment rate lower than the median value (column 2). In column (3) and (4), again, what we notice is that also with respect to fertility intention the reform seems to have impacted, through an indirect channel, especially in the provinces with a high number of firms above the threshold (column

4). As we expected, in provinces with few firms impacted by the reform, the coefficient is not statistically significant (column 3). In column (5) the results are reported only for those who are 25 and 26 years old, while in column (6) for all those who are between 27 and 35 years old. The result, as with regard to household formation intention shown in Panel A, it is statistically significant only in column (6). This does not surprise us, given the increasingly higher average age at which young Italians manage to leave their family of origin. Leaving the family is a first step towards the choice of having a child, and also on this front young Italians are lagging behind their European peers. In columns (7) and (8) we divide the sample between those who are married and those who are not. What we obtain is that the coefficient of our interest is significant in both cases, in contrast to household formation intentions. The coefficient in magnitude is larger for those who are already married by about four percentage points. However, this is not surprising. While it is very likely that married people already live together, it is not obvious that they have (or are waiting for) a child.

Table 7: Heterogeneous Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Yunemp(high)	Yunemp(low)	%firms(low)	%firms(high)	youngest	holdest	married	not married
Panel A: Household formation intentions								
*CourtEff*Post*	0.0215	0.2223***	0.0805	0.1487**	-0.0114	0.2261***	-0.1396	0.1412**
	(0.0545)	(0.0708)	(0.0493)	(0.0676)	(0.0566)	(0.0662)	(0.1591)	(0.0546)
Observations	6,691	5,918	3,944	8,784	3,060	9,666	2,272	10,451
R-squared	0.1682	0.2228	0.1915	0.1814	0.2381	0.1864	0.2980	0.1591
Panel B: Fertility intentions								
*CourtEff*Post*	-0.0082	0.1319*	-0.0015	0.1153*	0.0374	0.1101**	0.1541*	0.1138**
	(0.0404)	(0.0791)	(0.0389)	(0.0599)	(0.0474)	(0.0470)	(0.0845)	(0.0456)
Observations	9,252	9,366	5,241	13,420	3,931	14,730	4,145	14,514
R-squared	0.1189	0.1641	0.1633	0.1300	0.2888	0.1111	0.1571	0.1185
age	≥25	≥25	≥25	≥25	≥25 and ≤26	≥27	≥25	≥25
FE	district,time	district,time	district,time	district,time	district,time	district,time	district,time	district,time
controls	√	√	√	√	√	√	√	√

Notes: In Panel A the dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the Panel. In Panel B the dependent variable is the dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in the Panel. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. In each column we removed from the controls the variable that is used to split the sample. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

7 Conclusions

Using a representative sample of Italian youngster, this paper examines the differentiated aftermaths of the EPL reduction provided by the Jobs Act in 2015 in Italy on the household formation and fertility intentions of young Italians in various districts. To analyze this indirect effect that the reform of the labor market, the Jobs Act, through the different behaviour of employers, had on the perception of insecurity and subsequently on the family formation and fertility intentions of young Italians, we used an impact evaluation method: difference-in-differences.

As a prerequisite for this analysis, we started from the fact that in Italy there is a within-country variation in terms of de facto firing costs. This difference is mainly due to the different degrees of efficiency of the courts located in the various districts. Since in the pre-reform period, it was up to the judge to decide matters relating to individual and collective dismissals, the time of justice was an important factor for the employer in the choices about hiring, and of the type of contract. The Jobs Act, i.e. the contract with increasing protection, no longer provides for the dismissal to be decided by a judge, and thus changes the institutional setting that faces an entrepreneur.

We found that, in the post-reform period, the household formation and fertility intentions of young adults improved more in places where the courts were less efficient in the pre-reform period than in places where the courts were more virtuous. We further showed, studying the existence of heterogeneous effects, that the reform exerts influence above all on the intentions of both fertility and household formation of those who are graduates, who live in central-northern Italy and who live in regions with youth unemployment rate lower than the median. Additionally, in the regions with a higher concentration of firms above 10 employees, for the same efficiency of the courts, the outcomes of our interest have improved more than in areas with fewer companies above the threshold.[15]

To validate our results, we also implemented a series of robustness checks. These results made us think of an indirect effect of the reform on the perception of security of young adults. The reform may have had an impact on the choices of entrepreneurs in areas with less efficient courts, i.e. those that have benefited most from the change in legislation. The others, that already had fast courts at their disposal in the pre-reform period, have benefited less from the reform. This, in turn, may have influenced the perceptions of young adults. While many studies have investigated the determinants of household formation and fertility of young adults, to the best of our knowledge no one had focalized on a similar differentiated effect of a labor market

[15] We used the 10-employee threshold as a proxy for the 15-employee threshold, which would be the correct threshold but for which we had no available data. This result is in line with the provisions of the Jobs Act. In fact, as can be seen in the Institutional background section, the Jobs Act has changed the dismissal discipline for companies above 15 employees.

reform on the household formation and fertility intentions. As for the work that focused on the effects of the Jobs Act, no one has ever investigated this heterogeneous effect. Most of the studies, differently, focused on whether the Jobs Act created jobs or not.

Only recently De Paola et al. (2020) investigated the impact of the reduction in employment protection provided by the Jobs Act for large-firm employees. On the one hand, they found a negative impact on the childbearing probability of large-firm employees compared to small-firm employees, who were not affected by the reform. On the other hand, our study highlighted that the reform seems to have indirectly levelled out the fertility and household formation intentions of young Italians living in districts with more and less efficient courts. In particular, this gives the impression of being due to the reduction of firing costs that entrepreneurs in less efficient districts had to bear in the pre-reform period. Thus, acting as a strong disincentive to permanent hiring.

Future research could look into the impact that recent labor market reforms have had on the household formation and fertility intentions of young adults in other European countries, using policy evaluation methods.

8 Appendix A

A.1 Additional placebo and robustness checks

Table 8 shows the results of an additional robustness check. Taking up the initial household formation intentions dummy as the dependent variable, we added some interaction terms. Or rather, we removed youth unemployment at the provincial level from the controls and put it into regression as an independent variable, either interacting with the time variable and interacting with the indicator, as well as alone. Once again, the specifications used are several. In the first two columns, the threshold is set at 24 years, in column (1) the basic model is shown, while in column (2) the interactions are also included. The other pairs of columns follow the same logic, with the difference that the reference age threshold changes, respectively equal to 25 and 26 years old. From the magnitude and significance of the various coefficients, we can conclude that there is no relation between the macroeconomic context variable used above and the dependent variable, the dummy related to household formation intentions.

Table 8: Additional robustness checks on household formation intentions

	Household formation intentions					
	(1)	(2)	(3)	(4)	(5)	(6)
OUTCOMES						
*CourtEff*Post*	0.1247**	0.1161**	0.1406**	0.1343**	0.1922***	0.1739***
	(0.0525)	(0.0563)	(0.0562)	(0.0572)	(0.0596)	(0.0601)
Post*Yunemp		-0.0000		-0.0000		-0.0000
		(0.0000)		(0.0000)		(0.0000)
CourtEff*Yunemp		0.0015		0.0021		0.0027
		(0.0021)		(0.0021)		(0.0023)
Yunemp		0.0025		0.0023		-0.0024
		(0.0055)		(0.0060)		(0.0060)
Observations	14,232	14,232	12,728	12,728	11,124	11,124
R-squared	0.1659	0.1661	0.1720	0.1720	0.1728	0.1728
age	≥24	≥24	≥25	≥25	≥26	≥26
FE	district,time	district,time	district,time	district,time	district,time	district,time
controls	√	√	√	√	√	√

Notes: The dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the table. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. The variable Post*Yunemp is an interaction term between the youth unemployment rate at the provincial level and the year dummy. The variable Indic*Yunemp is an interaction term between the efficiency indicator of the courts and the youth unemployment rate at the provincial level. The variable Yunemp is the youth unemployment rate at provincial level. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

Table 9 shows the same test applied to fertility intentions. Even in this incident the dependent variable adopted is the one used in the baseline model. Once again, the specifications employed are several. In the first two columns, the minimum age threshold is set at 24 years, in column (1) the basic model is shown, while in column (2) the interactions are also included. The other pairs of columns follow the same logic, with the difference that the reference age threshold changes, respectively equal to 25 and 26 years. From the magnitude and significance of the various coefficients, we can conclude that here too there is no relation between the macroeconomic context variable used above and the dependent variable, the dummy related to fertility intentions.

Table 9: Additional robustness checks on fertility intentions

	Fertility intentions					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLE						
*CourtEff*Post*	0.0740**	0.0571	0.0998**	0.0768	0.1279***	0.1070*
	(0.0338)	(0.0446)	(0.0402)	(0.0509)	(0.0444)	(0.0546)
Post*Yunemp		0.0011		0.0021		0.0025
		(0.0014)		(0.0016)		(0.0018)
CourtEff*Yunemp		0.0000		0.0000		-0.0000
		(0.0000)		(0.0000)		(0.0000)
Yunemp		0.0005		-0.0017		-0.0011
		(0.0056)		(0.0053)		(0.0056)
Observations	20,741	20,741	18,661	18,661	16,742	16,742
R-squared	0.1503	0.1483	0.1337	0.1326	0.1253	0.1239
age	≥24	≥24	≥25	≥25	≥26	≥26
FE	district,time	district,time	district,time	district,time	district,time	district,time
controls	√	√	√	√	√	√

Notes: The dependent variable is the dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in the table. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. The variable Post*Yunemp is an interaction term between the youth unemployment rate at the provincial level and the year dummy. The variable Indic*Yunemp is an interaction term between the efficiency indicator of the courts and the youth unemployment rate at the provincial level. The variable Yunemp is the youth unemployment rate at provincial level. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

Table 10 shows the robustness to polynomial order for both outcomes of interest. The rationale for this robustness check is that the sensitivity of the reform does not change as the degree of inefficiency varies. As noted, the *CourtEff^2*Post* coefficient is not negative. As a consequence, our identification strategy holds.

Table 10: Robustness to polynomial order

	Household Formation	**Fertility**
	(1)	(2)
OUTCOMES		
*CourtEff*Post*	0.0828	0.0640
	(0.164)	(0.126)
*CourtEff^2*Post*	0.0322	0.0202
	(0.0715)	(0.0559)
Constant	0.237	0.209
	(0.224)	(0.234)
Observations	12,728	18,661
R-squared	0.172	0.134
age	>=25	>=25
FE	district,time	district,time
controls	√	√
P-value	0.00743	0.00582

Notes: The dependent variable in column (1) is the dummy related to household formation intentions, while in column (2) the dependent variable is the dummy related to fertility intentions. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. The variable *CourtEff^2*Post* is an interaction term between the efficiency indicator of the courts squared up and the year dummy, divided by one thousand to normalize the indicator. In the last row of the table is also shown the coefficient of the constant term. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, district and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

Table 11 replicates the main results on both outcomes but also using youngsters in the 18-25 age group. In this way the average age is much lower than the specification used in Tables 2 and 3 and the coefficients, while remaining with the same sign, are not statistically significant in all specifications. Coefficients that, in any case, are lower in magnitude. Despite that, the fact does not surprise us. It is likely that the mechanism we described above will not affect on young people who are still in high school or university. At that age, it is too early to make decisions about having a child or moving to live on your own, especially in light of the new habits of Italians and the statistics we reported in the "Introduction" section.

Table 11: Main results overall (age:17-34)

	Household formation intentions		**Fertility intentions**	
	(1)	(2)	(3)	(4)
OUTCOME				
*CourtEff*Post*	0.0060	0.0611	0.0147	0.0484*
	(0.0194)	(0.0383)	(0.0173)	(0.0256)
Observations	19,899	19,899	27,049	27,049
R-squared	0.0968	0.1267	0.1704	0.1973
age	overall	overall	overall	overall
FE	region,time	district,time	region,time	district,time
controls	√	√	√	√

Notes: The dependent variable is the dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the table. The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, different geographical level specifications and time fixed effect. The sample weights are applied. Standard errors are clustered at district level. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by:$p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

Table 12 shows the main results for both household formation and fertility intentions using a different dependent variable. More precisely, what we did was to aggregate in a different way the answers to the questions related to family formation and fertility intentions. The interest coefficients are statistically significant regardless of the specification used in Panel A, concerning family formation intentions. In contrast, in Panel B the interest coefficients are lower in magnitude, positive nevertheless not statistically significant. The notes of the tables show the specifications used and the methods of aggregation of the answers. These results are not particularly surprising because the difference in the aggregation of responses (see notes to Table 12) is less intuitive than that used in the rest of the paper.

Table 12: Different dependent variables

	(1)	(2)	(3)	(4)
Panel A: Household formation intentions				
*CourtEff*Post*	0.1581*	0.1538**	0.1435*	0.1501**
	(0.0880)	(0.0702)	(0.0817)	(0.0646)
Observations	12,728	12,728	12,728	12,728
R-squared	0.0262	0.0599	0.0646	0.0919
Panel B: Fertility intentions				
*CourtEff*Post*	0.0275	0.0431	0.0276	0.0532
	(0.0557)	(0.0467)	(0.0498)	(0.0413)
Observations	18,661	18,661	18,661	18,661
R-squared	0.0156	0.0604	0.0589	0.0979
age	≥25	≥25	≥25	≥25
FE	region,time	region,time	district,time	district,time
controls		√		√

Notes: In Panel A the dependent variable is a new dummy related to household formation intentions, and it remains unchanged in all the different specifications shown in the Panel. In order to create the new "household formation intentions" dummy, we have aggregated in a different way the answers of the survey. In particular, we have created a dummy equal to one if the answer to the question "Do you plan to live alone within the next year?" are "Yes, it is very likely" or "Probably not", and zero if the answer is "Certainly not, certainly later". In Panel B the dependent variable is a new dummy related to fertility intentions, and it remains unchanged in all the different specifications shown in the Panel. To create the new dummy of fertility intentions, we have aggregated differently the answers of the survey. In particular, we have created a dummy equal to one if the answer to the question "Over the next three years, do you expect to have (another) child?" are "Certainly yes", "Probably yes" "Probably not", and zero if the answer is "Certainly not". The variable *CourtEff*Post* is the diff-in-diff interaction term between the efficiency indicator of the courts and the year dummy, divided by one thousand to normalize the indicator. Controls include: age, marital status, presence of brothers and sisters, educational qualifications, parents' educational qualifications, youth unemployment rate at a provincial level, different geographical level specifications and time fixed effect. Standard errors are clustered at district level. The sample weights are applied. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10 = *, p < 0.05 = **, p < 0.01 = ***$.

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CHAPTER 3

The effects of the Affordable Care Act on time use

The effects of the Affordable Care Act on time use*

Abstract

Through the analysis of the American Time Use Surveys daily diary data, we study the impact of the Affordable Care Act on the time allocation of childless adults focusing on two key pillars of the Affordable Care Act: Medicaid expansion and Tax Premium Subsidies. We adopt a triple differences-in-differences approach that hinges on income eligibility thresholds and cross states variation in the time of implementation of these two pillars, to conclude that individuals newly eligible to Medicaid reduced their labour supply at the intensive margin, while potential beneficiaries of Tax Credit Premium Subsidies increased their labour supply at the extensive margin. In particular, our estimates suggest that people newly eligible to Medicaid may reduce long working hours and spend less time waiting for and receiving care. On top of that, they perform more household chores and management tasks, and also dedicate more time to caring for individuals from other households and volunteering. In contrast, potential beneficiaries of Tax Credit Premium Subsidies reduce their leisure time, on average. The rationales for these findings are discussed and our results are set in perspective of earlier studies.

Keywords: Affordable Care Act, Medicaid, Tax Credit Premium Subsidies, Health insurance, difference-in-difference-in-differences, Labor supply, Health care utilization, American Time Use Survey (ATUS).

JEL classification: I10, I13, I18, J01, J22.

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1 Introduction

In 2010 the Affordable Care Act (ACA) was enacted by the United States Congress and signed into law by President Barack Obama. It is widely recognized that ACA represents the largest expansion ever of the US health care system. Prior to ACA, only specific vulnerable groups -such as low-income families with children, pregnant women, the disabled and the elderly (the latter via Medicare)- had access to public health insurance with coverage and generosity varying widely across US states. Be that as it may, ACA did not enter immediately into force as the US Supreme Court found that it suffered from some forms of unconstitutionality. Only the first pillar of ACA was then implemented in 2010, the Dependent Coverage Mandate (DCM), which required employer-sponsored insurance plans to cover children, who have thus started to be covered until they turn twenty-six years old. The other two ACA pillars that we examine here, on the one hand, expanded Medicaid eligibility criteria which target households with income below and around the poverty line and, on the other hand, lowered the costs of private health insurance by introducing specific Tax Premium Subsidies, which are targeted at households with income just above the poverty line.

The Medicaid expansion was staggeringly implemented across US states between 2014 and 2015, with some states never putting it into force -following a 2012 Supreme court ruling that left to each state the choice of whether to expand Medicaid or not. In contrast, the Tax Credit Premium Subsidies entered into force in all states on the 1st January 2014. As a consequence, we exploit household income eligibility thresholds, combined with cross-state variation in the timing and implementation of these two ACA pillars to identify their impact on the time allocation of potential beneficiaries using over 4.000 daily activity diaries from the American Time Use Surveys (ATUS)[1] over the period 2012-2015.[2] With respect to Medicaid, in contrast to other studies that have assumed that those living in expansionary states after January 2014 are considered treated from the outset, we target individuals treated on the basis of the actual entry into force of the reform in that specific state, thanks to the continuous daily nature of ATUS.

Most of the earlier evaluation literature on ACA focused on effective coverage and health outcomes (Barbaresco et al. 2015; Antwi et al. 2013; Cantor et al. 2012; Sommers et al. 2012; Courtemanche et al. 2016; Kaestner et al. 2017; Frean et al. 2017). A handful of studies examined the labour supply effects of ACA, arriving at controversial conclusions (Aslim et al. 2020; Moriya et al.2016; Gooptu et al.

[1]There is to date a wide and established literature that used ATUS in economics (see, for instance, Hamersmesh et al. 2005, and Aguiar et al. 2013).

[2]We did not use the years after 2015 as the results of the 2016 elections cast great doubt on the future of the ACA. This reform, also informally called the "Obamacare reform act", was strongly desired by Obama and his administration, while it has always been attacked by US President Donald Trump. Even during the election campaign.

2016; Kaestner et al. 2017; Leung and Mas 2018). Our study sheds new light on the effect of Medicaid expansion and Tax Premiums on the hours effectively worked by exploiting daily activity diaries that provide information on the actual hours of work of a representative sample of childless Americans. Additionally, we can also investigate how these major ACA pillars affected other uses of time, which fosters our understanding of the overall effect of the reform on the daily lives of the targets. The only studies that investigated the time allocation effects of ACA centred on the Dependent Coverage Mandate (Colman and Dave 2018, Lenhart et al. 2017). In consequence of that, this study adds to the existing literature by providing new evidence on the labour supply and other time use effects of ACA.

An extensive literature studied the impact of Medicare (public health insurance for the elderly), Medicaid (public health insurance for the most vulnerable) and ACA on coverage and health outcomes. For example, Card et al. (2008) and Card et al. (2009) concluded that since the introduction of Medicare, the number of uninsured people aged over 65 has dramatically fallen. Looking at the ACA 2010 Dependent Coverage Mandate (DCM), Barbaresco et al. (2015), Antwi et al. (2013), Cantor et al. (2012) and Sommers (2012) found an increment in the probability to be insured in the post-DCM period for young adults. Focusing on the same reforms as we do here, Courtemanche et al. (2016), Kaestner et al. (2017) and Frean et al. (2017), among others, concluded that ACA increased significantly coverage in post ACA reform states, with the effects being stronger in states that put into force the Medicaid expansion in addition to the Tax premium credits. Generally, it is also found that visits to the general practitioner increased and there was an overall improvement in self-declared health conditions. There is on that account substantial convergence in the literature on the positive effects of expansive health reforms on coverage, and especially so for low-income childless adults. In contrast with this, the research stream that looked into the impact of ACA on labour market outcomes comes to controversial conclusions.

Labour and health care have always been extremely closely linked in the USA. Before these reforms, as a matter of fact, having a job was the easiest way to get health insurance. Much of the earlier work dealt with expansion coverage effects on job locks, job push, job mobility and job supply. Baicker et al. (2014) found no labour market effect of an earlier Medicaid expansion in Oregon. By way of contrast Dave et al. (2015) studied an earlier expansion of Medicaid for pregnant women, and found a reduction in labour supply. Barkowski (2020) also studied some earlier expansions and founds evidence of both job lock and job push. Garthwaite et al. (2014) studied the end of an early expansion program of Medicaid in Tennessee and concluded for a positive impact on labour supply at the extensive margin. Depew (2015) studied the impact of DCM on the labour supply of young adults and found a reduction in labour supply at the intensive margin. Aslim et al. (2020) and Moriya et al. (2016) found a

negative impact of Medicaid on the labour supply of childless adults, and a shift from full time to part time work. Thus, the results are still mixed.
Here we target the distinct effects of the Medicaid expansion and Tax premiums credits ACA pillars in 2014 and 2015 on the labour supply and other time uses of low-income childless adults of working age, a population which did not have access to public health care before ACA. We take a triple diff-in-diff estimation strategy as in Frean et al. (2017), allowing for differential effects in each of the first two years of implementation. Given the implicit increment in non-labor income, one may expect Medicaid expansion to reduce labour supply. Conversely, targets may increase labour supply to become eligible for Premium Tax Credits. As a consequence it is of paramount importance to control for both pillars, in order to gain a full understanding of the impact of ACA on the labour supply of the targets.
On top of that, this is the first study to investigate the influence of these two pillars on other time uses. As far as the time spent receiving and waiting for medical care is concerned, we investigate whether this has actually been reduced, due to the replacement of emergency room services (usually length) by visits to the general practitioner. Nonetheless, access to health coverage could also increment medical visits as well as moral hazard and/or risk taking behaviours. In addition to this time use category, we take into account household chores, household management tasks, volunteering and caring for people in other households, leisure and doing sports. The results of estimation of our model indicate an overall inflation in the time spent doing household activities for Medicaid targets and a reduction in leisure time for people eligible to Tax Premium Credits.
The rest of the paper is organized as follows. In section 2, a literature review is presented, both for health coverage/health related outcomes, labor market outcomes and studies which used ATUS data. Section 3 shows the institutional background, which contains both a summary of the latest health reforms in US and a more detailed explanation of the ACA reform. In section 4, we deal with the ATUS data and the descriptive statistics. Section 5 and 6 present respectively the conceptual framework and the empirical strategy. In section 7 empirical results, placebo and robustness checks are reported, as well as heterogeneous effects. Section 8 concludes. Appendix A provides a review of the literature related to health coverage and health related outcomes, while Appendix B discusses information related to the eligibility conditions of the various states, to the construction of the treatment thresholds, to the aggregation of the ATUS variables, as well as to additional robustness and placebo checks.

2 Related Literature

There is a wide literature on health and labour supply effects of Medicaid, Medicare and Affordable Care Act (ACA) and we summarize some of these studies below, without claiming to be exhaustive but rather with the aim to provide a general picture of the issues at stake. We also summarize some of the thin literature that targeted on other time uses.[3]

2.1 Labor market outcomes

The previous paragraph described a substantial convergence in the literature on the positive effects of expansive health reforms on coverage, especially for low income childless adults.

In this section we turn to another key issue, namely the impact of health reforms on the labour market. In the United States, health insurance has always been closely linked to employment. Before the ACA, the only way to get health insurance for most Americans was through employment, so that an expansion of public health insurance has the potential to have a major impact on the labour market. Health insurance is typically offered only to those in full-time employment, so an expansive coverage reform can also impact the choice of having a full-time or part-time job, as well as the choice of whether or not to work. Such a reform may also impact on the employment lock, i.e. the role of employer-provided insurance in deciding to work tout court, as well as on the job lock, i.e. the role of employer-provided health insurance in reducing job mobility. In addition, having access to health insurance that is not linked to a particular workplace may increase the likelihood that workers move from a job that provides private insurance to one that does not, but that is more in line with their skills (the so-called "job push" effect). For policy makers, knowing whether there are consequences on the labour market associated with a health care reform is a crucial component of a cost-benefit analysis of its effectiveness.

In this regard, a somewhat older research stream dealth with the labour market impact of some health reforms in the United States between the 1990s and the first decade of 2000.

Bansak (2008), by studying two Survey of Income and Program Participation (SIPP) waves, assessed the impact of the State Children's Health Insurance Program (SCHIP) on the job lock. He found that parents of children who were SCHIP-eligible were 5-6% more likely to change jobs than before the programme was introduced. This suggests that the introduction of public health insurance for children reduced the job lock among near-poor working parents. Hamersma et al. (2009) study the impact of early parental Medicaid expansions on job mobility. Principally, the subject of study was

[3] Appendix A1 also contains a review of the literature related to health coverage and health related outcomes.

the personal responsibility and work opportunities and reconciliation act of 1996. Thanks to this act, states gained flexibility to expand Medicaid to adults who previously were not eligible. They found that parental Medicaid expansions reduces job locks among low-income unmarried women. They also noticed that expanded eligibility moderately reduced the mobility of uninsured job seekers. Boyle et al. (2010) used data from the Current Population Survey (CPS) and an expansion from the 1990s in the U.S. Department of Veterans Affairs health care system aiming at assessing the effects of increased health insurance availability on labor market outcomes. They found that older workers are more likely to decrease work at both the intensive and the extensive margin. It increases the likelihood of working drops for an older worker by 3% compared to the pre-reform period. There is also evidence of an increment in "bridge jobs", i.e. part-time jobs that people choose to after retiring from a main job. An older worker is 8% more likely to switch to a part-time job than in the pre-reform period.

Garthwaite et al. (2014), unlike the others, studied the impact of public health insurance on labor supply by exploiting a large public health disenrollment. Specifically, their study targeted the case of the US state of Tennessee, which anticipated a reform of public health insurance in an inclusive manner through Tennessee's Medicaid system. In 2005, anyhow, following a reform, the state interrupted the expansion of TennCare, and about 170,000 residents lost their health insurance coverage. As a result, researchers detected an increase in job search behaviour and a sharp rise in job supply. Chiefly, they found an inflation at the extensive margin, which is quantifiable in a 2.5% increase in employment rates following the disenrollment. This rate is even higher if we take into account only childless adults, a category most affected by the reform. Moreover, they also observed a strong increment in private health insurance. The disenrollees started working to remain insured. What is more, the increase in labour supply is high and significant for those in the 40-64 age group, while it is smaller and not significant for young people. This can easily be explained by expected medical costs: average medical expenditures are strongly and positively associated with age (see Hartman et al. 2008). The researchers argued in their conclusions how the non-employers insurance options provided by the ACA would lead to a reduction in the labor supply.

Baicker et al. (2014) studied the case of Oregon. In 2008 there was a Medicaid's limited expansion for low-income, uninsured people who were selected through a lottery. The researchers evaluated the impact of Medicaid's expansion on labor supply. What they observed is that there was not a statistically significant impact of Medicaid on employment or earnings. By way of contrast, Dave et al. (2015) using data from the Current Population Survey (CPS) investigated the effect of the expansion of Medicaid eligibility for pregnant women in the late 1980s and early 1990s. According to their estimates, a 20% increase in Medicaid Eligibility during the period of analysis is associated with an 11-13% reduction in the likelihood that those who gave birth last year will be employed. In addition some researchers noticed that most of the reduction in the labor supply is associated with crowd-out, i.e. a shift from private to public insurance through shift in the labor supply. Barkowski (2020) studied early Medicaid expansions, especially for certain population groups, between the 1980s and 1990s and mainly their impact on employment outcomes. The results confirm the existence of both phenomena, job lock and job push. Principally, for male workers a 15% increase in the probability that a family member is eligible for Medicaid increases the rate of voluntary job quit by 14% over a period of 4 months.

A second stream of literature centres on the impact of Dependent Coverage Mandate (DCM) on the labour market of young Americans. Depew (2015) employed data from the American community survey (ACS) and reported a reduction in labour supply at the intensive margin, both in terms of hours worked and in terms of switching from full-time to part-time work. Along the same line, Bailey (2016), using data from the Current Population Survey (CPS), noticed that the reform had no positive effects on youth job mobility, suggesting that job lock is not a problem in the case of young adults.

Nevertheless, another possible explanation is that, unlike in the case of a reform such as those just described, or in the case of the Medicaid ACA, the dependent coverage mandate is only temporary and thus not strong enough to reduce job lock.

A third group of works focuses on the impact of the 2014 ACA expansion. Aslim et al. (2020) studied the impact of Medicaid on employment transitions of adults without dependent children. They witnessed an employment conversion from full-time to part-time employment in the expansion states after the reform. This phenomenon seems to be stronger for women, low-educated adults and those in the 45-64 age group. Moriya et al. (2016) using CPS data analyzed the impact of Medicaid expansion on full-time to part-time job switching, and they found a relatively limited evidence of an increment in part-time workers. Chiefly, they reported a modest augmentation in the number of people working 25-29 hours per week among workers with low educational attainment, and an equally modest increase among those over 60.

Be that as it may, Kaestner et al. (2017) found that the reform had a rather small impact, if compared to the substantial change in coverage. By way of contrast Gooptu et al. (2016) reported that there were no significant changes in employment or job switching or full versus part time status, and thus concluded that Medicaid had a limited impact on labor market outcomes. Similarly, Leung and Mas (2018) observed that the expansion of health care coverage through Medicaid did not have a significant effect on employment, hours worked and wages.

All in all, our reading of the literature on the labour market effects of health reforms in the US is that a consensus is yet to emerge.

2.2 Health reform and ATUS data

While many authors have looked at the impact of health reforms on coverage and labor market outcomes, only a few of them have analyzed the impact of such reforms on different outcomes as those observable on the American Time Use Survey (ATUS). The only works linking the Affordable Care Act and the ATUS are that of Colman and Dave (2018) and Lenhart et al. (2017), which studied the impact of DCM on young adults' time allocation.

Colman and Dave (2018) reported that the reform reduced labor supply of young adults, and on top of that they also analyzed what young people did with that extra time. What they found is a reduction in job lock, a reduction in average doctor's visits, an inflation in time spent socialising and an increment in time spent on educational and job search activities. Lenhart et al. (2017) in turn observed an increase in the number of people using their father's insurance, an increment in young people moving from full time to part-time work, and an augmentation in leisure time (especially watching television). That being said, they did not find an increase in time allocates to more productive use, such as investing in human capital accumulation (e.g. time spent in education).

To the best of our knowledge, no scientific paper has so far studied the impact of Medicaid and Tax credit on health outcomes, employment and time use using ATUS data.

Indeed, a CEPR report by Archambault and Baker (2018) observe that health insurance in states that have implemented Medicaid no longer depends on work, and studied the increase in voluntary part-time employment in 2014. Their estimate suggested that there were 1.1 millions more part-time workers in the post-reform period than in the pre-reform period. In addition, they analyzed time use choices associated to part time workers' extra time.

3 Institutional background

The Affordable Care Act, formally defined as the Patient Protection and Affordable Care Act and more commonly known as Obamacare, is a health care reform that was signed into law by President Barack Obama on 23 March 2010. This reform is to all intents and purposes the largest U.S. healthcare system's coverage expansion from Medicare, a reform passed in the mid-1960s of the last century.

Prior to the reform, the public health system was only available to certain groups of the population. Mainly, low-income families with children, pregnant women and the disabled were covered by public health insurance. As far as private insurance was concerned, this could have been obtained mainly through employment, although not all those in employment also had health coverage. So there was a large part of the population that did not have access to public insurance and, despite having a job, did not even have an employment-sponsored plan. These people then had to face extremely high costs to get insurance in the private individual market. The aim of the health insurance reform was to make health insurance coverage virtually universal, and to do this the purpose was to reform both the private market and the employer-sponsored plan through the introduction of premium subsidies and lower-cost private insurance plans sold on new health insurance exchanges. In addition, a necessary requirement to pursue the almost universal nature of the reform was also the expansion of public programs.

A first part of the reform that already went in this direction was the ACA's extension of the Dependent Coverage Mandate (DCM), which came into force in September 2010. This reform was related to employment-sponsored insurance (ESI) plans and impacted mainly on youth coverage. Prior to this reform, these plans covered the children of insured workers that were not enrolled in school at the age of 19, while those who were students could remain insured with their father's insurance until the age of 24. Additionally, even stricter rules were imposed depending on the marital status and whether or not young people had children. Although some states adopted laws that increased eligibility requirements for coverage (Monheit et al. 2011), these were not as comprehensive and known as ACA provisions. Thanks to DCM, insured children could continue to be covered until the age of 26, regardless of their student status, marital status or presence of children. Altough this measure came into force immediately in 2010, the other pillars of the ACA entered the scene later. In 2012, the Supreme Court decided that forcing all states to expand Medicaid was unconstitutional. In consequence of that, the choice of whether or not to expand Medicaid was left to each state, although the federal government was still responsible for the funds for any expansion.

Since 1 January 2014 the states began Medicaid expansion. The first 27 states expanded it on 1 January 2014, while others expanded in the following months and years, at different times. Many states, however, have not yet expanded Medicaid to date.

In Appendix B, Table B1 and Figure B1 summarize the expansion status and expansion dates for each State. In parallel, since 1 January 2014, all states started the expansion of the Tax credit's ACA pillar. This specific reform provided premium subsidies to those who wanted to obtain private insurance. Tax credit eligibility threshold, anyhow, is divergent in various states. Those states that expanded Medicaid also had a threshold of private insurance eligibility ranging from 138% to 400% of the Federal Poverty Line (FPL), while those that did not expand had Tax credit eligibility from 100% to 400% of the FPL. Accordingly, if the maximum income threshold to obtain Tax credit was fixed for all, the minimum threshold was different depending on whether Medicaid was present or not.

In the last column of Table B1, the minimum threshold for each individual state is reported. In addition, in the Appendix B we explain more precisely how we constructed the Medicaid and Tax credit thresholds. It is implicit that, since the FPL differs on the basis of the number of family members, this is an element to be taken into account when defining thresholds. We used data from the Kaiser Family Foundation for all information related to country expansion status, expansion date and other information on ACA features.[4] Based on this schematization of the reform, our aim is thus to study the effects of the ACA, and especially the two separate pillars: Medicaid and Tax credit.

Nonetheless, in order to create the treated and control groups, more detailed information about the individuals affected by the reform is essential, as well as a more specific view of the conditions of access in the pre-ACA period in the various states, both with regard to possible subsidies to private insurance and with regard to access to public insurance. With regard to the first point, and given the Medicaid planned to guarantee health insurance to all those below the 138% threshold of the FPL, it is very likely that children at this income level were already largely covered by both the first part of the ACA (the DCM described above) and the Children's Health Insurance Program (CHIP). As a consequence, it was decided not to take into account either children or young people up to the age of 26. Most of them were certainly covered in the pre-reform period and, in addition, without information about the dependence of individual young people on a possible private health insurance of their parents, it would have been impossible to divide young people into treated and untreated. On top of that, in many states, pre-reform laws had come into force in the pre-reform period, giving health coverage to adults with low incomes and dependent children.

Furthermore, all adults over the age of 64 were also eliminated from the analysis, as they also benefited from public assistance programs in the pre-reform period (Medicare, among others). Additionally, we eliminated adults with children from the analysis.

Basically, the group that is potentially most affected by the reform, both in the private and public sectors, is that of adults without dependent children, so called childless adults, which includes peoples between 27 and 64 years of age. This is the category that we use in our work. As regards the states more or less affected by the reform, it is necessary to assess the situation before 2014. At that time Medicaid programs generally provided coverage only to disabled people, elderly people, young people and children or families with children (and pregnant women). There were exceptions to these general rules, but individual state reforms that provided coverage to categories of people other than those listed above were difficult to pass, as

[4] The majority of studies cited in the literature use information from the Kaiser Family Foundation. Among others, see Sommers et al. (2012) and Wherry et al. (2016).

these had to be budget neutral for the federal government. In consequence of that, these programs were necessarily limited and did not involve large numbers of people. Thus, the 2014 reforms largely provided coverage for people who did not have coverage before, and also relaxed the eligibility conditions for some people who were already eligible before the reform.

4 Data and Descriptive Statistics

4.1 Data

To carry out our research, we exploited data from the American Time Use Survey (ATUS). This survey is sponsored by the Bureau of Labor Statistics and conducted by the U.S. Census Bureau. It is the first American federally administered and continuous survey on time use in the United States. The ATUS sample is randomly drawn from the CPS and covers a representative sample of all residents living in the U.S. who are at least 15 years of age, outside of those who perform military activities, those who are in nursing homes and those who are in prison. The main objective of the survey is to measure accurately and precisely how people use their time between distinct activities during the day. The individuals who complete the diary with the daily activities in the ATUS survey are randomly selected from a subset of households that have completed all the interviews in the Current Population Survey (CPS). The selected individuals are interviewed only once. About half of the households that are asked to participate in the ATUS decide to accept and then answer the questions. The ATUS respondent rate in the period 2012 - 2015 varies between 53.2% and 48.5

In the ATUS survey, people with young children, African-Americans and Hispanics are oversampled and thus we use Bureau of Labor Statistics (BLS) provided ATUS sample weights throughout our analysis. As far as sample size is concerned, around 10,000 household per year are interviewed. As far as the month of the interview is concerned, the monthly sample is divided into four randomly selected panels, i.e. one for each week. In order to provide reliable measurements of the time spent on the various activities both on weekdays and weekends, 10% of the sample is allocated on each day of the week from Monday to Friday. The remaining 50% of the sample is allocated as follows: 25% on Saturday and 25% on Sunday. The data is designed to be representative at both State and Country level.

The diaries are compiled by computer-assisted telephone interviewers and coded by BLS. In addition, when necessary BLS interviewers used conversation techniques that have been proven to reduce recall bias in laboratory settings (Schober and Conrad 1997). ATUS participants filled in the activity diary for the day before. This choice was made in light of the fact that retrospective reports about the use of time, as pointed out by Robinson (1985), are affected by recall bias and internal inconsistencies. In the diary all the activities are reported in a schematic and precise way, expressed in minutes, from 4 a.m. of the previous day until 4 a.m. of the diary day. Each activity is classified following a lexicon coding system with 17 first level categories, each of which has two further levels of detail. For simplicity, to each activity is assigned a six-digit code. The first two digits represent the main category (first level), the next two digits represent the second level category and the last two digits represent the third level, the most detailed. As an example, the ATUS code for *"Work, main job"* is 050101, which is part of the 0501 category, i.e. *"Working"*, which in turn is part of the 05 *"Work & Work-related activities"* category. Starting from these measures and classifications, we constructed a set of variables with the aim to obtaining as complete a measure as possible of the time that the respondents have spent carrying out the different subset of activities.

As far as the job is concerned, we created a variable that takes into account all the minutes that each specific person on that day dedicated to his/her job, to activities related to his/her job (such as socializing, relaxing and leisure as part of job) and to other income generating activities. What is more, we created dichotomous variables depending on whether the respondent declared to be employed or not, and whether he/she declared to have worked in his/her main job.[5] In this way we have two specific and distinct indicators available to examine the time spent at work, both at the intensive and the extensive margin. As far as the variables related to the *"Work & Work-related activities"* category is concerned, we likewise considered the variable related to *"Job search and interviewing"*. Be that as it may, few individuals answered this question that we thus did not investigate further. Besides this category, we constructed variables related to health care utilization, such as a measure relating to receiving or waiting for medical care services, both at the intensive and extensive margin. The other measures that we employed and that were built with the same method, i.e. aggregating separate classes of activities, are related to other outcomes such as personal activities, volunteer activities, household activities, household management, leisure and time spent doing sport. The categories used in relation to *"Work & Work-related activities"* were created taking into account the categories *"Market work"* and *"Other income generating activities"* used by Aguiar et al. (2013).

Similarly, following their approach, the *"Job search"* category was kept as a separate category. The same approach was also exploited for the creation of the aggregates relating to the other categories. On the other hand, for the health care and medical care category, as well as for the construction of other less aggregated categories, we largely followed Colman and Dave (2018).

For a more complete description of ATUS, see Hamermesh et al. (2005) or read the ATUS User's guide available on the Bureau of Labor statistics website (latest edition,

[5]This variable has a similar meaning compared to the aforementioned dummy.

updated 2019).[6]

In the various models we also employed a set of controls using CPS variables that account for respondent's socio-demographic characteristics such as age, education, marital status, race and ethnicity, gender, qualification, cohabitant status, metropolitan/urban area of residence, income thresholds and a dummy that controls for the fact that answers were made during the week or in the weekend.

On top of that, we matched unemployment rate data from the Bureau of Labor Statistics, which change on a monthly basis. In this way, to each individual corresponds the value of unemployment in the state in which he/she resides for that specifics month and year. In the survey there is also a variable related to whether the questionnaire is incomplete or not. The number of people with incomplete questionnaires is extremely low. For this reason the analyses were carried out considering these observations.[7]

4.2 Descriptive Statistics

Table 1 shows mean and standard deviation of the variables used in the main analysis, which are related to labour market as well as medical and health outcomes, but also to leisure, sport, household activities and others. The number of people considered in our sample is 10,494. These people are all aged between 27 and 64. They are all adults, either single or married or engaged, but without children. In the previous lines we have already explained the reason for this decision. More specifically, the sample was divided according to eligibility criteria. In column (1) there are the descriptive statistics related to those individuals who do not qualify for either Tax credit or Medicaid. Column (2) shows the data related to those who qualify for Medicaid and column (3) shows the data related to those who qualify for Tax credit, i.e. premium susbidies. For simplicity, in this section we will define "eligible Medicaid" and "eligible Tax credit" as those who could potentially benefit from the reforms. In other words, they are those who in both the pre-reform and post-reform period are eligible for the ACA expansions (according to the rules that came into force in 2014). The data on the use of time differ significantly across these various groups. Principally, with regard to the "work excluding job search" variable, those who are not treated work significantly more than the Tax credit eligible group, which in turn work significantly more than those who are eligible Medicaid. If we take as a reference the daily working hours declared only among those who work (i.e., who have declared to work at least a few minutes in their main occupation), we notice that the untreated on average declare to work about 8 hours and 42 minutes per day (522.18

[6] Furthermore, a detailed classification of the variables and the composition of the different aggregates used in our study is provided in the Appendix B.

[7] However, in a robustness check we repeated the analysis without considering the incomplete questionnaires and the results remain unchanged (see the Appendix B).

minutes), the Tax credit eligible 8 hours and 34 minutes (514.19 minutes), while the Medicaid eligible 7 hours and 54 minutes (474.77 minutes). Although there is a difference between the various groups, the gap is smaller than the "work excluding job search" overall variable that is not calibrated to the intensive margin. This is due to the fact that there are fewer unemployed people in the not eligible group than in the other two groups. And the unemployed are precisely the ones who drastically reduce average hours worked. Besides, those who are Medicaid eligible spend more time than anyone else looking for a job, followed by Tax credits eligible and finally the ineligibles. As far as the eligible group is concerned, the time related to "Medical care services" is higher for the Medicaid eligible, followed by Tax credit eligible and finally by the not eligibles. This seems to be in line with the literature on Emergency Department (ED) visits, according to which people without health insurance at the intensive margin spend more time in medical care than those with coverage. They wait until they are sick and do not implement preventive behaviour. In fact, they do not even have an attending physician to turn to. By definition, ED visits last longer than a medical examination or prevention.

Also with regard to the declared hours of sleep, those who sleep the most are Medicaid eligible, followed by Tax credit eligible and those who sleep the least are the ineligibles. The time spent in household activities is similar among the various groups, while the leisure time is much higher among those who are in the Medicaid eligible group. With regard to sport, the untreated do more activities than the others even though the time spent doing sport is similar. As far as socio-demographic characteristics are concerned, the three groups are similar.

With respect to the ethnicity, the percentage of Hispanics and black people is higher among the Medicaid eligibles. Specifically, in this group, foreigners are more than twice. By contrast, anyhow, more White Americans are in the non-eligible group than in the other two groups. As far as education levels are concerned, the difference is exceedingly high. 62% of not eligibles people finished college, compared to 26% of Medicaid eligibles. In turn, those who did not attend high school are 1% among the ineligibles and 22% among the Medicaid eligibles. As far as marital status is concerned, there are also big differences: there are more divorces and fewer married people between Medicaid eligibles and Tax credit group.

Table 1: Sample Statistics ATUS 2012 - 2015

Variable	**Not Eligible**	**Eligible Medicaid**	**Eligible Tax Credit**
Work excluding job search (in minutes)	321.29	166.13	274.89
	(295.20)	(253.08)	(287.49)
Work time among workers (in minutes)	522.18	474.77	514.19
	(192.84)	(193.84)	(179.65)
Job search and interviewing (in minutes)	1.79	5.07	3.39
	(23.08)	(33.49)	(30.36)
Medical care services (in minutes)	7.02	13.25	7.87
	(48.20)	(64.31)	(50.94)
Medical care services	88.78	87.64	85.52
if medical care services >0 (in minutes)	(148.88)	(144.61)	(146.95)
Sleep (in minutes)	493.87	537.56	512.59
	(114.35)	(147.70)	(132.74)
Voluntary activities and caring for other people (in minutes)	17.29	19.55	18.12
	(64.23)	(72.11)	(63.69)
Household activities, main (in minutes)	63.82	69.76	66.93
	(98.22)	(99.63)	(103.66)
Household management (in minutes)	12.15	8.83	10.07
	(37.76)	(33.43)	(36.82)
Leisure (in minutes)	248.83	355.11	282.49
	(182.72)	(238.98)	(201.94)
Sport/Exercise (in minutes)	21.58	13.04	14.89
	(55.76)	(44.74)	(52.01)
Age	47.73	48.14	47.56
	(11.97)	(11.86)	(12.26)
Family income group	13.87	3.62	9.72
	(0.93)	(1.73)	(1.79)
Woman (%)	0.48	0.50	0.49
	(0.50)	(0.50)	(0.50)
Metropolitan area (%)	0.87	0.79	0.81
	(0.34)	(0.40)	(0.39)
Hispanic (%)	0.06	0.17	0.11
	(0.25)	(0.38)	(0.31)
Black (%)	0.08	0.24	0.15
	(0.27)	(0.43)	(0.35)
White (%)	0.87	0.70	0.80
	(0.34)	(0.46)	(0.40)
Less than High school (%)	0.01	0.22	0.07
	(0.12)	(0.42)	(0.26)
High school (%)	0.22	0.37	0.37
	(0.41)	(0.48)	(0.48)
College drop (%)	0.15	0.16	0.18
	(0.36)	(0.36)	(0.38)
College (%)	0.62	0.26	0.37
	(0.49)	(0.44)	(0.48)
Divorced or separated (%)	0.15	0.29	0.22
	(0.35)	(0.45)	(0.42)
Married (%)	0.58	0.24	0.41
	(0.49)	(0.43)	(0.49)
Cohabitant (%)	0.10	0.13	0.09
	(0.29)	(0.33)	(0.29)
Observations	4,427	1,426	4,641

Notes: The ATUS sample refers to non-institutionalized civilians, ages 27 to 64, without children. In the computation of the descriptive statistics the sample weights are used. All the data reported in the table are expressed in minutes, except for the values related to the variable dummies that can only assume values 0 and 1. The minutes refer to the single day in which the interview took place. The number of observations reported refers to the maximum sample size. For some variables the sample is reduced due to missing information.

5 Conceptual framework

5.1 The Medicaid impact on labour supply

It is useful to discuss what the effects of ACA expansion might be, at least a priori. First of all, it is likely that the direct effects are mainly on insurance and employment outcomes. Employment and health insurance in the United States have always been closely linked because of the tax deductibility of employer-provided health insurance benefits and the absence of a universal and public health care system.
The optimisation of the labour-leisure trade-off predicts a reduction in labour supply associated with an expansive coverage reform. People, in fact, by obtaining coverage (or subsidies for coverage) would experience an increment in non-labor income, that would leads to a pure income effect. And in turn to a reduction in working time and an increase in leisure time. Nevertheless, to assess these impacts, it is necessary to disentangle the effects of the two major ACA's pillars: Medicaid expansion on the one hand and Tax credit expansion on the other.
Indeed, these effects, at least theoretically, may be rather different. As far as Medicaid is concerned, the latter might have impacted on the labour market through several channels. And there are also separate choices that people could make as a result. Health insurance in the pre-reform period was essentially provided by the employer and was only for those who had an open-ended contract. As we have already seen, public insurance covered specific categories of people, but these categories are not part of our analysis. Focusing on our group, i.e. childless adults between 27 and 64 years old, we try to identify any consequences of the reform. First of all, having public health insurance available can have a strong impact on the labour market if some people in the pre-reform period worked only in order to have access to health insurance, which otherwise they could not afford. This phenomenon is called "employment lock" in the literature. So what we expect, given the peculiarities of the reform, is a reduction of the employment lock for the treated sample.
Secondly, the reform could also have a negative impact on the "job-lock" (see Gruber 2000). With the term job-lock a large body of literature refers to the role of employer-provided health insurance in reducing job mobility. A reduction of this phenomenon could substantially lead to two consequences. A first possible consequence is that the reform could allow people to avoid to keep doing a job that is not suited for them just because of the maintenance of health insurance. Thus, the fact that health insurance is available outside the workplace means that workers might change from jobs that provide private insurance to employments that do not, but are more in line with their skills. In this respect the impact of the reform could then be positive, as it would lead to an augmentation in allocative efficiency. A second possible consequence could be related to the problem of "hours mismatch". That is, the unavailability of public health care might have distorted the optimal work/leisure trade-off. For example,

some individuals might have been working full time in the pre-reform period instead of part time in order to obtain health insurance through the employer (employer-provided health care insurance). Since health insurance in the post reform period is no longer dependent on work and thus an augmentation in part time rather than full time might be expected. Even in the unlikely case (given the high cost of insurance) in which someone paid for the coverage in the pre-reform period, in the post reform period out of pocket medical expenditures are cancelled. Nobody in the low income bracket has to pay for health insurance anymore. So, even in this incident, individuals may decide to work less for any given consumption profile.
Preliminary and descriptive data elaborated by Archambauld et al. (2018) show an increment in voluntary part-time employment in 2014. Voluntary part time workers are defined as those workers who choose to work fewer hours than full time workers by choice, unlike those who work part time only because it is the only job they have managed to get. Archambauld et al.(2018) estimated that in the post-reform period there were one million one hundred thousand more part-time workers than in the pre-reform period. In addition, there has also been a reduction in those who report working part time for economic reasons. This phenomenon moves in the opposite direction to what had happened during the great recession, when the involuntary part time rate had doubled in two years (2007 - 2009).
In the light of the data available to us, we can study whether there has actually been a reduction in working hours for treated Medicaid individuals compared to others. And we can verify this at both intensive and extensive margin. The only available outcome to investigate this issue is the "Job search activity". On the one hand, the reform could increase job search for beneficiaries, covered by public health insurance, that are looking for a job closer to their interests. On the other hand, those unemployed may decide not to look for a job, and those who already work part-time may decide not to look for a more remunerative one given the income effect brought about by the reform. So, the impact of the reform is ambiguous and depends on which of these effects prevails.

5.2 The Tax credit impact on labor supply

For those who are eligible for Tax credit premium subsidies, basically the effect we expect is different depending on the state of residence. Those who live in a state that has expanded Medicaid, have an incentive to work less since they are still covered by public insurance. In spite of that, they also have an incentive to continue working in order to maintain a health insurance better quality than the public one. For those living in states where Medicaid has not been expanded, nonetheless, according to Leung and Mas (2018), an increase in labour supply is expected, as they may wish to obtain or maintain Premium Subsidies. The alternative is to have to pay your own health insurance or not being insured tout-court.

5.3 Medicaid and Tax credit impact on medical and health care

It is less immediate, still, to figure out the impact of the cited reforms on the time spent receiving and waiting care. On the one hand, there may be a scale effect due to the expansion of coverage to uninsured individuals that generates an increase of the demand for medical care. On the other hand, there is a potential substitution effect between non-scheduled ED-based care to scheduled and routine visits to the doctor, or physician office-based care. As mentioned by Colman and Dave (2018), and according to the National Ambulatory Medical Care Survey (2011 and 2012 NAMCS, produced by the CDC) the median time spent in visits to the ER was 120 minutes, while the median time for visits with general practitioners was 17 minutes. Visits with a specialist doctor vary a lot in terms of duration in relation to the doctor's specialty, but in any case they also last much less than the time a person spends in ER. So this would make us think of a reduction in the time spent receiving treatment once coverage is obtained. Another potential impact is a reduction in preventive care once insured. This is a consequence of what is called "moral hazard" (see Ehrlick and Becker 1972).
If we imagine that both Medicaid and Tax credit lead to an expansion of coverage, the effect we could expect on the time spent receiving medical treatment could be both positive and negative. In consequence of that, it depends on which of the mechanisms we analyzed turns out to be stronger.

5.4 Medicaid and Tax credit impact on other outcomes

Besides analyzing the potential effects of both reforms on labor supply and coverage, it might also be interesting to analyze the second-order effects of the reforms on non-labor time use. Specifically, the reduction in labor supply brought about by Medicaid raises the question: what do people do with their extra time? What we expect is an increment in activities that are time-intensive. It is thus interesting to see the consequences on voluntary and household activities, leisure, as well as sport. Expected impact is positive for all the aforementioned variables, except Sport. As a matter of fact, there could be an ex ante moral hazard effect. In other words, obtaining insurance may induce individuals to take more health risks, as having coverage reduces the financial loss associated with illness. This effect could increase risky behaviours and, as we have seen, reduce investment in preventive medicine (Ehrlick and Becker 1972).

Be as it may, it seems to be quite a powerful disincentive the fact that being in good health is a value in itself, regardless of the fact that the individual is no longer responsible for any financial shock due to illness. The income effect of obtaining free or subsidized coverage can impact on the behaviour of newly insured persons in different ways. For instance, people may choose to spend a certain amount of money they had allocated to health coverage on junk food, cigarettes and alcohol or, vice versa, to buy a pool or gym subscription, and to buy organic food instead of buying at a discount store. The effect on sports activity, at least theoretically, appears to comply with the same rules also for those who benefit from Tax credit premium subsidies. Overall, Tax credit reform, as we have already seen, leads to an inflation in the labour supply of eligible individuals, so what we expect is a reduction in time spent doing household and leisure activities.

6 Empirical strategy

The objective of our study is to assess the impact of two pillars of Obamacare on various outcomes related to the labour supply and time use.

We identify their effects by means of a triple D-i-D specification similar to that of Frean et al.(2017). As Medicaid expansion and Tax premium subsidies identification frameworks are extremely different and target individuals with distinct levels of income below and around the poverty line, we enter separate terms of each of the two policies in our triple D-i-D identification framework:

$$\begin{aligned} Y_{ijt} &= A + \alpha Poverty_i + \beta StateMedicaid_{jt} + \gamma Year + \zeta Month + \eta States_j + \\ &+ \tau Poverty_i * States_j + \varrho Year * States_j + \delta Poverty_i * Post14 + \theta Poverty_i * \\ &* Post15 + \eta Premium + \lambda Premium * Post14 + \Lambda Premium * Post15 + \\ &+ mX_{ijt} + \rho U_{jt} + v \end{aligned} \tag{1}$$

Subscript i indexes the individual, j indicates the state of residence of the respondent, while t indicates the year of the survey, which ranges from 2012 to 2015. We only observe each individual once and individuals are surveyed continuously every day, month and year.[8] Consequently, Y_{ijt} refers to time-use outcomes for the individual i resident in state j at time t. The threshold variable *Poverty* is a dummy that assumes value 1 if the individual i is potentially eligible for Medicaid based on the ATUS income threshold of each individual being below the given Federal Poverty Line (FPL) threshold for Medicaid eligibility.[9] Likewise, *StateMedicaid* is a dummy that takes value 1 starting from the date on which a given state has expanded Medicaid, and 0 otherwise. *Year*, *Month* and *States* represent fixed effects for respectively

[8]For further details, see Section "Data".

[9]For further details, see Section "Institutional Background".

year, month and state. These are intended to capture time use and labour supply trends independent of the changes introduced by ACA, such as yearly or seasonal variations, and unobserved time-invariant state-specific factors or possible labour market shocks. *Poverty*States* are interaction variables between Medicaid income eligibility and state fixed effects. *Year*States* are interaction variables between years and states, which account for any state specific linear trends. The coefficients of interest that capture the effects of Medicaid expansions are, respectively, those on the variables *Poverty*Post14* and *Poverty*Post15*, that refer to individuals eligible to Medicaid in 2014 and 2015. Like in Frean et al. (2017), we allow for a differential impact in 2014 and 2015. Thus, the Medicaid treatment group includes childless adults who meet the income eligibility criteria for Medicaid and live in a state that implemented Medicaid by the time of their ATUS interviews. In contrast, the Medicaid control group includes childless adults either with income above the Medicaid eligibility threshold or that reside in states that had not (yet) implemented Medicaid expansion, whatever their income.

In contrast to other studies on the subject that assumed that those living in expansionary states in the months after January 2014 are considered treated from the outset, we involve individuals treated on the basis of the actual entry into force of the reform in that specific state, thanks to the continuous daily nature of ATUS.

Coming next to the Tax premium credits pillar of ACA, the variable *Premium* is a dummy that taking the value 1 if the individual i is potentially eligible for Tax premium credit, based on their ATUS income threshold being below the Federal Poverty Line (FPL) threshold for eligibility to Tax premium credits, and zero otherwise. Chiefly, the FPL threshold for eligibility varies between childless singles and childless couples, and also across states that did or did not (yet) implement the Medicaid expansion, and this variation helps us identify the effects at stake that are picked by the coefficients on the variables *Premium*Post14* and *Premium*Post15*, respectively, which inform about eligibility to Tax premium subsidies in 2014 and 2015. Because there were additional ACA features, such as, for example, enforcement penalties that varied across these two years, we follow Frean et al. (2017) and allow the effects of the two pillars to vary in 2014 and 2015.[10] The vector X_{ijt} includes socio-demographic controls related to age, gender, educational qualification, marital status, residence in a rural or urban area, ethnicity and income categories. It also includes a weekend dummy to account for whether the ATUS interview took place during a weekend day, as daily activities vary dramatically at weekends versus week days. What is more, the model also contains controls for monthly state unemployment levels (U_{jt}), collected from the Bureau of Labor Statistics, and that we merged to the ATUS, based on the interview month. The errors v are assumed to be normally

[10] Interactions between the dummy of Tax credit eligibility and the dummy for treated states are not included as they are obviously collinear with the variables *Premium*post14* and *Premium*post15*.

distributed, and we use robust standard errors clustered at a state level (to account for the possibility of non-independence of observations within the same state).
As we have seen in the Data section, ATUS overestimates weekends and also some demographic groups. Although we include controls for demographic characteristics and for weekend diaries, we also employed sample weights throughout the analysis.[11] The models are estimated using OLS.

7 Empirical results

Table 2 shows the estimates of our main specification (as specified in Equation 1 of Section 6) for the labour market outcomes of conditional and unconditional hours worked[12] (intensive margins) or employment (extensive margin). The sample analyzed includes childless adults, either single or partnered, in the age range from 27 to 64 years, in order to exclude from the analysis those who had already been beneficiaries of health coverage (for example, through DCM for young people aged less than 26, or Medicare for the elderly).[13]
In all specifications reported in Table 2, we control for state, year and month fixed effects. In addition, all regressions include a set of controls and the standard errors are clustered at the state level. Principally, we control for age, education, marital status, race and ethnicity, gender, qualification, marital status, cohabitant status, urban/rural area of residence, income thresholds, a dummy that controls for the fact that answers have been made during the week or in the weekend and the monthly state unemployment rate. In all specifications, the sampling weights attributed to individuals by the BLS are applied.
In column (1) of Table 2, we report estimates of our baseline model where the dependent variable is the number of working hours.[14] Remarkably, we find opposite effects of the two ACA pillars on the hours worked, with a reduction for those individuals eligible to Medicaid and an increase for Premium Tax credits potential beneficiaries. From the results in column (1), the negative effect on working hours for Medicaid eligibles is larger and statistically significant (at the ten percent level) only in 2015. This may be due to the fact that by 2015 more states implemented the expansion of Medicaid. On the contrary, working hours of potential recipients of Premium Tax credit increase significantly (at the ten percent level) in 2014 but the estimated coefficient becomes negative and not significant in 2015. As we mentioned before, all

[11]The dataset also includes a question whether or not the questionnaire is incomplete. In the main specifications, we considered all individuals. As an additional robustness check, we excluded from the estimation sample individuals with an incomplete questionnaire, which were only few observations, and indeed the estimation results including or excluding them are essentially the same.
[12]For more information, see Section "Data".
[13]For more information regarding previous reforms and the institutional background, see the Related Literature and Institutional Background sections.
[14]For further details and the variable definition, see Section "Data".

states implemented the Premium Tax credit pillar as from January 2014 while the Medicaid expansion was staggered in time between 2014 and 2015 and not all states implemented it.[15] This could explain why the Premium pillar causes an increment in hours worked in 2014, while Medicaid expansion reduces hours only in 2015. In terms of size of these effects, Medicaid expansion reduces hours by 45 minutes per day, while Premium Tax credit increases them by 30 minutes. These are very large effects.

When restricting the sample to individuals with positive hours of work on the diary day (see column (2) of Table 2), the Medicaid expansion effects become about twice as large and significant in both policy years, while the Tax Premium impact fades away. In fact, the overall hours responses (see column (1)) are driven by responses at the intensive margin (see column (2)) for Medicaid eligible, and at extensive margins (see column (3)) for Premium Tax credit potential beneficiaries. The results in Column 3 indicate that employment did not vary for people eligible to Medicaid but it increased for potential beneficiaries of Premium Tax credits by about 7 percentage points. These results are in line with the conceptual framework outlined earlier on. On the one hand, access to Medicaid can be seen as a positive income shock, which then leads to a drop in hours worked. On the other hand, to receive Premium Tax credits which are tied to purchasing (higher quality) private health insurance, potential beneficiaries enter employment.

To better understand what lies beyond these estimates, it is relevant to investigate further heterogeneity of responses. And that's what we're going to do, after several robustness checks, in the next Sections.

[15] Remember that eligibility to Medicaid in 2014 concerns residents of states that began to be treated in 2014 while 2015 Medicaid-eligibility interests residents of states that became eligible in 2015, as well as those living in states that started to be treated in 2014 and continued so in 2015.

Table 2: Baseline results

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost14	-10.3784	-99.2441**	0.0412
	(39.7109)	(43.0216)	(0.0707)
Povertypost15	-44.7430*	-77.2080*	-0.0412
	(25.6152)	(41.7552)	(0.0441)
Premiumpost14	32.0101*	-3.4873	0.0712**
	(16.9059)	(14.4326)	(0.0317)
Premiumpost15	-6.7497	-10.4471	0.0019
	(17.3339)	(19.7352)	(0.0246)
Observations	10,417	4,805	10,417
R-squared	0.2658	0.1821	0.2293
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

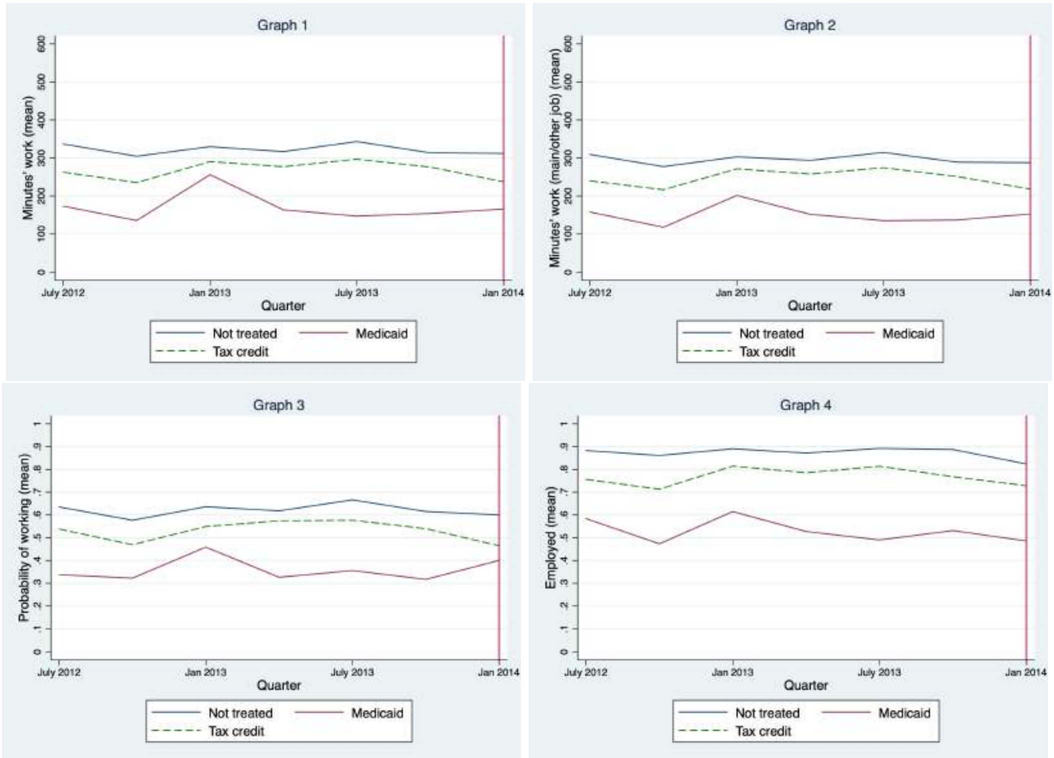
7.1 Parallel trends, placebo and other robustness analysis

The validity of our triple D-i-D analysis rests on the assumption that, in the absence of treatment, treated individuals (whether treated by Medicaid expansions or Tax premiums credits) would have followed the same trend as those in the control group. While we cannot directly test this assumption, we graphically inspect whether the outcomes followed distinct trends for the treated and the control group in the period before the reforms. Figure 1 plots the outcomes in the pre-reform period for people eligible to Medicaid or Tax premium credits of individuals with household income above eligibility thresholds and/or residents of states that did not (yet) implement these reforms (the untreated group). More precisely, in Figure 1 we show the quarterly trends for separate outcome variables: hours worked in the main job (graph 1 of Figure 1); hours worked in the main job as well as other jobs (graph 2 of Figure 1); the probability to work a positive number of hours in the day of the ATUS diary interview (graph 3 of Figure 1); and the employment probability according to the CPS questionnaire (graph 4 of Figure 1).[16]

[16]In order to obtain this information, we linked CPS information to the ATUS dataset.

While some of the ups and downs patterns in hours worked may be due to the post 2008 crisis, which hit especially the most vulnerable segment of the U.S. labour market, overall the graphs suggests that the parallel trend assumption in the pre-reform periods holds through.

Figure 1: Parallel trends



Notes: Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale as usual. The sample weights are applied.

Next, we run a placebo (see also Slusky 2015) and estimating the model only for the pre-reform period, but arbitrarily assuming that both Medicaid and Premium Tax credit treatment took place in 2013. Since the two reforms only came into force as from 2014, we should find no effect of these placebo reforms. Indeed, we find no significant effect of the pseudo-reforms (see the estimates in Table 3).

Table 3: Arbitrarily shift in the year of implementation of the reform

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost13	-12.1107	18.7159	-0.0278
	(30.8558)	(31.3181)	(0.0555)
Premiumpost13	9.1952	2.7904	0.0099
	(18.7833)	(15.8805)	(0.0323)
Observations	10,417	4,805	10,417
R-squared	0.2656	0.1826	0.2291
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Third, we investigate the existence of possible leads, that is, anticipatory effects with respect to the reform by including additional interaction terms between the income thresholds for the eligibility to, respectively, the Medicaid expansion and the Premium Tax credits and the pre-reform period 2013. The results of estimation remain substantially the same and the coefficients relating to the interaction terms for 2013 are not statistically significant, showing no anticipatory effects (see Table 4).

Table 4: Leads

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost14	-18.7518	-104.6176**	0.0245
	(37.1461)	(43.8658)	(0.0711)
Povertypost15	-52.8502**	-83.3138*	-0.0574
	(27.9514)	(47.8445)	(0.0453)
Povertypost13	-20.4124	-14.2094	-0.0401
	(27.9598)	(41.3768)	(0.0464)
Premiumpost14	38.4201**	-0.8661	0.0773***
	(16.5819)	(18.1221)	(0.0274)
Premiumpost15	-0.2791	-7.8006	0.0081
	(19.0121)	(24.2010)	(0.0267)
Premiumpost13	13.6142	5.5720	0.0135
	(19.4097)	(18.7316)	(0.0359)
Observations	10,417	4,805	10,417
R-squared	0.2659	0.1821	0.2294
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

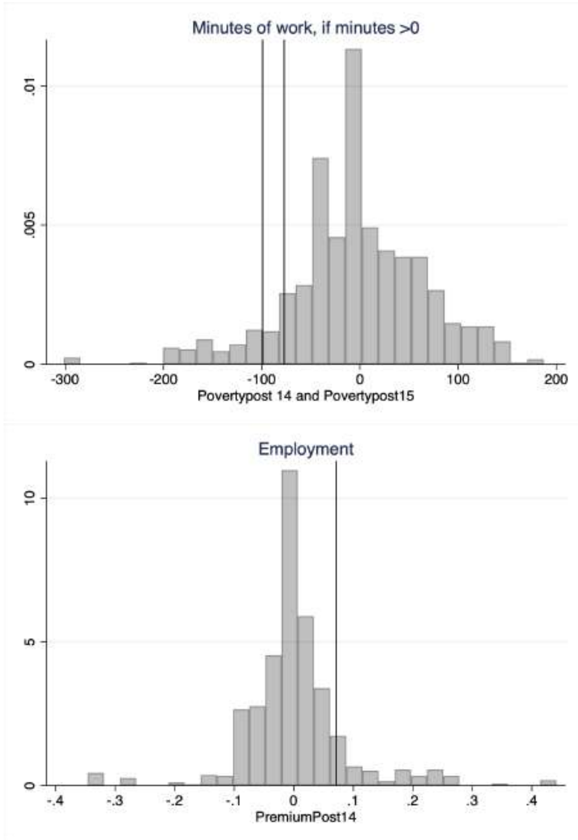
Notes: The model estimated by OLS is specified in Equation 1 and augmented with the interaction variables for 2013. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Correspondingly, we randomly distribute the eligibility condition of both reforms across individuals and states, but holding fixed the same number of people in the treated and control groups as in our main specification and we replicated this random allocation 1000 times.

The first graph reported in Figure 2 shows different estimates of the *Povertypost14* and *Povertypost15* coefficients obtained by applying various random allocations for the intensive margin specification reported in Table 2, column (2). The second graph, instead, shows the same results for the extensive margin specification, reported in Table 2, column (3) (*PremiumPost14* coefficient). In both figures the average of the estimated coefficients is centered at zero, and this shows that the relationship is not mechanical and automatic.

However, the states are few and then in the various simulations it can happen that by chance some combinations are extremely similar to the true one, making to fall back the true effect (reported in correspondence of the black line) inside the range.

Figure 2: Random allocation of the Medicaid and Tax Premium eligibility conditions



Notes: Each estimate shown in both graphs of Figure 2 is made by taking our main equation as the base equation. In the first graph the dependent variable is the variable related to the "Minutes of work", while in the other graph the dependent variable is the dummy related to the employment condition. The variables *Povertypost14* and *Povertypost15* are given by the interaction of the Poverty variable and the dummy that takes into account whether or not the state is treated by Medicaid in the year and month of the interview. The variable *Premiumpost14* is given by the interaction of the Premium variable and the dummy that takes into account whether or not the state is treated by the Tax credit reform in the year and month of the interview. In each model, the Medicaid and Tax credit treatment variables in month year were randomly assigned, holding the same percentage of treated and untreated states relative to the true treatment variable. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Each estimate shown in the figures is made by taking our main equation as the base equation. The y-axis shows the probability density function of the estimated coefficients. The black vertical line is placed in correspondence of the "true" estimated value of the coefficients, reported respectively in Table 2, column (2) (*Povertypost14*=-99.2441, *Povertypost15*=-77.2080) and column (3) (*Premiumpost14*=0.0712).

Furthermore, to check the robustness of our estimates, we remove from the estimation sample one state at a time and overall results are confirmed.[17]
Next, we arbitrarily change the threshold for being considered as worker in the intensive margin specification, either by changing the minimum hours worked on the diary day and being employed according to the CPS information (see Appendix B Table B7). These estimates suggest that regardless the rule and threshold we use to study the work dynamics at the intensive margin, the results remain stable. Over and above that, we took a distinct approach to defining treated and untreated states,

[17]Figure B2 in Appendix B shows the estimates of the coefficients of interest in the various specifications.

following both Leung and Mas (2018) and Kaestner et al. (2017) (see Appendix B Tables B3 and B4). Again, results remain stable.[18]

Also, we exclude from the control group individuals with income above $ 100,000 or, on the contrary, we include in the control group also people who earn more than $ 150,000 per year, which are not included in our main estimation sample, as presumably too divergent from the treated (see Appendix B Table B6). Additionally, we replicated the main results by dropping from the sample respondents with an incomplete questionnaire(see Appendix B Table B5). In all these cases, the estimation results remain unchanged and our conclusion hold through.

7.2 Heterogeneous effects

We believe it is important to investigate the existence of possible heterogeneous effects, in order to better qualify results obtained on the full sample. First of all, we check whether our findings on Medicaid impact can be explained by a reduction in overtime or second jobs or by a switches from full-time to part-time work schedules.

In order to investigate whether the reduction in hours is concentrated among individuals that held more than one job, we split the sample between those who declared to have a second (and possibly also a third) job and those who declared to have only one.[19] The results of estimation are reported in column (1) of Table 5, and show that removing those who have more than one job from the analysis reduces the magnitude of the estimates. As a consequence, part of the reduction of the labor supply may be due to individuals reducing hours worked in second and third jobs. Nonetheless, the significant drop in hours holds also for individuals that have only one job. Considering this fact, we check whether Medicaid eligibles may have switched from full-time to part-time work, by considering part-time work versus full-time work as an additional outcome.[20] We find that Medicaid eligibility reduces full time work and increases part-time work by 15 percentage points.

This result is consistent with the reduction in hours mismatch. In fact, someone who in the pre-reform period preferred to have a part time over a full time work, but who worked full time for the only purpose of obtaining health insurance, is likely to switch to part time work in the post reform period. This could be the mechanism driving our findings, also consistent with the findings of Archambault et al. (2018), who report an increment in voluntary part-time employment for the Medicaid eligibles in the post-reform period.

As for the impact on people eligible for Tax premium credits, the two positive coefficients of *Premiumpost14* and *Premiumpost15* of column (2) suggest an increase in

[18]For more details about the difference in the approaches, see Appendix B.

[19]Given our research design, the group of those that had two or more jobs is too small to be considered in its own.

[20]We create a dummy that has zero value if the individual has a part time job (according to CPS) and value 1 if the individual has a full time job.

full time work. Still and all, none of these are statistically significant.

In columns (3) and (4), we further split the sample between individuals aged above and below 50 years, respectively, to tentatively conclude that Medicaid and Tax premium eligibility impacted especially older workers labour supply. Also with regard to Tax credit over-50 eligible, the reform significantly affects the choice of full vs part time employment. In great detail, the rise in part-time work among over-50 Medicaid eligibles is more marked than in the full sample. Conversely, Premium Tax credit eligibility increased full-time work by about 8 percentage points for both years 2014 and 2015.[21]

[21]The sample used for the analysis in columns (2), (3) and (4) includes all individuals, regardless of whether they hold a second job or more jobs. However, we also rerun the analysis dropping those who have a second job and the results are essentially the same.

Table 5: Heterogeneous Effects (I)

Sample	**Dropping second job holders**	**Full sample**	**Sample aged under 50**	**Sample aged 50 and above**
Outcomes	Hours worked if hours >0 (1)	Full-time workers (2)	Full-time workers (3)	Full-time workers (4)
Povertypost14	-71.0738	-0.0261	0.0270	-0.0302
	(47.2789)	(0.0653)	(0.1099)	(0.1208)
Povertypost15	-79.7089*	-0.1501**	-0.0836	-0.1681*
	(42.1429)	(0.0594)	(0.0890)	(0.0932)
Premiumpost14	3.7123	0.0277	-0.0030	0.0810**
	(14.8736)	(0.0220)	(0.0340)	(0.0322)
Premiumpost15	-13.8686	0.0265	-0.0077	0.0802***
	(18.7240)	(0.0211)	(0.0392)	(0.0275)
Observations	4,241	8,170	4,156	4,014
R-squared	0.1898	0.1379	0.1841	0.1899
Years	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state
Controls	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1 and augmented with the interaction variables for 2013. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Full-time employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

With respect to heterogeneity of the main findings for subgroups of the population of interest, we take into account the characteristics of individuals, the macroeconomic characteristics of their area of residence and the industrial sectors in which they work.
Table 6 shows the results of the estimation of our main labour market outcomes for distinct age and gender groups.
The first four columns refer to the variable age. From the *Povertypost14* and *Povertypost15* coefficients of the first two columns emerges that the reduction of the time spent at work for the Medicaid treatment occurred both for the over 50 (column (1)) and for the under 50 (column (2)), but it was substantially driven by the over 50. Essentially confirming what reported in the previous table. From columns (3) and (4) *Premiumpost14* coefficient, it can be seen that the increment in the number of people working among the Tax credit treated has occurred mainly among the over 50. This could be due to the fact that, in terms of health insurance, people who are older are more sensitive to health coverage issue and therefore change their behaviour more

significantly and more quickly than young adults.

Columns (5) to (8) refer to the gender heterogeneity. The coefficients of *Povertypost14* and *Povertypost15* in columns (5) and (6) show the impact at the intensive margin of the reform on male and female Medicaid treated, respectively. Although, as in the previous heterogeneity, there are differences on the years in which the coefficients are significant depending on the sample split. Moreover, there is a higher reduction in hours worked among women than among men.

Premiumpost14 and *Premiumpost15* coefficients of columns (7) and (8) show the results for Tax credit treatment. Results suggest that the augmentation in labour supply at the extensive margin seems to be more attributable to women. It is likely that women represent the majority of those who had a hours mismatch in the pre-reform period. Similarly, it is equally likely that, given their lower labor market participation, they experienced a more pronounced increment in labor supply at the extensive margin as a response to Tax credit expansion.

Table 6: Heterogeneous Effects (II)

	Heterogeneous effect: Age				Heterogeneous effect: Gender			
Outcomes	over 50 Minutes of work, if minutes >0	under 50 Minutes of work, if minutes >0	over 50 Employment	under 50 Employment	man Minutes of work, if minutes >0	woman Minutes of work, if minutes >0	man Employment	woman Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Povertypost14	-67.5063	-78.9130**	0.1049	0.0302	-84.2195**	-37.8817	0.0325	0.0876
	(58.2797)	(37.7209)	(0.0803)	(0.0959)	(33.2010)	(60.4795)	(0.0903)	(0.1096)
Povertypost15	-124.8733*	-11.5712	0.0106	-0.0552	1.4775	-135.2317***	-0.0381	-0.0384
	(67.3799)	(73.8632)	(0.0627)	(0.0547)	(80.7712)	(35.0060)	(0.0551)	(0.0602)
Premiumpost14	-21.7532	14.7184	0.0763*	0.0694	-13.9118	2.3516	0.0278	0.0999*
	(21.5510)	(17.4319)	(0.0443)	(0.0452)	(20.3655)	(20.2163)	(0.0433)	(0.0537)
Premiumpost15	27.5342	-35.6332	-0.0012	0.0112	9.3624	-19.0956	-0.0221	0.0302
	(23.0305)	(28.9819)	(0.0471)	(0.0360)	(30.7106)	(24.9669)	(0.0390)	(0.0338)
Observations	2,376	2,429	5,674	4,743	2,511	2,294	5,117	5,300
R-squared	0.2739	0.2281	0.2415	0.2752	0.2344	0.2225	0.2617	0.2586
Years	12-15	12-15	12-15	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state	state	state	state
Controls	√	√	√	√	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

In Table 7 are reported the results of the analysis of heterogeneous effects on employment outcomes with regard to ethnicity and educational qualification.
The first four columns of table 7 are related to heterogeneity in terms of ethnicity. The *Povertypost14* and *Povertypost15* coefficients reported in columns (1) and (2) refer respectively to the impact of the Medicaid reform on "white" and "non-white" individuals. By "non-white" we mean all those who belong to ethnic minorities including blacks, hispanics, asians, indians and hawaians. The signs of the coefficients are all negative, but it is clear that the impact of the reform has been more pronounced on ethnic minorities than on "white" Americans. As opposed to this group, the coefficients for the "non-whites" are statistically significant (1%) for both years under analysis, and the magnitude of the coefficient for 2014 is about twice the coefficient value for the entire sample. In column (3) and column (4), in relation to results on the impact of Tax premium subsidies are reported for the "white"-"non-white" heterogeneity (extensive margin). It can be seen that, although the *Premiumpost14* coefficients related to the first year of the reform are positive for both groups, results are driven by the majority group of "white".
Columns (5) to (8), on the other hand, refer to the heterogeneity analysis according to individuals education. In order to have a sample large enough to perform the analysis, we decided to split the sample between those who have obtained at least a bachelor's degree and those who did not continue their studies. Among *Povertypost14* and *Povertypost15* coefficients reported in columns (5) and (6) the only significant coefficient is the one relative to the group of the non graduated. In the two subsequent and conclusive columns of Table 7, we report the results with respect to the extensive margin specification. The *Premiumpost14* coefficient, relating to the first year for those who have declared that they have not a college degree, it is the only significant. This coefficient is equal to about 9% and it is significant (1%).
Overall results on heterogeneity analysis based on individuals' education suggest that mainly non-graduates reduced labour supply as a response to the Reforms. Indeed, graduated subject are younger (on average) and are probably at the early stages of their careers, so that the analyzed reforms may not have had any impact on their labour supply choices.

Table 7: Heterogeneous Effects (III)

	Heterogeneous effect: Ethnicity				Heterogeneous effect: College			
Outcomes	white Minutes of work, if minutes >0 (1)	non white Minutes of work, if minutes >0 (2)	white Employment (3)	non white Employment (4)	no college Minutes of work, if minutes >0 (5)	college Minutes of work, if minutes >0 (6)	no college Employment (7)	college Employment (8)
Povertypost14	-84.9886	-166.3776***	0.0231	0.0166	-74.3392*	-67.9368	0.0168	0.1917
	(70.8745)	(59.4331)	(0.1161)	(0.0821)	(39.1104)	(53.2187)	(0.0798)	(0.1215)
Povertypost15	-119.2878*	-79.4530***	-0.0927	-0.0149	-83.2070	-19.6562	-0.0446	0.0681
	(66.2258)	(24.8968)	(0.0897)	(0.0672)	(64.1384)	(64.4162)	(0.0470)	(0.1594)
Premiumpost14	1.8674	-9.8569	0.0805**	0.0337	-5.2552	19.1138	0.0898***	0.0462
	(17.9484)	(35.6324)	(0.0383)	(0.0483)	(17.0999)	(27.0472)	(0.0332)	(0.0482)
Premiumpost15	-8.6304	-24.9721	0.0220	-0.0442	-10.8885	-2.7133	-0.0166	0.0223
	(21.4867)	(24.1253)	(0.0344)	(0.0543)	(17.1534)	(38.7172)	(0.0380)	(0.0473)
Observations	3,147	1,658	6,659	3,758	2,801	2,004	6,528	3,889
R-squared	0.2207	0.2617	0.2518	0.2624	0.1907	0.3097	0.2454	0.2850
Years	12-15	12-15	12-15	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state	state	state	state
Controls	√	√	√	√	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Table 8 shows the results related to our last set of heterogeneity analysis. In particular, we take into account the sector in which the workers operate and the economic and labor market conditions of the various states in which individuals reside. The first three columns report the results on the intensive margin for those working in the public sector, in the private sector and in the private "profit" sector respectively.[22]

Looking at *Povertypost14* and *Povertypost15* coefficients, it is clear that the reduction in working hours following the Medicaid reform is mainly driven by those working in the private sector (see *Povertypost15* coefficient reported in column (2)). In particular, when we focus on individuals working in the "for profit" industries, the coefficients are even larger, as suggested by estimates in columns (3). This is probably due to the fact that who work in the private sector "non-profit" is also moved by ideals and values, and not only by economic motivations. And so among these individuals there are also volunteers, a category not affected by the reform.

Columns (4) to (7) show results obtained after splitting the same according to the unemployment rate in the state, month and year for each individual interviewed. In particular, we identify states where the unemployment rate is below (see columns (4) and (6)) or above (see columns (5) and (7)) the average value. Looking at *Povertypost14* and *Povertypost15* coefficients of columns (5) and (6) we note that the negative impact of Medicaid on the hours worked is mainly driven by those who live in areas with a high unemployment rate. The 2015 coefficient for this group is significant (1%) and is 50 minutes higher in magnitude with respect to its value estimated for the full sample. If we assume that in states where unemployment is high, low-wage and low value-added workers have less choice, then employment-lock might be more frequent so that workers respond more strongly to a reform as Medicaid. Also results reported in column (7) provide evidence in favor of this hypothesis. If we look at the *Povertypost15* coefficient in column (7), we see that in states with high unemployment rate there has also been a reduction of the labor supply at the extensive margin.

Turning to the impact of the Tax credit reform, the *Premiumpost14* coefficient reported in column (6) suggest that the increase in the labour supply at the extensive margin is driven by those who live in an area with low unemployment rate. In column (7) the *Premiumpost14* and *Premiumpost15* coefficients are not statistically significant.

[22]The ATUS survey specifically asks individuals to self-report whether they work in the "for-profit" or "non-profit" private sector. Since there are often some typologies of unpaid and volunteer work in the "nonprofit" sector, as additional robustness we decide to repeat the heterogeneity only for those working in the private "for-profit" sector.

Table 8: Heterogeneous Effects (IV)

	Heterogeneous effect: Worker's sector			Heterogeneous effect: State unemployment rate			
Outcomes	govworker Minutes of work, if minutes>0	privworker Minutes of work, if minutes>0	privworker(profit) Minutes of work, if minutes>0	unemp<mean Minutes of work, if minutes>0	unemp>mean Employment	unemp<mean Employment	unemp>mean Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Povertypost14	-118.5507	-52.6210	-45.4560	-74.4453	-150.4625***	-0.0095	-0.0527
	(223.8977)	(52.0301)	(49.5319)	(163.0039)	(39.5578)	(0.1046)	(0.0759)
Povertypost15	-32.8877	-94.0801*	-136.6986**	-35.8680	-31.9535	-0.0372	-0.1415**
	(120.5536)	(51.4364)	(59.2571)	(146.6110)	(72.5717)	(0.0789)	(0.0632)
Premiumpost14	-2.3082	-1.0971	3.2277	-1.8653	-13.9917	0.1461**	0.0412
	(44.5281)	(16.0632)	(17.4906)	(33.6605)	(21.2374)	(0.0555)	(0.0473)
Premiumpost15	-62.1393	-3.2942	-18.6878	-2.3454	-61.6675*	0.0692	-0.1186
	(46.1719)	(20.0412)	(19.2990)	(35.5451)	(34.1513)	(0.0452)	(0.0990)
Observations	796	3,435	3,056	2,037	2,797	4,294	6,200
R-squared	0.3693	0.2165	0.2296	0.1069	0.0619	0.0755	0.0625
Years	12-15	12-15	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state	state	state
Controls	√	√	√	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

7.3 Other outcomes

Results on the impact of medicaid and Tax credit reforms on employment outcomes can be extended by exploiting ATUS data which provide several information on individuals' time use. In this section, we discuss main findings obtained by analyzing the impact of the health reforms on different time use outcomes.[23]

In particular, table 9 reports estimates for time spent using medical care services. This variable, which we call healthcare, includes the time spent throughout the day receiving and waiting for medical and care services, personal care services and health related self-care. The results reported in column (1) are related to the overall impact of the various pillars of the reform on the time spent on healthcare. None of the coefficient is significantly different from zero, probably because of the low number of people in the whole sample that have reported data related to this outcome. In columns (2) and (3) we report the intensive margin and the extensive margin reform impact on the healthcare variablen respectively.

Results shown in column (2) suggest a decrease in time spent on medical care in a broad sense, but only for the 2015, for those who are treated Medicaid. The coefficient of interest, *Povertypost15*, is large in magnitude and statistically significant (10%). This finding is consistent with the hypothesis of a substitution effect between ED visits and visits to the medical practitioner, which, by definition, have a shorter duration. All other coefficients reported in the table, although negative, are not statistically significant. In column (3) all coefficients, except for the coefficient for the second year of the Tax credit reform, are positive. As a consequence, these coefficients show an inflation in the number of people using medical care services, particularly with reference to Medicaid treated (Sommers et al. 2017; Courtemanche et al. 2018, among others). Be that as it may, the coefficients are not statistically significant.

[23] In Appendix B we report the composition of all the variables used in the various specifications. Each variable has been constructed starting from the variable available and reported in the ATUS lexicon.

Table 9: Other outcomes: Medical care services

Outcomes	Minutes spent on healthcare (1)	Minutes spent on healthcare, if minutes>0 (2)	Healthcare (extensive margin) (3)
Povertypost14	3.9481	-18.3704	0.0418
	(8.4170)	(59.1113)	(0.0393)
Povertypost15	-6.6453	-104.1326*	0.0171
	(6.3852)	(57.5479)	(0.0467)
Premiumpost14	-2.0346	-32.1143	0.0072
	(2.9679)	(27.6190)	(0.0192)
Premiumpost15	-2.5771	-10.1238	-0.0286
	(3.1544)	(28.2175)	(0.0182)
Observations	10,417	949	10,417
R-squared	0.0368	0.3131	0.0710
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. The probability of using health care is measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Table 10 shows the results on the impact of analyzed health reform on many time use variables. As opposed to our main outcomes and the medical care variable, in this analysis we take into account only the overall variable without evaluating the difference between the intensive and extensive margin. However we can quantify reforms impacts in terms of minutes, although any zero values in the variables examined can depress the results. In column (1) of table 10 are reported the results on the voluntary activities and caring for people variable, obtained summing the time used by individuals in caring for and helping non-household members and the time spent in volunteer activities.[24] The coefficients are not statistically significant except for *Povertypost15*. The positive sign and the magnitude of the coefficient shows that those who are treated by Medicaid in 2015 declare 17 minutes more of time (per day) spent taking care of people who do not belong to the family and doing voluntary activities compared to the control group. This finding can imply that those who received free insurance coverage reduced their working hours and seem to switch a portion of their earned free time to "give back" to their community. In this case, as in others in which the effect of the reforms are significant only for the second year, it is possible that the effects considered are indirect and, so, the change in outcomes takes some time to realize. There does not seem to be any impact on this variable as far as those who are treated by Tax credit are concerned.

Column (2) shows the results related to the variable "main household activities". The variable was obtained by summing the time spent by the individuals in carrying out the daily housework like cleaning, laundry, preparing lunch and dinner. The coefficients reported in column (2) are positive for both the years after the reform with respect to those who are treated by Medicaid, while are negative concerning those treated by Tax credit. Still and all, most coefficients are not significant. Again, as also highlighted in column (1), the only significant coefficient is *Povertypost15*. This coefficient shows that those treated by Medicaid increase, compared to the control group, the time spent on housework in 2015 of about sixteen minutes per day. As with personal care and volunteering activities, individuals seem to take up an additional slice of the larger amount of free time in housework.

Column (3) shows the results of another variable relating to time spent in household activities, a variable constructed with particular reference to those activities relating to household management. Results suggest that the Medicaid reform has led to an increment in the time spent dealing with financial management, personal organization and planning activities. This effect is significant for both 2014 and 2015. More in detail, the increase is about 7 minutes per day in the first year and 10 minutes per day in the second year. The magnitude of the coefficients relating to the impact of the Tax credit reform and the insignificance of the reform show that the reform did

[24]The variables considered to obtain the aggregate variables used in Table 10 are explained more completely in the Appendix B.

not have any effect on this group of individuals.

In column (4) are reported the results of the analysis using "Leisure" as a dependent variable, obtained by summing all the information related to socializing, communicating, relaxing and leisure, arts, entertainment and attending sports, recreational and social events. The objective is thus to study the effect of the reform on leisure time, except for the time spent in sports activities that we analyzed separately. The sign and magnitude of *Povertypost14* and *Povertypost15* coefficients, it imply that people in this group have increased leisure time in the post-reform period. However, the coefficients in question for both post-reform years are not statistically significant. The *Premiumpost14* coefficient of column (4), on the other hand, shows that during the first year after the reform those treated by Tax credit reform reduced the time spent in leisure activities by about 28 minutes a day. The coefficient is significant at the level of 10% and, although it is negative, it is lower in magnitude and is no longer significant in the second year of the reform (2015). This reduction in leisure time for those treated by Tax credit is in line with our main results. So, it is likely that individuals who did not work before the reform (or worked only part-time), have reduced the amount of time devoted to leisure in the post reform period.

In the fifth and last column of Table 10 we analyze the impact of the reform on the time spent doing sports activities. The *Povertypost14* coefficient is significant (10%) and tells us that those who have benefited from free health insurance, despite the increasing amount of free time available, reduced the time spent doing sports by about 9 minutes a day. [25] Indeed, there could be an ex ante moral hazard, whereby individuals, once obtained insurance, may reduce healthy behaviours, as the cost of a possible health problem in the post-reform period is no longer borne by the individuals.

[25]This is in line with what was said in the "Medicaid and Tax credit impact on other outcomes" Subsection.

Table 10: Other outcomes

Outcomes	Voluntary Activities (1)	Househact (main) (2)	Househact (management) (3)	Leisure (4)	Sport (5)
Povertypost14	-2.4166	5.0387	7.3634*	24.0635	-8.6022*
	(6.8484)	(10.8046)	(4.0669)	(38.2345)	(5.0917)
Povertypost15	17.3791*	16.2379*	10.2103*	3.8157	-4.7873
	(9.8876)	(9.6044)	(5.8848)	(28.0723)	(6.4906)
Premiumpost14	3.3028	-4.4470	0.6431	-27.6465*	-2.7158
	(3.6731)	(4.8748)	(1.9021)	(14.0864)	(3.8212)
Premiumpost15	3.7257	-2.4112	0.4430	-8.7893	-2.0171
	(3.3695)	(6.1096)	(1.7881)	(12.6086)	(3.4744)
Observations	10,417	10,417	10,417	10,417	10,417
R-squared	0.0455	0.1156	0.0507	0.1660	0.0602
Years	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state
Controls	√	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. In each column the overall variable is considered, without evaluating the difference between the intensive and extensive margin. In any case, it is still right to read the results in terms of minutes, although any zero values in the variables examined can depress the results. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

8 Conclusions

In this study we assess the impact of the two pillars of ACA that came into force in 2014 on separate outcomes. First, we provide evidence on the impact of Medicaid and Tax credit reform on childless low income adults. Due to the specific characteristics of the ATUS data, we are able to perform a granular analysis at individual level using detailed and certified information.

Our estimates show that Medicaid eligibles has significantly reduced labour supply at the intensive margin. In other words, they have reduced the hours worked during the day. Additional heterogeneity analysis suggest that part of the reduction of the labour supply may be due to individuals reducing hours worked in second and third jobs. Still and all the results, although smaller in magnitude, still hold true even among those who report a single job. There has indeed been a shift by this group of people from full-time to part-time work. This is in line with the estimates of Aslim et al. (2020) and Moriya et al. (2016). This shift seems to affect especially the group of those over 50 years old. This could be due to the fact that, in terms of health insurance, people who are older are more sensitive and then change their behaviour more significantly and more quickly than young adults. As far as gender is concerned, women are the most affected by the reform. As a matter of fact, women represent the majority of those who had a hours mismatch in the pre-reform period. In addition, the effect of the reform appears to be more pronounced on ethnic minorities, on non-graduates, on private workers and among those who live in states with high unemployment rate.

With respect to the group that obtained Tax credit Premium Subsidies, on the other hand, there was an increment in work at the extensive margin. This is at least partly due to the increase in the number of people working in states where Medicaid has not been adopted. In consequence of that, working is the only way for these people to obtain coverage subsidies. Again, the group most affected seems to be the over-50s. Among other groups, the reform has impacted more on over-50s, on women, on non-graduates, on "white" Americans, and on those living in states with low unemployment rates.

Secondly, we investigate how the ACA has impacted on the time spent receiving and waiting for medical care. Our results suggest that the beneficiaries of Medicaid reform have reduced the amount of minutes spent doing these activities. This is in line with what Colman and Dave (2018) found in relation to the Dependent Coverage Mandate, and probably a direct consequence of replacing ER-based health care with routine visits.

Third, we verified what Medicaid beneficiaries do in their extra time and what Tax credit beneficiaries forego. The first group increased the time spent in household activities and volunteer activities, while the latter reduced leisure time. In addition,

those who are treated by Medicaid experienced a reduction in time spent doing sport. This effect seems to be in line with a "moral hazard" effect.
All in all, the reform seems to have improved the health and financial profile of Americans.

9 Appendix A: Related Literature

A.1 Health coverage and health related outcomes

Previous research involving the impact of health reforms centred on various outcomes. The first outcome that has been investigated, of primary importance given the nature and objectives of the reform, is certainly the impact on the percentage of people with health insurance coverage. This outcome influences all the others. That being said, before the launch of ACA and its various pillars, there had never been a real universal reform. The only exception is Medicare, a reform that provides almost universal coverage for people over 65 years of age.

On that account, a first stream of literature centred either on Medicare or on less strong health coverage expansions as well as a few state-specific expansions. Card et al. (2008) and Card et al. (2009) studied the impact of Medicare on various health outcomes. First of all, they noted a sharp reduction in the number of people without health insurance after the age of 65. They also found a reduction in mortality rates for over-65s and an augmentation in routine doctor visits. Cardella and Depew (2014) focus on young people, analyzing data from the National Health Interview Survey (NHIS) for the pre-ACA period. In that context, young people could be covered by their parents' health insurance or by a government plan up to the age of 19. They found that when an individual turns 19, his/her likelihood of having health insurance decreases by 6%, which also has a negative impact on his or her reported health. Filkenstein et al. (2012) using individual level hospital discharge data for the entire state of Oregon, studied the impact of a lottery that in 2008 randomly assigned access to Medicaid health coverage to a low income uninsured adult population. In the post-lottery period, the authors noted a 25% increase in the probability of having health insurance for those who were selected. This had a positive impact on both their health care utilization, self-reported health and self-reported happiness.

A second stream of literature investigated the effect of the expansion of the first pillar of the Affordable Care Act: the Dependent Coverage Mandate (DCM). The DCM came into force in September 2010 and extended the age at which young adults can enrol as dependents of their parents to 26 years of age, thus remaining covered by their parents' employer-based health insurance plan. Due to good health, higher risk tolerance and high private health insurance prices, young adults have always had a low coverage rate. The aim of DCM is precisely to reduce the barriers to higher health care for young people. Barbaresco et al. (2015) using Behavioral Risk Factor Surveillance System (BRFSS) data find that DCM increases the probability of having health insurance, primary care doctor and excellent health of 23-25 year olds compared to 27-29 year olds (control group). Antwi et al. (2013), using data from the Survey of Income and Program Participation (SIPP), and also including the 16-18 year olds in the control group, found similar results. Cantor et al. (2012) using data from the

Current Population Survey 2005-2011 also discovered that the reform had a strong and rapid impact on the share of young adults with dependent coverage, a result that still holds even when they check for prior state reforms. Sommers et al. (2012) found similar results using CPS data. Sommers et al. (2013) using different data from the National Health Interview Survey and the Annual Social and Economic Supplement to the Census Bureau's Current Population Survey, considered young people aged 26 to 34 in the control group. They found a significant enlargement in coverage rate in the post-reform period, especially among unmarried adults, non-students and men. In addition, they proved that the first to acquire coverage after the reform are those who were in a worse health condition. Nonetheless, caution should be exercised when comparing the results of DCM with those of ACA. In addition to being profoundly divergent reforms, DCM is only temporary and targets an audience of young people who have exceedingly separate characteristics than those of the ACA expansion.

A third research stream focused directly on studying the aftermath of the 2014 ACA expansion and its various pillars on coverage and other health outcomes. Given the importance and uniqueness of this universal reform in the health sector, immediately at the turn of 2014 and 2015 various studies, most of them descriptive, studied the impact of the reform on coverage. Chiefly, some works that were carried out using federal survey data (Cohen and Martinez 2014; Long et al. 2014; Smith and Medalia 2015), suggested a large drop in the rate of uninsured persons, especially as far as low-income adults are concerned.

Sommers et al.(2015) looked into the same report using a private data source (self-reported data from the Gallup-Healthways Well-Being Index) and data from the first two Medicaid enrolment windows. They found a 7.9% reduction in uninsured people in the states that expanded Medicaid, as well as a 3.5% reduction in those who do not have a doctor and a 2.4% reduction in those who do not have easy access to medicines. Overall, they encountered significantly improved trends in self-reported coverage, access to care and medications for those living in the treated states. While this initial research provides us with an idea of the direction of the reform, in subsequent years researchers continued to study the reform impact both using separate types of data and using longer time periods, as well as more precise identification strategies and more advanced methods. In that direction, Courtemanche et al.(2016) estimated the causal effect of ACA on health insurance coverage using data from the American Community survey (ACS), and mainly they investigated the impact of ACA on states that implemented only some pillars of the reform compared to states that have also implemented Medicaid. What they found is that residents of states that adopted Medicaid increased the number of residents with health insurance by 5.9% compared to an increase of 2.8% among those that expanded only partially (e.g., only Tax credit).

Kaestner et al. (2017) studied the impact of the expansion of Medicaid eligibility on health insurance coverage, and find similar results to those just presented. What is more, they differentiated between categories of people who were already potentially eligible for some form of health insurance coverage in the pre-reform period and those who were not, finding higher coverage increases for the former group. They also noticed a small reduction in the number of people taking out private health insurance (between 0 and 4%).Wherry et al. (2016) using self-reported data from the 2010-2014 period of the National Health Interview Surveys also found increased coverage in states that expanded Medicaid (+7.4%). Thanks to the peculiarities of their data, they were also able to investigate the trend of other coverage-related outcomes such as the number of visits to the general practitioner (+6.6%), as well as diabetes and cholesterol diagnoses (up 5 and 6% respectively). These represent diagnoses that, in the absence of health coverage, might not have been carried out, with a negative impact on the life expectancy of low income childless adults. Simon et al.(2017) using BRFSS data addressed the impact of the reform on the rate of insured persons among low income childless adults, a category of people most impacted by the reform since in the pre-reform period they did not benefit from any health coverage. Researchers noticed in this group an increment in the number of insured persons of 17%, which is expected to be higher than that found in other studies on the subject. Additionally, they examined the impact of the expansion on other related outcomes and witnessed a positive impact on preventive care and self-assessed health.

Among all these studies, the one by Frean et al. (2017) is certainly of crucial importance. Although the focus is similar to that of the other works analyzed so far, Frean and colleagues' study investigated the causal impact of the reform and was the first study to disentangle the distinct coverage effects of the ACA's various provisions. In doing so, they used data from the ACS from 2012 - 2015. Overall, they explained that about 40% of the increase in insurance coverage from 2012-2013 to 2014-2015 is attributed to the creation of premium subsidies for exchange coverage. The remaining 60% is due to the inflation in Medicaid coverage. More specifically, according to their disaggregated estimates, Medicaid increased coverage among newly-eligible individuals by around 14 per cent, which should be added to the less discussed but crucial "woodwork effect", i.e. an increase in the number of policyholders among those who were previously eligible for Medicaid before the ACA but who were not enrolled. This phenomenon is evident in all states, regardless of whether they have expanded Medicaid or not. Another interesting result is the crowding-out effect: they found no evidence of significant crowding-out between employed sponsored coverage and Medicaid.

Sommers et al. (2017) studied the impact of ACA on health care use and self-reported health three years after the reform. Authors used data collected from a random-digit-dialing telephone survey by low-income adults in three states: Kentucky (which expanded Medicaid), Arkansas (which expanded only private insurance to low-income adults) and Texas (which did not expand coverage in any way). In the two States that expanded, the rate of uninsured persons was reduced by 20% compared to Texas that did not expand.

As mentioned earlier, some researchers assessed the impact of ACA expansion on a wide range of health outcomes. Ghosh (2017), using data from a rich national pharmacy transactions database, investigated the impact of Medicaid expansion on drug prescription. Since in the pre-reform period there were many unmet medical needs among those who then acquired coverage through the reform, the use of prescription drugs was an overriding proxy for assessing the impact of the reform. They found that in the first 15 months of the expansion, prescriptions of medicines paid by Medicaid increased by 19% in the states where the expansion took place compared to others, for a total of around 12.8 million additional prescriptions. Courtemanche et al. (2019) using BRFSS data examined the impact of behaviours related to health risks of non-elderly adults three years after the implementation of the reform. Their results offered a mixed evidence, at least in some dimensions, as they found more preventive services but less healthy lifestyles.

Courtemanche et al. (2018) using BRFSS data noticed more access to health care among non-elderly adults. Their results were more marked in magnitude especially for those states that expanded both Medicaid and Tax credit. They also detected an increased likelihood of having a primary care doctor and check-ups, but no effects regarding risky behaviours or health outcomes.

Thus, the literature survey suggests that Medicaid enlarged coverage, allowing new policyholders to use more care and enjoy better health. Less is known about the condition of already insured in the post reform period. Indeed, one rationale offered by policymakers of states that did not implement the reform was that it could have overburdened the health care system, and eventually compromised access to care for the population already insured with Medicare.

Be that as it may, Carey et al. (2020), using data from the ACS, looked into the impact of Medicaid expansion on those who were already insured with Medicare and found no negative spillovers for the already insured population where Medicaid was expanded. Although some studies reported somewhat distinct outcomes (Glied and Hong 2018; McInerney et al. 2017, among others), the most plausible explanation seems to be that providers might have anticipated the demand shock resulting from the expansion, thus avoiding the negative impact on those who were already insured.

10 Appendix B

B.1 Medicaid Expansion States and Tax Credit Eligibility Threshold

Table B1 reports information related to states that have and have not expanded Medicaid. In fact, after the 2012 Supreme Court decision, the Medicaid expansion's choice was left to each state. The other ACA pillar (Tax credit premium subsidies), still, was pursued by all states through sizable income-based Tax credits for individuals in an income bracket between 100% and 400% of the FPL who are not eligible for Medicaid. The insurance can now be purchased through subsidize premiums for private insurance purchased on an online platform. Be that as it may, in states where Medicaid has come into force, the minimum threshold in order to access to Tax credit has been raised to 138%. In the last column of the table, we then reported the minimum threshold for Tax credit eligibility, which differs between states that have and have not expanded Medicaid. As far as the maximum threshold is concerned, it remains at 400%, the same in all states.[26]

[26] The data source for the construction of the table reported below were taken from the Henry J.Kaiser Family Foundation.

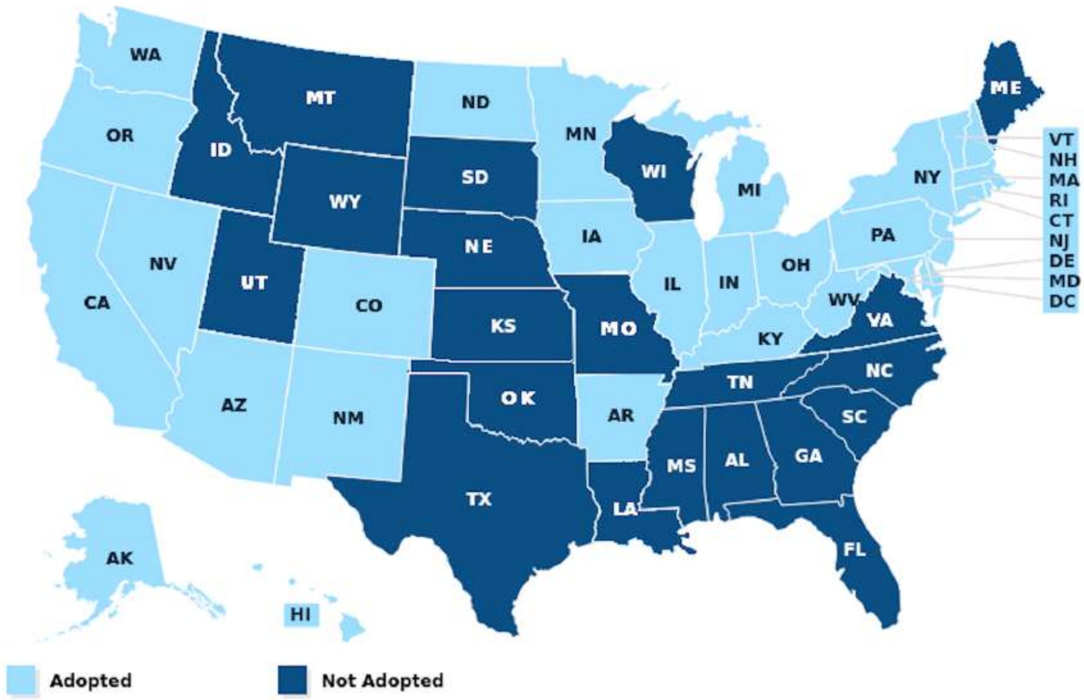
Table B1: Medicaid Expansion States and Tax Credit Eligibility Threshold

States	**Medicaid Expansion Status**	**Date of Expansion**	**Tax Credit Eligibility Threshold**
Alabama	Not Expanding	-	100%
Alaska	Expanded	01/09/2015	138%
Arizona	Expanded	01/01/2014	138%
Arkansas	Expanded	01/01/2014	138%
California	Expanded	01/01/2014	138%
Colorado	Expanded	01/01/2014	138%
Connecticut	Expanded	01/01/2014	138%
Delaware	Expanded	01/01/2014	138%
District of Columbia	Expanded	01/01/2014	138%
Florida	Not Expanding	-	100%
Georgia	Not Expanding	-	100%
Hawaii	Expanded	01/01/2014	138%
Idaho	Not Expanding	-	100%
Illinois	Expanded	01/01/2014	138%
Indiana	Expanded	01/02/2015	138%
Iowa	Expanded	01/01/2014	138%
Kansas	Not Expanding	-	100%
Kentucky	Expanded	01/01/2014	138%
Louisiana	Not Expanding	-	100%
Maine	Not Expanding	-	100%
Maryland	Expanded	01/01/2014	138%
Massachusetts	Expanded	01/01/2014	138%
Michigan	Expanded	01/04/2014	138%
Minnesota	Expanded	01/01/2014	138%
Mississippi	Not Expanding	-	100%
Missouri	Not Expanding	-	100%
Montana	Not Expanding	-	138%
Nebraska	Not Expanding	-	100%
Nevada	Expanded	01/01/2014	138%
New Hampshire	Expanded	15/08/2014	138%
New Jersey	Expanded	01/01/2014	138%

States	Medicaid Expansion Status	Date of Expansion	Tax Credit Eligibility Threshold
New Mexico	Expanded	01/01/2014	138%
New York	Expanded	01/01/2014	138%
North Carolina	Not Expanding	-	100%
North Dakota	Expanded	01/01/2014	138%
Ohio	Expanded	01/01/2014	138%
Oklahoma	Not Expanding	-	100%
Oregon	Expanded	01/01/2014	138%
Pennsylvania	Expanded	01/01/2015	138%
Rhode Island	Expanded	01/01/2014	138%
South Carolina	Not Expanding	-	100%
South Dakota	Not Expanding	-	100%
Tennessee	Not Expanding	-	100%
Texas	Not Expanding	-	100%
Utah	Not Expanding	-	100%
Vermont	Expanded	01/01/2014	138%
Virginia	Not Expanding	-	100%
Washington	Expanded	01/01/2014	138%
West Virginia	Expanded	01/01/2014	138%
Wisconsis	Not Expanding	-	100%
Wyoming	Not Expanding	-	100%

Figure B1 shows on a map the states that have/have not expanded Medicaid between 2014 and 2015.

Figure B1: Medicaid Expansion/Non Expansion States



Notes: States' decisions about adopting the Medicaid expansion are as of December 31, 2015.
Sources: The status for each state is based on KFF (Kaiser Family Foundation) tracking and analysis of state executive activity. For a up-to-date overview of states that have expanded Medicaid see: https://www.kff.org.

B.2 Construction of Medicaid and Taxcredit thresholds and Detailed Activity Codes used for the construction of the Dependent Variables

As the CPS - ATUS data that we have available does not contain precise information about the income of the individual person interviewed, we used the income class variable. The data on the federal poverty line were taken from the U.S. Bureau Census over the years (2012-2015) for childless households consisting of one and two individuals.

Starting from these data, we calculated the distinct thresholds for Medicaid and Tax credit eligibles. Since income classes have an income range of $5,000, we had to make choices about which classes to consider with respect to the various thresholds. In almost all cases, the choice that has been made is the most conservative, thus excluding some people from the range rather than including many people who actually have a higher income class. In any case, below are reported the specific rules we followed in this respect.

Medicaid - single without children

The 138% FPL threshold in order to access to "Medicaid" (single without children) is $16,105 (FPL base year 2014, single without children: $11,670). The time use data have the variable for income classes "hefaminc". In this respect we consider as falling within the category all income classes below threshold 5 (hefaminc=5), ranging from $12,500 to $14,999. We do not include the subsequent threshold (hefaminc=6) because it ranges from $15,000 to $19,999, while instead the threshold to access Medicaid stops at $16,105.

Medicaid - childless couples

The 138% FPL threshold for access to "Medicaid" (couples without children) is $21,707. (FPL base year 2014, childless couples: $15,730). In this respect we consider as included in the threshold all the income brackets below threshold 6 (hefaminc=6), which ranges from $15,000 to $19,999. We do not use the subsequent threshold (hefaminc=7) because it ranges from $20,000 to $24,999, while instead the threshold to access Medicaid stops at $21,707.

Tax credit - single without children - States where Medicaid has not expanded

The 100% FPL threshold for obtaining "Tax credit" (single without children) in states where Medicaid has not been expanded is $11,670. As lower income threshold, we use the threshold 5 (hefaminc=5), which ranges from $12,500 to $14,999. If we employ the fourth category, which ranges from $10,000 to $12,499, all income from $10,000 to $11,670 would be out of the threshold but we would still take it into account. The upper income threshold 400% FPL to access "Tax credit" (single without children) in states where there is no Medicaid is $46,680. As upper income threshold we use threshold 11 (hefaminc=11), which ranges from $40,000 to $49,999. In this way, those who are in the income group $46,680-$49,999 are considered to be eligible. If we used only up to income threshold 10 ($35,000-$39,999), yet, we would not consider all those in the income category $40,000-$46,680, although we would actually have to consider them.

Tax credit - childless couples - States where Medicaid has not expanded

The 100% FPL threshold for access to "Tax credit" (childless couples) in states where there is no Medicaid expansion is $15,730. As a lower income threshold we employ threshold 6 (hefaminc=6), which ranges from $15,000 to $19,999. It would not make sense to exploit threshold 7 as the lower threshold, which ranges from $20,000 to $24,999. By choosing threshold 6, still, those who earn between $15,000 and $15,730 are erroneously considered as falling within the income bracket. As for the upper income threshold 400% FPL to access "Tax credit" (childless couples) is $62,920. As upper income threshold we use threshold 12 (hefaminc=12), which ranges from $50,000 to $59,999. In this way those who are in the income bracket $59,999-$62,920 are not, erroneously, considered to be in the income bracket. Still and all, using income threshold 13 ($60,000-$74,999) would be a mistake.

Tax credit - single without children - States where Medicaid has been expanded

The 138% FPL threshold for access to "Tax credit" (single without children) in states where Medicaid is expanded is $16,105. As a lower income threshold we consider threshold 6 (hefaminc=6), which ranges from $15,000 to $19,999. If we employ threshold 7, which goes from $20,000 to $24,999, all income from $16,105 to $19,999 would not, erroneously, be considered. By choosing threshold 6, still, those who earn between $15,000 and $16,105 are also considered to be in the income bracket. As for the upper income threshold 400% FPL to access "Tax credit" (single without children) in states where Medicaid is epxanded is $46,680. As upper income threshold we utilize threshold 11 (hefaminc=11), which ranges from $40,000 to $49,999. In this way those who are in the income bracket $46,680-$49,999 are considered to be in the income bracket. If we used only up to income threshold 10 ($35,000-$39,999), yet, we would not consider all those in the income bracket $40,000-$46,680, although we should consider them.

Tax credit - childless couples - States where Medicaid has been expanded

The 138% FPL threshold for access to "Tax credit" (childless couples) in states where Medicaid has been expanded is $21,707. As lower income threshold, we employ threshold 7 (hefaminc=7), which ranges from $20,000 to $24,999. If we exploit threshold 8, which ranges from $25,000 to $29,999, all incomes from $21,707 to $25,000 would, erroneously, not be considered. By choosing threshold 7, nonetheless, those who earn between $20,000 and $21,707 are wrongly considered to be in the income bracket. As for the upper income threshold 400% FPL to access "Tax credit" (childless couples) in states where Medicaid is expanded is $62,920. As upper income threshold we use threshold 12 (hefaminc=12), which ranges from $50,000 to $59,999. In this way those in the income bracket $59,999-$62,920 are not, erroneously, considered to be in the income bracket. Be that as it may, if we also utilize income threshold 13 ($60,000-$74,999), we would consider everyone in the income bracket $62,920-$74,999, although we should not actually consider them.

Table B2: Detailed Activity Codes used for the construction of the Dependent Variables (First and Second-tier codes)

Category	Activity Included
Work	t0501 Working, t0502 Work-related activities, t0503 Other income-generating activities, t0599 Work and work-related activities, n.e.c.*
Medical care	t0804 Medical and care services, t0805 Personal care services, t0103 Health-related self care. **
Household main act.	t0201 Housework, t0202 Food & drink preparation & clean-up, t0203 Interior maintenance, repair & decoration. ***
Household management	t0209 Household management. ***
Leisure	t1201 Socializing and communicating, t1202 Attending or hosting social events, t1203 Relaxing and leisure, t1204 Arts and entertainment (other than sports), t1205 Waiting associated with socializing, relaxing and leisure, t1299 Socializing, relaxing and leisure, n.e.c.; t1302 Attending sports/recreational events, t1303 Waiting associated with sports, exercise,& recreation, t1304 Security procedures related to sports, exercise, & recreation.
Volunteer Activities	The whole category t04 Caring for & helping Nonhousehold (NonHH) Members; t1501 Administrative & support activities, t1502 Social service & care activities (except medical), t1503 Indoor & outdoor maintenance, building & clean-up activities, t1504 Participating in performance & cultural activities, t1505 Attending meetings, conferences & training, t1506 Public health & safety activities.
Sport	t1301 Participating in sports, exercise, and recreation.

* However, we repeated the analysis using only the variable "t0501 Working", or using only the first two aggregates "t0501 Working and t0502 Work-related activities", and the results remain the same. To create the "Minutes of work" variable at the intensive margin we used the fact that t0501 was greater than 0 as a constraint. All the others intensive margin variables used in the analysis follow the same logic.
** To create the variable "Medical care" at the intensive margin we focused on the category t0804, and used the fact that Medical care was greater than 0 as a constraint.
*** We also tried to aggregate variables related to the macro-category "Household Activities" in a different way, but the results remain substantially the same.

B.3 Treated and not treated States: other approaches

As already mentioned in the main text, some researchers who carried out analyses related to the impact of Medicaid reform have used distinct rules and approaches than ours. We discuss these in more detail below, to then test the robustness of our results to the use of these different rules.

Kaestner et al. (2017) targeted only on Medicaid and they considered as treated both states that had no expansion in the pre reform period and expanded Medicaid during 2014, as well as states that expanded Medicaid during 2014 and had prior but limited Medicaid expansion. The authors considered both groups, albeit separately. The states belonging to the first group are: AK, KY, MI, NH, NV, NM, ND, OH, WV (9). The states belonging to the second group are: AZ, CA, CO, CT, HI, IA, IL, MD, MN, NJ, OR, RI, WA (13).
All states that are not listed above are in the control group. The only exceptions are the states that have expanded Medicaid post 2014, which are removed from the analysis. To simplify, they consider as "treated" for the whole 2014 those states that have expanded in the following months. As far as the category of individuals is concerned, they counted in the analysis families with children, which we eliminated, and also young adults over the age of 22. In both these categories, the risk of having already had treatment in the pre-reform period is much higher than in our case.

Leung and Mas (2018) and also Aslim (2020) utilized only 42 states, divided between states in which childless adults were eligible in the pre-reform period (especially in 2013) that expanded Medicaid, and states that did not expand. All states that had some form of coverage for childless adults in the pre-reform period were eliminated. So the states were categorised as follows: Expansion State (treatment group): AR, CA, IL, IA, KY, MD, MA, NV, NJ, NM, ND, OH, OR, RI, WA, WI, WV, MI, NH, PA, IN. Non Expansion State (control group): AK, AL, FL, GA, ID, KS, LA, ME, MO, MS, MT, NE, NC, OK, SC, SD, TN, TX, UT, VA, WY. States not considered: AZ, CO, CT, DE, DC, HI, MN, NY, VT. Wisconsis was considered treated even though it did not expand as it created a program that covers young adults up to 100% FPL.

Results using different treated and not treated group

In Tables B3 and B4 we replicated our main results by using different treated and not treated group. More specifically, in Table B3 we used Kaestner et al.(2017) approach and in Table B4 Leung and Mas (2018) approach. The results remained substantially the same with regard to sign, significance and magnitude.

Table B3: Main results using Kaestner et al. (2017) approach

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost14	-15.6593	-98.5355**	0.0377
	(43.5937)	(48.6345)	(0.0779)
Povertypost15	-47.5812	-81.0902*	-0.0369
	(28.8510)	(43.1801)	(0.0497)
Premiumpost14	33.9811*	-3.8853	0.0791**
	(17.1470)	(15.4622)	(0.0334)
Premiumpost15	-15.5922	-19.6868	0.0004
	(17.0799)	(19.2146)	(0.0263)
Observations	9,600	4,444	9,600
R-squared	0.2644	0.1807	0.2275
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Table B4: Main results using Leung and Mas (2018) approach

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost14	-27.5139	-90.8325*	-0.0175
	(51.6733)	(53.4432)	(0.0902)
Povertypost15	-49.9607	-97.4519**	-0.0404
	(31.8694)	(46.1366)	(0.0535)
Premiumpost14	26.2068	0.7971	0.0581*
	(18.2873)	(15.8360)	(0.0342)
Premiumpost15	-17.1988	-16.2876	-0.0089
	(18.0702)	(20.3611)	(0.0280)
Observations	9,008	4,166	9,008
R-squared	0.2627	0.1770	0.2282
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

B.4 Additional placebo and robustness checks

In Table B5 we replicated our main results by removing from the analysis the questionnaires that had been marked as incomplete by the BLS. The results remain substantially the same with regard to significance, sign and magnitude.

Table B5: Main analysis excluding incomplete questionnaires from the sample

Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)
Povertypost14	-27.5139	-90.8325*	-0.0175
	(51.6733)	(53.4432)	(0.0902)
Povertypost15	-49.9607	-97.4519**	-0.0404
	(31.8694)	(46.1366)	(0.0535)
Premiumpost14	26.2068	0.7971	0.0581*
	(18.2873)	(15.8360)	(0.0342)
Premiumpost15	-17.1988	-16.2876	-0.0089
	(18.0702)	(20.3611)	(0.0280)
Observations	9,008	4,166	9,008
R-squared	0.2627	0.1770	0.2282
Years	12-15	12-15	12-15
Cluster level	state	state	state
Controls	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

A further robustness test is shown in Table B6. In the first three columns of Table B6 we replicated our main results (reported in Table 2) by removing from our sample, and more precisely from the control group, those individuals whose income class is greater than $99,999 per year. Conversely, in the columns from (4) to (6) we replicated the results of our main table adding to the sample also those who have an income included in a class that exceeds 150,000 $. As noted from the magnitude and significance of the coefficients, the results are very similar to the main results.

Table B6: Change of the control group income class limits

	Income class ≤ $99999 per year			No income class limits		
Outcomes	Minutes of work	Minutes of work, if minutes>0	Employment	Minutes of work	Minutes of work, if minutes>0	Employment
	(1)	(2)	(3)	(4)	(5)	(6)
Povertypost14	3.8876	-82.8014*	0.0505	-11.8322	-89.7419*	0.0317
	(37.7946)	(41.6587)	(0.0687)	(41.1685)	(46.3918)	(0.0691)
Povertypost15	-54.7318**	-73.1161*	-0.0557	-43.9773*	-78.2790*	-0.0432
	(24.7324)	(39.1532)	(0.0490)	(24.1479)	(43.4726)	(0.0417)
Premiumpost14	46.8410**	11.9839	0.0861**	28.2175*	-2.8769	0.0631**
	(22.2636)	(19.5668)	(0.0416)	(16.1085)	(12.9888)	(0.0295)
Premiumpost15	-15.5860	0.6063	-0.0173	-5.2254	-7.3063	-0.0010
	(19.0852)	(26.2068)	(0.0332)	(17.2116)	(20.4658)	(0.0224)
Observations	7,835	3,467	7,835	11,300	5,307	11,300
R-squared	0.2648	0.1901	0.2339	0.2728	0.1874	0.2327
Years	12-15	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state	state
Controls	√	√	√	√	√	√

Notes: The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. Employment probabilities are measured on a 0-1 scale. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

Table B7 shows additional robustness related to the impact of Medicaid at the intensive margin. We changed the minimum threshold of daily minutes of "Main job and other jobs" in order to be considered workers and then included in the intensive margin analysis.

In the various columns are reported the results related to the various specifications that, starting from column (1), raise the threshold (which was previously set at 0) to 30, 60, 90 120 and 180 minutes respectively. The results remain substantially stable. The only differences from the main specification (Table 2) are given by the fact that *Povertypost14* coefficients are statistically significant at the 1% level (except in column (1), which the coefficient remain significant at the 5% level, and in column (5), which we will discuss later). In addition, in all specifications except that in column (1), the *Povertypost15* coefficient remains negative but is never statistically significant.

Finally, in column (5) we replicated our main result on treated Medicaid group at the intensive margin using as a eligibility rule the CPS information related to the employment status, and so considering only those who are marked as employed. Even if this variable is less precise than the ATUS one, that is related to the individual on the single day of the interview, the results remain substantially stable. The coefficient of *Premiumpost14* shows an inflation in the number of minutes worked even at the intensive margin for Tax credit treated.

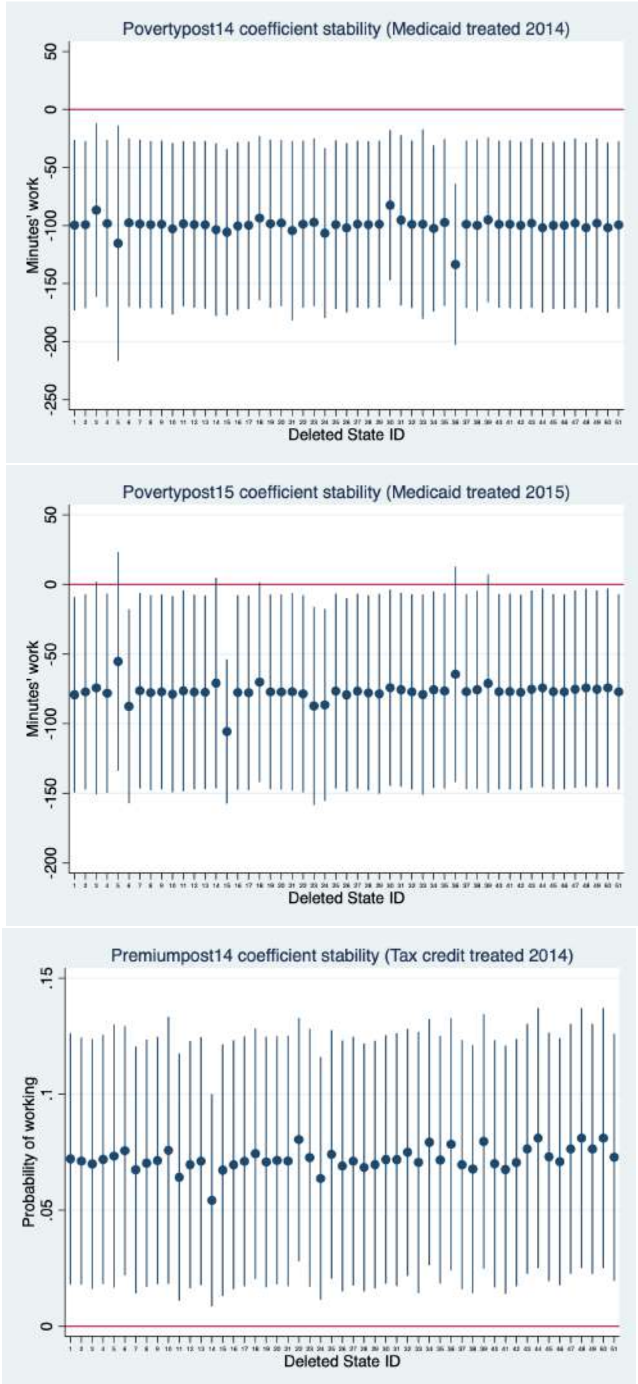
Table B7: Threshold change for being considered as worker in the intensive margin specification

Outcomes	Main job ($\geq 30min$) (1)	Main job ($\geq 60min$) (2)	Main job ($\geq 120min$) (3)	Main job ($\geq 180min$) (4)	Main job (CPS variable) (5)
Povertypost14	-99.2441**	-104.1980***	-97.5129***	-95.2047***	-52.7554
	(43.0216)	(36.9212)	(31.3689)	(33.1416)	(58.0631)
Povertypost15	-77.2080*	-39.2107	-55.0047	-50.2833	-115.8149***
	(41.7552)	(43.6187)	(41.3883)	(44.2281)	(32.5818)
Premiumpost14	-3.4873	-4.6641	-7.7351	-5.6831	35.1802**
	(14.4326)	(11.3999)	(13.2099)	(13.1714)	(15.8682)
Premiumpost15	-10.4471	-14.1550	-11.9371	-1.2077	-10.0814
	(19.7352)	(16.7690)	(18.5790)	(18.3534)	(19.5832)
Observations	4,805	4,463	4,309	4,189	8,170
R-squared	0.1821	0.1441	0.1361	0.1257	0.3330
Years	12-15	12-15	12-15	12-15	12-15
Cluster level	state	state	state	state	state
Controls	√	√	√	√	√

Notes: Each column reports the results following the column specification (2) in Table 2, with the difference that a modified threshold for the intensive margin specification is taken into account in the various columns (from column (1) to (4)). For the variable definition in column (5), information from the CPS are used. The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied. Hours of work are actually measured in minutes per day, preserving the diary format. The statistical significance of the test that the underlying coefficient is equal to zero is denoted by: $p < 0.10$ = *, $p < 0.05$ = **, $p < 0.01$ = ***.

The three graphs reported in Figure B2, instead, are additional robustness related to the stability of the coefficients. We replicated our main analysis 51 times, removing one by one all the states.
Graph 1 shows the stability of the coefficient for the first year (2014) with respect to Medicaid treated at the intensive margin. Graph 2 reports the stability of the coefficient for the second year (2015) for Medicaid treated at the intensive margin. Finally, in graph 3 the stability of the coefficient for the first year of Tax credit treated at the extensive margin is reported. Overall, results are stable.

Figure B2: Coefficients stability



Notes: The graphs in Figure B2 show the coefficients for the main results reported in Table 2, re-estimated by removing each state from the analysis one by one in order to verify the stability of the results. More specifically, graphs 1 and 2 in Figure B2 are respectively related to checking the stability of *Povertypost14* and *Povertypost15* coefficients.Hours of work are actually measured in minutes per day, preserving the diary format. Graph 3 is dedicated to verify the stability of *Povertypost15* coefficient. Employment probabilities are measured on a 0-1 scale as usual. The model estimated by OLS is specified in Equation 1. Controls include: age, gender, educational qualification, marital status, metropolitan area of residence, ethnicity, income categories, weekend dummy, monthly state unemployment levels, geographical and time fixed effect. Standard errors are clustered at State level. The ATUS weights are applied.

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