The London School of Economics and Political Science

Essays in Political Economy

Nicola Fontana

A thesis submitted to the Department of Economics
for the degree of Doctor of Philosophy

June, 2021

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without my prior written consent.

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of approximately 65,000 words.

Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with Professor Tim Besley and Doctor Nicola Limodio, and I contributed 33% of this work. A version of this paper has been published in the *American Economic Review: Insights*, Vol. 3(2), June 2021, pp. 251-65.

I confirm that Chapter 3 was jointly co-authored with Professor Tommaso Nannicini and Professor Guido Tabellini, and I contributed 33% of this work.

Acknowledgments

This thesis is the final step of a long and intense journey that has been made possible thanks to the help and support of numerous extraordinary researchers that deserve my highest gratitude.

I would like to thank my supervisor Guy Michaels, for his constant support and guidance, and his ability to provide tools to make my own decisions. I am also grateful to my supervisor Stephen Machin, for all the fruitful and insightful discussion we had over the years. Special mentions go to Tim Besley and Guido Tabellini which taught me how to develop as a rigorous researcher. Finally, I want to thank Tommaso Nannicini and Eliana La Ferrara which helped me invaluably at the beginning of my career as a young master student. I am also grateful to the rest of the LSE and the Centre for Economic Performance faculty and researchers.

I owe many thanks to Marco Tabellini, whose support during the last stages of this journey has been invaluable, and to all my other coauthors, Nicola Limodio, Marco Manacorda and Gianluca Russo, who contributed to this thesis and my research agenda.

I am forever grateful to my friends and colleagues in London, they made this experience unique and unforgettable during the PhD roller coaster. Thank you, Ale, Andre, Bonni, Carla, Celine, Daniel, Derek, Di Song, Eusebio, Gaia, Kilian, Luca, Łukasz, Maddi, Maicolone, Marco, Marti, Matteo, Maxi, MCF, Mighe, Monica, Nicolas, Nike, Phebio, Ricky, Sara, Tillman, Tommi, Vinci, Viola, Zane. Thank you to the ULU tribe, they made the Sunday nights much more fun. Thank you to Alessandra, she has been the salt that makes everything taste better. For Bobbi a thank you is not enough, the daily routine would have been impossible without Mansell/Coke Street.

Thank you to my friends back in Italy, they made hard each time I have to leave. Thank you Albertolotti, Albi, Ale, Andre, Biondi, Clara, Elisa, Ferdi, Garats, Gaspa, Giovi, Giulia, Giulio, Luca, Luci, Marghe, Marti, Matte, Michi, Nazzo, Nere, Ricky, Ripa, Serena.

However, more than anything else, this thesis was possible thanks to the unconditional love and support of my family: Camilla, Giovanna and Massimo. This thesis is dedicated to them.

Abstract

This thesis consists of three chapters, in which I study the role of globalization and how political views are formed. The first chapter studies the rise of backlash against tourism as a form of anti-globalization sentiment, looking at the role of Airbnb. I construct a spatially disaggregated dataset to study the consequences of Airbnb in London. First, I document that 1 additional Airbnb tourist per 1000 residents increases complaints against tourists by 2.2 per cent. Secondly, I explore the roots of these reactions. I find that higher Airbnb penetration decreases neighbourhood quality, while the housing market is marginally affected. These negative externalities can be explained by a lack of monitoring and coordination by hosts, which are key differences compared to traditional hotel accommodations. Finally, I show that the deterioration of neighbourhood quality markedly reduces social capital, and worsens attitudes towards globalization, with higher support for Brexit.

The second chapter documents how firms in tradable sectors are more likely to be subject to external competition to limit market power while non-tradable firms are more dependent on domestic policies and institutions. We combine an antitrust index with firm-level data from Orbis covering more than 12 million firms from 94 countries and find that profit margins of firms operating in non-tradable sectors are significantly lower in countries with stronger antitrust policies.

The third chapter studies the impact of the Italian civil war and Nazi occupation of Italy in 1943–45 on postwar political outcomes. The Communist Party, more active in the resistance movement, gained votes in areas where the Nazi occupation was both longer and harsher, mainly at the expense of centrist parties. This effect persists until the late 1980s. These results suggest that civil war and widespread political violence reshape political identities in favour of the political groups that emerge as winners.

Contents

List of Tables

List of Figures

Chapter 1

Backlash against Airbnb: Evidence from London

Nicola Fontana
Department of Economics and Center for Economic Performance, London School of Economics and Political Science

Abstract

Anti-globalization sentiments have been on the rise in recent years. In urban contexts, these attitudes may take the form of backlash against tourism. In this paper, I examine the role of Airbnb, a major short-term rental platform, in explaining the rising discontent against tourists. To do so, I construct a rich and spatially disaggregated dataset to study the consequences of Airbnb penetration in London. First, I document that 1 additional Airbnb tourist per 1000 residents increases complaints against tourists by 2.2 per cent. Secondly, I explore the roots – pecuniary and non-pecuniary – of these reactions. I find that higher Airbnb penetration is associated with a decrease in neighbourhood quality, while the housing market is only marginally affected. These negative externalities can be explained by a lack of monitoring and coordination by hosts, which are key differences between short-term renting and traditional hotel accommodations. Finally, I provide evidence that the deterioration of neighbourhood quality markedly reduces social capital, as measured by the number of charitable organizations, and worsens attitudes towards globalization, leading to higher support for Brexit.

1.1 Introduction

A vast literature has studied the rising backlash against globalization, suggesting that opposition to international trade (Colantone and Stanig, 2018; Autor et al., 2020) and to immigration (Becker and Fetzer, 2016; Halla et al., 2017; Dustmann et al., 2019) have played a key role in fueling these views.[1] Some recent studies suggest that these grievances have economic but also social roots (Abramitzky and Boustan, 2017; Tabellini, 2020). In this paper, I examine the backlash against tourists, a phenomenon which has increased dramatically across European cities in recent years (Peeters et al., 2018), and I interpret it as a form of urban backlash against globalization.

To the extent that anti-globalization attitudes are linked to populism, these dynamics may suggest that also large cities, which have so far largely resisted populist waves (Rodden, 2019; Broz et al., 2020), may eventually undergo political shifts similar to those already experienced by rural areas.[2]

The growing backlash against tourism has coincided with extraordinary growth in visitors numbers. Based on the latest official statistics (UNWTO, 2020), the number of international overnight tourists grew to 1.5 billion in 2019, which is 53.4 per cent higher than in 2010. This has been driven by a rising middle class across the world and by the significant reduction in airfares triggered by low-cost carriers. However, the key factor has been the rise of short-term renting, facilitated by digital platforms such as Airbnb, which represents the first and largest platform on the market.[3] By substantially reducing transaction costs, the emergence of these intermediation services allow existing housing units to be rent to short-term visitors, rapidly increasing the capacity for overnight stays. Even so, the rise in tourism has been a powerful engine of economic growth, with a direct GDP contribution growing yearly at 3.6 per cent rate (WTTC, 2019). A concern is that, however, this growth has been highly concentrated in a handful of destinations around the world, with almost 50 per cent of global tourism concentrated in 100 cities (Yasmeen, 2019).

City governments in some of these hotspots are trying to cope with so-called "over-tourism", a term coined by the media to describe the consequences of having too many visitors that may fuel the backlash against tourists.[4] While it is recognized that tourists are beneficial for some local areas and sectors, such overcrowding brings costs, which are borne by residents. Tourists may increase cost of living, with locals "crowd out" from touristic neighbourhoods. Moreover, residents may find that pavements, roads and public transports are clogged by tourists and they may deal with more and more common late-night misbehaviours.[5]

Motivated by these facts, in the first part of this paper, I investigate the relationship between rising backlash against tourists and Airbnb penetration in London. Then, I study the pecuniary and non-pecuniary roots of the observed discontent against tourists. In particular, I investigate whether the grievances against tourism stem from higher house prices and rents, or from worsening of quality of life in the neighbourhoods. Finally, I ask whether the deteriorating quality of neighbourhoods reduces social capital and residents' support for globalization.

I perform my analysis in the context of London, for which I construct a rich and spatially disaggregated dataset at electoral ward-year level, where wards represent the primary unit of English electoral geography. My dataset has three novel features. First, the main proxy of backlash against tourists is the number of

[1] Mudde and Kaltwasser (2017), Margalit (2019) and Guriev and Papaioannou (2020) provide thorough reviews of the existing literature on these phenomena.

[2] Highlighted in the media, *e.g.* The Guardian, January 2020, *Overtourism in Europe's historic cities sparks backlash*; The Economist, October 2018, *The backlash against overtourism*.

[3] From 2008 to 2019 in London the number of rooms available on Airbnb grew from 0 to 135,000, almost matching the number of hotel rooms at 159,000. See Quattrone et al. (2016) for a discussion on Airbnb spread in London.

[4] Highlighted in the media, *e.g.* The Guardian, January 2020, *Overtourism in Europe's historic cities sparks backlash*; The Economist, October 2018, *The backlash against overtourism*.

[5] "Airbnb Party flats" are a well know issues, *e.g.* The Guardian (2017) *It sounded like Fabric was upstairs' - Airbnb rental used for all-night party*.

complaints against tourists, which I build from a unique source of geolocalized complaints sent to local authorities. Thanks to this direct measure, I can test precisely the relationship between Airbnb penetration and backlash against tourism. Such measure allows me to exactly capture the "voice of losers" and those unhappy with the status quo, even when these groups represent a minority whose discontent would not be captured by vote shares or average house prices, which are more standard measures typically used in political economy or urban economics to explore analogous questions.

Second, I introduce a new measure of Airbnb penetration that accounts for the "intensity" of Airbnb tourists presence by considering the length of stay and the number of guests in each listing. Notably, this measure does not suffer from the problem, recurring in the literature, of inactive listings not removed from the Airbnb website, as it uses actual reviews to infer the number of guests in the area. Moreover, I distinguish between types of Airbnb tourists (families *vs.* non-families), by ethnicity (following Tzioumis, 2018) and by type of accommodation (room *vs.* entire property).

Third, I complement the dataset with a rich set of neighbourhood quality measures (complaints about negative behaviours, anti-social behaviour crime rates, and proxies for congestions of local services), proxies for social capital (number of charitable, youth and political organizations) and anti-globalization views (Brexit vote share). These measures allow me to shed light on non-pecuniary mechanisms and social implications, so far unexplored by the existing literature.

The empirical analysis is performed at the electoral ward-year level, controlling for ward fixed characteristics as well as flexible time effects for each local authority within London.[6] In the baseline specification I also include a wide range of pre-determined and geographic characteristics interacted with year fixed effects to control for different evolution depending on initial and fixed characteristics. In addition, I use a shift-share instrumental variable strategy, as in Barron et al. (2020), to address the concern that Airbnb penetration might be itself influenced by time-varying ward conditions not captured by the demanding set of controls described. The "share" part of the IV exploits spatial variation of historical point of touristic interests. The "shift" component exploits time variation in Airbnb worldwide popularity. The validity of this strategy hinges on two critical assumptions, conditional on controls: i) determinants of the spatial distribution of historical sites from hundreds of years ago are not informative of current trends and ii) worldwide Airbnb popularity is not informative of wards unobservable trends.

Notably, hotel penetration is not a confounding factor in my identification strategy. First, hotel industry penetration is almost constant in the sample period considered, therefore, mostly absorbed by ward fixed effects. Second, controlling for flexible trends (by the local authority or by "central" wards) ensures that common trends are captured. Third, the instrument proposed does not predict hotel penetration. Fourth, adding hotel penetration as a regressor alters neither the significance nor the magnitude of my results.

I begin my analysis by documenting a positive relationship between Airbnb penetration and backlash against tourists. For each additional tourist every 1000 residents, which represents the median impact in London, complaints against tourists increase by 2.2%.[7] There exist at least two explanations for this finding. First, discontent might arise from the impact that a permanent reallocation of housing supply, from long to short-term rentals, has on prices. Second, the high turnout of tourists in residential areas might affect the quality of the neighbourhood.

Evidence on the first channel comes from Barcelona (Garcia-López et al., 2019), Amsterdam (Almagro and Domínguez-Iino, 2020), Los Angeles (Koster et al., 2019), Berlin (Duso et al., 2020) or the entire US (Barron et al., 2020). These papers show how an increase in Airbnb penetration is linked to a rise in prices

[6] See Appendix Section 1.A.1 for details on London administrative structure. Each ward is uniquely assigned to one of the 33 London local authorities (or boroughs).

[7] However, the magnitude of the effect can vary widely given the extreme heterogeneity of Airbnb presence across London. As an example, Airbnb penetration in central London is, on average, ten times larger than in the median ward.

caused by the permanent shift of properties from long to short-term renting in cities with fixed housing supply. However, in the context of London, I find limited evidence of this channel.

I explore the second channel documenting how neighbourhood quality is impacted by Airbnb presence. I do this in several ways. First, I show that public transports congestion, proxied by underground entries and exits flows, increases across London in areas with higher Airbnb penetration. Secondly, I document a rise in crime rates for anti-social behaviours. The estimated effects are sizeable: anti-social behaviour crime rates increase by 2.6 per cent in a ward with median Airbnb penetration. Third, complaints against rubbish in the streets increase due to Airbnb penetration. However, it is important to underline that not all complaints are increasing, suggesting that i) residents are not complaining more in general or that, ii) local authorities are still investing in areas with high Airbnb penetration.[8] These results suggest that the roots of backlash are also linked to non-pecuniary motivations, which cannot be captured only by dynamics in house prices. A potential explanation is that, while house prices just capture a net, average effect, benefits and costs are unequally distributed and perceived across different subgroups of residents. My direct approach to measuring complaints and neighbourhood quality allows to capture such heterogeneity and to unveil new patterns. To the best of my knowledge, this is the first paper to explicitly link Airbnb penetration to the backlash against tourism and to provide evidence that this occurs through a decline in neighbourhood quality.

To rationalize these results, I highlight the differences between short-term renting and traditional accommodations in hotels. First, the absence of formal monitoring over guests may induce both negative behaviours from tourists and negative selection, with more disruptive tourists choosing Airbnb properties to take advantage of looser constraints (*quality* externality). Second, as Airbnb supply is extremely flexible and not regulated, local services may fail to adjust and hosts do not internalize the impact of an increasing number of visitors on congestion of public services (*quantity* externality).[9]

Consistently with the hypotheses described, I provide evidence that negative externalities are triggered by a lack of control from Airbnb hosts. I observe fewer complaints where more families are present among Airbnb tourists, as well as in areas where most guests rent just one room and share the property with the host, rather than renting the entire property. The former result confirms that less disruptive tourists induce fewer complaints, and the latter suggests that monitoring through the presence of hosts mitigates negative behaviours from Airbnb guests. In a heterogeneity analysis, I provide evidence that complaints are decreasing when integration between tourists and residents is more likely, suggesting that more cosmopolitan areas are more prone to welcome tourists. Noticeably, I do not observe a decrease in population linked to Airbnb penetration but only minor changes in composition, reassuring that my results are not driven by specific dynamics of residents' sorting.

Finally, I provide suggestive evidence that deteriorating quality of neighbourhoods, through short-term renting, reduces social capital and residents' support for globalization. Following Guiso et al. (2016), I measure social capital by the number of charitable, youth and political organization. Using these proxies, I document that social capital and civic engagement decrease when Airbnb penetration rises. Moreover, I show that higher Airbnb penetration increases support for Brexit, suggesting a rising anti-globalization sentiment. This result provides another potential channel, on top of the one already discussed in the literature (Broz et al., 2020; Eichengreen, 2018), that may fuel anti-cosmopolitan and populists sentiments. It is also consistent with Colantone and Stanig (2018), that shows how support for the Leave option in the Brexit referendum was systematically higher in regions hit harder by economic globalization.

Previous literature My paper contributes to three strands of the literature. First, it is related to the growing body of research on the impact of short-term renting. A first set of papers highlights the impact

[8] Complaints about roads' or green areas' status are not affected by Airbnb penetration.

[9] Hotel industry has an almost constant supply in the period studied and it is often heavily regulated.

on the house and long-term rent prices. Sheppard and Udell (2016), Garcia-López et al. (2019), Koster et al. (2019), Duso et al. (2020) and Barron et al. (2020) find that house and long-term rent prices increase, taking advantage of a similar empirical strategy as the one presented here, in New York, Barcelona, Los Angeles, Berlin and the United States, respectively.[10] Calder-Wang (2020) uses a structural model of residential choice to estimate the effect of the increased opportunity for landlords to rent short term for on the equilibrium rents across different housing types and demographic groups. A second set of papers studies the impact of Airbnb on the hotel industry, showing how Airbnb presence negatively affected hotel revenues (Zervas et al., 2017; Farronato and Fradkin, 2018; Schaefer and Tran, 2020). The paper closest to mine is Rondon (2019), which focuses on electoral consequences of Airbnb penetration in Barcelona, and shows how areas with more Airbnb experience higher abstention and are more likely to vote for the party that campaigned in favour of home-sharing regulations.

I complement this recent literature in four ways. First, I discuss and provide direct evidence of the linkage between Airbnb penetration and the observed backlash against tourists. Second, I present novel evidence of an additional non-pecuniary impact of short-term renting on neighbourhood quality. I also highlight the consequences of the deterioration of local amenities on social capital and political views. Third, I suggest that the lack of monitoring by Airbnb and Airbnb hosts is a key difference from standard hotel tourism, which represents the mechanism driving the documented backlash. Fourth, I introduce a new measure of Airbnb penetration. While the literature has focused mainly on the number of listings, I define penetration as the number of Airbnb guests nights over residents population. The measure that I use has two key advantages: i) it does not suffer from the problem of inactive listings not removed from Airbnb website, as it uses actual reviews to infer the number of guests in the area; and ii) it accounts for the heterogeneity in the size of listings and length of stay of guests.

The second strand of the literature I contribute to is the one examining the determinants of neighbourhood quality. From business composition (Almagro and Domínguez-Iino, 2020) to school quality (Bayer et al., 2007), several explanations have been advanced.[11] My contribution is to highlight the role played by short-term renting industry and its potential impact on residents' behaviour. My working hypothesis is that disruption experienced by residents may induce a lower willingness to contribute to the neighbourhood quality. This is consistent with the idea that, in neighbourhoods in which social networks are tighter, the willingness to contribute to the local area is higher[12]. Not only tourists will misbehave, but their misbehaviour may induce similar responses by residents.

Finally, my work is related to the growing literature explaining how anti-globalization sentiments and social capital are shaped. Dustmann et al. (2019) shows that a larger share of refugees leads to an increase in the vote share for right-leaning parties with an anti-immigration agenda, but that this is not true in large urban municipalities. Autor et al. (2020) finds that trade-exposed electoral districts simultaneously exhibit stronger support for both radical-left and radical-right views. The results in my paper shed light on the additional channel of short-term tourism, which may shape social capital and political views, in particular looking at anti-globalization sentiments using Brexit votes. Cities have largely resisted these trends and, for this reason, results presented in this project may complement immigration and trade literature that explained these phenomenons.

The remainder of the paper is organized as follows. Section 2.3 presents the data. Section 1.3 lays out the empirical strategy and presents the first stage results from the IV strategy. Section 1.4 studies the

[10] Differently from other studies, Koster et al. (2019) takes advantage of discontinuous regulation between Los Angles county and neighbourhood areas. Duso et al. (2020) takes advantage of policy changes in Berlin.

[11] Almagro and Domínguez-Iino (2020) uses a structural approach to study the endogenous link between amenities and residents location sorting, and how this shapes welfare distribution. Airbnb, in their context, drives the shift in housing supply. They also document an increase in house and long-term rent prices.

[12] This has been shown by comparing homeowners and long-term rentals, and it becomes even more salient if short-term renters are present (Putnam, 1993; Sims, 2007).

impact of Airbnb penetration on the backlash against residents and on pecuniary and non-pecuniary roots. Section 1.5 investigates the mechanisms behind the externalities generated by Airbnb penetration. Section 1.6 documents the consequences in terms of social capital and anti-globalization support. Section 2.5 summarizes the main robustness checks, which are then described in detail in the Appendix. Section 2.6 concludes.

1.2 Data

My analysis relies on a panel of 624 London electoral wards for years between 2002 and 2019, where each ward is uniquely assigned to a local authority (or "borough").[13] I exclude from the sample City of London local authority due to its unique characteristics. To study the economic and political effects of Airbnb penetration, I combine data from several sources. Appendix 2.6 fills in the details. Appendix Table 1.C.1 reports summary statistics for the main variables presented in this Section.

1.2.1 Airbnb Penetration

Data on Airbnb penetration come from InsideAirbnb.com and Tomslee.net, independent sources that web-scrape the Airbnb website monthly and collect all publicly available information. My measure of Airbnb penetration is defined as follow:

$$Airbnb\,penetration_{it} = \frac{Airbnb\,tourists\,nights_{it}}{Residents\,nights_{i2007}} \tag{1.1}$$

It represents the average number of tourists using Airbnb that a resident would meet in a random day in ward i and year t. The numerator in Equation (1.1) is computed in the following way:

$$Airbnb\,tourists\,nights_{it} = \sum_{j} Reviews_{jit} \times \frac{1}{0.69} \times Guests_j \times Nights_j \tag{1.2}$$

where $Reviews_{jit}$ is the number of reviews received in year t by listing j in ward i.[14] To convert the number of reviews into the number of Airbnb visits, I rescale the former by 0.69, which is the percentage of guests that leave a review (Fradkin et al., 2020). I obtain the number of $Airbnb\,tourists\,nights$ by taking into account the number of guests the property can accommodate ($Guests_j$) and the number of minimum nights a host requests ($Nights_j$). This measure of Airbnb tourists nights produces an overall figure for 2018 that is very similar to official statistics in Airbnb (2018), 6.88 million vs 6.82 million. The denominator in Equation (1.1) is the number of residents in 2007 in ward i times 350, where I assume each resident spends 15 days outside London.[15]

Airbnb penetration before 2008 is set to zero, as the platform was founded in 2008 in San Francisco. Web scraped data start in 2013, which is the first year Airbnb presence become relevant in London and in most of the popular destinations (see *e.g.* Garcia-López et al., 2019). However, I can recover Airbnb penetration before 2013 by looking at the number of reviews ever received by the listings in 2013, conditional on the listing not being removed from the platform. Results are analogous when restricting the sample to 2013-2019, which still represents the longest panel of Airbnb presence in the literature.

[13] I fix the boundaries at 2011 electoral wards. See Appendix Section 1.A.1 for details on London administrative structure.

[14] I assign each listing based on latitude and longitude. Even if Airbnb alters the exact location by a factor ranging between 0 and 150 meters, given the size of each ward the number of wrongly assigned listed is neglectable. Since guests have 14 days maximum to fill a review, whose time of filling is, therefore, representative of the period of the visit.

[15] Here and in the rest of the paper when considering per residents measure I fix local population at its 2007 level (source: Office of National Statistics).

This measure of Airbnb penetration captures the intensity of Airbnb tourism with respect local population and it departs from previous literature, Garcia-López et al. (2019), Barron et al. (2020), Almagro and Domínguez-Iino (2020), Duso et al. (2020) or Koster et al. (2019), which studies housing market outcomes using the number of properties listed on Airbnb website. This approach has two main advantages: i) since it considers only actual reviews, it automatically excludes listings present on Airbnb website but not active; ii) it represents more precisely the number of tourists in the area, as it takes into account the size of the flat and duration of stay.

To further explore mechanisms and heterogeneity of my results, I identify families using Airbnb using keywords (*e.g.* ”children”, ”wife”, etc.) in the review content. Moreover, I distinguish among guests renting a room or renting an entire property. Finally, following Tzioumis (2018) I assign an ethnicity based on first name guests' ethnicity using the first name of the reviewer.

In Figure 1.8.1 I plot the geographic distribution of *Airbnb penetration* in 2019, where areas more (less) exposed are denoted in red (yellow), and bins are defined according to 2018 quintiles. Exposure is decreasing with distance from the city centre, except for a cluster at the extreme west denoting Heathrow Airport area. In 2013 (see Appendix Figure 1.C.2) Airbnb was more concentrated in the city centre and surrounding areas were overall less exposed.[16] Conversely, an analogous measure of hotel penetration for 2019 (Appendix Figure 1.C.4) shows a higher concentration in fewer locations, mainly in Centre-West London (Westminster and Chelsea area).[17]

1.2.2 Outcomes of interests

Complaints against tourists I measure backlash against tourism with the number of complaints against tourists per resident. I web scrape “FixMyStreet”, an online service where residents can submit geolocalized complaints which are forwarded to the local authority in charge. Users can comment on each complaint, and I count each comment as a separate complaint when building my measure. Data collected span the period 2007-2019 and contain around 1.3 millions complaints (included comments) in 17 categories.[18] I identify complaints against tourists from the description associated with each complaint if specific keywords were used (*e.g.* ”tourist”, ”Airbnb”, etc.).

Classic measures of backlash against specific groups, such as political support or newspaper articles (Tabellini, 2020, Dustmann et al., 2019 or Colantone and Stanig, 2018) are non-applicable in this context given the granularity of the analysis. Differently from measures of backlash proposed by the political economy of discontent (*e.g* vote shares) or measures of net welfare change proposed by the urban economics literature (*e.g* house prices), my outcome variable can capture the “voice of losers” and those unhappy with the status quo, even when these groups represent a minority, whose discontent would not be captured by previously cited measures. An additional key advantage of my measure is that it consists of actual complaints, and it does not require any sentiment analysis to infer a negative attitude towards tourists.

Housing market A permanent shift in housing supply may induce an increase in house prices and long-term rents, which is a potential source of discontent by residents. This is why I collect data on both house prices and long-term rents.

Data on house prices are from UK Land Registry, which reports the details of the universe of transactions

[16] While the Olympic Games 2012 and years before saw a very modest presence of Airbnb, a turning point event is also represented by the acquisition of London-based rival CrashPadder.

[17] In Appendix Section 1.B.2 I provide a detailed description on how I recover the number of hotel tourists nights.

[18] In Appendix Table 1.C.3 I report the 17 categories. Top 4: Rubbish (23.0%), Road Status (21.4%), Fly-tipping (18.0%), Green Area Status (8.8%). FixMyStreet was founded in 2007, see Appendix Figure 1.C.5 for aggregate take-up rates.

from 1995 to 2020. I match each transaction with Energy Performance Certificates to recover the size of the property, which enables me to compute the median price per square meter in ward i and year t. To guarantee representativeness, I exclude ward-year observations with less than 10 transactions.

Rent data come from Urban Big Data Centre (UBDC), with primary source being Zoopla, a popular UK property comparison-online platform. I construct the median long-term rent price for ward i in year t from 2011 to 2016.[19] Both house and long-term rental prices are adjusted using CPIH index (2015=100, source: Office for National Statistics).

Neighbourhood quality An alternative channel that may explain the observed backlash against tourists is represented by a drop of neighbourhood quality. I consider three measures of neighbourhood quality: i) a proxy for congestion, ii) the number of complaints from residents, and iii) anti-social behaviour crime rates.

I build the proxy for congestion by using Transport for London (TFL) data on all entries and exits from underground stations in each ward-year by resident population as-of 2007.[20]

As for complaints, I start from the measure described above, looking at complaints related to the local area quality, instead of that on tourists. Complaints are divided into 17 categories, and I construct the number of complaints by the resident population in 2007 in ward i in year t for each category. I further distinguish among three main types. First, complaints *susceptible to the presence of tourists and to a change in residents' behaviour* (complaints about rubbish, fly-tipping or flyposting). Second, complaints *susceptible to change in residents' behaviour* but not to the presence of tourists (complaints about car parking or dog fouling) would act as a proxy of civic engagement. Third, I consider complaints about the roads' status and green areas' status. I use this category of complaints as a *placebo group*, to verify that it is not the case that i) residents are complaining more in general, or that ii) local authorities are not investing at all in very touristic areas.

Finally, I consider the number of anti-social behaviour crimes per 2007 residents at the ward-year level. Anti-social behaviour is defined by the police as "behaviour by a person which causes, or is likely to cause, harassment, alarm or distress to persons not of the same household as the person" (Anti-social behaviour Act 2003 and Police Reform and Social Responsibility Act 2011). The key difference with the previously-described complaints measure is the non-subjective nature of crime rates, which are derived from official reports, hence verified by Police officers, and are less affected by residents' biased reporting nuisances.[21]

Social capital Deteriorating quality of local amenities may reduce social capital and reduce residents' support for globalization. Following Guiso et al. (2016), I measure social capital as the number of charitable organizations per resident population in 2007.[22] Similarly, to capture how local networks within the neighbourhood evolve, I consider the number of youth and political organizations per 2007 residents. The source of these data is the "Point of Interest" dataset (2011-2019) from Digimap (2020), which reports the

[19] Zoopla reports advertised, not realized, rent prices. Original data are reported at MSOA-quarter level. Mapping from MSOA, an alternative geographic classification, to wards is described in 1.B.1. I compute the average within year of quarterly data.

[20] For wards not containing a station but with stations within 500 meters I consider the distance squared weighted average number of entries/exits for all stations within 500 meters from ward boundaries.

[21] Original data are provided at the month-MSOA level. I thank CEP Community - Crime group for sharing the data with me. Appendix Section 1.B.3 presents detailed definitions of anti-social behaviours.

[22] Another natural variable to consider as a proxy of social capital would be voter turnout. Three issues prevent me to use it. First, national elections results are not available at my unit of analysis (*i.e.* ward) but only at the constituency level. There are 73 parliamentary constituencies in Greater London: with only 5 general elections from 2002 onwards, I suffer from small sample biases. Second, local elections (for which we have results at ward level) are not representative. Third, electorate reported is an endogenous variable as, to vote, individuals have to register. Information on the number of people meeting criteria to be able to register is not available at the ward-year level.

exact coordinates and a precise sector categorization.[23]

Political outcomes To capture anti-globalization sentiments I leverage on the 2016 EU "Brexit" Referendum. The Brexit Referendum has been widely associated with globalization sentiments (Colantone and Stanig, 2018) and political dissatisfaction (Fetzer, 2019). BBC manually collected results at ward level, as official sources report data only at local authority level.[24] Even though a subset of local authorities is represented (14 over 33), the data feature a satisfactory level of geographic representation (see Appendix Figure 1.C.7).

1.2.3 Demographic and geographic variables

I complement my dataset with various demographic variables. I collect the share of workers by sector and the share of workers by occupation (2001 Census); the share of residents by ethnicity, the share of residents by nationality, the share of residents by educational attainment, the share of homeowners (2011 Census). All variables are collected at Output Area level, which maps uniquely into wards. I also collect data on population by age group at the ward-year level from 2002 to 2018 (Office National Statistics). Caveats on population counts may apply in this context as ONS can only provide estimates from secondary sources (see Suárez Serrato and Wingender, 2016 for an example of mismeasurement in population estimates in no-Census years). To mitigate these concerns I also collected information on the median electricity consumption at ward level as an alternative proxy of population (source: UK Government, Department for Business, Energy & Industrial Strategy; 2013-2018).

Moreover, taking advantage of GIS software, I compute the following measures of "centrality" for each ward: the distance from each ward centroid to Charing Cross, which is considered the London city centre; the distance from each ward centroid to London 2012 Olympic Games venues, as London 2012 Olympic Games involved major renewing of certain areas; the distance from each ward to the closest underground station, as the underground network represents a crucial characteristic of London structure and it is a proxy of how well connected to other locations a ward is.

Finally, I collect public data on schools to check whether ward population composition is changing over time. At the school level, I collect data on the share of pupils in ward i and year t for which English is not the first language and the share of pupils entitled to free meals. Both variables are provided by the UK Government and available for the period 2011-2019. Schools in London are highly competitive and almost all of them run admissions locally, considering small catchment areas. Pupils enrolled in a school can be considered a reliable proxy of the wards' pupil population.

1.3 Empirical Strategy

To study the social and economic effects of Airbnb penetration, I estimate the following model:

$$Y_{ibt} = \beta Airbnb\, Penetration_{ibt} + X_{it}\gamma + \eta_i + \delta_{bt} + \epsilon_{ibt} \tag{1.3}$$

where Y_{ibt} represents the outcome of interest in ward i, local authority b and year t, and *Airbnb Penetration* is the measure described in section 1.2.1. X_{it} is a rich set of interactions between year dummies and 2001 share of workers by sector, 2001 share of workers by occupation, 2001 log of house prices per square meter, distance from ward centroid to Charing Cross, distance from ward centroid to the closest London 2012

[23] There are 620 sectors and 9 categories. I thank Dr Lindsay Relihan, Nick Groome and Ordinance Survey team for their support with this data.

[24] BBC (2017), *Local voting figures shed new light on EU referendum*

venue, distance from ward boundaries to the closest underground station. I also include ward i fixed effects (η_i) and local authority b time trend (δ_{bt}). Inclusion of local authority specific time trend is important given the peculiarity and autonomy of each local authority. This rich set of fixed effects implies that β is estimated from changes in Airbnb penetration within the same ward over time, compared to other wards in the same local authority in a given year and compared to wards with similar pre-determined and geographical characteristic in a given year.

Standard error computation follows Conley (1999), Conley (2010) and Hsiang (2010). I consider a spatial correlation parameter of 14 km and a serial correlation parameter of 10 years.[25]

Two opposite forces may be at force to bias results. On the one hand, we may expect Airbnb penetration to be higher in wards becoming more attractive due to local amenities and in higher quality neighbourhoods. On the other hand, Airbnb penetration might settle in otherwise declining wards, where residents and long-term renters do not want to live. The concern is that, despite the rich set of controls, any time-varying unobservable variation included in ϵ_{ibt} that correlates both with Airbnb penetration and the outcome of interest will lead to biased OLS estimates for β in equation (1.3).

To address these concerns, I instrument Airbnb penetration following a shift-share IV strategy as in Garcia-López et al. (2019), Barron et al. (2020) or Almagro and Domínguez-Iino (2020). The "share" part of the IV exploits spatial variation from the spatial distribution of historical monuments and buildings per square kilometres, as their presence represents an attractive feature for tourists. The "shift" part exploits time variation in the worldwide popularity of Airbnb as proxied by the Google search volume for the word "Airbnb".[26]

$$Airbnb\,Penetration_{it} = Historical\,Sites_i \times Google\,Trend\,Airbnb_t \tag{1.4}$$

The exclusion restriction can be expressed as follows. Both factors are orthogonal to unobservable ward temporal variation ϵ_{ibt}, conditional on covariates and fixed effects. First, I do not expect worldwide Airbnb popularity to be informative of ward specific unobservable trends. Second, I assume that determinants of the spatial distribution of monuments from hundreds of years ago are not informative of current trends that may affect the outcome of interests.

Similarly, we can say that the key identifying assumption behind the instrument is that wards with a higher number of historical monuments must not be on different trajectories for the evolution of economic and social conditions in subsequent years (see also Goldsmith-Pinkham et al., 2020 and Borusyak et al., 2020). This assumption can be violated if the characteristics of wards with the higher number of historical monuments had persistent confounding effects on tourism patters as well as on changes in the outcomes of interest.

I deal with this concern in two different ways. First, I show that the pre-period change in outcomes of interest is uncorrelated with subsequent changes in Airbnb penetration predicted by the instrument (Appendix Section 1.D.1). Second, in my baseline specification, I control for interactions between year dummies and several 2001 wards characteristics and proxies of "centrality" that might be linked to a higher number of

[25] Parameters choice follows from the fact that the radius of the median local authority would be 2 km if they were perfect circles. This implies that I am assuming that spatial correlation vanishes 3 complete local authorities from each ward centroid. For the autocorrelation parameter, I consider 10 years as Airbnb started in London in 2009. Note that Greene (2018) recommends at least $T^{0.25}$, even considering the longest panel (2002-2019) I am being more conservative. Results are similar by changing parameters value and by considering clustering at local authority level as described in Appendix Section 1.D.7 and reported in Appendix Figure 1.E.5.

[26] In Appendix Figure 1.C.8, I plot the geographical distributions of historical monuments and buildings, notably they are not only concentrated in the city centre. In Appendix Figure 1.C.9, I plot the time evolution of trend for the word "Airbnb" according to Google, denoting a stable growth over time.

historical monuments and may have had a time-varying effect on economic and social conditions across wards.

In terms of instrument relevance, in all my specifications, I obtain a strong first stage relation. Table 1.8.1 presents first stage results for the relationship between Airbnb penetration and my instrument. Kleibergen-Paap F statistic for weak identification, using the described spatially-corrected standard errors, is reported. In Column 1 I consider only ward and year FE. In column 2 I introduce local authority flexible time trends while, in Columns 3 and 4, I progressively include the set of controls interacted with year fixed effects. Column 4 reports my baseline specification. In all cases, the F-stat is well above 10, and there is a strong and significant relationship between Airbnb penetration and the instrument proposed. Appendix Section 1.D.1 further explores the robustness of this empirical strategy.

1.4 Impact of Airbnb on the neighbourhood

This Section outlines the first set of contributions of this paper. First, Airbnb penetration is associated with backlash from residents (Section 1.4.1). Second, I study what the causes of observed backlash against tourists are in relation to Airbnb penetration, in particular looking at pecuniary and non-pecuniary channels. While house and long-term prices react only marginally in the London context (Section 1.4.2), Airbnb penetration is associated with a decrease in neighbourhood quality (Section 1.4.3).

1.4.1 Backlash against Tourism

Abundant anecdotal evidence suggests that the increase in short-term renting in London has fueled residents' discontent. Airbnb, as one of the earliest and most widespread players, has been often accused to foster "touristification" and "killing" city centres.[27] Motivated by this discussion, in Table 1.8.2, I study the effect of Airbnb penetration on the number of complaints against tourists per person received by local authorities. Throughout the paper, Panels A and B always present, respectively, OLS and IV estimates. I also report the KP F-stat for weak instruments and years considered for each specific outcome variable.

Column 1 and 2 of Table 1.8.2 report the effect on the baseline measure of complaints against tourists, the log of complaints per 2007 residents. The coefficient in Column 2, Panel B, implies that an increase of one Airbnb tourist every 1000 residents increases complaints against tourists by 2.2%, while over the 2013-2019 period complaints against tourists grew on average by 7%. The positive relation between Airbnb penetration and complaints against tourists is invariant to the exclusion of the rich set controls interacted with year fixed effects described in Section 1.3 (Column 1), to the use of a logarithm version of the penetration measure (Column 3), and the result still holds when looking at i) a dichotomous dependent variable taking value one if there has been at least one complaint against tourists in the ward i in year t (Column 4), or ii) including in the complaints measure only the original complaint and not all the subsequent comments to it (Column 5).

I consider an increase of one more tourist using Airbnb every 1000 residents which represents the median growth the period 2013-2019. The growth of Airbnb has been a common phenomenon across all wards (see Figure 1.8.1) but with substantial heterogeneity, with central London experiencing a growth ten times larger than the median ward. An increase of one tourist every 1000 residents can also be interpreted as one more tourists within 150 meters when taking into account London density.

This result confirms that backlash against tourism and Airbnb presence are linked. This is particularly valuable in a setting where considerable heterogeneity in the distribution of gains and losses is present.

[27] Financial Times, September 2019, *Are Airbnb investors destroying Europe's cultural capitals?*; The Guardian, May 2019, *How Airbnb took over the world.*

This would not be possible using more standard measures typically used in the political economy (*e.g* vote shares) or in the urban economics (*e.g* house prices) literature, as they would capture just a net effect. My outcome variable, on the contrary, can capture the "voice of losers" and those unhappy with the status quo, even when these groups represent a minority, whose discontent would not be captured by previously cited measures.

Finally, it is worth commenting on the fact that IV estimates are stronger in magnitude than OLS. The downward bias of OLS can be rationalized by i) omitted factors are negatively related to the number of complaints, and ii) locations of rising Airbnb are positively selected. The most likely omitted factor is a positive trend experienced by certain neighbourhoods, which is likely to reduce overall complaints. I provide suggestive evidence on positive selection of popular Airbnb neighbourhoods by showing that average 2013 amenities are higher in areas in the top quartile of 2019 Airbnb penetration than in the bottom quartile (Appendix Table 1.E.3).[28]

Motivated by this evidence I proceed in my analysis and study the roots of such backlash. There exist at least two explanations for this finding. First, discontent might arise from the impact that a permanent shift in housing supply from long- to short-term rent has on prices, which is a channel I explore in the next Section. Second, the high turnout of tourists in residential areas might affect the quality of the neighbourhood, which I discuss in Section 1.4.3.

1.4.2 Housing market

So far, the literature has focused on the impact of Airbnb on the housing market, looking at house prices and long-term rents. The documented positive effect of Airbnb on both prices was then, anecdotally, linked to the backlash received by Airbnb in many popular destinations. It is then natural to start my analysis of the potential roots of the previously described backlash from these outcomes. Results are presented in Table 1.8.3. Interestingly, when looking at the IV specification with full controls (Panel B), I find no statistically significant effects of Airbnb penetration on both house prices and long-term rents.

In Column 1, I consider the impact of Airbnb penetration on the log of the median house price per square meter. OLS estimate implies a statistically significant increase in house priced by 0.2% for every additional tourist by 1000 residents, but the effect becomes statistically not different from 0 when looking at IV specification. Over 2013-2019 the median ward experienced a 30% growth rate in house prices, suggesting that Airbnb has only a limited impact, if any, on house prices in London. In column 3, I study the effect of Airbnb penetration on house prices without any rescaling by house size. Results are fully consistent. This also confirms the fact that previous evidence is not driven by the fact that, when presenting results for price-per-square-meter, I am just focusing on properties for which I can retrieve property size matching transaction data with energy certificates.[29]

Similarly, I find no effect of Airbnb penetration on long-term rents ask prices, which I measure in Column 4 by the median rent at the ward level. This is very interesting considering that the median ward experienced an 8 per cent growth rate in long-term rents over 2013-2016.

These results depart from previous literature, which found a positive and significant effect of Airbnb pene-

[28]Similar results holds if I consider 2007. The choice of setting 2013 as "initial period" is based on the fact that before 2013 Airbnb penetration was limited, as reported also in Garcia-López et al. (2019), and on data available as outcomes of interest.

[29]Consistent with above discussion when looking at house prices, OLS suffers from an upward bias. A positive trend in neighbourhood quality in certain neighbourhoods is positively correlated with both house prices and Airbnb penetration, which positively biases my estimates for β. This was not the case for recent evidence for Amsterdam in Almagro and Domínguez-Iino (2020) or for the entire US in Barron et al. (2020), which find downward biased OLS estimates.

tration on house prices and rents. However, my findings are robust to using alternative measures of Airbnb penetration closer to the ones proposed by the literature. In column 2 and 5, I look at the impact of the number of entire Airbnb properties over the number of dwellings in 2011 (see Appendix Section 1.B.2 for details on how I construct this measure) on house prices and rents, respectively. Consistently with the literature, the magnitude of the coefficients using this alternative measure is bigger, but estimates remain not statistically different from 0. This suggests that an additional explanation for this discrepancy may rely on how standard errors are computed, as I explicitly allow for correlation over space and time. As described in Appendix Section 1.D.7, just clustering at ward or local authority level may deliver standard errors too narrow.

Just looking at house prices in London, and assuming house prices internalize all benefits and costs for residents, we may be tempted to conclude that Airbnb has no effect on the welfare of residents or, looking at previous studies, that Airbnb has a positive effect. Heterogeneity of the impact of Airbnb across population subgroups is key to reconcile such zero effect with the rise of backlash, which I documented above in the paper and which is in line with what trade or migration and trade literature has found (*e.g.* Tabellini, 2020, Autor et al., 2020). House prices just capture an average effect: while few homeowners benefit from the rise in house prices values, many long-term renters are paying the cost.

Moreover, among Airbnb hosts in London, only half are reporting to live in London and around 3 per cent controls 43 per cent of all Airbnb listings in 2019, suggesting a high level of professionalism which may contribute to a rising inequality not captured by measures such house prices.[30]

To unveil such inequality, it is crucial to explore alternative measures of residents' welfare, such as neighbourhood quality and congestion, which is what I do in the next subsection.

1.4.3 Neighbourhood quality

In this Section, I explore the impact of Airbnb on neighbourhood quality as a source of backlash against tourists. This effect may arise for two key distinctive characteristics that differentiate short-term renting from hotel accommodations.

First, Airbnb supply can adjust almost immediately to market demand (Farronato and Fradkin, 2018).[31] As a consequence, local services, which face higher adjustment costs, may fail to timely react, causing a drop in overall neighbourhood quality and higher congestion. I refer to this effect as to a *quantity* externality.

Second, neither hosts nor Airbnb monitors guests during their visits.[32] An absent host may induce: i) negative behaviours, ii) negative selection on the type of guests as they may want to take advantage of the absence of monitoring, and iii) more disruptive behaviours not only when in the property but also in the local area due to the lack of any verification procedures.[33] I call this a *quality* externality.

Congestion In Column 1 of Table 1.8.4, I provide evidence regarding the first type of externality. An increase of 1 tourist per 1000 residents increases the number of entries and exits per resident by 0.6 per cent, which is sizable given the almost constant average usage of underground services over the last decade.

[30] I define "professional" every host that manages more than 5 listings.

[31] Moreover central planner has no control over where Airbnb properties will be, this is not the case in the hotel industry, which is often heavily regulated. See Sections 55 and 57 of the Town and Country Planning Act 1990 for further details

[32] Airbnb advertise its service saying that guests can "live like a local" and "feels like at home".

[33] "Airbnb Party flats" are a well know issues, *e.g.* The Guardian (2017) *It sounded like Fabric was upstairs' - Airbnb rental used for all-night party*. Incentives on the hosts' side are limited as hosts are often offered an insurance plan by Airbnb itself to protect their flat by damages. Moreover consider that in a hotel an ID and a payment card is immediately registered, in Airbnb everything is carried online posing a question of traceability.

This result is particularly interesting in London, where underground represents the major system of transportation, with 4 millions of passenger journeys every day (Larcom et al., 2017).

Entries and exits may not be representative of actual congestion if the supply of trains increases. However, using aggregated data from TFL (Appendix Table 1.C.6) I show that this is not the case, *i.e.* supply does not change, with more than half of the lines not increasing operated kilometres in 2006-2019 period, and the ones that increased their supply doing so in relation to the opening of new portions of the network, or to the start of night service.

Complaints about local area To document the *quality* externality, I report the impact on the number of complaints related to the local area quality per resident in Table 1.8.4. In Column 2 I consider my preferred measure: the log of the number of complaints about rubbish per resident. The IV specification shows that an increase in Airbnb penetration is associated with a 2.8 per cent increase in complaints, confirming a decline in neighbourhood quality. In Columns 3 and 4 I consider alternative measures, complaints regarding fly-tipping and flyposting, respectively, with similar results.

As discussed in Section 2.3, these measures are susceptible to both tourists and residents negative behaviours and in Section 1.6 I explicitly discuss how the presence of short-term tourists may reduce civic engagement by local citizens.

Finally, to be able to claim that these complaints are linked with a lower quality of local area I rule out that: i) local authorities, which are in charge of waste collection, do not have stopped investing in these areas, or ii) residents are generally complaining more. This is what I do in Table 1.8.4, where I try to look at the effect of Airbnb penetration on complaints about road status (Column 5) and green area status (Column 6), which are local amenities that are not expected to be influenced by the presence of Airbnb tourists or residents misbehaviour and can be thought as placebo measures. Consistently with my prior, I find no effects.

Anti-social behaviours crime rates To further document the *quality* externality, in Column 7 of Table 1.8.4, I document how an increase in Airbnb penetration is associated with a 2.6 per cent increase in anti-social behaviour crime rates. As described in Section 2.3, Police crime rates are a more objective measure as crimes are verified by Police officers. This helps mitigate concerns about biased reporting by residents, which instead may affect the complaints reported directly by residents. This result confirms how neighbourhood quality is negatively affected by Airbnb presence.

Comparing this result to the magnitudes usually uncovered by the crime literature, I find this is a sizable effect. In Draca et al. (2011) a 10 per cent increase in police activity reduces crime by around 3 to 4 per cent. In my context, a similar impact is obtained by decreasing Airbnb penetration by around 1.5 tourists per 1000 residents, which is close to the 60$^{\text{th}}$ percentile in the Airbnb penetration measure in 2019.

1.5 Mechanisms and heterogeneity of backlash

Once established that i) Airbnb penetration is associated with more complaints regarding tourists and ii) negative externalities on congestion and neighbourhood quality are a potential root of this backlash, I provide evidence on why the absence of monitoring causes rising backlash (Section 1.5.1), as discussed in Section 1.4.3. In addition, in Section 1.5.2, I document how Airbnb does not affect the population in London wards, ruling out the possibility that residents' movements drive my results (Section 1.5.2). Finally, in Section 1.5.3, I provide evidence about the heterogeneity of the results depending on the ethnic composition of the neighbourhood.

1.5.1 Monitoring

To back the intuition that lack of monitoring is a key factor that distinguishes short-term accommodation from hotels - and a factor that may drive negative behaviours and negative selection of guests - I present two results. First, complaints are reduced when monitoring is less important as tourists are less disruptive (families). Second, the same happens when monitoring is easier because hosts are present (room renting). I consider the following specification:

$$Y_{ibt} = \beta_1 Airbnb\,Pen_{ibt} + \beta_2 Airbnb\,Pen_{ibt} * HD_{ibt} + \beta_3 HD_{ibt} + \gamma X_{it} + \eta_i + \delta_{bt} + \epsilon_{ibt} \tag{1.5}$$

where $Airbnb\,Pen$ represents the Airbnb penetration measure described in Section 2.3 and and HD is a dummy variable capturing the heterogeneous effects of either i) families or ii) room renting. The definition of such dummy for the two cases is specifically described below. As in the main specification, I instrument Airbnb penetration with the usual shift-share instrument described in Section 1.3 and the interaction $Airbnb\,Pen * HD$ with the interaction between the instrument and the HD dummy.[34]

Families In Table 1.8.5 Column 1, I interact Airbnb penetration with a dummy equal to one when 15 per cent or more tourists are part of a family. Consistent with the idea that monitoring is an issue in the Airbnb context, and assuming families are more prone to behave properly, I find that when more families are present, Airbnb penetration is associated with 1.2 per cent fewer complaints.[35]

This result also dampens an alternative explanation of my results. One could argue that the observed misbehaviours arise due to a lack of repeated interactions among neighbours and lack of knowledge of local rules due to the high turnout of residents. However, if that was the case, we should find no differences when comparing more or less disruptive types of tourists, as what would drive the results is this lack of cohesion rather than a negatively selection. The fact that when tourists are not negative selected, assuming families properly behave, suggests, however, that this alternative explanation of lack of cohesion is less likely to hold.

Room renting On Airbnb a guest can either rent an entire property or just a room. In Table 1.8.5 Column 2, the heterogeneity dummy equals one if the share of guests-nights renting just a room is greater than 50% of the total Airbnb tourists-nights. Results show that the number of complaints in the area decreases by 1.2 per cent when most of the tourists are in rooms. This is consistent with the idea that the absence of monitoring may foster negative behaviours or negative selection and confirms that, on the contrary, monitoring by hosts prevents negative behaviours or negative selection.

An alternative explanation is that negative sorting is not driven by the absence of monitoring, but rather by lower prices in Airbnb accommodation with most disruptive tourists attracted by lower prices. Average price per night per room in an Airbnb accommodation is 70 £ while a hotel room costs, on average, 170 £ in London. At the same time, renting just a room is cheaper than renting an entire property (55 *vs.* 96 when considering per room prices). If negative selection is simply driven by lower prices we should observe more disruptive behaviours when most of the tourists are using cheaper accommodations. However, the result presented in Column 2, Table 1.8.5 suggests that, when most tourists in the area are renting just

[34] Equation (1.5) is generic. In some of results described below HD will be constant, and it will be then absorbed by ward fixed effects. This specification is extremely demanding and F-Stat of the first stage occasionally falls below 10, while when excluding the wide set of controls interacted with year fixed effects the F-stat is always above 10. Moreover, given the added complexity, F-stat is computed starting from standard errors clustered at ward level, while standard errors reported are corrected following Conley (1999), parameters considered: 14 km and 10 years.

[35] See Section 2.3 for how I defined families. The average share of families tourists-nights present in my sample in 2017-2018 is 12 per cent, while according to Airbnb (2018) families represent 14 per cent of total guests. Sample size drops because, by definition, I need Airbnb tourists to be present in the area and this may not be the case in the first years of Airbnb presence in London.

rooms, there are fewer complaints in the area.

1.5.2 Exit or complain?

A natural consequence of the decline in neighbourhood quality may be that residents decide to leave the area. These movements may drive my results if not orthogonal to Airbnb penetration, as in a "vote with your feet" framework (Tiebout, 1956). This would be certainly true in a frictionless city, however, constraints as homeownership and school enrollment may limit movements and increase complaints (Hirschman, 1970). In the data, I observe no change in ward population, as reported in Column 1 of Table 1.8.6. In Columns 2 to 5, I check whether the age composition of ward population is affected. I just observe minor changes in the age structure, with fewer residents below 18 (Column 2), and few more aged 25-34 (Column 4).[36]

Pupils composition To further verify whether neighbourhood composition is affected, I consider the impact of Airbnb penetration on i) the ward-specific share of pupils for which English is not the primary language, and ii) the ward-specific share of pupils entitled to a free meal registered in a school. In both cases, I find no statistically significant results, as reported by Columns 6 and 7 of Table 1.8.6. This is also consistent with the idea that residents with higher constraints are less likely to move. School admission is a relevant constraint to movings given that most schools in London run admission locally with small catchment areas.

Homeowners Consistent with the idea that more constrained individuals are less likely to move and file a complaint instead, I find more complaints in areas with higher homeownership. In Column 8 of Table 1.8.6, I interact, in Equation (1.5), Airbnb penetration with a dummy equal to 1 if the share of residents owning a flat according to 2011 Census is above the median London value. I find that, in wards where homeownership is above the median, each additional Airbnb tourist per 1000 residents increases complaints against tourists by 2% more than in wards below the median.

1.5.3 Cosmopolitan areas

Diversity is a recognized driver of social divisions (Easterly and Levine, 1997; Alesina et al., 1999). We may expect, in parallel with migration literature (Dahlberg et al., 2012; Tabellini, 2020), fewer complaints against tourists if i) local areas are more cosmopolitan, and ii) ethnic distance between tourists and residents is lower.

First, in Column 3 of Table 1.8.5, I show that the number of complaints is lower in areas with higher ethnic diversity. I show this by interacting Airbnb penetration with a dummy equal to one if the 2011 ward ethnic fractionation index is above the median value for London.[37] The coefficient on the interaction term is negative, and it suggests that more cosmopolitan areas are more prone to welcome tourists. The result holds also when the fractionalization index is based on the share of 2011 nationality composition of the local area (Column 4).

Second, I study the heterogeneity driven by the ethnic distance between Airbnb tourists and resident. I construct a dummy that equals one if, in a given ward-year, most of the residents are of ethnicity j and most of the Airbnb tourists are from an ethnicity different from j, for a given j.[38] Therefore the dummy takes

[36]Results are confirmed when using, as an alternative proxy, the median electricity consumption in the ward. This is important as population estimated measures from ONS may suffer from profound forecast biases, see Suárez Serrato and Wingender (2016) as an example in the US context.

[37]Following a vast literate (Alesina and La Ferrara (2005)), I consider as an index of ethnic fractionalization: $ELF_i = 1 - \sum_j sh_i^j$ where sh_i^j is the share of the ethnic group j over total population according to 2011 Census in ward i.

[38]Consider for example black as ethnicity. This ethnic distance dummy is equal to 1 if the ward i is above median

value one when the ethnic distance is wider. Table 1.8.5 Column 5 shows that diversity in composition between tourists and residents drives up the complaints as well.

An alternative explanation for the above results is that residents may be complaining about the influx of certain ethnicities, rather than about the presence and negative behaviours of tourists. Studying properly this hypothesis is appealing but goes beyond the purpose of this paper and it is an interesting avenue for future research. Literature has focused on peer-to-peer discrimination from the user perspective. Both black male hosts (Edelman and Luca, 2014) and black male guests (Edelman et al., 2017) are discriminated in the Airbnb platform. Similar results have been found regarding Uber and Lyft (Ge et al., 2020). Still very little is know on how peer economies may be related to local discrimination.

1.6 Consequences of Airbnb penetration

I have studied the roots and mechanisms of backlash of Airbnb penetration. In this Section, I study how the deterioration of local neighbourhoods due to Airbnb can have profound effects on social capital (Section 1.6.1) and attitudes towards globalization (Section 1.6.2).

Social capital has been used to explain an impressive range of phenomena (economic growth Knack and Keefer, 1997; institution's design and performance Djankov et al., 2003, etc.). Similarly, recent waves of populism have profound consequences in our societies and cities, when compared to rural areas, have largely resisted this trend (Broz et al., 2020; Rodden, 2019). It is then particularly important to shed light on the social and political consequences that the inflow of tourism through short-term renting may trigger in cities.

Evidence presented in this Section confirms the concerns of the many who oppose this new wave of tourism (the media even refer that to that as "overtourism"): such a high turnout of temporary residents is harmful for social capital formation, local networks formation, civic engagement and may favour anti-cosmopolitan sentiments.[39]

1.6.1 Social Capital

In Table 1.8.7 I study the impact of Airbnb penetration on social capital measures. In Column 1, I document the effect of Airbnb penetration on the number of charitable organizations (Guiso et al., 2016) following Airbnb penetration. Looking at Panel B, an increase of 1 Airbnb tourist per 1000 residents is associated with a drop of 2.1 per cent in the number of charitable organization per resident. Moreover, as shown by Columns 2 and 3, the number of youth organization per resident and the number of political organizations, proxies for local networks formation and civic engagement, drop by 0.6% and 0.5% respectively, when looking at full IV specification.

Residents' behaviour Consistently with the idea that Airbnb penetration may affect social capital, I also document how residents' behaviour and civic engagement deteriorate in areas more affected by Airbnb. In Columns 4 and 5 of Table 1.8.7, I report the impact of Airbnb penetration on complaints on car parking and dog fouling, which are two types of misbehaviour intimately linked to residents and unaffected by the presence of tourists.

In both cases I observe an increase of misbehaviour by residents: Airbnb penetration not only has a direct impact on neighbourhood quality due to the presence of tourists but it deteriorates social capital and

when looking at 2011 share of black residents and when the ward-year is above median when looking at the share of non-black Airbnb tourists visiting.

[39]The Guardian, January 2020, *Overtourism in Europe's historic cities sparks backlash.*

residents' civic engagement. This is in line with previous literature comparing the behaviour of home-owners and long-term renters (Sims, 2007) and it is even more salient if short-term renters are present. In neighbourhoods in which local networks are weaker, the willingness to contribute to the local area is lower (Putnam, 1993).

1.6.2 Brexit

Finally, I directly explore the political consequences or Airbnb penetration. In Table 1.8.7, I study its impact on the share of citizens supporting Brexit at the 2016 EU Referendum. Given the nature of the referendum, I cannot exploit the panel structure of my data but the empirical strategy remains similar. I estimate the following equation:

$$Y_{ib} = \beta Airbnb\,Penetration_{ib} + \gamma X_i + \delta_b + \epsilon_{ib} \tag{1.6}$$

where Y_{ib} is the vote share of leave option in 2016 Brexit EU referendum, $Airbnb\,Penetration_{ib}$ represents the Airbnb penetration in 2016, X_i is a set of ward level characteristics, and δ_b are local authority fixed effects. I instrument $Airbnb\,Penetration_{ib}$ with the shift-share IV described in Section 1.3 and I progressively increase the set of controls.

In Column 6, I include local authority fixed effects, the distance from the ward centroids to Charing Cross, and the distance from ward polygon to the closest underground station. Moreover, I control for the 2011 share of residents with a university degree, and the 2011 share of the population over 65, as these two factors have been shown to be determinant in predicting Brexit support. I find a positive and significant coefficient on both OLS and IV specifications.

In Column 7, I include the 2011 share of residents from the United Kingdom, 2011 share of residents from the European Union, and 2011 share of residents from the Commonwealth countries and the 2011 share of white residents. Looking at Column 7, an increase of 1 Airbnb tourist per 1000 residents is associated with an increase of 0.59 percentage points for Brexit support. The magnitude of this effect is quantitatively very relevant: it is equivalent to the impact of an increase by 1 percentage point of the share of university graduates, which is commonly recognized as a key factor in determining Brexit support.

A potential concern of this result is that, given its cross-sectional nature, I cannot control for fixed characteristics of the neighbourhood. Nevertheless, I control for all the major factors that the literature has described to be relevant for Brexit support: ethnic, age, and education composition (Colantone and Stanig, 2018; Fetzer, 2019), which takes care of most of the identification constraints.

Future research should take advantage of individual or household level surveys to study in even greater depth the impact of Airbnb penetration on perceptions over local neighbourhoods, trust in people, civic engagement, and political views.

1.7 Robustness

A number of robustness checks to the main results and preferred specifications have already been presented in the text. In this Section, I present additional checks and I refer to Appendix Section 1.D for further details.

In Appendix Section 1.D.1 I discuss the robustness of first stage results reported in Column 4 of Table 1. First, I consider an alternative measure for historical sites, using historical buildings from Historic England, and an alternative measure for the shift component, using Google trend for "Airbnb London". Second, I test alternative specifications for the first stage. Third, I construct alternative measures of Airbnb presence. Appendix Table 1.E.2 shows that results are robust in each of these cases. Finally, I verify the robustness of

Column 4, Table 1.8.1 by i) modifying the starting year of analysis and ii) excluding one local authority at the time.

In Appendix Section 1.D.2, 1.D.3 and 1.D.4, I replicate the OLS and IV specifications of Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) using measures of Airbnb penetration built in different ways. Results are robust. First, I show that results are similar if I consider the number of beds rather than the number of people a property can accommodate in the expression for Airbnb Penetration. This is done to tackle the concern that the number of people a property can accommodate may be inflated with hosts not providing proper accommodation for each guest (Appendix Table 1.E.4).

Second, I check that my results are not driven by the fact that data before 2013 have been imputed conditional on properties being still listed on Airbnb. When restricting the sample to 2013 onwards, all results are robust, except for results on underground congestion, for which, however, I have a very short panel dataset (Appendix Table 1.D.3).

Third, I consider as an alternative measure of Airbnb penetration the number of entire properties listed on Airbnb over the number of dwellings, in line with previous literature (Garcia-López et al., 2019); Barron et al., 2020). My results do not change (Appendix Table 1.E.6).

In Appendix Section 1.D.5, I discuss whether my identification strategies (both OLS and IV) captures waves of hotel tourism rather than Airbnb tourism. I provide various evidence to show that it is not the case. First, hotel industry penetration is almost constant, therefore, fully absorbed by ward fixed effects. Second, controlling for flexible trends (by the local authority or by "central" wards) ensures that common trends are captured. Third, the instrument proposed does not predict hotel tourism penetration (see Appendix Section 1.B.2 for details on how I construct this measure). Fourth, adding hotel tourism penetration as a regressor alters neither the significance nor the magnitude of my results (Appendix Table 1.E.7).

In Appendix Section 1.D.6, I explicitly take into account the seasonality of tourism flows by considering a monthly version of Equation (1.3). Results for complaints measures and anti-social behaviour crime rates - the only outcome for which I can credibly estimate a monthly regression - are presented in Appendix Table 1.E.8. All results are in line with the baseline yearly specification, suggesting that even when taking into account monthly trends, Airbnb penetration is still associated with more complaints and a drop in neighbourhood quality.

In Appendix Section 1.D.7 I discuss parameter choice (14 km and 10 years) for standard errors correction following Conley (1999), Conley (2010) and Hsiang (2010). Results are similar by changing parameter values and by considering clustering at ward or local authority level. However, not taking into account flexible spatial and time correlation (*i.e.* just clustering) may deliver standard errors too narrow, with the consequence of not significantly different from zero estimates being wrongly interpreted (Appendix Figure 1.E.5).

Finally, in Appendix 1.D.8, I discuss multiple hypothesis testing procedures that I run to ensure that my results are not false rejections of the null of no statistical significance. Results are presented in Appendix Table 1.E.9. Irrespective of the method considered, I am reassured that I find that p-values computed using standard procedures are unaffected by potential problems arising due to multiple hypothesis testing.

1.8 Conclusions

In this paper, I examine the backlash against tourists, a phenomenon which has increased dramatically across European cities in recent years, and I interpret as a form of urban backlash against globalization.

Before Covid19 pandemic, tourism flows management was at the forefront of the political debate, and tourists met increasing opposition on both economic and social grounds in many popular locations, especially in Europe. In this paper, I exploit variation in the number of Airbnb tourists received by London neighbourhoods between 2002 and 2019 to jointly study the consequences of mass short-term tourism inflow.

Using a panel dataset with unique features in terms of information richness and spatial disaggregation, and demanding OLS and IV specifications, I find that Airbnb tourism triggered hostile reactions. Exploring the causes of such backlash, I provide evidence that resident backlash is unlikely to have only pecuniary roots, as the impact of Airbnb penetration on the housing market and long-term rents is, on average, limited.

The main driver of backlash is, instead, declining neighbourhood quality. I find that Airbnb penetration increases congestion of the underground system, complaints by residents on the local area, and anti-social behaviour crime rates. Exploiting variation in the type of tourists and type of accommodations chosen, I document that residents' backlash is lower if tourists are less "disruptive" (*i.e.* families) or more monitored (*i.e.* hosts are present as they rent just a room), suggesting that lack of control as a potential key difference between Airbnb and hotel tourism.

In terms of long-term consequences, findings show how a higher Airbnb penetration is associated with decreasing social capital, lower civic engagement, and larger support for anti-globalization views. These results are particularly important given the urban context studied, with cities that have largely resisted populist trends (Broz et al., 2020; Rodden, 2019), and open up the way for a new avenue of research.

These set of results reconcile with the vast literature that has studied the rising backlash against globalization (see Mudde and Kaltwasser, 2017; Margalit, 2019 and Guriev and Papaioannou, 2020 provide thorough reviews of the existing literature). Tourism, as international trade and immigration, despite a beneficial economic impact may trigger opposition due to non-pecuniary roots. The fact that Airbnb penetration induces a drop in social capital and foster anti-globalization sentiments confirms that it can have profound effects on social dynamics and political views.

My results suggest that monitoring is crucial to guarantee future sustainable development of the touristic industry and to avoid disruptive consequences for residents.

Future research should engage in a formal comparison between rural and urban areas, both within the UK and across countries, studying cities that have been most affected by Airbnb penetration. The cross-cities comparison becomes particularly relevant to get a deeper understanding of the link between tourism, populism and social capital, described for the first time in this paper.

At the same time, a separate analysis should address the inequality implications of the fact that Airbnb benefits accrue to a few homeowners while most residents are paying the costs. Finally, studying in greater detail the difference among types of tourists and the selection induced by short-term renting is a challenging area of research. Even in the post Covid19 era, it will be important to regulate tourists flows to make sure that the latter will not disproportionately redirect to destinations that are not well-prepared to welcome a significant mass of people. Airbnb and low costs flights allow high flexibility in location choices and this is why these are important phenomena to monitor.

Tables

Table 1.8.1: First Stage

	Airbnb Penetration (x1000)			
	(1)	(2)	(3)	(4)
Historical Sites x	1.331	0.712	0.538	0.527
Google Trend/100	(0.142)***	(0.085)***	(0.076)***	(0.075)***
Observations	11232	11232	11232	11232
R-Squared	0.553	0.762	0.804	0.808
F-Stat FS	88.0	70.0	50.1	49.3
Ward FE	X	X	X	X
Year FE	X			
LLA x Year FE		X	X	X
Vars 2001 x Year FE			X	X
Geo x Year FE				X
Years	2002-2019	2002-2019	2002-2019	2002-2019

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. *Historical Sites* is the number of historical sites per km^2 (source: Digimap, 2020) and *Google Trend* represents the worldwide search volume of the word "Airbnb" in Google. In Column 1 I include year and ward fixed effects. In Column 2 I include ward fixed effect and local authority time trends. Column 3 adds to Column 2 the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter. Column 4 adds to Column 3 the interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** p<0.01; ** p<0.05; * p<0.1.

Table 1.8.2: Backlash against tourists

	ln(Complaints against tourists per resident (x1000))		Complaints against tourists per resident (x1000)	At least one complaint against tourists per resident	ln(Complaints against tourists per resident (x1000)) - No comments
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS					
Airbnb Penetration (x1000)	0.008	0.008	0.022	0.006	0.006
	(0.004)**	(0.004)**	(0.011)*	(0.002)***	(0.003)**
Panel B: IV					
Airbnb Penetration (x1000)	0.017	0.022	0.074	0.021	0.017
	(0.009)*	(0.012)*	(0.039)*	(0.010)**	(0.009)*
Observations	8112	8112	8112	8112	8112
F-Stat FS	66.8	43.7	43.7	43.7	43.7
Ward FE	X	X	X	X	X
LLA x Year FE	X	X	X	X	X
Vars 2001 x Year FE		X	X	X	X
Geo x Year FE		X	X	X	X
Years	2007-2019	2007-2019	2007-2019	2007-2019	2007-2019

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100*. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Dependent variable in Columns 1 and 2 is the log of complaints against tourists per 2007 residents. In Column 3 I consider the complaints against tourists per 2007 residents. In Column 4 a dummy equal to 1 if at least one complaint was lifted against tourists in the ward-year. In Column 5 I exclude from the number of complaints the comments received by the original complaint. To avoid taking the log of a zero, one is added to the number of complaints before taking logs in Columns 1, 2 and 5. In Column 1 I include ward fixed effect and local authority time trends. Columns 2 to 5 add the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to closest underground station. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** p<0.01; ** p<0.05; * p<0.1.

Table 1.8.3: Housing Market

	ln(Median house price per sqm)		ln(Median house price)	ln(Median rent)	
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS					
Airbnb Penetration (x1000)	0.002		0.009	0.004	
	(0.001)**		(0.001)***	(0.002)*	
Entire properties		0.008			0.014
over dwellings (x100)		(0.002)***			(0.008)*
Panel B: IV					
Airbnb Penetration (x1000)	-0.003		0.006	0.003	
	(0.002)		(0.006)	(0.005)	
Entire properties		-0.014			0.018
over dwellings (x100)		(0.009)			(0.032)
Observations	11231	11231	11231	11231	3514
F-Stat FS	46.1	19.5	47.5	47.5	18.0
Ward FE	X	X	X	X	X
LLA x Year FE	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X
Geo x Year FE	X	X	X	X	X
Years	2002-2019	2002-2019	2002-2019	2011-2016	2011-2016

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Columns 2 and 5 consider as a measure of Airbnb presence the number of entire properties listed on Airbnb over the number of dwellings in 2007. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100*. Dependent variable in Columns 1 and 2 is the log of median house prices per m^2 (transaction price, source: UK Land Registry). In Column 3 I consider log of median house prices. In Columns 4 and 5, I consider the average of the quarterly median monthly asking rents (source: Zoopla from UBDC). In all columns, I include ward fixed effect and local authority time trends, the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to the closest London 2012 venue and distance ward to closest underground station. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** $p<0.01$; ** $p<0.05$; * $p<0.1$.

Table 1.8.4: neighbourhood quality

	ln(Entry and exit in tube per resident)	ln(Complaints per resident (x1000)) Rubbish	Fly-tipping	Flyposting	Road status	Green area status	ln(Anti Social behaviour per resident (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Airbnb Penetration (x1000)	0.003	0.014	0.007	0.007	0.006	0.000	0.004
	(0.001)***	(0.004)***	(0.005)	(0.004)	(0.004)	(0.002)	(0.001)***
Panel B: IV							
Airbnb Penetration (x1000)	0.006	0.029	0.028	0.048	0.010	-0.004	0.026
	(0.003)*	(0.015)**	(0.014)**	(0.020)**	(0.014)	(0.005)	(0.007)***
Observations	3640	8112	8112	8112	8112	8112	5616
F-Stat FS	24.7	43.7	43.7	43.7	43.7	43.7	36.9
Ward FE	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X
Years	2007-2017	2007-2019	2007-2019	2007-2019	2007-2019	2007-2019	2011-2019

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100*. Dependent variable in Column 1 is the log of entry and exit from underground stations in the ward per 2007 resident (source: TFL). In Columns 2 to 6, I consider the log of complaints per 2007 residents by category (source: FixMyStreet). In Column 7 I consider the log of anti-social behaviour crime per 2007 residents (source: Police statistics). All columns include ward fixed effect, local authority time trends and the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station. To avoid taking the log of a zero, one is added to the dependent variables before taking logs in Columns 2 to 6. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** p<0.01; ** p<0.05; * <0.1.

Table 1.8.5: Mechanism - Monitoring and Inclusion

	ln(Complain against tourists per person (x1000))				
	(1)	(2)	(3)	(4)	(5)
Panel A: IV					
Airbnb Penetration x	-0.003	-0.008	-0.006	-0.032	0.006
Heterogeneity dummy	(0.002)	(0.002)***	(0.003)**	(0.018)*	(0.004)
Airbnb Penetration (x1000)	0.008	0.008	0.011	0.049	0.003
	(0.004)**	(0.004)**	(0.005)**	(0.025)*	(0.000)***
Heterogeneity dummy	0.002	0.011			0.002
	(0.004)	(0.004)***			(0.004)
Panel B: IV					
Airbnb Penetration x	-0.012	-0.012	-0.012	-0.041	0.017
Heterogeneity dummy	(0.005)**	(0.003)***	(0.005)**	(0.022)*	(0.007)**
Airbnb Penetration (x1000)	0.018	0.021	0.020	0.064	0.007
	(0.014)	(0.016)	(0.011)*	(0.032)**	(0.007)
Heterogeneity dummy	0.011	0.011			-0.010
	(0.005)**	(0.004)***			(0.006)
Heterogeneity considered	More 15 pct families	More 50 pct no flat	ELF - ethnicity	ELF - nationality	Discrepancy ethnicity
Observations	4531	4531	8112	8112	4531
F-Stat FS	6.9	9.0	9.9	9.4	9.1
Ward FE	X	X	X	X	X
LLA x Year FE	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X
Geo x Year FE	X	X	X	X	X
Years	2010-2019	2010-2019	2007-2019	2007-2019	2010-2019

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100* and its interaction with the heterogeneity dummy. Dependent variable is the log of complaints against tourists per 2007 residents. *More 15 pct families* is a dummy equal to 1 if more than 15% of tourists-nights recorded in the ward-year are assigned to a family. *More 50 pct non-entire property* is a dummy equal to 1 if more than 50% of tourists-nights recorded in the ward-year are spent not in entire property. *ELF - ethnicity* is a dummy equal to 1 if, in 2011, the ward has an ELF index (based on ethnicity) above the median. *ELF - nationality* is a dummy equal to 1 if, in 2011, the ward has an ELF index (based on nationality) above the median. *Discrepancy ethnicity* is a dummy equal to 1 if the ward i is above median when looking at 2011 share of ethnicity j residents and when the ward-year is above median when looking at the share of non j ethnicity Airbnb tourists visiting. All Columns include ward fixed effect, local authority time trends, the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station. To avoid taking the log of a zero, one is added to the number of complaints before taking logs. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** p<0.01; ** p<0.05; * p<0.1.

Table 1.8.6: Population and composition

	ln(Total Population)	Share of population 0-18	19-34	35-64	65+	Share of pupils first lang. not English	free meals	ln(Complain against tourists per person (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS								
Airbnb Penetration (x1000)	0.002	-0.072	0.103	-0.025	-0.006	0.168	0.010	0.008
	(0.001)**	(0.031)**	(0.043)**	(0.027)	(0.012)	(0.071)**	(0.031)	(0.004)**
Airbnb Penetration x								0.001
Share homeowners above median								(0.001)
Panel B: IV								
Airbnb Penetration (x1000)	0.009	-0.113	-0.100	0.211	0.001	-0.126	0.318	0.022
	(0.006)	(0.054)**	(0.155)	(0.111)*	(0.056)	(0.367)	(0.220)	(0.012)*
Airbnb Penetration x								0.020
Share homeowners above median								(0.012)*
Observations	10608	10608	10608	10608	10608	5595	5595	8112
F-Stat FS	39.9	39.9	39.9	39.9	39.9	34.2	34.2	7.2
Ward FE	X	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X	X
Years	2002-2018	2002-2018	2002-2018	2002-2018	2002-2018	2011-2019	2011-2019	2007-2019

Note: The sample includes a panel of 624 electoral wards in Greater London. *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100* and its interaction with the heterogeneity dummy. Dependent variable in Column 1 is the log of ward population. In Column 2 to 5, it is the share of population by age group. In Column 6 and 7, it is the share of pupils (over total pupils enrolled in the ward) for which English is not their first language and entitled for free meals. Dependent variable in Column 8 is the log of complaints against tourists per 2007 residents. To avoid taking the log of a zero, one is added to the number of complaints before taking logs in Column 8. *Share homeowners above median* is a dummy equal to 1 if, in 2011, the ward has a share of homeowners above the median. All Columns include ward fixed effect, local authority time trends, the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station. To avoid taking the log of a zero, one is added to the number of complaints before taking logs. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years. *** p<0.01; ** p<0.05; * p<0.1.

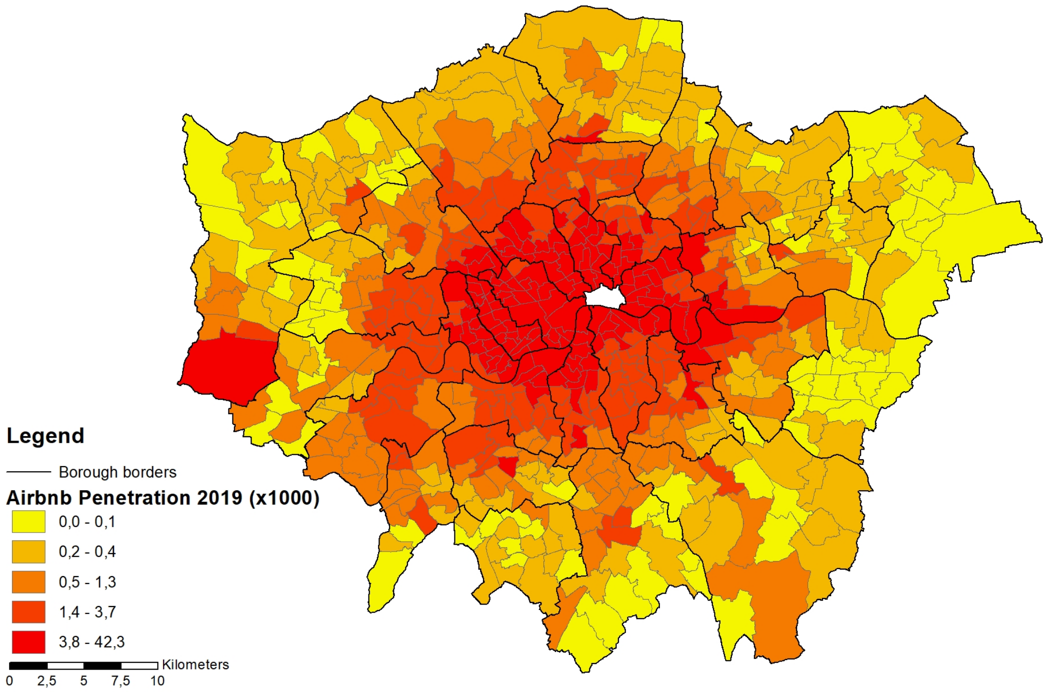
Table 1.8.7: Social Capital and Brexit

	ln(Organizations per person (x1000))			ln(Complaints per resident (x1000))		Share of people voting	
	Charitable	Youth	Political	Car Parking	Dog fouling	Leave Brexit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Airbnb Penetration (x1000)	0.000	-0.001	0.000	0.015	0.025	0.697	0.567
	(0.002)	(0.001)*	(0.000)	(0.006)**	(0.011)**	(0.079)***	(0.069)***
Panel B: IV							
Airbnb Penetration (x1000)	-0.021	-0.006	-0.005	0.004	0.006	0.605	0.589
	(0.009)**	(0.003)*	(0.002)**	(0.002)**	(0.002)***	(0.336)*	(0.303)*
Observations	5616	5616	5616	8112	8112	280	280
F-Stat FS	36.9	36.9	36.9	43.7	43.7	15.4	14.1
Ward FE	X	X	X	X	X		
LLA x Year FE	X	X	X	X	X		
Vars 2001 x Year FE	X	X	X	X	X		
Geo x Year FE	X	X	X	X	X		
Geo Controls						X	X
Educ and age Controls						X	X
Ethnicity, Nationality							X
Years	2011-2019	2011-2019	2011-2019	2007-2019	2007-2019	2016	2016

Note: *Airbnb penetration* represents the number of Airbnb tourists nights over residents nights. Instrument in Panel B is *Historical Sites per km^2 times Airbnb Google Trend/100*. Dependent variable in Column 1 to 3 is the log of the number of organizations per 2007 residents (source: Digimap, 2020). In Columns 4 and 5 I consider the log of complaints per 2007 residents by category (source: FixMyStreet). In Columns 6 and 7, I consider the share of votes in favour of Leave in 2016 EU referendum. In Columns 1 to 5 I include ward fixed effect, local authority time trends and the interaction of year dummies with 2001 share of workers by sector, 2001 share of workers by occupation and 2001 log of house prices per square meter and interaction of year dummies with distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station. Columns 6 and 7 control for geographical variables (local authority fixed effects, distance ward centroid to Charing Cross, distance ward to the closest underground station), the share of the population over 65 in 2011 and the share of the population with a university degree (or higher). Column 7 add share of white population in 2011, share of UK citizen in 2011, share of EU citizen in 2011 and share of Commonwealth citizen in 2011. To avoid taking the log of a zero, one is added to the dependent variables before taking logs in Columns 1 to 3. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameters considered: 14 km and 10 years in Columns 1 to 3, 14 km in Columns 4 and 5. *** p<0.01; ** p<0.05; * p<0.1.

Figures

Figure 1.8.1: Airbnb Penetration in 2019



Note: Airbnb penetration (equation 1.1) in 2019 x 1000 at ward level. Bins are represented by 2018 quintiles of Airbnb penetration.

1.A Appendix – Institutional background

1.A.1 London administrative structure

Greater London is formed by 33 local authorities or "boroughs". I exclude "City of London" given its peculiarity, it contains the historic centre with the first settlement located here and the primary central business district of London. It is also a separate ceremonial county, being an enclave surrounded by Greater London, and is the smallest county in the United Kingdom.[40] Median area of a local authority is 38.7 km^2 while median population (2011 Census) is 255,511 residents. Each borough is administered by borough councils which are elected every 4 years. Boroughs are the principal local authorities in London and are responsible for running most local services, such as schools, social services, waste collection and roads. Some London-wide services are run by the Greater London Authority, and some services and lobbying of government are pooled within London Councils. Some councils group together for services such as waste collection and disposal. Each borough council is a local education authority.

The Greater London Authority (GLA), known colloquially as City Hall, is the devolved regional governance body of London, with jurisdiction over both the City of London and the ceremonial county of Greater London. It is a strategic regional authority, with powers over transport, policing, economic development, and fire and emergency planning. The GLA is responsible for the strategic administration of Greater London. It shares local government powers with the councils of 32 London boroughs and the City of London Corporation.

Unit of analysis of this paper is the electoral ward. Each ward is fully contained in a borough. In my sample I consider 624 wards. Median area is 1.86 km^2 while median population (2011 Census) is 13,015 residents. The ward is the primary unit of English electoral geography and each is represented by three councillors.

1.B Appendix – Data Sources and Description

1.B.1 Data management

Unit of analysis is the electoral ward. Each ward is fully contained in a borough.

Census Geography

The main geographies directly associated with the Census are Output Areas (OA). The OA is the lowest geographical level at which census estimates are provided. Output areas are fully contained in electoral wards so whenever data are provided at OA geography level the mapping is straightforward. OAs are further aggregated at lower layer super output areas (LSOA) and then into the middle layer super output areas (MSOA).

Mapping different geographies

Thanks to specific mapping provided by the Office for National Statistics (ONS) it has been possible to map:[41]

[40] It is a common practice to exclude City of London, see for example Draca et al. (2011).

[41] I am deeply grateful to the Open Geography portal from the Office for National Statistics (ONS) for their constant support throughout this project. They provided excellent support for all my data enquirers regarding mapping and boundaries of different geography levels.

- Output areas 2001 Census into Output areas 2011 Census
- LSOA or MSOA 2011 Census into 2011 electoral wards. LSOA or MSOA are not perfectly contained in a ward. When looking at absolute numbers (*e.g.* number of rented properties) I compute the share of the area in which each LSOA or MSOA is split across different wards and I assign proportionally data to the corresponding ward. When looking at relative numbers (*e.g. average rent*) I compute the shares of the ward represented by each specific LSOA or MSOA forming the ward and used them to compute a weighted average of the index of interest. Data originally reported at MSOA level: crime rates and rents data.
- Postcodes into Output areas 2011 Census. Data originally reported at postcode level: house prices.
- Electoral wards of any calendar year into 2011 electoral wards. When a ward is split across multiple wards I assigned its data proportionally to the area split. When two wards form a new ward I combine their data with a weighted average based on the areas of the two old wards.

1.B.2 Airbnb Penetration

InsideAirbnb is a source more reliable than Tomslee. In Appendix Table 1.C.2 I report the dates and sources of each web scraped as reported by InsideAirbnb.com and Tomslee.net. Each web scrape can be thought as a "snapshot" of all publicly available information on Airbnb.com. Given InsideAirbnb reports a much richer set of variables in case two "snapshots" from different sources were available I used InsideAirbnb. If two "snapshots" from the same source are available for the same month I kept the one closer to the 15 of the month. After applying these restrictions I have one snapshot in 2013, one in 2014, four snapshots in 2015, eight snapshots in 2016, five snapshots in 2017, eight snapshots in 2018 and twelve snapshots in 2019. In Appendix 1.D.3 I discuss results when restricting years after 2013 given it is the year in which the series of snapshots started.

Thanks to these data I am able to compute the following measure of Airbnb penetration:

$$Airbnb\,penetration_{it} = \frac{Airbnb\,Tourists\,nights_{it}}{Residents\,nights_{i2007}}$$

It represents the average number of tourists using Airbnb a resident would meet in a random day in ward i and year t. The numerator is computed in the following way:

$$Airbnb\,tourists\,nights_{it} = \sum_j Reviews_{jit} \times \frac{1}{0.69} \times Guests_j \times Nights_j$$

To compute $Airbnb\,tourists\,nights_{it}$ I started from the number of reviews each listing j, received in a given year t.[42] Each listing is assigned to a ward i given its latitude and longitude.[43] I adjust the number of reviews reported taking into account that only 69% of guests leave a review, following results in Fradkin et al. (2020), obtaining the number of visits using Airbnb. Results are similar if ignoring this adjustment given it is just a constant multiplicative factor. Notice that reviews are hidden until either guests and host submit a review or 14 days had expired. Prior 8th May 2014 both guests and hosts had 30 days after the checkout date to review each other and any submitted review was automatically posted to the website. Review rate before 8th May 2014 was 68%. I multiply the resulting number of visits by the number of guests the property can accommodate and by the number of minimum nights a host requests.[44] Results are similar if considering the number of beds in the property, as explained in Appendix Section 1.D.2. My

[42] Guests have 14 days maximum to fill a review, they are then representative of the period of the visit.

[43] Exact location is not provided, Airbnb alters the exact location by a factor ranging between 0 and 150 meters, given the size of each ward the number of wrongly assigned listed is neglectable.

[44] These informations have been fixed at the last available date

measure of Airbnb tourists nights reports very similar figures when compared to official statistics reported in Airbnb (2018) for 2018 (6.88 million vs 6.82 million).

The denominator of $Airbnb\,penetration_{it}$ is the number of residents in 2007 in ward i times 350, where I assume each person spends 15 days outside London.

Alternative measure

Literature, as Garcia-López et al. (2019), Barron et al. (2020), Almagro and Domínguez-Iino (2020), Duso et al. (2020) or Koster et al. (2019) has mainly focused on the number of properties listed on Airbnb website. To replicate their results I consider:

$$Airbnb\,penetration = \frac{Entire\,Properties\,on\,Airbnb}{Number\,Dwellings_{2011}} \tag{1.7}$$

where at the nominator I am considering the number of entire properties listed on Airbnb website in ward i and year t while at the denominator I consider the number of dwellings in 2011 (source: Census 2011). It can be interpreted as the share of properties that are on the short-term market. Assuming a constant supply of dwellings in London, it measures the shift in housing space from residents to tourists. As the rest of the literature has been focusing on housing market it makes sense to consider this types of measures, however, given my context I preferred to consider a measure that measures the intensity of Airbnb penetration, in terms of tourists in the area, more precisely by: i) taking into account the size of the flat; ii) taking into account the duration of stay.

Notice that a recurrent concern with this type of measure as in equation 1.7 is that it relies on the assumption that only active listings are left on the platform. This issue is tackled in the literature in various ways (*e.g.* by restricting only to the one receiving reviews, see Barron et al., 2020) but concerns still apply, especially when constructing the panel dataset for periods for which a web scrape is not available where the standard approach is to assume that a listing has been active since the first day of listing continuously. My measure presented in Section 1.2.1 leverages on the actual reviews received. I will then capture activity by the number of reviews, even if a non-active listing is still present on the website it won't be an issue. However, I may still suffer from the problem that I am only leveraging on listings that manage to survive at least till 2013 (the first year for which I have a web scrape). The same issue applies, however, to the measure discussed in equation 1.7.

As robustness I replicate my analysis with this measure, results are unchanged, see Appendix Section 1.D.4.

Hotel penetration

Data on number of tourists in hotels are not available at the unit of analysis used in this paper. More generally they are usually provided from professional data providers that take advantage of detailed surveys. In order to estimate the number of tourists using "standard" accommodation industry I proceed in the following way. Similarly to Airbnb penetration (equation 1.1) I define hotel penetration as:

$$Hotel\,penetration_{it} = \frac{Hotel\,Tourists\,nights_{it}}{Residents\,nights_{i2007}} \tag{1.8}$$

It represents the average number of tourists using hotels a resident would meet in a random day in ward i and year t.[45] The numerator is computed in the following way:

$$
\begin{aligned}
&Hotel\,Tourists\,nights_{it} = \\
&\quad = N.\,London\,rooms_t \times \frac{N.\,Hotels_{it}}{Tot.Hotels_t} \times 3 \times 365 \times Occupation\,rate_t
\end{aligned}
\tag{1.9}
$$

where I consider the total number of hotel rooms reported by van Lohuizen and Smith (2017) and PwC UK Hotel forecast (2016, 2017, 2018 and 2019). Total hotel rooms are assigned proportionally across London according to the distribution among wards of hotels in year t.[46] I then assume that each room can fit on average 3 guests giving the number of guests that can potentially be present each day in a ward i. I then consider the yearly equivalent by multiplying 365 by the average annual occupation rates reported by van Lohuizen and Smith (2017) and PwC UK Hotel forecast. The denominator of hotel penetration is the number of residents in ward i in 2007.

In Appendix Figure 1.C.3 and 1.C.4 I plot the geographical distribution of hotel penetration measure in 2013 and 2019 respectively. The geographic distribution displays a clear cluster in Centre-West London (namely Westminster and Chelsea area) and one in the Heathrow Airport area. Moreover, I do not observe any remarkable change in the geographical distribution when comparing the two years.

1.B.3 Neighbourhood quality

Anti-social behaviours definition

Anti-social behaviour is defined by the police as "behaviour by a person which causes, or is likely to cause, harassment, alarm or distress to persons not of the same household as the person" (Anti-social behaviour Act 2003 and Police Reform and Social Responsibility Act 2011).

London Metropolitan Police website divides anti-social behaviours into three main categories, depending on how many people are affected:

- Personal antisocial behaviour is when a person targets a specific individual or group.
- Nuisance antisocial behaviour is when a person causes trouble, annoyance or suffering to a community.
- Environmental antisocial behaviour is when a person's actions affect the wider environment, such as public spaces or buildings.

Under these main headings, antisocial behaviour falls into one of 13 different types: vehicle abandoned; vehicle nuisance or inappropriate use; rowdy or inconsiderate behaviour; rowdy or nuisance neighbours; littering or drugs paraphernalia; animal problems; trespassing; nuisance calls; street drinking; prostitution-related activity; nuisance noise; begging; misuse of fireworks.

[45] For hotels I considered any "serviced" room, which include both hotel rooms as well as bed and breakfast and hostels

[46] Source of the number of hotels in ward i-year t is Digimap (2020) which is available only from 2011 onwards. For all previous years, I considered the average distribution over the 2011-2019 period

1.B.4 Other variables

1.B.5 Demographic and geographic variables

As a control in baseline specification, I consider the share of workers by sector and by occupation. Sectors considered are: i) Agriculture, hunting and forestry (A), Fishing (B), Mining and quarrying (C), Electricity, gas and water supply (E), Construction (F); ii) Manufacturing (D) iii) Wholesale and retail trade, repairs of motor vehicles, motorcycles and personal and households goods (G), Hotels and restaurants (H), Transport, storage and communications (I); iv) Financial intermediation (J), Real estate, renting and business activities (K); v) Public administration and defense; compulsory social security (L), Education (M), Health and social work (N), Other community, social and personal services activities (O), Activities of private households as employers and undifferentiated production activities of private households (P), Extraterritorial organizations and bodies (Q).[47] Occupations considered are: i) managers and senior officials, ii) professional occupations and associate professionals/technical occupations, iii) administrative and secretarial occupations and skilled trades occupations, iv) personal services occupations and sales and customer services occupations v) process, pant and machine operatives and elementary occupations.

[47] The letter in parenthesis refers to the NACE Rev. 1.1 section reported in the original data

1.C Appendix – Additional Tables and Figures

Table 1.C.1: Summary Statistics

	Mean	Sd	Min	Max	Obs	Sample
Panel A: Airbnb variables						
Airbnb listing	29.3	85.2	0.0	1,392	11,232	2002-2019
Airbnb tourists nights	2,513	9,574	0	194,643	11,232	2002-2019
Airbnb tourists nights per resident	0.61	2.38	0.00	42.26	11,232	2002-2019
Entire properties on Airbnb over 2011 (x100)	0.28	0.92	0.00	15.94	11,232	2002-2019
Panel B: Housing variables						
Median house price per square meter	5,024	2,413	1,853	24,889	11,231	2002-2019
Median rent price	1,488	480	664	6,963	3,519	2011-2016
Panel C: Complaints and neighborhood quality variables						
Complaints against tourists per resident	0.01	0.15	0.00	6.97	8,112	2007-2019
Complaints about rubbish per resident	2.85	15.91	0.00	282.59	8,112	2007-2019
Complaints about fly-tipping per resident	2.08	13.05	0.00	275.24	8,112	2007-2019
Complaints about flyposting per resident	0.44	2.78	0.00	86.07	8,112	2007-2019
Complaints about park status per resident	1.04	6.41	0.00	108.71	8,112	2007-2019
Complain about road status per resident	2.60	11.53	0.00	303.51	8,112	2007-2019
Entry/exit underground stations per resident	2.51	3.23	0.17	35.64	3,657	2007-2017
Anti social behaviour crimes per resident	40.52	28.52	7.31	470.60	5,616	2011-2019
Panel D: Social capital and political variables						
Charitable organizations per resident	0.31	0.56	0.00	12.09	5,616	2011-2019
Youth organizations per resident	0.06	0.08	0.00	0.83	5,616	2011-2019
Political organizations per resident	0.01	0.06	0.00	1.59	5,616	2011-2019
Share of votes supporting Brexit	40.5	14.5	15.0	79.0	280	2016-2016
Panel E: Ward characteristics						
Total population	12,948	2,586	4,608	35,210	10,608	2002-2018
Area (km^2)	2.55	2.58	0.39	29.03	624	

Note: Column Sample reports the year for which a variable is available. *Airbnb tourists nights over 2007 resident (x1000)* represents the main measure of Airbnb presence, called *Airbnb Penetration* and described in Section 2.3. All variables in Panel C and Panel D (with the exception of *Entry/exit underground stations per resident* and *Share of votes supporting Brexit* are multiplied by 1000. In all variables reporting data per resident the reference population is 2007 resident population.

Table 1.C.2: Webscrape dates and source

Webscrape date	Source	Webscrape date	Source
2013-12-21	Tomslee	2018-05-11	InsideAirbnb
2014-05-13	Tomslee	2018-07-07	InsideAirbnb
2015-01-17	Tomslee	2018-08-08	InsideAirbnb
2015-04-06	InsideAirbnb	2018-09-10	InsideAirbnb
2015-09-02	InsideAirbnb	2018-10-06	InsideAirbnb
2015-12-25	Tomslee	2018-11-04	InsideAirbnb
2016-01-09	Tomslee	2018-12-07	InsideAirbnb
2016-02-02	InsideAirbnb	2019-01-13	InsideAirbnb
2016-03-03	Tomslee	2019-02-05	InsideAirbnb
2016-06-02	InsideAirbnb	2019-03-07	InsideAirbnb
2016-08-07	Tomslee	2019-04-09	InsideAirbnb
2016-09-22	Tomslee	2019-05-05	InsideAirbnb
2016-10-03	InsideAirbnb	2019-06-05	InsideAirbnb
2016-12-26	Tomslee	2019-07-10	InsideAirbnb
2017-01-21	Tomslee	2019-08-09	InsideAirbnb
2017-03-04	InsideAirbnb	2019-09-14	InsideAirbnb
2017-04-19	Tomslee	2019-10-15	InsideAirbnb
2017-06-19	Tomslee	2019-11-05	InsideAirbnb
2017-07-28	Tomslee	2019-12-09	InsideAirbnb
2018-04-08	InsideAirbnb		

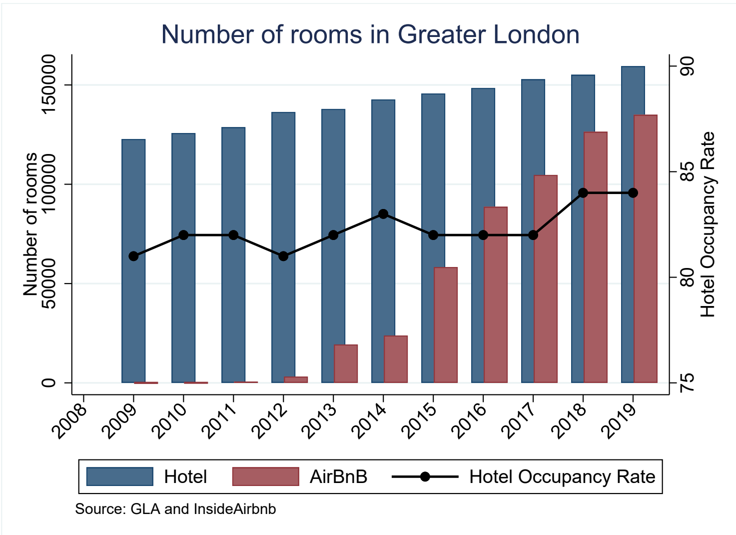
Note: Dates of webscrapes carried on by InsideAirbnb.com and Tomslee.net.

Table 1.C.3: FixMyStreet complaints categories

Complaints about	Share over total	Complaints about	Share over total
Rubbish	24.6%	Drain	1.8%
Road status	21.4%	Car parking	1.3%
Flytipping	18.0%	Dead animal	1.1%
Green area status	8.8%	Dog foul	0.8%
Street lights	6.8%	Admin	0.7%
Abandoned vehicle	6.6%	Street furniture	0.2%
Flyposting	3.8%	Dangerous structure	0.1%
Traffic sign	2.2%	Public toilet	0.0%
Other	1.8%		

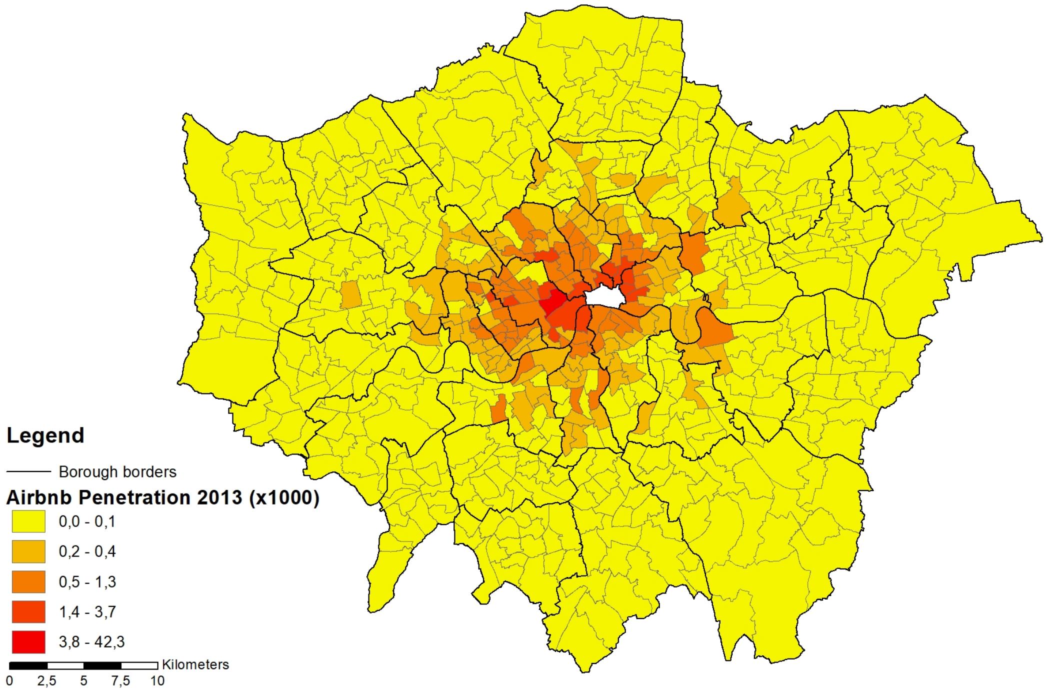
Note: Categories in FixMySteet after aggregating similar ones and share of complaints over total number of complaints (2007-2019)

Figure 1.C.1: Evolution hotel and Airbnb rooms in London



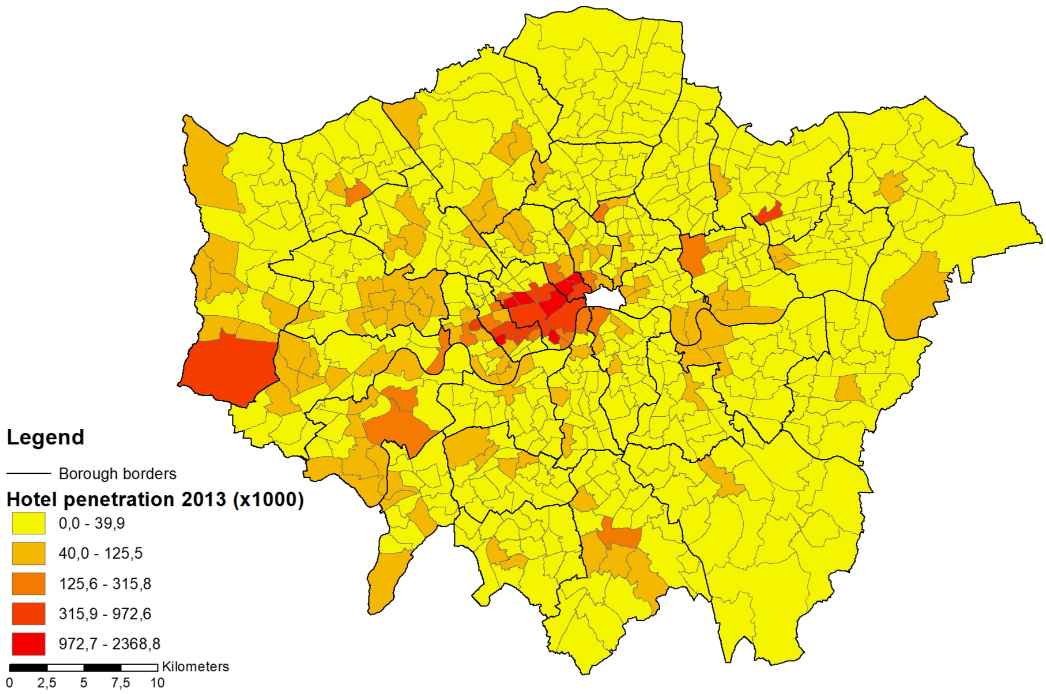
Note: Plotting number of hotel rooms in London (blue, source: Greater London Authority and PwC) and number of Airbnb rooms (red, source: Tomslee.net and InsideAirbnb.com) on the left axis. Plotting hotel occupancy rate (black line, source: PwC) on the right axis.

Figure 1.C.2: Airbnb Penetration in 2013



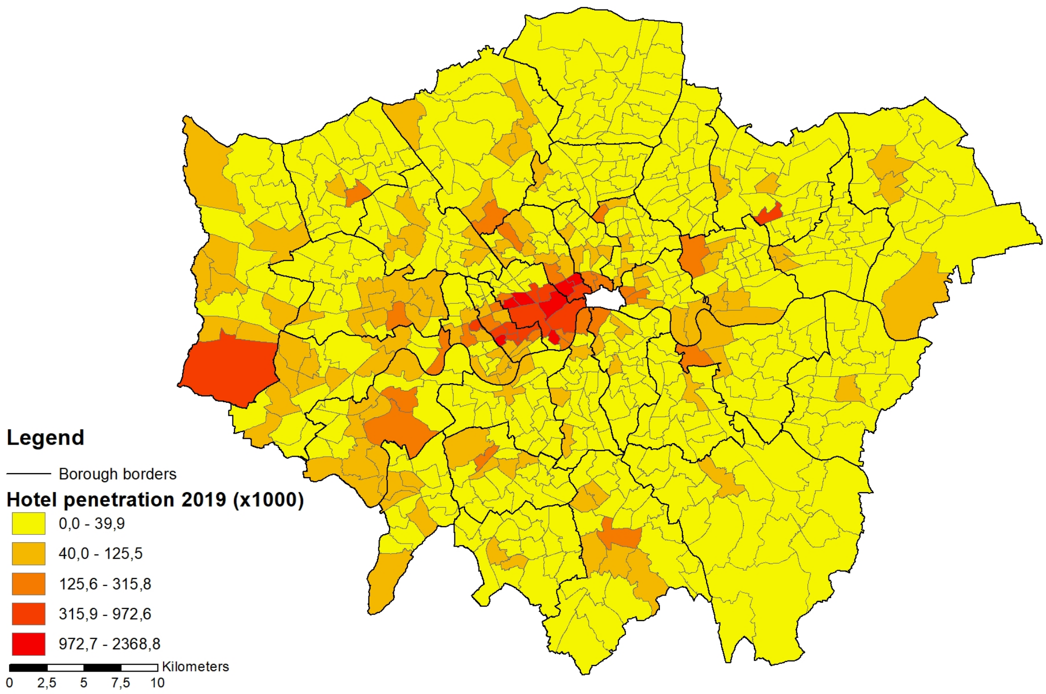
Note: Airbnb penetration (equation 1.1) in 2013 x 1000 at ward level. Bins represent 2019 quintiles of Airbnb penetration.

Figure 1.C.3: Hotel Penetration in 2013



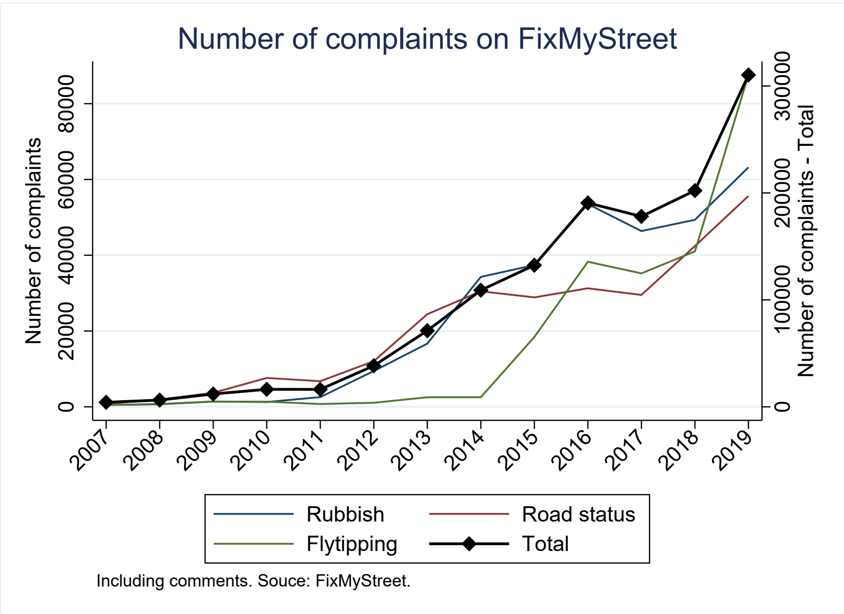
Note: Hotel penetration (equation 1.8) in 2013 x 1000 at ward level.

Figure 1.C.4: Hotel Penetration in 2019



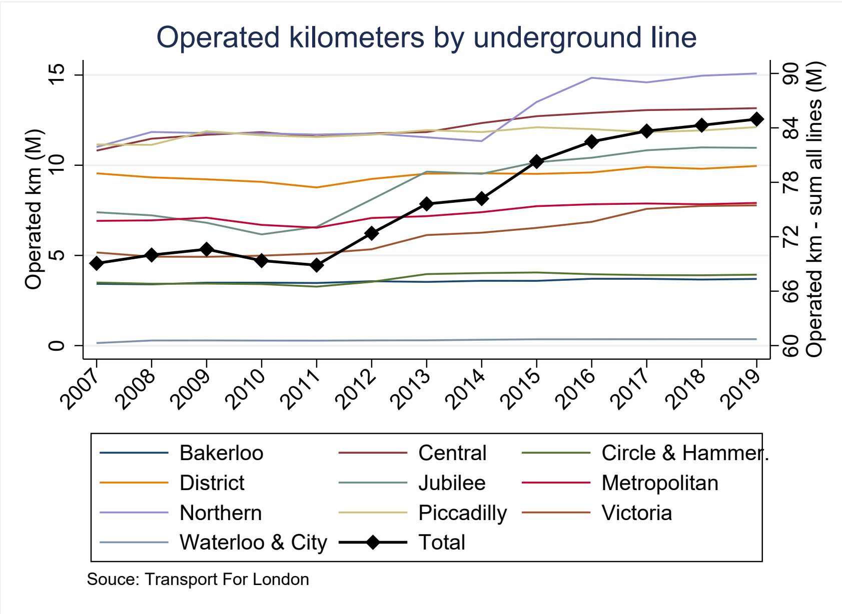
Note: Hotel penetration (equation 1.8) in 2019 x 1000 at ward level.

Figure 1.C.5: Number of complaints on FixMySteet



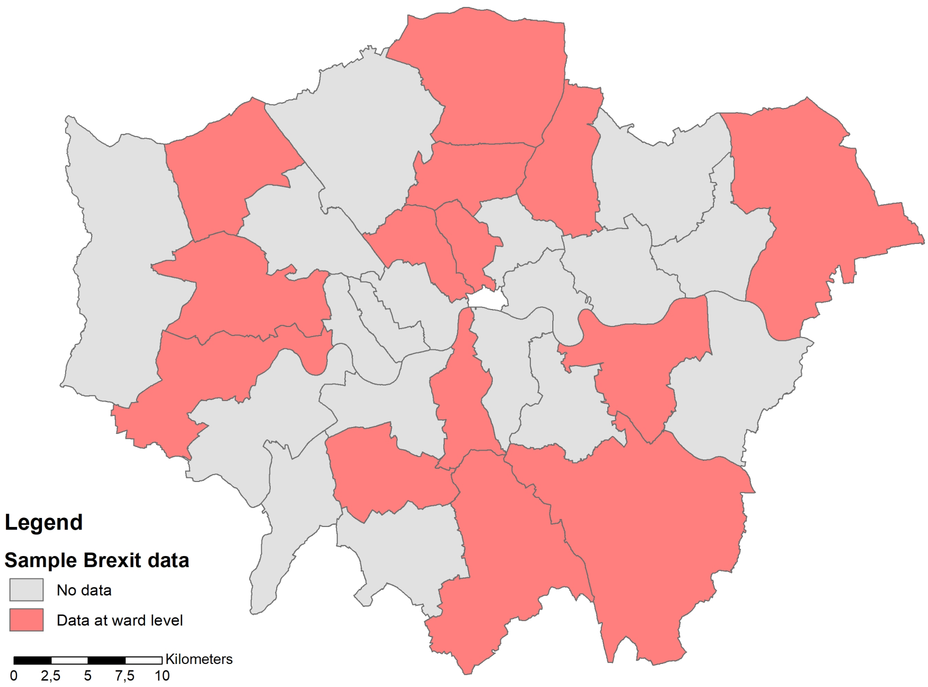
Note: Plotting total number of complaints and number of complaints for the three major categories. Including number of complaints and subsequent comments. Source: FixMyStreet (founded in 2007).

Figure 1.C.6: Operated kilometers by underground line



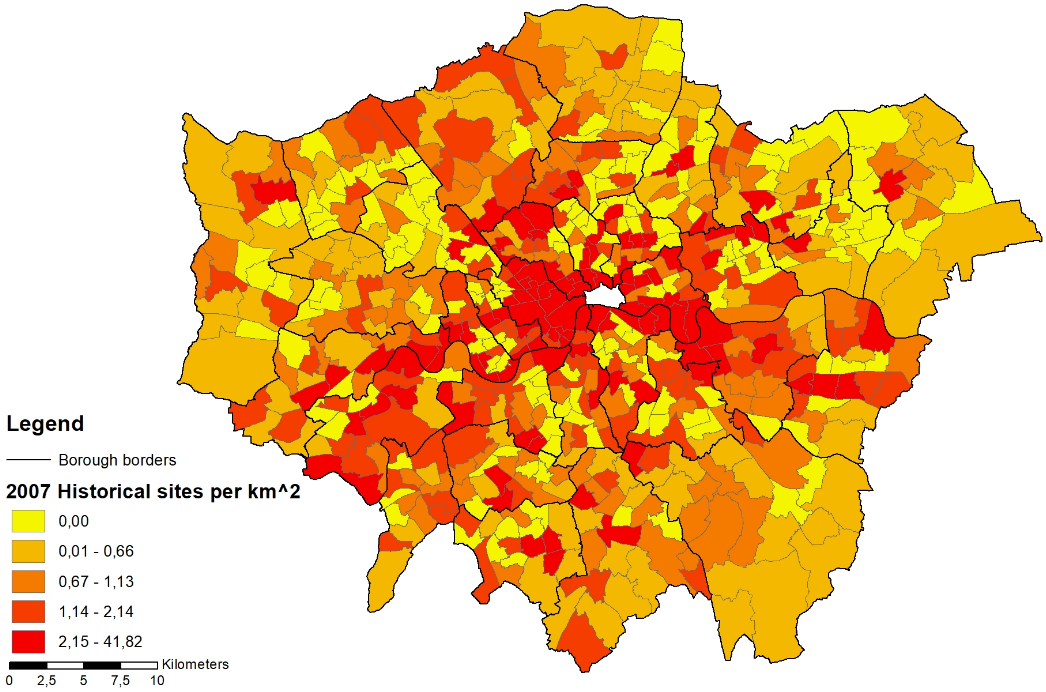
Note: Plotting operated kilometers by line. Central, Jubilee, Northern, Piccadilly and Victoria line since August 2016 are running Night services. Source: Trasport for London.

Figure 1.C.7: BBC Brexit Sample at ward level



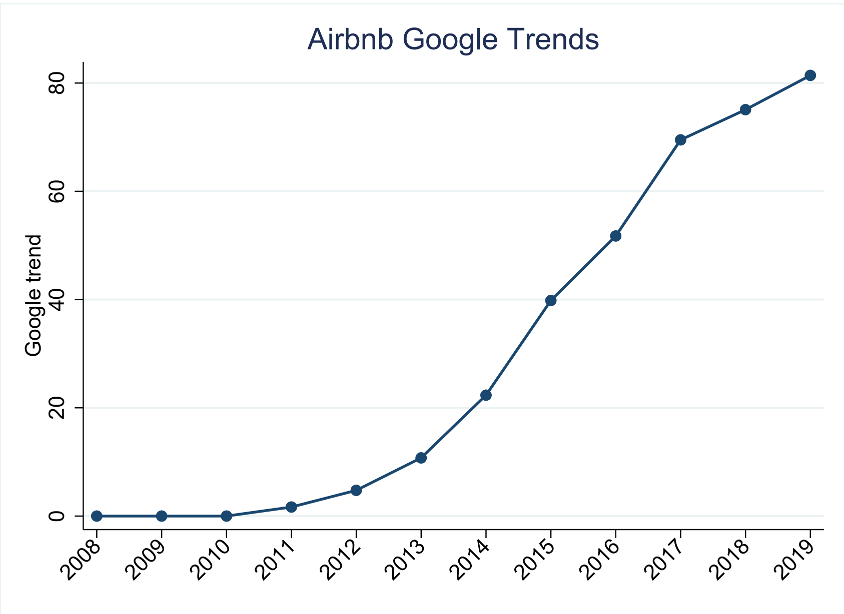
Note: Plotting local authority that reports data at ward level (source: BBC)

Figure 1.C.8: Geographical distribution of point of historical touristic interest



Note: Plotting number of historical monuments and buildings by square kilometres (source: Digimap, 2020). Bins represents quintiles of the distribution.

Figure 1.C.9: Geographical distribution of point of historical touristic interest



Note: Plotting time evolution of worldwide search volume for the word "Airbnb" (source: Google Trend). Google reports an index at the month level, that represents the search volume with respect to the month with maximum search volume. The index then ranges from 0 (no searches for the word "Airbnb") to 100 (maximum search volume ever recorded for the word "Airbnb"). I consider the yearly average of these monthly indexes.

1.D Appendix – Additional Results and Robustness

1.D.1 First Stage Robustness

In this section I provide robustness checks for the IV strategy proposed in Section 1.3.

Pre-trends The validity of the shift-share instrument constructed in equation 1.4 in the main text rests on one key assumption: areas with a higher share of historical sites must not be on different trajectories for the evolution of economic and social conditions in subsequent years (see also Goldsmith-Pinkham et al., 2020 and Borusyak et al., 2020). In Appendix Table 1.E.1 I test for pre-trends, regressing the pre-period (2002-2007) change in the outcomes of interest against the 2008-2019 change in Airbnb penetration predicted by the instrument. The estimated equation controls for local authority fixed effects, 2001 share of workers by sector, 2001 share of workers by occupation, 2001 log of house prices per square meter, distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to the closest underground station:

$$Y_{ib}^{2007} - Y_{ib}^{2002} = \beta(Airbnb_{ib}^{2019} - Airbnb_{ib}^{2008}) + \gamma X_i + \delta_b + \epsilon_{ib} \tag{1.10}$$

Unfortunately, among my outcomes, I can only consider median house prices and population. Reassuringly, coefficients (reported in Panel B) are never statistically significant. Also, and importantly, they are quantitatively different from the baseline IV estimates, reported in Panel A. These results indicate that historical sites are not in wards that were already undergoing economic or political changes.[48]

In the remaining on the section I described various modifications to the baseline specification of the first stage presented in 1.8.1.

Alternative shift and shares As first robustness, I consider alternative measures both for the shares and the shifts of equation 1.4. I consider a different source for the "share" component in Column 1 of Appendix Table 1.E.2 using the number of historical buildings of grade I and II star reported by Historic England. In Column 2, I modify the "shift" component and consider the Google Trend for "Airbnb London". In both cases, results are robust to these modifications. I will present robustness only for the specification of Column 4 of Table 1.8.1 with the full set of controls.[49]

Alternative specifications Second, I consider instead of *Airbnb Penetration* alternative specifications. In Column 3 of Appendix Table 1.E.2, I consider the log of Airbnb Penetration (to avoid taking the log of a zero, one is added to the number of Airbnb Penetration before taking logs). In Column 4 I consider just the numerator of equation 1.1.

Alternative measures of Airbnb presence Third, to verify that my results do not depend strictly on the assumptions made in constructing my measure of Airbnb penetration I consider alternative measures of Airbnb presence. In Column 5 of Appendix Table 1.E.2, I consider in the numerator of equation 1.1 the number of beds per listing instead of the number of people a listing can accommodate. In Column 6 I consider as the numerator of equation 1.1 the number of Airbnb visits (*i.e.* ignoring adjustment by the number of guests the property can accommodate and the minimum number of nights to consider) and as the denominator the 2007 residents. In Column 7, to reconcile with the literature on the impact of Airbnb on house prices and hotel industry, I consider the number of entire properties listed on Airbnb over the number of dwellings in 2011. All these modifications do not alter the relevance of my instrument. Finally, to verify

[48] In Panel B I am considering standard errors clustered at ward level as Hsiang (2010) procedures fails to deliver spatially corrected standard errors

[49] Also the other specifications are robust to the modification proposed, results available upon request

whether my instrument is capturing Airbnb penetration or simply a higher presence of tourists in Column 8 I consider the number of Accommodation establishments per 2007 residents. In this case, I do not find any first stage suggesting that my instrument is not predicting the presence of hotels, and then of "regular" tourists but only of Airbnb tourists.

Starting year Baseline specification includes all sample years from 2002 onwards. Airbnb, however, was born in 2008 in Los Angeles and it became popular in 2013. Moreover, in 2013 it is the first year I observed data, all previous years have been imputed using reviews of listings still existing in 2013. To make sure that my results did not follow from the starting year I progressively modify the starting year moving the first year of analysis one year earlier in each first stage regression. The specification is the one of Column 4, Table 1.8.1. Results are presented in Appendix Figure 1.E.1 where I am reporting the coefficient of each regression where I am changing the starting year of thr analysis reported on the x-axis. Results are stable until the very end when the coefficient is not significantly different from zero anymore. Moreover, the F-statistic drops below ten when 2016 (or later) is the starting year. Most likely both these issues arise due to the wide set of different trends I am including in the regression. However, it is important to notice that even if I restrict my attention after 2008 (when Airbnb was born) or after 2013 (when Airbnb became popular and when my data collection starts) I do not observe any significant difference from the baseline specification.

Exclude one by one a local authority In Appendix Figure 1.E.2 I report estimates of Column 4, Table 1.8.1 where I am excluding, one by one a local authority. Only when excluding Westminster the first stage results is not robust anymore. This is not surprising given the prominent role in the tourism industry played by the Westminster borough as within its boundary we can find many popular destinations like Buckingham Palace or Hyde Park.

1.D.2 Data quality robustness: beds

As described in Appendix Section 1.B.2 I take advantage of the information of how many people a listing can accommodate to infer the number of visitors in a given listing. A potential concern is that this information misrepresents real numbers as Airbnb hosts may inflate it by allowing people on sofas, etc. To make sure that this is not a problem in Appendix Table 1.E.4 I replicate OLS and IV specifications of Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) replacing number of people a flat can accommodate with number of beds in the flat in the expression for *Airbnb tourists nights* in expression 1.2. Results are unchanged.

1.D.3 Data quality robustness: webscrape dates

As described in Appendix Section 1.B.2 the first "snapshot" of reviews is available in 2013. That means that all information prior 2013 has been inferred conditional on the listing being still active in 2013. To make sure that this is not a problem in Appendix Table 1.E.5 I replicate OLS and IV specifications of Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) restricting my sample from 2013 onwards. All results are robust with the exception of results on underground congestion, for which, however, I have a very short panel dataset.

1.D.4 Alternative measure of Airbnb penetration

I replicate OLS and IV specifications of Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) using as a measure of Airbnb penetration the number of entire flats listed on Airbnb over the number of dwellings described in Appendix Section 1.B.2. Results are reported in

Appendix Table 1.E.6 and they are unchanged.

1.D.5 Airbnb or Hotel tourists?

A potential concern is that the effects described are not due to the presence of short-term tourists but it is just a proxy of overall rising tourism. While in principle this can be true I tackle this issue in different ways. See Appendix Section 1.B.2 where I describe how I construct a measure of hotel tourists penetration comparable to Airbnb tourists penetration.

First, the hotel industry is relatively fixed. In my data, I observe only minor changes in the supply of hotels. Similarly, when looking at the total number of hotels rooms they increase only by 16,000 from 2013 to 2019 while Airbnb number of rooms increased by almost 87,000, see Appendix Figure 1.C.1. In 1.E.3 I plot the median ward, 25th percentile ward and 75th percentile ward from the distribution of number of hotels per square kilometre. It confirms that the number of establishment is relatively fixed. Even more important the geographic distribution of hotels is almost constant with the clustered identified in Appendix Figure 1.C.4 in which I am plotting hotel tourists distribution for 2019. This appears evident when compared to the same figure in 2013 in Appendix Figure 1.C.3. This guarantees that most of the variation in the tourists using hotel rooms will be captured by ward fixed effects. Moreover, even if a set of neighbourhoods (*e.g.* a specific local authority or all areas closer to the city centre) are becoming more popular, it will be captured by the specific trends described in Section 1.3.

Second, as described in Appendix Section 1.D.1 the instrument proposed in Section 1.3 does not predict the hotel presence nor the number of guests in hotel accommodations. This reassures that the variation used in the IV strategy is orthogonal to hotel presence.

Third, when adding as a regressor the predicted number of hotel tourists per residents as described in Appendix Section 1.B.2 the significance of my estimates is not affected. In Appendix Table 1.E.7 I am reporting the OLS and IV specifications of Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) adding Hotel Penetration measure described in Appendix Section 1.B.2. No results are affected. Moreover, increasing by one standard deviation the number of hotel tourists per resident (1.5) delivers much smaller results than an increase in one standard deviation in Airbnb penetration (2.4) suggesting that i) the impact from Airbnb tourism penetration is robust to the inclusion of hotel tourism penetration and ii) it is more relevant in explaining the dynamics documented in this paper.[50]

1.D.6 Monthly results

Tourism, and Airbnb tourism as well, is subject to a high degree of seasonality with the peak season from June to September (and a second small peak in December). In Appendix Figure 1.E.4 I present quarterly aggregated data for international visitors and Airbnb visitors nights. Aggregating data at year level is a necessary step because i) most of the variables are available only at year level and ii) many variables contain meaningful variation only looking at relatively long time intervals.

Nevertheless, for some outcomes it is possible to credible estimate the model presented in Equation 1.3 at the month level, meaning that I will consider month-year specific time local authority trends as well as interacting pre-determined and geographic characteristics with month-year dummies.[51] Specifically, I

[50] Standard deviations reported are for the measures multiplied by 1000 and 10 as reported in the Appendix Table 1.E.7. Various concerns with respect to this regression apply: i) it is hard to think a credible instrumental strategy for hotel penetration, for this reason, I considered only the OLS, with all the common caveats; ii) hotel penetration may be a bad control if Airbnb penetration heavily affects also hotel presence and businesses as suggested by Farronato and Fradkin (2018)

[51] Standard errors consider a 120 time parameter and the usual 14km geographic correlation.

can do it for: complaints against tourists, complaints about rubbish, road status and car parking and social behaviour crime rates.[52]

Results are presented in Appendix Table 1.E.8. Results are all in line with baseline year specification suggesting that even with a demanding specification that takes into account monthly trends Airbnb penetration is associated with more complaints and a drop in neighbourhood quality.

1.D.7 Standard Errors

As described in Section 1.3 throughout the paper I consider standard errors corrected following Conley (1999), Conley (2010) and Hsiang (2010) with the following parameter choice: 14 km and 10 years. Parameters choice follows from the fact that the radius of the median local authority is 2 km if they were perfect circles. That implies that I am assuming that spatial correlation vanishes 3 complete local authorities from each ward centroid. For the autocorrelation parameters, I considered 10 years as Airbnb started its presence in London in 2009, note that Greene (2018) recommends at least $T^0.25$, even considering the longest panel (2002-2019) I am being more conservative.

To validate my choice I report in 1.E.5 the 10% confidence intervals of Table 1.8.2, Panel B Column 2 specification when changing the parameters of interest. In particular, I report all the combinations with time parameter equal to 2, 5, 10, 15, 20 and distance parameter equal to: 1, 5, 10, 14, 15, 20, 25. For completeness, I also report the confidence interval clustering at local authority and level. Notably, the clustered standard errors are the smallest, this reinforces our concerns in not considering explicitly autocorrelation and spatial correlation. When turning our attention to parameters combination it is evident how the "time" parameter does not alter confidence intervals while it is the distance parameters determining how wide the confidence intervals will be. Around the parameter choice (14 km and 10 years) results remain significant and confidence intervals almost identical. Wider standard errors appear when considering very limited time parameters jointly with a high distance parameter. Given that this is happening from parameter choices far from the baseline specification I am reassured over my choice.

1.D.8 Multiple hypothesis testing

Given the numerous outcomes considered under the same treatment, a concern is that we may falsely reject at least some null hypothesis of no effect. A vast literature has tackled the issue of multiple hypothesis testing. I perform various tests.

False Discovery Rate (FDR) q-values One of the most popular ways to deal with this issue is to follow Anderson (2008) to compute sharpened False Discovery Rate (FDR) q-values.[53] The FDR is the expected proportion of rejections that are type I errors (false rejections). The procedure is extremely simple because this takes the p-values as inputs, I can easily consider pvalues coming after Conley (1999) correction. A drawback of this method is that it does not account for any correlations among the p-values. Anderson (2008) notes that in simulations the method seems to also work well with positively dependent p-values, but if the p-values have negative correlations, a more conservative approach is needed.[54]

[52] I report results for each category of complaints, namely: susceptible to the presence of tourists and negative behaviours by residents, placebo and susceptible to negative behaviours by residents. All other measures display similar patterns, results available upon request

[53] Code is available from Anderson's website

[54] Note that sharpened q-values can be less than unadjusted p-values in some cases when many hypotheses are rejected because if there are many true rejections, you can tolerate several false rejections too and still maintain the false discovery rate low.

Familywise error rate (FWER) An alternative procedure aims to control the familywise error rate (FWER), which is the probability of making any type I error. I calculate Westfall-Young (Westfall and Young, 1993 stepdown adjusted p-values, which also control the FWER and allow for dependence amongst p-values.[55]. This method uses bootstrap resampling to allow for dependence across outcomes. Given the added complexity imposed by the fact that I am now controlling for dependence amongst p-values I consider standard errors clustered at ward level[56]

Joint test that no treatment has any effect A third approach, suggested in Young (2018), rather than adjusting each individual p-value for multiple testing, it conducts a joint test of the hypothesis that no treatment has any effect, and then uses the Westfall-Young approach to test this across equations.[57] Also here, given the added complexity imposed by the fact I am now controlling for dependence amongst p-values I consider standard errors clustered at ward level. Looking at randomization-c p-value for the joint-test of the significance of treatment measure in each equation as a whole is 0.0017 while randomization-t p-value for the Westfall-Young multiple testing test of the significance of any treatment measure in each equation as a whole is 0.0035. We can then reject the hypothesis that no treatment has any effect.

In Appendix Table 1.E.9 I report original p-values in Column 1, sharpened q-values following Anderson (2008) in Column 2 and p-values corrected following Westfall and Young (1993) in Column 3. In table footnote I report the Young (2018) pvalue of the joint test of the hypothesis that no treatment has any effect. I tested all the main specifications, namely Panel A and B of Table 1.8.2, Column 2; Table 1.8.3, Columns 1 and 4; Table 1.8.4; Table 1.8.6, Columns 1 to 5, 7 and 8; Table 1.8.7, Columns 1 to 3.

Comparing Columns 1 and 2 p-values and sharpened q-values following Anderson (2008) are similar. Only in one case original p-values wrongly reported a significant result, *i.e.* when looking at log median rent in the OLS regression. Also in columns 1 and 3 differences are limited. In only 4 cases a significant result is not significant anymore when considering corrected p-values: when looking at log median house price, log population, share of pupils for which English is not the first language and log number of political organizations per residents, in all cases in the OLS regressions.

[55] Stata code available as `randcmd`

[56] I report bootstrap-t as it is generally considered superior to the -c because its rejection probabilities converge more rapidly asymptotically to nominal size, Hall (1992). I consider 1999 randomization iterations

[57] Stata code available as `randcmd`

1.E Appendix – Robustness Tables and Figures

Table 1.E.1: Pre-Trends

	ln(Median house price per sqm)		ln(population)	
	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
Panel A: baseline				
Airbnb Penetration (x1000)	0.002	-0.003	0.002	0.009
	(0.001)**	(0.002)	(0.001)**	(0.006)
Panel B: pretrend				
Airbnb Penetration (x1000)	0.001	0.003	0.000	0.001
2019-2008	(0.001)	(0.003)	(0.001)	(0.002)
Years Dep. Var.	2002-2007	2002-2007	2002-2007	2002-2007

Note: Note: this table reports baseline IV estimates in Panel A as in Table 1.8.3, Column 1 and 1.8.6, Column 1. Panel B regresses the 2002-2007 change in outcomes against the 2008-2019 change in instrumented Airbnb penetration. All regressions include borough fixed effects, 2001 share of workers by sector, 2001 share of workers by occupation, 2001 log of house prices per square meter, distance ward centroid to Charing Cross, distance ward centroid to closest London 2012 venue and distance ward to closest underground station. F-stat First Stage refers to the K-P F-stat for weak instrument. Conley (1999) standard errors, parameter considered: 14 km and 10 years in Panel A. Standard errors clustered at ward level in Panel B. *** p<0.01; ** p<0.05; * <0.1.

Table 1.E.2: First Stage Robustness

	Airbnb Penetration (x1000)		ln(Airbnb Penetration (x1000))	Airbnb Tourist Nights	Airbnb penetration bed	Airbnb visits per resident	Airbnb property over dwelling	Accommodation per resident
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Sites x	0.361	0.422	0.030	2131.325	0.260	0.094	0.120	0.004
Google Trend/100	(0.041)***	(0.062)***	(0.008)***	(307.542)***	(0.038)***	(0.013)***	(0.025)***	(0.006)
Observations	11232	11232	11232	11232	11232	11232	11232	5616
R-Squared	0.827	0.808	0.938	0.794	0.812	0.812	0.874	0.974
F-Stat FS	79.4	47.0	14.7	48.9	46.0	54.2	22.2	0.4
Ward FE	X	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X	X
Instrument	Historic England	Airbnb London	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Years	2002-2019	2002-2019	2002-2019	2002-2019	2002-2019	2002-2019	2002-2019	2011-2019

Note: The sample includes a panel of 624 electoral wards in Greater London from 2002 to 2019. The specification considered is the same as Column 4, Table 1.8.1 unless differently specified. In Column 1 I consider Historical Buildings per square kilometers as share. In Column 2 I consider Google Trend for "Airbnb London" as shift. In Column 3 I consider the log of Airbnb Penetration (to avoid taking the log of a zero, one is added to the number of Airbnb Penetration before taking logs). In Column 4 I consider just the numerator of expression 1.1. In Column 5 I consider in the numerator of equation 1.1 the number of beds per listing instead of number of people a listing can accommodate. In Column 6 I consider as numerator of equation 1.1 the number of Airbnb visits (*i.e.* ignoring adjustment by number of guests the property can accomodate and minimum number of nights to consider) and as denominator the 2007 residents. In Column 7 I consider the number of entire properties listed on Airbnb over number of dwellings in 2011. In Column 8 I consider the number of Accommodation establishments per 2007 residents

Table 1.E.3: Selection

	Top quartile	Bottom quartile	Difference
Median house price per square meter	6235.624	3276.900	2958.724
	(2307.165)	(2307.165)	0.000
Complaitns about rubbish per resident (x1000)	0.249	0.282	-0.033
	(0.635)	(0.635)	0.608
Charitable organizations per resident (x1000)	0.990	0.147	0.843
	(1.356)	(1.356)	0.000
Population	12121.709	12049.653	72.056
	(2245.219)	(2245.219)	0.322

Note: Quartiles are defined based on 2019 Airbnb Penetration. First two columns report the average and standard deviation (in parenthesis) of top and bottom quartile for all wards-year before 2013. Last column reports the difference of average values and the pvalue of a two side test for the difference being equal to 0.

Table 1.E.4: Robustness: using beds in the flat

	ln(Complaints against tourists per resident (x1000))	ln(Median house price per sqm)	ln(Median rent)	ln(Tube entry/exit per resident)	ln(Complaints Rubbish per resident (x1000))	ln(ASB per resident (x1000))	ln(Charitable Organizations per resident (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Airbnb Penetration (x1000)	0.014	0.003	0.008	0.006	0.025	0.007	0.000
	(0.007)**	(0.001)**	(0.005)	(0.001)***	(0.007)***	(0.002)***	(0.004)
Panel B: IV							
Airbnb Penetration (x1000)	0.044	-0.006	0.006	0.012	0.059	0.053	-0.043
	(0.024)*	(0.004)	(0.010)	(0.006)*	(0.030)**	(0.013)***	(0.017)**
Observations	8112	11231	3514	3640	8112	5616	5616
F-Stat FS	40.7	43.2	45.1	26.3	40.7	33.8	33.8
Ward FE	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X
Years	2007-2019	2002-2019	2011-2016	2007-2017	2011-2019	2007-2019	2011-2019

Note: I replicate results presented in Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) replacing number of people a flat can accommodate with number of beds in the flat in the expression for *Airbnb tourists nights* in expression 1.2.

Table 1.E.5: Restrict from 2013 onwards

	ln(Complaints against tourists per resident (x1000))	ln(Median house price per sqm)	ln(Median rent)	ln(Tube entry/exit per resident)	ln(ASB per resident (x1000))	ln(Complaints Rubbish per resident (x1000))	ln(Charitable Organizations per resident (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Entire properties	0.008	-0.001	0.002	0.000	0.003	0.017	0.004
over dwellings (x100)	(0.004)**	(0.000)	(0.002)	(0.000)	(0.001)**	(0.004)***	(0.001)***
Panel B: IV							
Entire properties	0.035	-0.004	-0.001	0.002	0.028	0.040	-0.002
over dwellings (x100)	(0.018)*	(0.004)	(0.003)	(0.003)	(0.010)***	(0.021)*	(0.007)
Observations	4368	4367	2330	1654	4368	4368	4368
F-Stat FS	33.7	33.7	52.0	12.3	33.7	33.7	33.7
Ward FE	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X
Years	2013-2019	2013-2019	2013-2016	2013-2017	2013-2019	2013-2019	2013-2019

Note: I replicate results presented in Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) restricting my sample from 2013 onwards.

Table 1.E.6: Alternative measure of Airbnb Penetration: dwellings

	ln(Complaints against tourists per resident (x1000))	ln(Median house price per sqm)	ln(Median rent)	ln(Tube entry/exit per resident)	ln(ASB per resident (x1000))	ln(Complaints Rubbish per resident (x1000))	ln(Charitable Organizations per resident (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Entire properties	0.026	0.008	0.014	0.006	0.009	0.040	-0.004
over dwellings (x100)	(0.015)*	(0.002)***	(0.008)*	(0.004)	(0.004)**	(0.018)**	(0.009)
Panel B: IV							
Entire properties	0.096	-0.014	0.018	0.034	0.114	0.129	-0.091
over dwellings (x100)	(0.052)*	(0.009)	(0.032)	(0.018)*	(0.029)***	(0.065)**	(0.037)**
Observations	8112	11231	3514	3640	5616	8112	5616
F-Stat FS	18.6	19.5	18.0	17.9	15.6	18.6	15.6
Ward FE	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X
Years	2007-2019	2002-2019	2011-2016	2007-2017	2011-2019	2007-2019	2011-2019

Note: I replicate results presented in Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) using as a measure of Airbnb penetration the number of entire flats listed on Airbnb over the number of dwellings described in Appendix Section 1.B.2

Table 1.E.7: Include Hotel Penetration

	ln(Complaints against tourists per resident (x1000))	ln(Median house price per sqm)	ln(Median rent)	ln(Tube entry/exit per resident)	ln(Complaints Rubbish per resident (x1000))	ln(ASB per resident (x1000))	ln(Charitable Organizations per resident (x1000))
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS							
Airbnb Penetration (x1000)	0.007	0.002	0.003	0.002	0.013	0.004	0.000
	(0.003)**	(0.001)**	(0.002)*	(0.001)***	(0.004)***	(0.001)***	(0.002)
Hotel Penetration (x10)	0.016	-0.003	0.013	0.010	0.021	0.004	-0.016
	(0.009)*	(0.003)	(0.012)	(0.004)**	(0.013)*	(0.011)	(0.011)
Panel B: IV							
Airbnb Penetration (x1000)	0.023	-0.004	0.001	0.005	0.030	0.034	-0.025
	(0.012)*	(0.002)*	(0.004)	(0.004)	(0.017)*	(0.009)***	(0.010)**
Hotel Penetration (x10)	-0.009	0.006	0.016	0.008	-0.005	-0.046	0.027
	(0.012)	(0.005)	(0.010)	(0.005)	(0.022)	(0.019)**	(0.015)*
Observations	8112	11231	3514	3640	11231	5616	5616
F-Stat FS	15.6	16.0	34.2	22.1	15.6	14.6	14.6
Ward FE	X	X	X	X	X	X	X
LLA x Year FE	X	X	X	X	X	X	X
Vars 2001 x Year FE	X	X	X	X	X	X	X
Geo x Year FE	X	X	X	X	X	X	X
Years	2007-2019	2002-2019	2011-2016	2007-2017	2007-2019	2011-2019	2011-2019

Note: I replicate results presented in Tables 1.8.2 (Column 2), 1.8.3 (Columns 1 and 4), 1.8.4 (Columns 1, 2 and 7) and 1.8.7 (Column 1) adding Hotel Penetration measure described in Appendix Section 1.B.2.

Table 1.E.8: Month level regressions

	ln(Complaints against tourists per resident (x1000))	ln(Complaints per resident (x1000)) Rubbish	Road status	Car Parking	ln(ASB per resident (x1000))
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS					
Entire properties	0.006	0.025	0.013	0.003	0.019
over dwellings (x100)	(0.003)**	(0.009)***	(0.005)***	(0.002)	(0.008)**
Panel B: IV					
Entire properties	0.036	0.137	0.041	0.022	0.264
over dwellings (x100)	(0.015)**	(0.039)***	(0.038)	(0.009)**	(0.072)***
Observations	97344	97344	97344	97344	67391
F-Stat FS	39.3	39.3	39.3	39.3	31.4
Ward FE	X	X	X	X	X
LLA x Year-Month FE	X	X	X	X	X
Vars 2001 x Year-Month FE	X	X	X	X	X
Geo x Year-Month FE	X	X	X	X	X
Years	2007-2019	2007-2019	2007-2019	2007-2019	2011-2019

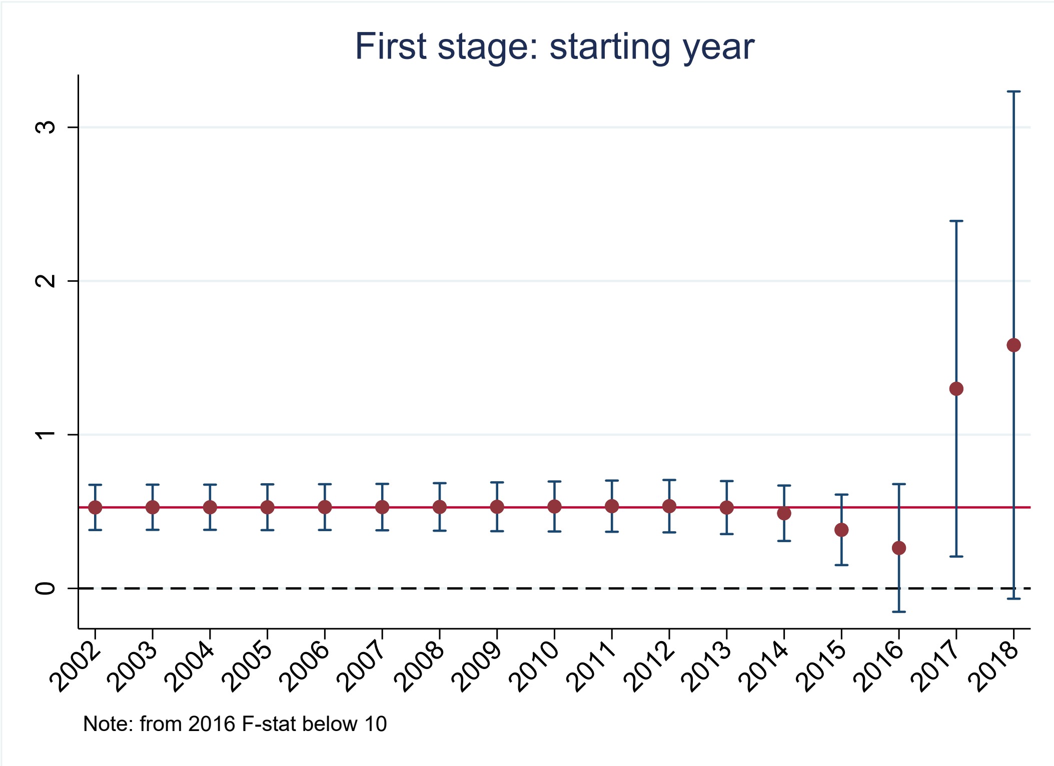
Note: I replicate results presented in Tables 1.8.2 (Column 2), 1.8.4 (Columns 2, 5 and 7) and 1.8.7 (Column 4) considering month level data.

Table 1.E.9: Multiple hypothesis testing

	Original p-value	Anderson (2008)	Westfall-Young (1993)
	(1)	(2)	(3)
Panel A: OLS			
ln(complaints against tourists per resident (x1000))	0.038	0.087	0.001
ln(median house price per sqm)	0.015	0.067	0.105
ln(median rent)	0.089	0.123	0.053
ln(entry and exit in tube per resident)	0.000	0.003	0.067
ln(complaints per resident (x1000) Rubbish)	0.001	0.016	0.000
ln(complaints per resident (x1000) Fly-tipping)	0.149	0.138	0.122
ln(complaints per resident (x1000) Flyposting)	0.129	0.137	0.050
ln(complaints per resident (x1000) Road status)	0.108	0.133	0.129
ln(complaints per resident (x1000) Green area status)	0.800	0.339	0.844
ln(anti social behaviour per resident (x1000))	0.004	0.035	0.018
ln(Population)	0.045	0.096	0.247
Share population 0-18	0.020	0.070	0.003
Share population 19-34	0.016	0.067	0.039
Share population 35-64	0.355	0.227	0.562
Share population 65+	0.623	0.272	0.767
Share of pupils first language not English	0.019	0.070	0.111
Share of pupils with free meals	0.757	0.326	0.885
ln(Organizations per resident (x1000) - Charitable)	0.845	0.352	0.763
ln(Organizations per resident (x1000) - Youth)	0.058	0.097	0.108
ln(Organizations per resident (x1000) - Political)	0.590	0.270	0.709
Panel B: IV			
ln(complaints against tourists per resident (x1000))	0.064	0.097	0.000
ln(median house price per sqm)	0.103	0.133	0.038
ln(median rent)	0.562	0.270	0.055
ln(entry and exit in tube per resident)	0.056	0.097	0.027
ln(complaints per resident (x1000) Rubbish)	0.048	0.096	0.003
ln(complaints per resident (x1000) Fly-tipping)	0.037	0.087	0.006
ln(complaints per resident (x1000) Flyposting)	0.016	0.067	0.001
ln(complaints per resident (x1000) Road status)	0.484	0.270	0.001
ln(complaints per resident (x1000) Green area status)	0.490	0.270	0.219
ln(anti social behaviour per resident (x1000))	0.000	0.003	0.007
ln(Population)	0.115	0.137	0.167
Share population 0-18	0.036	0.087	0.000
Share population 19-34	0.519	0.270	0.119
Share population 35-64	0.056	0.097	0.049
Share population 65+	0.981	0.417	0.308
Share of pupils first language not English	0.732	0.323	0.118
Share of pupils with free meals	0.149	0.138	0.046
ln(Organizations per resident (x1000) - Charitable)	0.013	0.067	0.006
ln(Organizations per resident (x1000) - Youth)	0.064	0.097	0.053
ln(Organizations per resident (x1000) - Political)	0.012	0.067	0.021

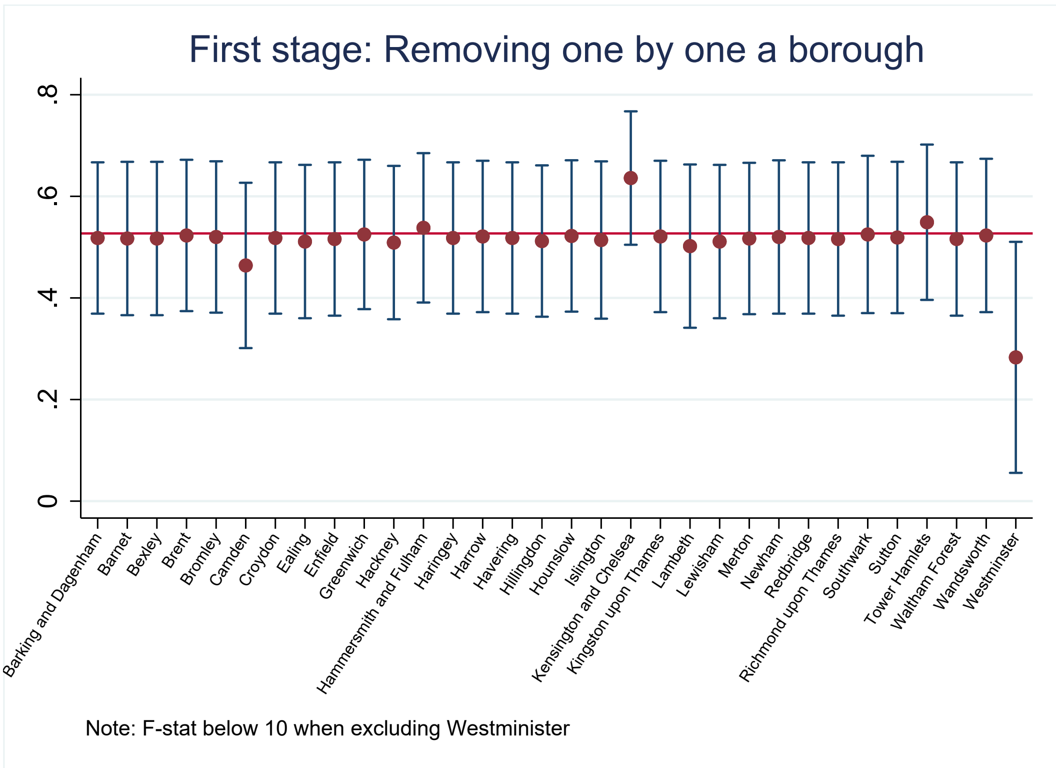
Note: Column 1 contains pvalues computed using Conley (1999) as described in Section 1.3. Column 2 contains sharpened q-values following Anderson (2008). Column 3 stepdown adjusted p-values following Westfall and Young (1993). Randomization-t p-value for the Westfall-Young multiple testing test of the significance of any treatment measure in each equation as a whole is 0.0035 following Young (2018)

Figure 1.E.1: First stage: modify starting year



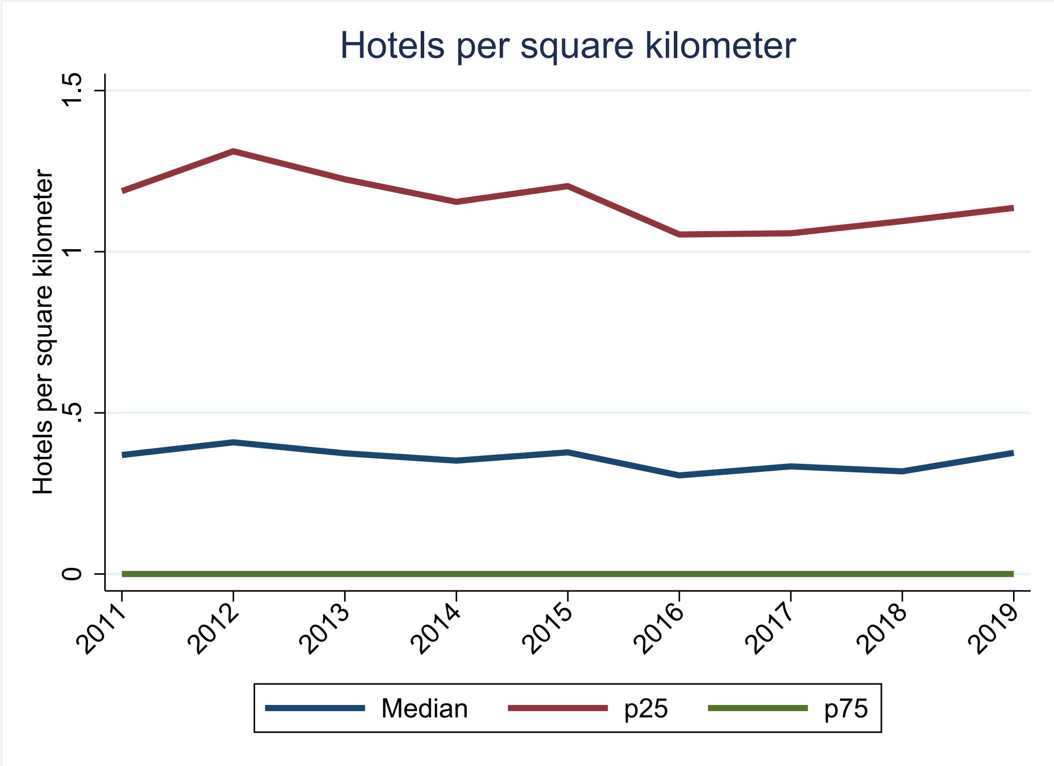
Note: Plotting coefficients of Column 4, Table 1.8.1 where I modify the starting year of analysis. Starting years are reported on the x-axis.

Figure 1.E.2: First stage: Removing one by one a local authority



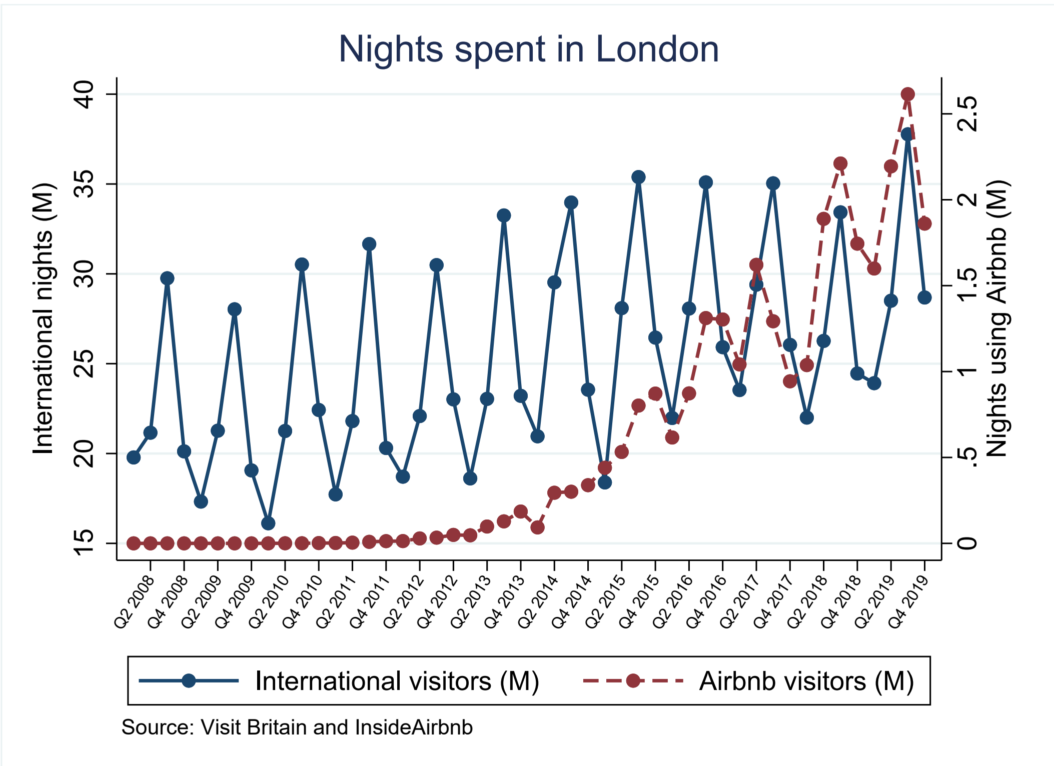
Note: Plotting coefficients of Column 4, Table 1.8.1 where I exclude one by one a local authority from the analysis. Excluded local authorities are reported on the x-axis.

Figure 1.E.3: Seasonal variation in tourists nights



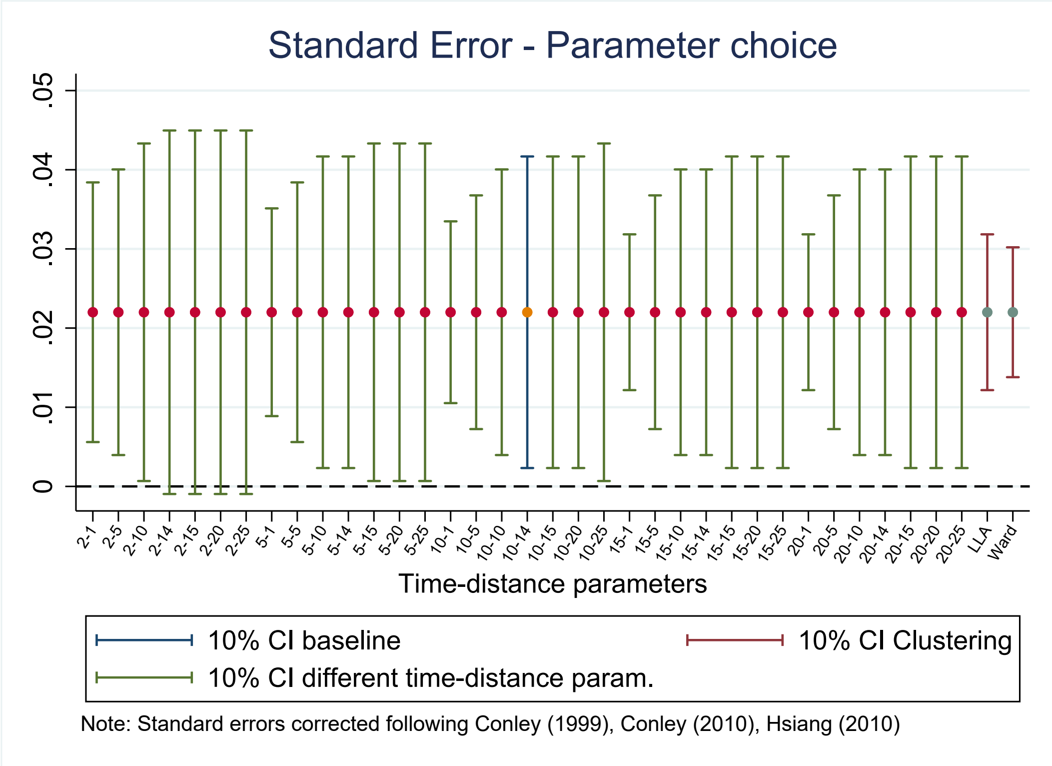
Note: Plotting median ward, 25th percentile ward and 75th percentile ward from the distribution of number of hotels per square kilometre.

Figure 1.E.4: Seasonal variation in tourists nights



Note: Plotting number of international visitors nights (left y-axis) and nights of tourists using Airbnb (right y-axis) by quarter.

Figure 1.E.5: Different Standard Errors computation



Note: Plotting 10% confidence intervals of Table 1.8.2, Panel B Column 2 specification. On the x-axis reporting the combination of time-distance parameters considered in Conley (1999) standard error correction. Last two data points report standard errors clustering at Local Authority and ward level respectively.

Chapter 2

Antitrust Policies and Profitability in Non-Tradable Sectors

Tim Besley
Department of Economics and STICERD, London School of Economics and Political Science
Nicola Fontana
Department of Economics and Center for Economic Performance, London School of Economics and Political Science
Nicola Limodio
Department of Finance, BAFFI CAREFIN and IGIER, Bocconi University

Abstract

Firms in tradable sectors are more likely to be subject to external competition to limit market power while non-tradable firms are more dependent on domestic policies and institutions. This paper combines an antitrust index available for multiple countries with firm-level data from Orbis covering more than 12 million firms from 94 countries, including 20 sectors over 10 years and finds that profit margins of firms operating in non-tradable sectors are significantly lower in countries with stronger antitrust policies compared to firms operating in tradable sectors. The results are robust to a wide variety of empirical specifications.

This paper examines how institutions designed for enforcing competition in markets affect economic performance. A central role of the state in building a market economy is to guarantee that there are benefits of competition to ensure static and dynamic efficiency. But the way that this is done varies across sectors of the economy. Those sectors that are subject to international competition have natural exposure to competition if trade is liberalized, while those that are not are more dependent on domestic policies that encourage entry and limit the abuse of market power.

The core empirical implication that we explore here, is that the institutions that affect competition policy should have a heterogenous effect on tradable and non-tradable sectors. To investigate this, we require firm-level data. Hence, we have assembled a dataset of 12 million firms covering 20 sectors across 94 countries over a period of 10 years (2006-2015) based on Orbis (2016). To measure antitrust policy, we exploit the Total Scope Index Score constructed by Hylton and Deng (2007) which is based on assessments of competition law made by legal experts and practitioners. This is available for a wide variety of countries.

Our core findings exploit variation in antitrust both across and within different sectors and countries. Using the Orbis profitability measure, we show that profitability is related to an index of antitrust policy but *more strongly* in non-tradable sectors. The results that we present are robust to a variety of alternative specifications.

Our baseline results are based on a cross-country and cross-sector analysis where we show evidence of systematic heterogeneity in the relationship between antitrust policies and firm outcomes. We show that, in countries with stronger antitrust policies, the profit margins of firms operating in non-tradable sectors are significantly lower than those operating in tradable sectors. The results are economically meaningful suggesting, for example, that if China adopted France's antitrust index, we would expect a 19% fall in the average profit margin. We also find that concentration is lower in non-tradable sectors when antitrust policy is strong. In contrast, changes in antitrust are associated with negligible effects on tradable sectors, in line with the hypothesis that international markets serve to discipline firms in such sectors.

These findings underline the limits of trade liberalization as a means of promoting competition since, in our sample, about 82% of firms operate in non-tradable sectors. So, without rigorous competition policy, there may be limited scope to introduce more competition into important sectors such as wholesale, retail, transportation, construction, and real estate.

Our results are consistent with the idea that institutions matter, in the form of competition law and enforcement, for sectors of the economy where international competition is weak. Moreover, the finding in our paper is specific to the antitrust measures; other measures of "good institutions" do not appear correlated with profitability in the non-tradable sectors of the economy. It therefore adds a new dimension to debates about how a strong institutional environment can be conducive to growth and development beyond the previous focus on such things as lowering the threat of expropriation (La Porta et al., 1998), minimizing rent extraction (Acemoglu et al., 2001) or securing legal protection and infrastructure (Besley and Persson, 2011).

The remainder of the paper is organized as follows. The next section discusses related literature. In section 2.2, we present the conceptual framework that motivates the test that we use. Section 2.3 presents the data and section 2.4 presents the core empirical results. In section 2.5, we discuss a key concern about interpreting the results along with some robustness checks. Section 2.6 contains concluding remarks.

2.1 Related Literature

The paper is related to an emerging body of literature on antitrust policy and its consequences. Our findings complement recent work studying the role of antitrust and firm margins in the United States (Gutiérrez and

Philippon, 2017, De Loecker et al., 2020), that we extend to additional countries and across sectors with different degrees of tradability. Debates about the role of antitrust and its ability to affect firm behavior are long-standing. Block et al. (1981) show that antitrust efforts and penalties generate a deterrent effect on cartels, that lead firms to set a price between the competitive the oligopolistic price. Gutiérrez and Philippon (2017) find that the profitability and concentration of US industries increased in the past two decades due to decreasing domestic competition, in line with Grullon et al. (2019). These results are consistent with our antitrust measure showing a decline in the stringency of antitrust policies in the United States, as highlighted by Faccio and Zingales (2017) for the telecommunication sector.

Gutiérrez and Philippon (2017) study the increase in competitiveness in Europe, showing that increased antitrust enforcement has led to lower concentration and profits, without negative effects on innovation. Alfaro et al. (2016) show that higher prices in the product market induce more integration, exploiting plausibly exogenous variation induced by trade policies. This result is in line with our findings on declines in both firm profits and concentration as antitrust policy intensifies.

The paper also relates to the large literature on the impact of trade liberalization by showing that antitrust policies may help to offset the absence of external competition for non-tradable sectors.Devarajan and Rodrik (1989) study how imperfect competition and scale economies affect the size and scope of trade liberalization. Pavcnik (2012) investigates the effects of trade liberalization on plant productivity in Chile and finds evidence of within plant productivity improvements, leading to higher aggregate productivity, in line with Krishna and Mitra (1998) in India and Amiti and Konings (2007) in Indonesia. Bernard et al. (2011) offer a general equilibrium model of multiple-product, multiple-destination firms, with heterogeneity in productivity across firms as well as product attributes within firms. This illustrates the heterogeneous effects of trade liberalization across countries, across and within firms. Using a structural model and matched plant-product data, De Loecker (2011) shows that the gains from trade liberalization are substantially smaller than previously estimated.

While there is a large and growing literature on the impact of trade liberalization on tradable sectors, much less is known about non-tradables. Among the prominent contributions in this smaller field, Goldstein et al. (1980) develop and estimate a general import function with tradable and non-tradable goods, finding a marginal role for non-tradable goods. Xu (2003) examines how trade liberalization can affect the boundary between tradable and non-tradable goods, leading firms to switch the tradability of their products. Rodrik et al. (2004) estimate the contributions of institutions, geography, and trade in determining income levels, finding that institutional measures are key, particularly in codetermining trade patterns. Finally, Kovak (2013) develops a specific-factors model of regional economies showing that prices of non-tradable goods and services move in line with tradable goods prices following liberalization.

Finally, the paper is related to a large literature on the benefits of competition for consumers (see Vickers, 1995 for an overview). In static models, these typically come from driving prices closer to marginal cost while, in dynamic models, there is a role for competition in encouraging the development and adoption of cost-reducing technologies and also in product innovation (*e.g.* Aghion et al., 2001). There is an increasing realization that the benefits of competition have not been emphasised sufficiently in the design of development strategies, where incumbents often enjoy unchecked market power (see, for example, the work of the United Nations Conference on Trade and Development (UNCTAD, 2008)).

2.2 Empirical Hypotheses

The paper tests two hypotheses relating antitrust policies and profitability. Equilibrium profit margins depend on ex-post price competition, entry and exit, all of which are affected by competition policy in general and antitrust policy in particular. While there is a range of potential models that could be used to motivate this, many make ambiguous predictions about the relationship between competition and profitability once entry and exit are allowed, especially when firms are heterogenous (see, for example, Syverson (2019) and Covarrubias et al. (2019)).

Effective antitrust institutions try to regulate market conduct by powerful firms through facilitating entry of new and more efficient firms, thereby benefitting consumers directly. This will create entry which tend to lower prices and profits. Firms that face international competition through imports face additional pressure on prices and profitability that non-tradable firms do not get exposed to.[1] This yields:

Prediction 1 *All else equal, profit margins will tend to be higher in non-tradable sectors.*

Our second hypothesis makes a stronger claim that we should observe a stronger marginal effect from an improvement in antitrust institutions in non-tradable sectors, *i.e.,*

Prediction 2 *Strengthening antitrust policies will tend to lower profit margins in non-tradable sectors more than in tradable sectors.*

Although we regard this as plausible given that competition is likely to be weaker in non-tradable sectors, it is not a direct implication of some models. There are direct effects of antitrust policies on prices that will tend to have a higher marginal impact where competition is weaker as we would expect in non-tradable sectors. But things are somewhat less clear cut in models where antitrust policy affects entry and exit.

Ultimately, it is an empirical question whether we find a relationship between antitrust policies and profit margins which is stronger in non-tradable sectors. This is what we explore for the remainder of the paper.

2.3 Data

In this section, we describe core features of the data; Online Appendix 2.A.1 fills in the details. Our core sample is based on the universe of firms contained in Orbis (Bureau van Dijk, BvD) from 2006 to 2015.[2] The dataset contains each firm registered and reports financial statements.[3] Each firm in the data is assigned to a sector using the reported NACE Rev. 2 sector code.

We will differentiate between whether a firm operates in a *tradable* or a *non-tradable* sector. In the baseline, tradable sectors are Agriculture, forestry and fishing (A), Mining and quarrying (B) and Manufacturing (C). All other sectors are labelled as non-tradable. As a robustness check, we follow Mian and Sufi (2014), and include Information and communication (J) among the *tradable* sectors.[4]

2.3.1 Profitability

Our core variable is the firm-specific profit margin according to the Orbis Handbook defined as the Profit/Loss before Tax and External Items over Operating Revenue (times 100).[5] The main analysis uses the average

[1] Although domestic regulators might also have a say about mergers of foreign firms if they operate in domestic markets, this is likely to be hard to detect in the data.

[2] In 2015 there are only 109,043 firms with a non-missing observation for the profit margin. In all other years, there are at least 5.5 million. (The results are robust to dropping 2015 completely, see Column 5, Online Appendix Table 2.D.5.)

[3] It is not possible to distinguish firms going out of business from firms simply not reporting data

[4] We excluded the sector "Activities of extraterritorial organisations and bodies (U)" altogether.

[5] Profit/Loss before Tax and External items is the sum of Operating Profit (which is equal to Gross Profit, *i.e.* Operating Revenue minus Costs of Goods Sold including any interest payments on this, minus Other Operating Expenses) and Loss with Financial Profit/Loss (which is equal to Financial Revenue minus Financial Expenses).

profit margin of all firms in the country-sector after having computed the average profit margin for each firm over the ten year period. We trim the top/bottom 1% of the firms to account for reporting errors, and, in a robustness check, we also trim at the top/bottom 5% of the country-sector cells, see Online Appendix Table 2.D.8.[6] The core sample is defined for 20 sectors and 94 countries containing over 12 million firms of which about 10 million are classified as operating in a non-tradable sector.

To ensure the concentration measures are representative of the country-sector firm composition, we impose a minimum number of firms with relevant data for the country-sector to be included in the data. The usual cutoff that we use is 20 firms in a country-sector but as a robustness check, we will vary this cutoff from 0 (i.e., no restriction at all) to 3000 firms per country-sector.[7]

As an alternative way of aggregating data, we compute each concentration measure at country-sector-year level and then we take the average of these concentration measures over the ten years that we study (we will use the label *Average* to refer to the concentration measures computed in this way). To assess robustness, we will also look at year-by-year results.[8] We also compute a Herfindahl-Hirschman Index (HHI) based on Total Assets and Gross or Net Sales for each country-sector: Even though it is less theoretically sound, we regard the HHI-based Total Assets as better measured since sales are missing for many more firms.[9]

2.3.2 The Antitrust Measure

To measure antitrust policy at a country level, we use the *Total Scope Index Score (Scope Index)* from Hylton and Deng (2007).[10] They code antitrust laws and policies around the world (112 countries in the most recent version) in order to have a metric for the strength of antitrust laws. The authors examine the effects of various components of competition law and assign a score depending on how the national law specifies procedures, penalties, and enforcement.[11] The total index score is the sum of the scores for each category as elaborated further in Hylton and Deng (2007). The minimum possible total index score is 0 and the maximum is 30. For our analysis, it is important to point out that it is principally (if not exclusively) a *de jure* index; it has no direct measure of the effectiveness of antitrust policies in practice. Section II of Hylton and Deng (2007) discusses the methodology extensively. [12] In the empirical analysis below, we average the index over the ten year period of our data (2006-2015). We will also test the robustness of the results to using the budget allocated to the antitrust regulator using data from Bradford et al. (2019).

The highest value of the index is for France with a score of 26 while the US has a value of 24. China (20) scores below the median value while Mexico (23) and India (22) sit just above and below the median respectively. We show in the Appendix that the antitrust index is correlated in a common sense way with a

[6] Trimming is performed at country-sector level after having computed the average profit margin over the ten years period for each firm.

[7] This means that we need at least 20 firms to have at least one financial statement in the data over the ten year period.

[8] We include only those country-sectors where the number of observations is greater than or equal to the cutoff based on the average number of firms used to compute the yearly concentration measures.

[9] We have also experimented with predicting gross (or net) sales using total assets, *i.e.* regressing gross sales on total assets, sector fixed effects and the interaction term at country-year level (or at country level when we predict values of averages over ten years). We then used imputed values where the sales variable is missing (with negative values excluded).

[10] The most recent version of the dataset can be found at *http://www.antitrustworldwiki.com/antitrustwiki/index.php* .

[11] Categories considered: Territorial Scope, Remedies, Private Enforcement, Merger Notification, Merger Assessment, Dominance, and Restrictive Trade Practices.

[12] A special case is represented by Europe. Hylton and Deng (2007) present both regulation from the European Commission and for each country member of the EU, reporting the national antitrust law and the national antitrust law integrated with EU regulation. We consider measures of European-wide Antitrust policies in Online Appendix Table 2.D.5, in which we consider the European Union to a single country with similar results to our baseline specification.

range of variables which represent the quality of institutions.[13] Although these are not causal connections, they suggest that there are important sources of country-level unobserved heterogeneities that probably affect the antitrust regime, thereby reinforcing the need to include country fixed effects in all our regressions. We will return to this when we evaluate whether it is the antitrust index or some more generalized measures of ”good” institutions that is driving the results.

2.3.3 Summary Statistics

Summary statistics on the distribution of profitability, concentration, and the antitrust index are given in Table 2.6.1. This shows how these variables vary within countries, across sectors and across countries within sectors.

Panel A gives the average profit margin both overall and disaggregated using our tradable/non-tradable distinction. The average profit margin in non-tradable sectors is higher with a mean of 7.68 (standard deviation 9.47) when compared to the tradable sectors with a mean of 5.18 (standard deviation 6.63). These descriptive statistics are consistent with Hypothesis 1 based on the assumption that tradable sectors are, on average, more exposed to international trade. The between-country variation is somewhat greater than the within-country variation, thereby suggesting that country-specific factors are at work in determining these differences. Panel B shows that the HHI measure is also higher on average for the non-tradable sectors. It is 4.87 (standard deviation 9.02) for the non-tradable sectors while for the tradable sectors it is 4.03 (standard deviation 8.83).

In Panel C, we give the fraction of country-sectors in our sample that are classified as tradable according to our baseline definition and that used in Mian and Sufi (2014). Our definition suggests that 16% of country-sector observations are in the tradables sector while using the Mian and Sufi (2014) definition, it is 22%. Summary statistics in Table 2.6.2 are consistent with our sample being composed of 10.5 million firms operating in the non-tradable sector out of a total of 12.8 millions firms. It means that tradable sectors represent 17.9% (21.8% using the Mian and Sufi (2014) definition) of our sample. We conclude that most firms are not exposed to international trade and that looking at the competitive impact of trade therefore gives only a partial picture of factors driving firm performance and profitability.

Finally, in Panel D, we report the means and standard deviations of our two core antitrust variables. The wide range of differences in the expenditure measure are particularly striking.

2.4 Core Results

In this section, we outline the main approach taken and core results.

[13]Specifically, we run the following regression:

$$A_c = \alpha + \chi Z_c + \varepsilon_c$$

where A_c is the *Total Scope Index Score* of Hylton and Deng (2007) and Z_c can be any of the following: the log of GDP per capita, the Economic Freedom, Civil Liberties and Political Rights Indices from Freedom House, the democracy and executive constraints measures from PolityIV and the Rule of Law Index from the World Justice Project. The results in Online Appendix Table 2.C.2 show that countries with higher GDP have a better antitrust regime on average using the measure from Hylton and Deng (2007). The index is positively correlated with economic freedom but negatively correlated with political and civil rights. Countries that are more democratic and have stronger executive constraints also have higher scores on the antitrust index; stronger rule of law is also positively correlated with the index.

2.4.1 Empirical Approach

Let P_{cs} be a core profitability or concentration measure in country c and sector s. Then our main regression specification is as follows:

$$P_{cs} = \delta_c + \delta_s + \beta_1 [A_c \times N_s] + \beta_2 N_s + \beta_3 A_c + \varepsilon_{cs} \tag{2.1}$$

where δ_c are country fixed effects, δ_s are sector fixed effects, A_c is the antitrust index and N_s is dummy variable which is equal to one if a sector is classified as non-tradable. P_{cs} and A_c are averaged within-countries using all data between 2006 and 2015.[14] The standard errors are clustered at country level.[15] The inclusion of country fixed effects absorbs the general background variation due to different economic and political circumstances affecting the environment in which firms operate. And sector fixed effects account for technology and other fixed differences at that level.

A test of Hypothesis 1 is whether $\beta_2 > 0$, *i.e.* profit margins are higher in non-tradable sectors due to the absence of foreign competition. However, this coefficient can be estimated only when we exclude sector fixed effects. Nonetheless, we will report what the data say in that case. Similarly, we cannot estimate β_3 when we include country fixed effects from (2.1). But when we exclude them, as we do in one of our specifications below, we expect to find that $\beta_3 < 0$.

Hypothesis 2 says that we should have $\beta_1 < 0$ *i.e.* the antitrust measure matters only for non-tradable sectors. Our most demanding test of Hypothesis 2 is where both country and sector fixed effects are included.

2.4.2 Results

Table 2.6.2 reports the core results.

Column (1) excludes sector fixed effects and finds, in line with Hypothesis 1, that non-tradable sectors tend to have higher profit rates reflecting what we found in the raw data and consistent with the hypothesis that they face weaker competition. In this case, Hypothesis 2 also holds as we find that having a higher value of the antitrust index lowers profit margins more strongly in non-tradable sectors.

Column (2) of Table 2.6.2 has sector fixed effects but no country fixed effects. There is weak evidence that the antitrust measure is negatively correlated with profit margins. But, once again, there is a stronger negative correlation between the antitrust measure and profit margins in non-tradable sectors.

Column (3) has both country and sector fixed effects so constitutes our main specification against which we will assess the robustness of our findings. Now, we have a negative and significant estimate of β_1 in line with Hypothesis 2. It indicates that a one standard deviation increase in the antitrust index of a country is associated with a decline of 10.3 percent of a standard deviation in the profit margin of firms operating in a non-tradable sector. This corresponds to an absolute decline in the profit margin of 0.88 points, corresponding to a 13% drop compared to the mean profit margin. Another way to look at this is to suppose that if China, which has an index value of 20, moved to France's score of 26 (1.45 times the standard deviation of the *Scope Index*) then it would lead to a decrease in the average profit margin of $-0.103 \times 1.45 \times 8.53 = -1.27$, which corresponds to a 19% fall in the average profit margin given the sample average of 6.80. So the effect that we have uncovered is economically meaningful.

[14] In the robustness check, section we repeat the analysis year-by-year.

[15] We have explored alternative clustering of standard errors at country-sector level, equivalent to the Huber-White estimator, and unadjusted standard errors. We have also experimented with weighted regression using the number of firms in each country-sector as a weight. Our results are robust to these changes alternative approaches. See Online Appendix Table 2.D.8.

In Column (4), we show that these findings hold up if we instead use the Mian and Sufi (2014) definition of tradable sector, which reclassifies the Information and Communication sector as tradable along with Agriculture, Manufacturing and Mining. This classifies nearly half a million firms in our sample as tradable. The key coefficient, β_1, is somewhat larger in absolute magnitude. In Column (5), we use a measure of the expenditures allocated for antitrust purposes instead of the antitrust index and the results are qualitatively similar. Finally, Column (6) use the asset-based HHI described above as the left hand side variable. The coefficient β_1 remains negative and significantly different from zero, suggesting that a one standard deviation increase in the antitrust index of a country is associated with the sector operating in a non-tradable sector of that country to be less concentrated by 13.5 percent of a standard deviation. This implies an absolute decline in the HHI of 1.22 points, corresponding to a 27.2% drop relative to the mean level of concentration.[16]

Taken together, these results are supportive of both hypotheses suggested above. Profit margins are higher in non-tradable sectors and, since tradable sectors face more discipline from import competition, the antitrust policy environment matters most for non-tradable sectors.

2.4.3 Sectoral Heterogeneity

We have grouped the coefficients *a priori* based on whether the sector is classified as tradable or non-tradable. As a reality check, we allow for a separate relationship between the antitrust index and sector, *i.e.* we estimate

$$P_{cs} = \delta_c + \delta_s + \sum_{\text{non-tradable sectors}} \beta_s \left(\delta_s \times A_c\right) + \varepsilon_{cs} \tag{2.2}$$

where β_s is the sector specific correlation between the antitrust variable and our outcome of interest. This allows us to assess whether it is the non-tradable sectors that are indeed driving the result. We plot the coefficient β_s for each of our 17 non-tradable sectors where the interval gives the 95% confidence interval in Figure 2.6.1. The three tradable sectors represent the reference group (Agriculture, Manufacturing and Mining).

The first thing to note is that no sector has a positive and significant coefficient. Those that have negative and significant coefficients are transportation and storage, accommodation and food Service, finance and insurance, and real estate. Electricity, gas, steam and air-conditioning, and defence and social security are borderline significant. When we test whether all coefficients are equal to each other, we reject the null hypothesis (pvalue=0.001). As expected we also reject the null hypothesis that they are all equal to zero at the same time (pvalue=0.0007).

2.5 Robustness

We now discuss two robustness checks; many others can be found in the Online Appendix. The first investigates whether it is the antitrust regime that matters rather than just "good" institutions and the second looks at other ways of cutting the data.

2.5.1 Antitrust or Other Country Characteristics?

We show in Table 2.C.2 (Online Appendix) that the antitrust index is correlated with variables that we expect to reflect the economic and institutional environment, specifically: the log of GDP per capita, the Economic Freedom, Civil Liberties, and Political Rights Indices from *Freedom House*, the democracy and executive constraints measures from *PolityIV* and the Rule of Law Index from the *World Justice Project*.

[16]Online Appendix Table 2.D.2 repeats the analysis using an HHI constructed with gross or net sales with similar results.

This raises a possible concern that our results may be driven by these institutional differences rather than the antitrust environment. To assess this, we take these institutional variables and interact them, one by one, with the non-tradable dummy rather than the antitrust index. Specifically we run:

$$P_{cs} = \delta_c + \delta_s + \beta_1 \left[Z_c \times N_s \right] + \varepsilon_{cs} \tag{2.3}$$

where Z_c are the variables from Table 2.C.2 (Online Appendix). If it is the antitrust regime that our measure is capturing then we should not expect to find any significant correlation between Z_c and lower profit margins in non-tradable sectors.

The results are in Table 2.6.3, which shows across the board that there is no significant correlation between these other background economic and institutional variables, and a lower profit margin in non-tradable sectors in spite of the fact that Table 2.C.2 found them to be strongly correlated with the index itself. We would particularly flag that this is true for GPD per capita and democracy indicators. This suggests that our findings are indeed driven by something specific to the antitrust environment as measured in Hylton and Deng (2007).

2.5.2 Alternative Ways of Constructing the Data

To stress test the data that we have and their reliability, we now explore what happens when we try different rules for assembling our profit margin data, different years, and splitting the sample across observations where the measurement is likely to be more reliable. The results are in Table 2.6.4.

To include a country-sector in the data, we required that there were at least 20 observations in a cell. Columns (1) through (3) vary this. In Column (1), we drop any restriction on cell size completely and the core finding is robust. Columns (2) and (3) become more stringent for inclusion with 50 and 200 observations being needed for inclusion. The latter is particularly demanding for quite a few countries and the sample now falls from 94 countries to 63. Yet the results are robust. In Online Appendix Table 2.D.4, we report similar regression results for various other cutoffs, finding results that are in line with Table 2.6.4.

We used the data averaged across all years between 2006 and 2015. In Columns (4), (5) and (6) we pick three representative years (2007, 2011 and 2014) to show that size and significance of the main coefficient of interest does not change. This happens despite losing a few countries in each column compared to the full time period. In Online Appendix Table 2.D.4 and 2.D.5 we provide results for each year separately as well as alternative ways of aggregating data.[17]

Finally, we address the concern that our results could be driven by poor data quality in some countries. Column (7) of Table 2.6.4 offers one important robustness check: it restricts the sample to countries that have at least 19 sectors with sufficient data to be included (where 20 is the maximum number of sectors possible). This serves as a check on data quality since some countries have limited data coverage in Orbis that leads to the exclusion of entire sectors. Our findings are not affected by imposing this restriction. In Online Appendix Table 2.D.7, we present other alternatives with similar conclusions.

2.6 Concluding Comments

This paper has explored a specific aspect of institutional quality, namely the strength of *de jure* antitrust policy. For tradable goods, exposure to import competition serves as a disciplining mechanism for firms, leading to lower profit margins. However, for non-tradable goods, what firms do depends on how governments set the framework for and implement competition policy. This is important since 82% of all firms

[17] The only insignificant sub-sample is for 2015 for which we have a much smaller sample size.

in our data operate in non-tradable sectors and there is little scope for international competition to improve their performance.

To explore this, we have built a global dataset based on Orbis and used an "off-the-shelf" measure of antitrust policy constructed by legal scholars. We find that stronger antitrust policy depresses profit margins but only in non-tradable sectors. This suggests that competition policy is particularly important in parts of the economy that are not exposed to import competition. This is particularly relevant given that we find that there is a greater concentration in non-tradable sectors. Our data cover a range of diverse economies including some low income and emerging market countries. This has pros and cons; data quality is likely to be lower in less-developed parts of the world, but we benefit from having more variation in the range of antitrust policies to learn from.

Although we find robust results for the measures that we use, we acknowledge that antitrust is only one aspect of what matters in determining profitability and firm performance. Moreover, even though we have ruled out the claim that our findings are simply a reflection of "good institutions" in general, there are some aspects of competition policy that we could be picking up since they may be correlated with the antitrust index that we use, a prime example being the regulation of entry.[18] Future work based on other specific dimensions of policy would therefore be valuable.

The paper fits into wider debates about cross-country developments in competition policy, in particular the respective roles of technology versus antitrust policies in shaping profits.[19] While our findings cannot adjudicate between these views, they do underline a potential role for antitrust policies in explaining cross-country differences in profitability. But it also suggests an interaction between this and how far a country is exposed to international competition. Our results suggest that looking at the benefits of competition across the whole economy is important and perhaps deserves more attention. This is relevant in advanced countries too, such as the U.S., where there are concerns about the potential consequences of "going soft" on competition policy, something which is likely to matter most where there is little competition from abroad. One interesting topic for future research is to investigate whether heavily protected tradables sectors also seem to respond to antitrust institutions similar to non-tradables.

Our paper also contributes to the wider agenda of opening up the "black box" of institutional and policy differences. Competition policy is very specific and, while related to other commonly used measures of institutional difference, seems to have quite specific effects. The findings also support, therefore, for increased efforts to make competition policy more effective.

[18] We have looked at whether the World Bank Doing Business indicator of the regulation of entry yields similar results but have found no evidence of this.

[19] See, for example, Autor et al. (2020) and Philippon (2019)

Tables

Table 2.6.1: Summary Statistics

	Obs	Mean	Sd	Sd between	Sd within	Min	Median	Max
Panel A								
Average Profit Margin	1,224	7.27	9.11	10.78	6.58	-18.85	5.52	50.76
Average Profit Margin (tradable)	201	5.18	6.63	6.01	3.58	-9.71	4.33	42.28
Average Profit Margin (non-tradable)	1,023	7.68	9.47	11.34	6.63	-18.85	5.76	50.76
Panel B								
HHI Assets	1,245	4.73	8.91	6.51	7.39	0.00	0.97	89.31
HHI Assets (tradable)	206	4.03	8.33	9.94	4.22	0.00	1.02	88.45
HHI Assets (non-tradable)	1,039	4.87	9.02	6.42	7.47	0.00	0.96	89.31
Panel C								
Tradable sector (Baseline)	1,224	0.16	0.37			0	0	1
Tradable sector (Mian and Sufi)	1,224	0.22	0.41			0	0	1
Panel D								
Total Scope Index Score	1,110	21.93	3.69			8.00	23.00	27.00
Budget USD million (2006-2010)	1,015	21.05	45.47			0.00	7.14	258.12

Note: The unit of analysis is a country-sector. In Panel A, C and D we use data on country-sectors with at least 20 firms with non missing data on the average profit margin. In Panel B we consider the country-sectors with at least 20 firms with non-missing data on Assets. All variables are averaged over the entire sample period (2006-2015). The average profit margin and HHI Assets have been calculated after trimming the top/bottom 1% of firms within each country-sector.

Table 2.6.2: Main results: Profit Margin and Concentration

	Average Profit Margin (std)					HHI assets (std)
	(1)	(2)	(3)	(4)	(5)	(6)
Non-tradable sector x	-0.171	-0.0819	-0.103	-0.137	-0.0502	-0.135
Antitrust Index - β_1	(0.0512)***	(0.0420)*	(0.0411)**	(0.0374)***	(0.0202)**	(0.0554)**
Non-tradable sector - β_2	0.284					
	(0.0513)***					
Antitrust Index - β_3		-0.104				
		(0.0535)*				
Antitrust Index	Scope Index	Scope Index	Scope Index	Scope Index	Budget (USD)	Scope Index
Tradable definition	Baseline	Baseline	Baseline	Mian and Sufi	Baseline	Baseline
Sample	2006-2015	2006-2015	2006-2015	2006-2015	2006-2010	2006-2015
Observations	1,110	1,110	1,110	1,110	913	1,122
R-squared	0.402	0.330	0.631	0.633	0.648	0.447
N firms	12,800,308	12,800,308	12,800,308	12,800,308	9,200,182	20,017,937
N firms non-tradable	10,515,246	10,515,246	10,515,246	10,004,777	7,432,724	17,105,026
N countries	94	94	94	94	75	97
N sectors	20	20	20	20	20	20
Mean dependent variable	6.80	6.80	6.80	6.80	6.28	4.49
St. Dev. dependent variable	8.53	8.53	8.53	8.53	8.00	9.05
Country FE	YES	NO	YES	YES	YES	YES
Sector FE	NO	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 2.1. The sample is defined as in Table 2.6.1, Panel A. Standard errors are clustered at a country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or loss before tax and external items over operating revenue. The variable Antitrust Index measures the intensity of antitrust activities, as defined by Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. The variable non-tradable is a dummy variable equal to one for all sectors except Agriculture, Manufacturing and Mining in Column 1 to 3, 5 and 6, adding sector J (Information and Communication) following Mian and Sufi (2014) in Column 4; see the Online Appendix Section A.1.1 for details. In Column (5) the antitrust measure is the Budget in USD from Bradford et al. (2019); see Section 2.3 for details. Column 5 uses data only for 2006-2010 due to the limited years covered by this antitrust policy measure. In Column (6) the dependent variable is the Herfindahl - Hirschman Index which measures the industrial concentration based on firm assets, ranging from 0 (perfect competition) to 100 (monopoly). Country fixed effects are included in Column (1), sector fixed effects are included in Column (2), while Column (3)-(6) include both country and sector fixed effects. *** $p<0.01$; ** $p<0.05$; * $p<0.1$.

Table 2.6.3: Institutional Indexes

	Average Profit Margin (std)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-tradable sector x	-0.0784	-0.0609	0.0678	0.0701	-0.0374	-0.0323	-0.0782
Institutional Index - β_1	(0.0486)	(0.0550)	(0.0514)	(0.0494)	(0.0539)	(0.0581)	(0.0524)
Observations	1,209	1,183	1,193	1,193	1,140	1,140	984
R-squared	0.630	0.628	0.634	0.634	0.631	0.631	0.628
N firms	13,487,883	13,487,281	13,488,237	13,488,237	13,435,159	13,435,159	13,093,172
N firms non-tradable	11,134,868	11,134,347	11,134,185	11,134,185	11,090,519	11,090,519	10,793,174
N countries	118	112	117	117	110	110	89
N sectors	20	20	20	20	20	20	20
Mean dependent variable	7.20	7.03	7.03	7.03	7.07	7.07	7.09
St. Dev. dependent variable	9.02	8.89	8.89	8.89	8.84	8.84	8.88
Institutional variable	Log GDP pp	Economic Freedom	Civil Liberties	Political Rights	Polity IV	Executive Constraints	Rule of Law
Country FE	YES	YES	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 2.3. The sample is defined in Table 2.6.1, Panel A. Standard errors are clustered at a country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The variable denoted as the Institutional Index measures various country characteristics; see the Online Appendix Section A.3 for details and sources. All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

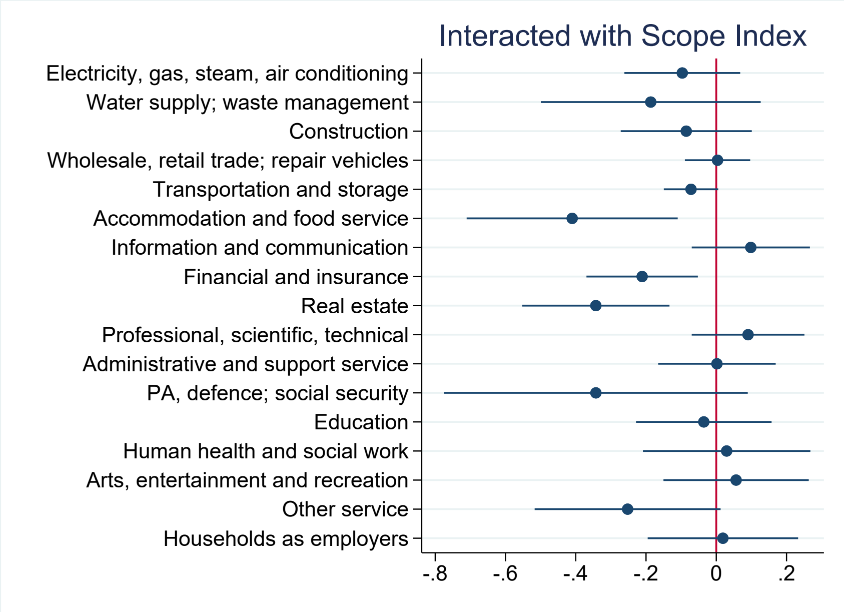
Table 2.6.4: Other robustness

	Average Profit Margin (std)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-tradable sector x	-0.123	-0.12	-0.0857	-0.14	-0.148	-0.149	-0.101
Antitrust Index - β_1	(0.0380)***	(0.0367)***	(0.0482)*	(0.0596)**	(0.0560)***	(0.0355)***	(0.0555)*
Cutoff	0	50	200	20	20	20	20
Countries	All	All	All	All	All	All	At least 19 sectors
Sample	2006-2015	2006-2015	2006-2015	2007	2011	2014	2006-2015
Observations	1,389	955	769	877	970	904	633
R-squared	0.541	0.688	0.697	0.692	0.582	0.633	0.646
N firms	12,802,233	12,795,362	12,775,024	5,530,434	5,990,410	6,118,058	11,583,782
N firms non-tradable	10,516,750	10,510,797	10,494,919	4,355,641	4,907,369	5,185,113	9,489,360
N countries	109	80	63	83	88	84	33
N sectors	20	20	20	20	20	20	20
Mean dependent variable	7.49	6.24	5.83	8.13	6.30	7.50	5.25
St. Dev. dependent variable	9.71	8.09	7.64	8.63	7.79	7.76	6.91
Country FE	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 2.1. The sample is defined as in Table 2.6.1, Panel A unless specified. We include the country-sectors with at least 0, 50 or 200 firms with non-missing data to calculate the average profit margin in Columns 1, 2 and 3. Columns 4-6 report the results year-by-year for three years (2007, 2011 and 2014). Column 7 restricts the sample to countries with at least 19 sectors with at least 20 firms with non-missing data to measures the average profit margin. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The variable Antitrust Index measures the intensity of antitrust activities, as defined by Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. The variable non-tradable is a dummy variable equal to one for all sectors except Agriculture, Manufacturing and Mining. All columns include both country and sector fixed effects. In Columns 4-6 the average profit margin is computed after trimming the top/bottom 1% of firms within each country-sector-year. *** p<0.01; ** p<0.05; * p<0.1.

Figures

Figure 2.6.1: Results by sector: Profit Margin



Note: This graph augments the analysis of Table 2.6.2, Column 3, by including an interaction between the Antitrust Index and a sector dummy. We report these interactions (*i.e.* β_s in equation 2.2). Excluded sectors are the sectors in the *Baseline* definition of Tradable (Agriculture, Manufacturing and Mining). The sample is defined as in Table 2.6.1, Panel A. Standard errors are clustered at a country level, we report the 95% confidence interval. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The Antitrust Index measures the intensity of antitrust activities, according to Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. All specifications include both country and sector fixed effects.

2.A Online Appendix – Data Sources and Description

2.A.1 Orbis Dataset

We collect the universe of firms contained in Orbis (Bureau van Dijk, BvD) from 2006 to 2015[20]. We refer to the 2016 version of the dataset, we got access in September 2017 The Orbis dataset reports financial statements for each firm ever registered in the each period. [21] The data includes: a unique firm identifier, country code (ISO, 2 digits), NACE Rev. 2 main section code and yearly data on operating revenue, net income, total assets, profit margin, price earning ratio, number of employees, gross sales, net sales, financial revenues and financial expenses.[22] The profit margin is defined (see the Orbis Handbook) as Profit/Loss before Tax and External Items over Operating Revenue (times 100).[23] The original dataset contains roughly 160 million of observations. However, only 130 million of these report a sector code.

Sectors

We assign each firm to one sector using the NACE Rev. 2 main section code reported in Orbis as the reference (we will refer to this as a firm's "sector" unless otherwise specified). The list of sectors is in Appendix Table 2.C.1. We divide sectors into *tradable* and *non-tradable*. In the baseline, tradable sectors are: Agriculture, forestry and fishing (A), Mining and quarrying (B) and Manufacturing (C).

Missing NACE codes If the NACE Rev. 2 main section code is missing we rely on the following codes present in the data with the following order giving the hierarchy used in filling the gaps:

1. NACE Rev. 2 Core code (4 digits). We convert the 4 digits NACE codes to the main section code using the first two digits as shown in Online Appendix Table 2.C.1
2. NACE Rev. 2 Primary code(s).
3. NACE Rev. 2 Secondary code(s).
4. NAICS 2012 Core code (4 digits). We map the 4 digit NAICS 2012 codes to NACE Rev. 2 4 digits code (and then we are able to assign automatically the corresponding NACE Rev. 2 main section). The source of the mapping tables is Eurostat. If more than one NAICS code is assigned to more than one NACE Rev. 2 main section then we conduct manual checks.
5. NAICS 2012 Primary code(s).
6. NAICS 2012 Secondary code(s).
7. US SIC Core code (3 digits). We map the 3 digits US SIC codes (1987 version) to NAICS 2007 codes and then to NAICS 2012 codes (above mapping then applies). Source: US Census. Going through the NAICS codes is necessary as a direct mapping from US SIC to NACE Rev. 2 does not exist. Manual checks were also carried on here to ensure that to each US SIC code only one NACE Rev. 2 main section
8. US SIC Primary code(s).
9. US SIC Secondary code(s).

[20] In 2015 there are only 109,043 firms with non missing profit margin. In all other years there are at least 5.5 millions

[21] It is not possible to distinguish firms going out of business from firms simply not reporting data

[22] Operating revenue, net income, total assets, gross sales, net sales, financial revenue and financial expenses are reported in thousands of US Dollars. Profit margin in percentage points

[23] Profit/Loss before Tax and External items is the sum of Operating Profit (which is equal to Gross Profit, *i.e.* Operating Revenue minus Costs of Goods Sold, minus Other Operating Expenses) and Loss with Financial Profit/Loss (which is equal to Financial Revenue minus Financial Expenses)

Creating a unique NACE code By construction, the original dataset has repeated observations for the same firm whenever the firm operates in more than one sector (either it was a main, primary, or secondary sector). Most of the time, the only information that varies is the sector code reported while data from financial statements are constant. Having converted everything, we delete duplicates in NACE Rev. 2 main section. However, some duplicates may remain whenever different NACE Rev. 2 main section codes are reported. As a first step, we look at which sector represents the biggest share of sales. If there are duplicates (mainly due to missing information on sales), we keep the observation with the smallest number of missing observations in the financial statements. If this procedure does not resolve all cases of duplication, we randomly select among the duplicated codes for that observation.

The Mian and Sufi division between tradable and non-tradable sectors As a robustness check in Table 2.6.2, following Mian and Sufi (2014), we also include Information and communication (J) among the *tradable* sectors (the *Mian and Sufi* definition).Mian and Sufi (2014) classify 294 4 digit 2012 NAICS industry codes as non-tradable, tradable, construction or other industries. They also report the percentage of the entire 2007 US labour force represented by each industry. We match the 2012 NAICS 4 digits code to NACE Rev. 2 4 digit codes and sum tradable and total labour force by NACE Rev. 2 main section code. We compute the relative share of tradable within each NACE Rev. 2 main section code. Sectors A, B, C, J are the only four sectors with positive shares of the labor force in tradable sectors according to the Mian and Sufi (2014) definition (6.3%, 60%, 86% and 7.2% respectively). Computing the number of industries classified as"tradable" in each NACE Rev. 2 main section code delivers similar results. In particular: 25% of industries in code A, 80% of code B, 87% of code C, 5.9% of code J are categorized as tradable by Mian and Sufi (2014).

Data Cleaning The original data contains some extreme outliers. We therefore used a trimmed version of all variables. Specifically, we trimmed the top and bottom 1%.[24] This trimming exercise is performed at a country-sector level in the main sample.

The cross-section sample

The unit of analysis is a country-sector. For the main analysis, we use the average profit margin constructed by taking the average profit margin of all firms in a country-sector over a ten year window. In this exercise, trimming is performed at a country-sector level. We refer to this as the *mean* concentration measure.

To ensure that these concentration measures are representative of the country-sector firm composition, we require a minimum number of observations for the relevant country-sector for it to be included in the data. The baseline cutoff is at 20. However, as a robustness check, we also consider 0 (i.e. no restriction at all), 10, 30, 40, 50, 200 or 3000 firms per country-sector.[25] The baseline cross sectional sample at a country-sector level represents around 13 million firm-level observations.[26]

Appendix Figure 2.C.1 reports the number of firms with non-missing profit margin data disaggregated by continent. This shows that most of our firms are not coming from developed countries. We have a significant number of firms located in Asia, Eastern Europe and Latin America, and relatively few, from North America.

Appendix Figure 2.C.2 reports the number of firms in each sector. This shows the importance of focusing

[24]Results not trimmed and trimming top and bottom 5% are presented as robustness in Appendix Table 2.D.8, no differences arise

[25]As we first compute the average of firms balance sheets it means that we will need at least 20 firms to have at least one balance sheet data reported in the ten years period

[26]Just to recall all the restrictions imposed: we drop all firms not reporting any sector code, we drop all the firms in the top/bottom 1% of the distribution of the variable of interest (*e.g.* profit margin, assets, ...) at country-sector, we drop all firms not part of a country-sector reporting data for the variable of interest for at least 20 firms

firms outside manufacturing, in contrast to much work on level behavior. We will also be exploiting data from some large sectors such as retail and construction.

Appendix Figure 2.C.3 reports the total number of sectors available for each country averaged by continent when we use our requirement of at least 20 firms per country-sector. It shows that 58 (out of 123) countries have more than 10 sectors with more than 20 firms. Most of them are in Western Europe, Eastern Europe, and North America. This graph, combined with Appendix Figure 2.C.1, suggests that some countries (*i.e.* look for example at Latin America) may have many firms reported, but most of them will come from the same sectors, rather than being spread equally among many.

Appendix Figure 2.C.4 reports the percentage of country-sectors with more than 20 firms. It shows that almost every country has at least 20 firms in the Financial and Insurance sector. In addition, 10 sectors (out of 21) have more than 50% of our sample countries represented.

Alternative Aggregation As an alternative way of aggregating the data, we compute each concentration measure at a country-sector-year level before averaging over the ten years (we will call this variable the *average* concentration measure). Results using this alternative aggregation approach (or yearly concentration measures) are similar to the baseline as we show in Appendix Tables 2.D.4 and 2.D.5 below. [27]

The HHI Index as a Measure of Concentration We have also computed Herfindahl-Hirschman Index (HHI) for each country-sector using the total assets and gross or net sales from Orbis. We prefer to use HHI based on total assets for two main reasons: i) when looking at non-tradable sectors, it is not at all straightforward how to consider sales and ii) the sales variables in Orbis contain many missing observations. To mitigate this problem we have tried imputing gross (or net) sales based on the relationship between this variable and total assets. Specifically, we regress gross sales on total assets, sector fixed effects and country$\times$year fixed effects (or at country fixed effects when we predict values of averages over ten years). Predicted values are then imputed only if the original variable (gross sales in the example) is missing. Negative predicted values are also excluded.

Capturing Entry and Exit In the original Orbis dataset, we are unable to observe entry and exit. Specifically, we cannot distinguish whether missing values are due to firm not yet existing/ceasing to exist. We therefore proceed as follows:

We define entry in the following way: a firm enters in year t if we start observing data from the financial statement in year t. We apply this procedure for the 2008-2015 period[28].

We define exit in the following way: a firm exits our sample in year t if we do not observe data from the financial statement in any of the following years. We apply this procedure for the 2006-2012 period.[29]

Since we are only interested in firms reporting data, we follow these procedures before trimming the variables in the data from financial statements. We compute the share of firms entering (exiting) for each year-country-sector over the total number of firms in the country-sector. Finally, we compute the average entry (exit) share of firms for each country-sector. In the analysis, we restrict to country-sectors with at least 20 firms reporting data from their financial statements.

[27] Defining the cutoff to have the country-sector included in the sample, is straightforward: we include the country-sectors with a number of observations greater than or equal to the cutoff. If we consider the average of these yearly concentration measures we compute the average number of firms used to compute the yearly concentration measures. Trimming top and bottom 1% is performed at country-sector-year

[28] Applying the same procedure in 2006, first year of data, and 2007 may result in wrong entry assignments

[29] Applying the same procedure in 2013-2015 may result in wrong exit assignments. We exclude three years when looking at firm exit because 2015 reports a substantially lower number of firms

2.A.2 The Antitrust Measure

We use the *Total Scope Index Score (Scope Index)* from Hylton and Deng (2007).[30] They code antitrust laws and policies around the world (112 countries in the most recent version) in order to create a metric of antitrust laws. This is constructed by examining various components of competition law and assign a score depending on how national laws govern conduct, penalties or enforcement.[31] The total index score is the sum of the scores for each sub-category. The minimum value is 0 while the maximum is 30. This is mainly a *de jure* index and does not measure the effectiveness of these laws. Section II of Hylton and Deng (2007) discusses the methodology at length. [32] We average the index of our ten year period (2006-2015).

Figure 2.C.5 shows the geographical distribution of this index.

It gives a sense of the country coverage and areas of the world where antitrust laws are rated to be stronger or weaker. There are notable countries without data, including most of sub-saharan Africa.

We show in the Appendix Table 2.C.2, that the antitrust index is correlated in a common sense way with a range of variables which represent the quality of institutions. Specifically, we run the following regression:

$$A_c = \alpha + \chi Z_c + \varepsilon_c \tag{2.4}$$

where A_c is the *Total Scope Index Score* of Hylton and Deng (2007) and Z_c is variously: log of GDP per capita, the Economic Freedom, Civil Liberties and Political Rights Indices from Freedom House, the democracy and executive constraints measures from PolityIV, and the Rule of Law Index from the World Justice Project. Appendix Table 2.C.2 shows that countries with higher GDP are classified, on average, as having a better antitrust regime according to Hylton and Deng (2007). The index is positively correlated with economic freedom but negatively correlated with political and civil rights. Countries that are more democratic and have stronger executive constraints also have a higher score on the antitrust index. And stronger rule of law is positively correlated with the index. Although these are not causal relationships, it suggests that there are important sources of country-level unobserved heterogeneity that are likely to affect the antitrust regime, thereby reinforcing the need to include country fixed effects in all our regressions.

To supplement this index, we use Bradford et al. (2019) to measure the budget (in USD) allocated by each country for antitrust agencies as an alternative measure for antitrust policies [33]. This is available only up to 2010. However, we will use it alongside the scope index as robustness check in Table 2.6.2. To ensure a valid comparison with other results, we will average the concentration index measures over the period 2006-2010.

We also run the analysis using the Competition Law Index (*CLI*) from Bradford and Chilton (2018) [34]. This is similar to the *Scope Index* and covers a larger group of countries. However, it is also only available up to 2010. Results (available upon request) show a negative, although insignificant correlation between this and our measure of profitability in a specification similar to column (3) of Table 2.6.2. To ensure a valid comparison we average the concentration index measure only over the period 2006-2010 when we do this.

[30] The most up to date dataset can be found here. We access the data in May 2018

[31] Categories considered Territorial Scope, Remedies, Private Enforcement, Merger Notification, Merger Assessment, Dominance, and Restrictive Trade Practices.

[32] A special case is represented by Europe. Hylton and Deng (2007) present both regulation from the European Commission and for each country member of the EU, reporting the national antitrust law and the national antitrust law integrated with EU regulation. We ignored the purely European Commission law and whenever there was a conflict between purely national and national with EU regulation antitrust law (*i.e.* both reported in the same year) we had the latter to dominate. We consider measures of European-wide Antitrust policies in Appendix Table 2.D.5, in which we consider the European Union to a single country with similar results to our baseline specification.

[33] Data can be accessed `here` (Comparative Competition Enforcement Dataset, accessed August 2019).

[34] Data can be accessed `here` (Comparative Competition Law Dataset).

In an effort to capture the effectiveness of antitrust policies we looked at the yearly Global Competitiveness Report from World Economic Forum[35]. We particularly focus the Executive Opinion Survey question which asks respondents: "In your country, to what extent does anti-monopoly policy promote competition?" where the answer can be from 1 (does not promote competition) to 7 (effectively promotes competition).[36] We compute an average for this variable over the ten years period of analysis (2006-2015). Results (available upon request) show a negative, although insignificant, correlation in our main specification akin to Column (3) of Table 2.6.2 when using this alternative indicator of antitrust policy.

2.A.3 Other variables

We have also collected a range of country-level variables to use in our analysis: GDP per capita, PPP (constant 2011 international $), (source: World Bank); Summary index of Economic Freedom of the World, (source: Fraser Institute); Civil Liberties Index and Political Rights Index, (source Freedom House); Polity IV and Executive Constraints Index, (source: Centre for Systemic Peace); Overall score among Rule of Law, (source: World Justice Project). For all these variables we compute the average over the ten years period of analysis (2006-2015).[37]

2.A.4 Summary Statistics

Summary statistics on the distribution of profitability, concentration, and the antitrust index are given in Table 2.6.1. This shows how these variables vary within country across sector and across country within sector. Panel A gives the average profit margin both overall and disaggregated using our tradable/non-tradable distinction. The average profit margin in non-tradable sectors is higher with a mean of 7.68 (standard deviation 9.47) compared to a mean of 5.18 (standard deviation 6.63) for the tradable sectors. These raw data are consistent with Hypothesis 1 based on the idea that tradable sectors are more exposed to international trade. The between country variation is somewhat greater than the within country variation suggesting that country-specific factors are at work in determining these differences. Panel B shows that the HHI measure based on assets is also higher on average for the non-tradable sectors. It is 4.87 (standard deviation 9.02) for the non-tradable sectors while for the tradable sectors it is 4.03 (standard deviation 8.83).

In Panel C, we give the fraction of country-sectors in our sample that are classified as tradable according to our baseline definition and that used in Mian and Sufi (2014). Our definition suggests that 16% of country-sector observations are in the tradables sector while using the Mian and Sufi (2014) definition, it is 22%. Summary statistics in Table 2.6.2 are consistent with our sample being composed of 10.5 million firms operating in the non-tradable sector out of a total of 12.8 millions firms. It means that tradable sectors represent 17.9% (21.8% using the Mian and Sufi (2014) definition) of our sample. We conclude that most firms are not exposed to international trade and that looking at the competitive impact of trade therefore gives only a partial picture of factors driving firm performance and profitability.

Finally, in Panel D, we report the means and standard deviations of our two core antitrust variables. The wide range of differences in the expenditure measure are particularly striking.

Appendix Table 2.C.3 presents summary statistics for additional key variables used in the analysis. HHI

[35]Each year Global Competitiveness Report from World Economic Forum, see for example 2015-2016 version here

[36]Nicholson (2008) looks at the relationship between this *De Facto* measure and Hylton and Deng (2007)

[37]With the exception of Rule of Law index available only in 2012, 2014 and 2015. Sources: GDP per capita, PPP (constant 2011 international $): World Bank provides now data at constant 2017 international here (accessed April 2018). Link reported refers to the 2017 international as no link for constant 2011 is available. Summary index of Economic Freedom of the World: here (accessed June 2018). Civil Liberties Index and Political Rights Index: here (accessed June 2018). Polity IV and Executive Constraints Index: here (accessed April 2018). Overall score among Rule of Law: here (accessed June 2018)

gross sales and HHI net sales have been computed after trimming the variable of interest in the sample at 1% at country $\times$ sector level. We are also restricting the sample to country-sectors with at least 20 firms reporting data in the variable of interest. In Panel B, we restrict the attention to country-sectors with at least 20 firms reporting average profit margin.

2.B Online Appendix – Additional Results and Robustness

In this section, we present some additional results and a range of robustness checks.

Entry and exit We investigate one of the many possible mechanisms behind the negative relationship between antitrust, profits, and concentration. Our main hypothesis is that antitrust policy may induce competition by lowering the regulatory burden and fixed costs. A corollary of this may imply that antitrust induces a differentially positive effect on entry and exit in non-tradable sectors compared to tradable ones. As a result, the following equation verifies whether antitrust is associated with the firm entry and exit (measured as described in Online Appendix, section 2.A.1). We regress $Entry_{cs}$, which measures the average entry of firms in country c and sector s on an interaction between the antitrust index, $Antitrust_c$, and the dummy taking unit value for sectors classified as non-tradable, $Non-Tradable_s$, including country and sector fixed effects. The same regression is also presented for the average share of exiting firms:

$$Entry_{cs} = \beta_1 Antitrust_c \times Non-Tradable_s + \delta_c + \sigma_s + \epsilon_{cs}. \tag{2.5}$$

Column (1) of Appendix Table 2.D.1 shows that one standard deviation higher antitrust index is associated with a 6.78 percent increase in the standard deviation of the share of firms entering in country c and sector s. This is statistically significant, corresponding to an increase of 2.2% relative to the mean. Changes in antitrust policy do not correlate well with the exit of firms, as shown in Column (2). This is true both in terms of significance and the point estimate is an order of magnitude smaller than for entry. The finding in Column (1) is consistent with antitrust policy lowering barriers to entry, which may increase the likelihood of new firms entering (or existing firms growing in size). At the same time, the lack of response on exit is in line with antitrust policy leading to lower profits, but insufficiently so to drive firms from the market.

Alternative HHI We consider alternative concentration measures and verify their robustness with our main results. Column (6) of Table 2.6.2 shows that the HHI based on assets, is negatively correlated with the antitrust in non-tradable sectors. We repeat this analysis in Appendix Table 2.D.2 by using two different concentration measures: a) the HHI based on gross sales in Column (1) and b) the HHI using net sales in Column (2). The results of Online Appendix Table 2.D.2 are in line with Table 2 both in terms of sign and magnitude.

Cutoff We modify the sample threshold defining our sample as we did in Table 2.6.4, Columns 1-3. In Section 5 of the paper, we considered only country-sector cells containing at least 20 firms and disregard all country-sector cells with a smaller number of firms. This generates comparable cells across countries and sectors. Online Appendix Table 2.D.4 replicates our baseline specification presented in the Column (3) of Table 2.6.2, including country and sector fixed effects and only changes the minimum number of firms necessary to include a country-sector cell with cutoffs between 0 and 3000 firms. The results in all columns are statistically indistinguishable from those in Table 2.6.2. The loss of statistical significance in columns further to the right is most likely related to power issues.

Alternative samples We show that our results are robust to different sampling strategies as we did in Table 2.6.4, Columns 4-6. Online Appendix Tables 2.D.4 and 2.D.5 explore the specification presented in Column (3) of Table 2.6.2 on the sample for the years 2006-2015. In Column (1), we verify that our main result is unaffected if we change the timing of our sample and take the average profit margin over the 2006-2010 period to make it comparable with column (5) of Table 2.6.2. Column (2) shows that our main result is unchanged if we analyze the average of yearly average profit margin at country-sector level, rather than first averaging profit margin for each firm over the ten years and then average by country-sector. We then repeat the analysis by defining our sample as the mean over a single year and verify that the results are statistically indistinguishable from the core results. Hence, Columns (3) to (8) of Online Appendix Table 2.D.4 report our main result for single years: from 2006 to 2011. Online Appendix Table 2.D.5 reports the same coefficients in a year-by-year fashion from 2012 until 2015 in Columns (1) to (4). The latter is the only year that shows a marginally insignificant estimate, but with a much smaller sample: the number of country-sector cells in 2015 is 167, compared to roughly 900 for all other years, and around 1.5% of the firms reporting data, compared to other years.

To ensure that including 2015 does not alter our results, we look at Column (3) of Table 2.6.2 for the years 2006-2014. The results are identical.

We now allow the EU to be treated as a single country; the results are in Online Appendix Table 2.D.5. Here, we calculate the average profit margin (and the HHI index) treating all EU countries as a single country. The countries considered in the EU in 2006 (the first year of data) are: Austria, Belgium, Cyprus, Czech Republic, Denmark, Estonia, France, Finland, Germany, Hungary, Italy, Ireland, Latvia, Lithuania, Luxembourg, Greece, Malta, Netherlands, Poland, Portugal, Slovakia, Slovenia, Spain, and Sweden. Bulgaria and Romania (which joined in 2007) and Croatia (which joined in 2013) are excluded. Columns (6), (7) and (8) replicate the specifications in Columns (3), (5) and (6) of Table 2.6.2. The basic findings are robust to aggregating the EU into a single entity. However, the coefficient in Column (6) is not significant although we cannot reject the coefficient being the same as Column (3) in Table 2.6.2. In addition to the results shown, we averaged (weighting by size of country-sector) each country-sector within EU (both for our antitrust indexes and the concentration measures) and the results are almost identical.

Weighting Profit Margin by Operating Revenues This sub-section shows that our results are almost identical if, when computing the average profit margin at country-sector, we consider a weighted average by operating revenues instead of the simple average. Results are shown in Appendix Table 2.D.6 in which we replicate Columns (1) to (5) of Table 2.6.2. Only Column (5) is not significant although we cannot reject the coefficient being the same as Column (5) in Table 2.6.2. All the other coefficients of interest are very similar and, if anything, larger.

Data quality Here we explore whether our results depend on the poor data quality of some countries. Column (7) of Table 2.6.4 offers one important robustness check: it restricts the sample to countries that have at least 19 sectors with sufficient data to be included (where 20 is the maximum number of sectors possible). This serves as a check on data quality since some countries may have limited data in Orbis which leads to the exclusion of entire sectors. We now verify that this is not a problem in our setting. Online Appendix Table 2.D.7 shows a further robustness check based on Column (7) of Table 2.6.4 when the minimum number of sectors per country is varied from 5 to 20. The point estimate is unchanged as this threshold moves from 5 in Column (1), to 10 in (2), to 15 in (3) and to 20 in (4). As in previous tests, a higher threshold leads to a small number of observations and firms, weakening the statistical precision but leaving the point estimate unchanged.

Various robustness Additional robustness checks are in Online Appendix Table 2.D.8. Column (1) presents results from a weighted regression, with the weight given by the number of firms in the country-sector with a non-missing profit margin[38]. The next two columns consider different "trimming" strategies. In our baseline estimates, we had trimmed the top and bottom 1% of firms, in line with much of the literature that

[38]In this case we want to give more relevance to country-sectors with more firms represented. This is different from what we do in Online Appendix Table 2.D.6 where we are giving more weight to larger firms when computing average profit margin at country-sector level.

uses Orbis data. Our findings are robust to not doing any trimming (Column (2)) and to more restrictive trimming at 5% in Column (3). In Column (4), we also verify that our results are robust to a different way of trimming by removing the top/bottom 1% of concentration measures. Columns (5) and (6) vary the way in which we cluster our standard errors. Column (5) has unadjusted standard errors and Column (6) clusters at a country-sector level (equivalent to robust standard errors).

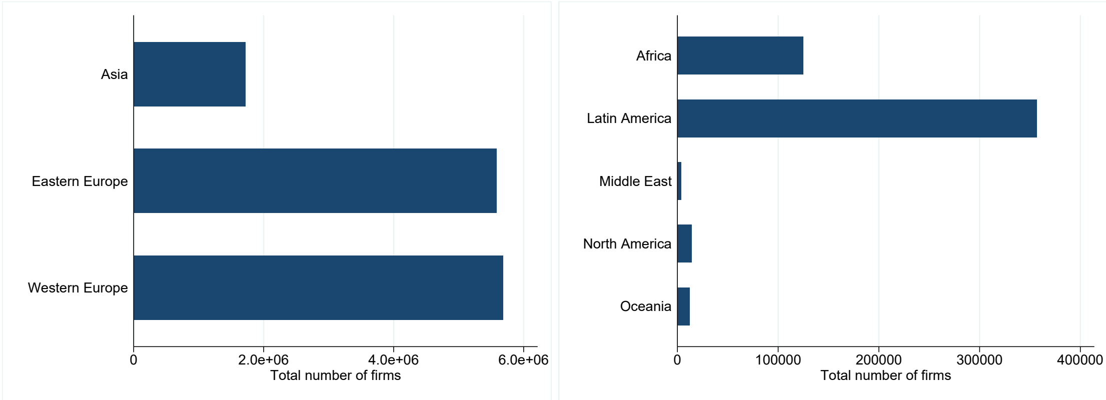
2.C Online Appendix – Additional Tables and Figures

Table 2.C.1: Nace Revision 2 codes

Main section	Description	2 digits
A	Agriculture, forestry and fishing	01 – 03
B	Mining and quarrying	05 – 09
C	Manufacturing	10 – 33
D	Electricity, gas, steam and air conditioning supply	35
E	Water supply; sewerage, waste management and remediation activities	36 – 39
F	Construction	41 – 43
G	Wholesale and retail trade; repair of motor vehicles and motorcycles	45 – 47
H	Transportation and storage	49 – 53
I	Accommodation and food service activities	55 – 56
J	Information and communication	58 – 63
K	Financial and insurance activities	64 – 66
L	Real estate activities	68
M	Professional, scientific and technical activities	69 – 75
N	Administrative and support service activities	77 – 82
O	Public administration and defence; compulsory social security	84
P	Education	85
Q	Human health and social work activities	86 – 88
R	Arts, entertainment and recreation	90 – 93
S	Other service activities	94 – 96
T	Activities of households as employers; undifferentiated goods and services producing activities of households for own use	97 – 98
U	Activities of extraterritorial organisations and bodies	99

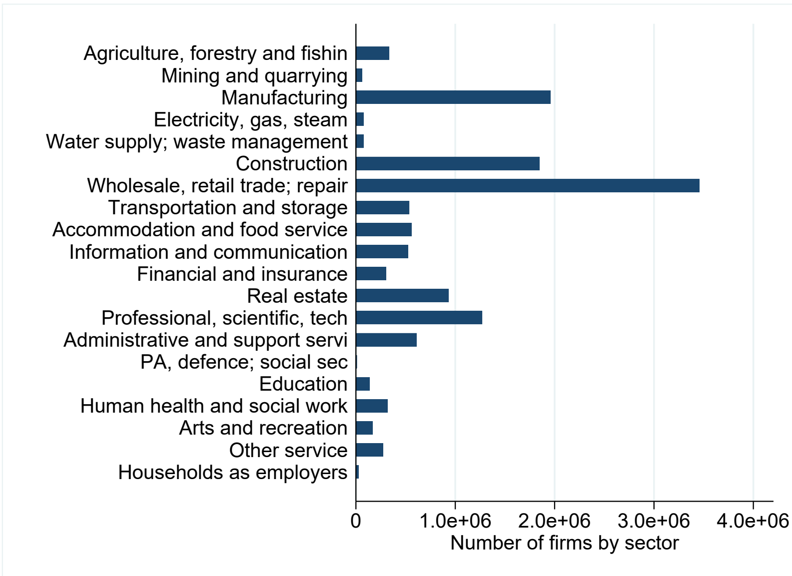
Note: We report Level 1 Sectors in NACE Rev 2.

Figure 2.C.1: Number of firms by continent



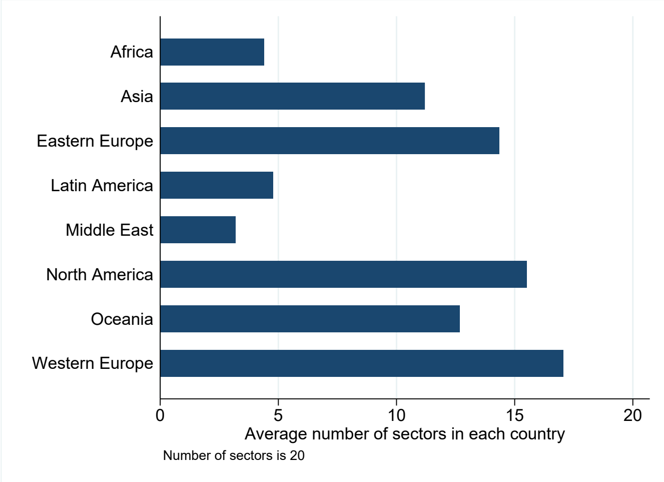
Note: Number of firms with non-missing profit margin in at least one of the ten years of sample period (2006-2015) by continent. The sample is as defined in Table 2.6.1, Panel A.

Figure 2.C.2: Number of firms by sector



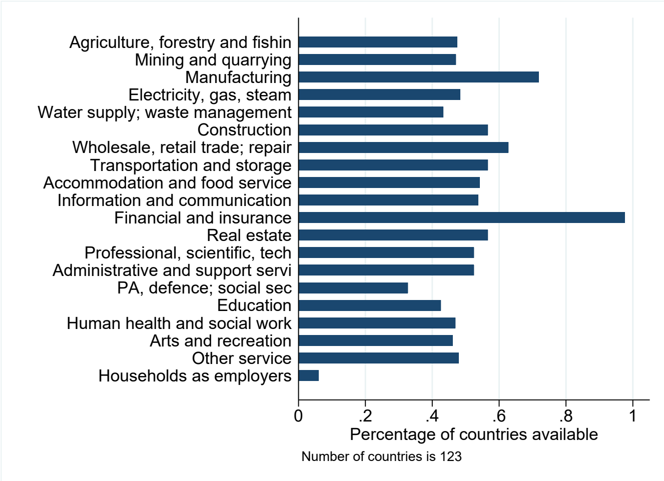
Note: Number of firms with non-missing profit margin data in at least one of the ten years of sample period (2006-2015) by sector. The sample is as defined in Table 2.6.1, Panel A.

Figure 2.C.3: Average number of sectors by continent



Note: Average number of sectors in each country averaged by continent. Sample defined as in Table 2.6.1, Panel A.

Figure 2.C.4: Percentage of countries with at least 20 firms in the sector



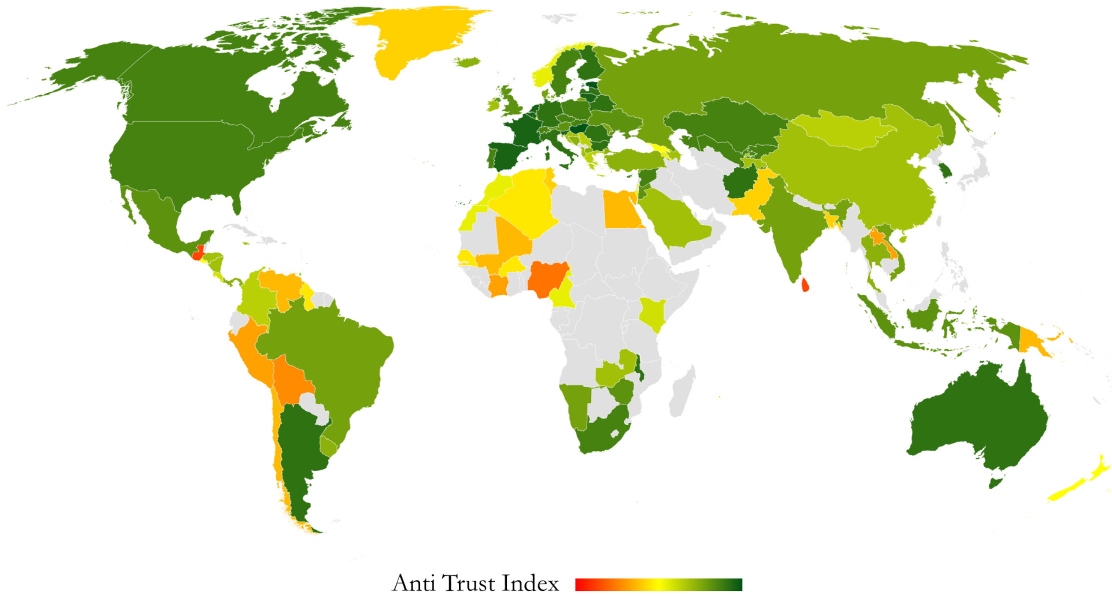
Note: Percentage of countries in each sector. Sample defined as in Table 2.6.1, Panel A.

Table 2.C.2: Antitrust Index

	Antitrust Index (std)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Institutional Index - χ	0.438	0.248	-0.375	-0.326	0.243	0.303	0.354
	(0.122)***	(0.117)**	(0.109)***	(0.101)***	(0.107)**	(0.110)***	(0.106)***
Observations	107	106	108	108	104	104	83
R-squared	0.132	0.053	0.120	0.096	0.048	0.071	0.125
N countries	107	106	108	108	104	104	83
Mean dependent variable	20.06	19.99	20.17	20.17	20.11	20.11	20.29
St. Dev. dependent variable	4.66	4.63	4.63	4.63	4.69	4.69	4.78
Institutional variable	Log GDP pp	Economic Freedom	Civil Liberties	Political Rights	Polity IV	Executive Constraints	Rule of Law

Note: This table presents OLS estimates using the specification in equation 4. Standard errors are robust standard errors and reported in parentheses. The dependent variable Antitrust Index is an index measuring the intensity of antitrust activities, as defined by Hylton and Deng (2007). The Institutional Index variable represents various country characteristics, see Online Appendix Section 2.A.3 for details and sources. *** p<0.01; ** p<0.05; * p<0.1.

Figure 2.C.5: Geographical distribution Scope Index (average 2006-2015)



Note: The geographical distribution Scope Index (averaged for the period 2006-2015). The red areas represent a low value of the antitrust index (minimum equals to 5) while the green areas represent a high value of the antitrust index (maximum equals to 27). We do not have data for grey areas.

Table 2.C.3: Additional Summary Statistics

	Obs	Mean	Sd	Min	Median	Max
Panel A						
Number of firms reporting profit margin	1,224	11,023	40,800	20	785	906,758
Number of firms reporting total assets	1,245	16,645	54,686	20	1077	977,687
HHI Gross Sales	1,115	3.56	6.40	0.00	0.47	78.71
Number of firms reporting gross sales	1,115	10,105	34,323	20	568	604,246
HHI Net Sales	1,116	3.56	6.38	0.00	0.46	78.71
Number of firms reporting net sales	1,116	10,186	35,324	20	570	656,511
Pct new firm	1,623	0.09	0.03	0.01	0.10	0.13
Pct closed firm	1,592	0.04	0.03	0.00	0.03	0.14
Panel B						
GDP per capita	1,209	27,706	17,433	1524	23782	121,724
Economic Freedom	1,183	7.20	0.66	3.91	7.28	9.03
Civili Liberties	1,193	2.24	1.50	1.00	1.70	7.00
Political Rights	1,193	2.30	1.82	1.00	1.20	7.00
Polity IV	1,140	17.02	4.96	0.00	19.00	20.00
Executive Constraints	1,140	6.16	1.46	1.00	7.00	7.00
Rule of Law (2012, 2014, 2015)	984	0.64	0.14	0.33	0.61	0.88

Note: The unit of analysis is country-sector. In Panel A we consider the country-sectors with at least 20 firms with non missing financial statements data to compute the variable of interest. Panel B considers the country-sectors with at least 20 firms with non missing average profit margin. All variables are averaged over the entire sample period (2006-2015). HHI Gross Sales and HHI Net Sales have been computed after trimming the sample at 1% at country-sector level. Net or gross sales present many missing values, we predict non-negative missing values using total assets, sector fixed effect, and the interaction term between sector and total assets, looking separately at each country.

2.D Online Appendix – Additional Results and Robustness

Table 2.D.1: Entry and Exit

	Pct new firms (std)	Pct closed firms (std)
	(1)	(2)
Non-tradable sector x	0.0678	-0.00196
Antitrust Index - β_1	(0.0305)**	(0.0224)
Observations	1,367	1,351
R-squared	0.850	0.888
N firms	54,993,643	48,119,148
N firms non-tradable	47,385,227	41,461,838
N countries	100	99
N sectors	20	20
Mean dependent variable	0.09	0.04
St. Dev. dependent variable	0.03	0.04
Country FE	YES	YES
Sector FE	YES	YES

Note: This table presents OLS estimates using the specification in equation 1, where the unit of observation is a country-sector cell, and the country-sectors contain at least 20 firms with non-missing financial statements data. Standard errors are clustered at country level and reported in parentheses. The Antitrust Index is from Hylton and Deng (2007). The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. All coumns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.2: Alternative HHI indexes

	HHI Gross Sales (std)	HHI Net Sales (std)
	(1)	(2)
Non-tradable sector x	-0.099	-0.103
Antitrust Index - β_1	(0.0575)*	(0.0554)*
Observations	1,006	1,008
R-squared	0.366	0.367
N firms	11,136,415	11,236,380
N firms non-tradable	9,589,379	9,668,482
N countries	89	89
N sectors	20	20
Mean dependent variable	3.23	3.25
St. Dev. dependent variable	6.17	6.18
Country FE	YES	YES
Sector FE	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in 2.6.1, Panel A, unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. The Antitrust Index is from Hylton and Deng (2007). The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. The variable Herfindahl - Hirschman Index (HHI) is an index measuring the concentration of an industry based on firm net sales (Column 1) or gross sales (Column 2), it ranges between 0 (perfect competition) and 100 (monopoly). All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.3: Alternative cutoffs

	Average Profit Margin (std)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-tradable sector x	-0.123	-0.101	-0.115	-0.132	-0.12	-0.0857	-0.0614
Antitrust Index - β_1	(0.0380)***	(0.0443)**	(0.0423)***	(0.0408)***	(0.0367)***	(0.0482)*	(0.0545)
Cutoff	0	10	30	40	50	200	3000
Observations	1,389	1,193	1,041	994	955	769	394
R-squared	0.541	0.618	0.671	0.679	0.688	0.697	0.761
N firms	12,802,233	12,801,449	12,798,690	12,797,083	12,795,362	12,775,024	12,382,718
N firms non-tradable	10,516,750	10,516,129	10,513,853	10,512,348	10,510,797	10,494,919	10,168,878
N countries	109	104	88	83	80	63	40
N sectors	20	20	20	20	20	20	19
Mean dependent variable	7.49	7.05	6.70	6.50	6.24	5.83	5.78
St. Dev. dependent variable	9.71	8.78	8.27	8.23	8.09	7.64	7.82
Country FE	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in Table 2.6.1, Panel A unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The Antitrust Index is from Hylton and Deng (2007). The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. We consider the country-sectors with at least 0, 10, 30, 40, 50, 200 or 3000 firms with non-missing data for the average profit margin in Columns 1 to 7. All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.4: Alternative samples

	Average Profit Margin (std)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-tradable sector x	-0.153	-0.129	-0.0676	-0.14	-0.152	-0.0858	-0.188	-0.148
Antitrust Index - β_1	(0.0397)***	(0.0356)***	(0.0398)*	(0.0596)**	(0.0632)**	(0.0449)*	(0.0423)***	(0.0560)***
Sample	Mean 2006-2010	Average 2006-2015	2006	2007	2008	2009	2010	2011
Observations	1,000	954	845	877	904	923	952	970
R-squared	0.655	0.655	0.682	0.692	0.627	0.647	0.656	0.582
N firms	8,998,328	5,343,366	5,104,345	5,530,434	5,690,555	5,723,592	5,634,093	5,990,410
N firms non-tradable	7,216,377	4,347,578	4,016,206	4,355,641	4,434,258	4,549,090	4,728,485	4,907,369
N countries	89	83	81	83	84	85	86	88
N sectors	20	20	20	20	20	20	20	20
Mean dependent variable	6.62	6.73	7.47	8.13	6.24	5.53	6.65	6.30
St. Dev. dependent variable	8.34	7.35	8.28	8.63	7.78	8.22	8.11	7.79
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in Table 2.6.1, Panel A, unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The Antitrust Index is from Hylton and Deng (2007). The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. In Column 1 we consider average profit margin over 2006-2010 period. In Column 2 we first take the average by year and then average by country-sector. Columns 3-8 report the results year by year for 2006-2011. In Columns 2-8 average profit margin has been computed after trimming the top/bottom 1% of firms within each country-sector-year. All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.5: Alternative samples

	Average Profit Margin (std)							HHI assets (std)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-tradable sector x	-0.135	-0.118	-0.149	-0.185	-0.103	-0.0464	-0.0936	-0.15
Antitrust Index - β_1	(0.0478)***	(0.0437)***	(0.0355)***	(0.110)	(0.0411)**	(0.0489)	(0.0238)***	(0.0786)*
Sample	2012	2013	2014	2015	Mean 2006-2014	EU unique	EU unique	EU unique
Antitrust Index	Scope Index	Scope Index	Scope Index	Scope Index	Scope Index	Scope Index	Budget (USD)	Scope Index
Observations	996	995	904	168	1,110	696	551	704
R-squared	0.632	0.638	0.633	0.682	0.629	0.638	0.669	0.448
N firms	6,631,818	6,923,192	6,118,058	90,270	12,793,410	12,800,468	9,305,869	20,018,149
N firms non-tradable	5,478,139	5,743,993	5,185,113	80,217	10,508,781	10,515,350	7,516,707	17,105,174
N countries	88	88	84	25	94	71	55	74
N sectors	20	20	20	19	20	20	20	20
Mean dependent variable	6.52	7.03	7.50	10.20	6.80	7.89	7.40	5.66
St. Dev. dependent variable	8.04	8.08	7.76	9.07	8.53	9.30	8.80	9.50
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in 2.6.1, Panel A, unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The Antitrust Index is from Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. Columns 1-4 report the results year by year for the period 2012-2015, where the average profit margin has been calculated after trimming the top/bottom 1% of firms within each country-sector-year. In Column 5, we the average profit margin is for the period 2006-2014. In Columns 6-8, all the countries member of European Union in 2006 are treated as a single country. In Column 7, antitrust index is Budget in USD as defined in Bradford et al. (2019) (see Section 4 for details) and covers the period 2006-2010. In Column 8, the dependent variable is the Herfindahl - Hirschman Index (HHI) measuring the concentration of an industry based on firm assets, ranging from 0 (perfect competition) to 100 (monopoly). All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.6: Weighting Profit Margin by Operating Revenues

	Average Profit Margin (std)				
	(1)	(2)	(3)	(4)	(5)
Non-tradable sector x	-0.219	-0.0768	-0.151	-0.157	-0.0290
Antitrust Index - β_1	(0.0676)***	(0.0640)	(0.0593)**	(0.0511)***	(0.0447)
Non-tradable sector - β_2	0.0814				
	(0.0730)				
Antitrust Index - β_3		-0.169			
		(0.0635)***			
Antitrust Index	Scope Index	Scope Index	Scope Index	Scope Index	Budget (USD)
Tradable definition	Baseline	Baseline	Baseline	Mian and Sufi	Baseline
Sample	2006-2015	2006-2015	2006-2015	2006-2015	2006-2010
Observations	1,066	1,066	1,066	1,066	889
R-squared	0.314	0.376	0.540	0.541	0.547
N firms	12,330,345	12,330,345	12,330,345	12,330,345	8,892,370
N firms non-tradable	10,115,761	10,115,761	10,115,761	9,622,786	7,169,720
N countries	93	93	93	93	75
N sectors	20	20	20	20	20
Mean dependent variable	6.01	6.01	6.01	6.01	6.33
St. Dev. dependent variable	7.26	7.26	7.26	7.26	7.18
Country FE	YES	NO	YES	YES	YES
Sector FE	NO	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in Table 2.6.1, Panel A, unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. When aggregating at country-sector we weight firms' profit margin by operating revenues. The Antitrust Index is from Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. All columns include both country and sector fixed effects. *** $p<0.01$; ** $p<0.05$; * $p<0.1$.

Table 2.D.7: Restricting number of sectors per country

	Average Profit Margin (std)			
	(1)	(2)	(3)	(4)
Non-tradable sector x	-0.103	-0.103	-0.105	-0.0800
Antitrust Index - β_1	(0.0458)**	(0.0514)*	(0.0597)*	(0.0469)
Number of sectors per country	5	10	15	20
Observations	1,065	1,004	909	120
R-squared	0.616	0.624	0.626	0.779
N firms	12,797,276	12,791,836	12,772,003	5,200,463
N firms non-tradable	10,513,150	10,509,322	10,496,408	4,585,427
N countries	65	57	49	6
N sectors	20	20	20	20
Mean dependent variable	6.42	5.98	5.72	7.47
St. Dev. dependent variable	8.21	7.69	7.41	8.07
Country FE	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES

Note: This table presents OLS estimates using the specification in equation 1. The sample is as defined in Table 2.6.1, Panel A, unless otherwise specified. In Columns 1 to 4, we consider those countries with at least 5, 10, 15 or 20 sectors and with at least 20 firms with non-missing average profit margin data. Standard errors are clustered at country level and reported in parentheses. The dependent variable, Profit Margin, is defined by Orbis as the profit or losses before tax and external items over operating revenue. The Antitrust Index is from Hylton and Deng (2007). Both of these variables are standardized and averaged between 2006 and 2015. The variable non-tradable is a dummy variable taking the value one for all sectors other than Agriculture, Manufacturing and Mining. All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

Table 2.D.8: Other robustness

	Average Profit Margin (std)					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-tradable sector x	-0.105	-0.105	-0.122	-0.0956	-0.103	-0.103
Antitrust Index - β_1	(0.0528)*	(0.0385)***	(0.0408)***	(0.0406)**	(0.0466)**	(0.0380)***
Observations	1,110	1,123	1,087	1,089	1,110	1,110
R-squared	0.797	0.647	0.639	0.651	0.631	0.631
N firms	12,800,308	13,132,056	11,783,874	12,701,900	12,800,308	12,800,308
N firms non-tradable	10,515,246	10,793,295	9,683,463	10,420,144	10,515,246	10,515,246
N countries	94	96	91	94	94	94
N sectors	20	20	20	20	20	20
Mean dependent variable	6.80	7.09	6.74	6.78	6.80	6.80
St. Dev. dependent variable	8.53	8.92	8.32	7.79	8.53	8.53
Country FE	YES	YES	YES	YES	YES	YES
Sector FE	YES	YES	YES	YES	YES	YES
Weighted	Yes	No	No	No	No	No
Trim at firm level	1%	No	5%	1%	1%	1%
Trim at concentration measure level	No	No	No	1%	No	No
Standard errors	Cluster Country	Cluster Country	Cluster Country	Cluster Country	OLS	Cluster Country-Sector

Note: This table presents OLS estimates from equation 1. The sample defined as in 2.6.1, Panel A, unless otherwise specified. Standard errors are clustered at country level and reported in parentheses. Column 1 reports the result from a weighted regression where the weights are the number of firms in each country-sector with not missing profit margin. In Column 2, we do not trim our data. In Column 3, we trim top/bottom 5% firms based on average profit margin distribution within each country-sector level. In Column 4, we trim top/bottom 1% country-sectors based on average profit margin. In Column 5, we do not adjust standard errors. In Column 6, we consider standard errors clustered at country-sector level (*i.e.* Robust standard errors). Unless otherwise specified, all columns report the same specification as in Column 3 of Table 2.6.2. All columns include both country and sector fixed effects. *** p<0.01; ** p<0.05; * p<0.1.

2.E Online Appendix – Data References

Bradford, Anu, Adam S. Chilton, Christopher Megaw, and Nathaniel Sokol. 2019. "Competition Law Gone Global: Introducing the Comparative Competition Law and Enforcement Datasets." *Journal of Empirical Legal Studies*, 16(2): 411–443.

Bradford, Anu, and Adam Chilton. 2018. "Competition law around the world from 1889 to 2010: The competition law index." *Journal of Competition Law and Economics*, 14: 393–432.

Bradford et al. 2006-2010. "Comparative Competition Enforcement.", http://comparativecompetition org/data (accessed August 2019).

Bureau van Dijk, 2006–2015. "Orbis 2016", (accessed September 2017).

Center for Systemic Peace. 2006-2015. "Polity IV Project.", https://www.systemicpeace.org/inscrdata.html (accessed April 2018).

Comparative Competition Law Dataset. 2006-2010. "Competition Law Index", http://comparativecomp org/data/ (accessed August 2019).

World Economic Forum. 2006-2015. "Executive Opinion Survey." *Global Competitiveness Report*, (accessed April 2018).

Fraser Institute. 2006-2015. "Economic Freedom of the World.", https://www.fraserinstitute.org/studies/economic-freedom (accessed June 2018).

Freedom House. 2006 -2015. "Country and Territory Ratings and Statuses.", https://freedomhouse.org/report/freedom-world (accessed June 2018).

Hylton, Keith N. and Fei Deng. 2007. "Antitrust around the world: An empirical analysis of the scope of competition laws and their effects." *Antitrust Law Journal*, 74(2): 271–341.

Hylton, Keith N. et al. 2006-2015. "Antitrust World Reports.", http://antitrustworldwiki.com (accessed May, 2018).

World Bank. 1990 - 2019. "International Comparison Program: GDP per capita, PPP (constant 2011 international $).", https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.KD (accessed April 2018).

World Justice Project, 2006 - 2015. "Rule of Law Index.", https://worldjusticeproject.org/our-work/research-and-data/wjp-rule-law-index-2020/current-historical-da (accessed June 2018).

Chapter 3

Historical Roots of Political Extremism: The Effects of Nazi Occupation of Italy

Nicola Fontana
Department of Economics and Center for Economic Performance, London School of Economics and Political Science
Tommaso Nannicini
Department of Social and Political Sciences and IGIER, Bocconi University; CEPR; IZA
Guido Tabellini
Department of Economics and IGIER, Bocconi University; CIFAR; CEPR; Ces-Ifo

Abstract

We study the impact of the Italian civil war and Nazi occupation of Italy in 1943–45 on postwar political outcomes. The Communist Party, which was more active in the resistance movement, gained votes in areas where the Nazi occupation was both longer and harsher, mainly at the expense of centrist parties. This effect persists until the late 1980s. These results suggest that civil war and widespread political violence reshape political identities in favor of the political groups that emerge as winners. This benefits extremist groups and hurts moderates, since the former are more involved in violent conflict.

3.1 Introduction

It is well understood that the presence of large extremist parties can affect the functioning of democratic institutions. The origin of political extremism is less well understood, however. Throughout the 1960s and 1970s, extremist parties in Italy and France gathered over 30% and over 20% of the votes, respectively, while they were virtually absent in Austria, Germany, and the Anglo-Saxon countries. How can we explain such large differences in neighboring countries with a similar economic structure and at the same level of economic development?

In this paper, we investigate whether political extremism emerges as a legacy of civil wars and foreign occupations. As discussed in Walter (2017), extremist groups have an advantage during civil wars, because their radical ideology makes them more successful in solving the collective action problem and in organizing violence. When they win the civil war, extremist groups can capitalize on their success and turn it into political support in democratic elections. Moreover, civil wars can directly impact on the party system, as military factions evolve into political organizations. A civil war can also exacerbate political conflict and lead to radicalization, although the opposite reaction is also possible. The goal of this paper is to study these different effects in an advanced democracy.

We study the domestic political consequences of the Italian civil war and Nazi occupation during the final two years of World War II. The intensity of the conflict varied across Italy, since the Allies freed Southern and much of Central Italy almost immediately, while Northern-Central Italy remained under Nazi occupation for much longer. Moreover, the Nazi troops became particularly aggressive toward partisans and civilians in the last stage of the war.

Using data at the municipality level, we first look at the country-wide association between postwar votes to extreme-left parties and the duration and violence of Nazi occupation, after controlling for several observables. The postwar vote share of the Communist Party is higher where the Nazi occupation was longer and more violent. These correlations persist until the end of the ”First Republic” in the early 1990s. We instead find no correlation with voters' turnout.

To identify a causal effect, the rest of the paper exploits the fact that the battlefront between the Germans and the Allies remained stuck for over six months near the so called "Gothic line," a defensive line cutting Northern-Central Italy from West to East. We apply a geographic Regression Discontinuity Design (RDD) to municipalities just above and just below the Gothic line, comparing their voting outcomes in the postwar national elections. The compound treatment is that, North of the line, the violent German occupation was both longer and harsher, and so were the civil war and the fighting by the resistance movement.

Our main result from RDD is that the vote share of the extreme-left parties in postwar elections is larger in municipalities just North of the line. This effect is quantitatively important (about 9 percentage points or higher for the communists in the 1946 elections), and again persists until the end of the "First Republic". The communist gain above the line is mainly at the expense of the catholic vote share, although this finding is less robust, suggesting that the communists may also have gained votes from other moderate or center-left parties. Municipalities North of the line are also less likely to vote for the extreme right-wing parties linked to the fascist regime, but this effect occurs later in time and it is smaller than the vote loss of the catholics. Thus, political polarization increased where the civil war and Nazi occupation lasted longer. Again, we find no difference in voters' turnout.

What drives these effects? We contrast two possible explanations. First, a longer exposure to civil war and foreign occupation might directly affect voters' political attitudes. The Italian Communist Party was more active in the resistance movement than the others, and it had opposed Mussolini from the start (the Catholics instead had voted him in office). The shared emotions associated with the violent German occupation could have led voters to identify with the political party that, more than others, was the symbol of the victorious resistance movement. Second, a longer Nazi occupation might have affected postwar political organizations, since North of the line the resistance movement remained active for longer, and this may have given an advantage to the Communist Party in building grassroots organizations.

The evidence is more consistent with the first hypothesis, that is, the channel going through voters' attitudes rather than political organizations. First, in the OLS analysis, the communist vote share in postwar elections is correlated with the occurrence of violence, but not with the presence of partisan brigades. Second, in the RDD analysis, we find that partisan brigades were equally widespread just North and just South of the line, and their presence had no influence on the effects of the Nazi occupation on voting outcomes (i.e., the treatment effect is homogeneous in areas with and without partisan brigades). Third, the extreme right-wing parties, which were obviously more free to self-organize North of the line, did not benefit from this greater freedom, on the contrary they garnered more support South of the line. Fourth, in 2015 we conducted a random survey of about 2,500 individuals resident in 242 municipalities within 50 Km of the Gothic line. Memory of the civil war is stronger North of the line and amongst individuals who have a left-wing political orientation. There is also some weak evidence of mildly more anti-German attitudes North of the line.

Despite its importance, the empirical literature on these issues is not very large. Ochsner and Roesel (2016) and Ferwerda and Miller (2014) have applied geographic RDD to WWII data - see also Kocher and Monteiro (2015) - while Dell and Querubin (ming) exploits discontinuities in the US military strategies during the Vietnam war. Dehdari and Gehring (2018) and Grosfeld and Zhuravskaya (ming) apply geographic RDD to study other historical episodes. Our empirical findings are consistent with an important tradition in political science, which has studied key historical junctures such as external or civil wars, when new parties are born and young generations build new political identities breaking with the past (Mayhew 2004, Campbell et al. 1960, Sundquist 2011). Balcells 2011 studies the political attitudes of war veterans in the Spanish civil war of 1936–38 and finds results consistent with ours. Costalli and Ruggeri (2015) also study the effect of the Italian civil war on the immediate postwar election, and some of their findings are consistent with ours, although they do not look at the Nazi occupation as treatment, only focus on the 1946 election, and do not exploit any geographic RDD to make causal inference. A few papers have studied the effects of civil wars in Africa, generally showing that such events reinforce ethnic identities and increase political participation (Blattman 2009, Bellows and Miguel 2009, Bauer et al. 2016), while other papers show that civil wars tend to increase violence and radicalization (Canetti and Lindner 2015, Canetti-Nisim et al. 2009, Grosjean 2014, Miguel et al. 2011). Finally, our paper is also related to a larger literature on the persistence of political attitudes and cultural traits (Acharya et al. 2015 , Voigtländer and Voth 2012, Fouka and Voth 2013, Avdeenko and Siedler 2016 Lupu and Peisakhin 2017, Iwanowsky and Madestam 2017, Rozenas and Zhukov 2019).

3.2 Data

This section describes our variables. Appendix 3.A provides a historical summary of the relevant period. Appendix 3.B provides more detail on the data. The unit of observation is the municipality.

3.2.1 Political outcomes

We measure political outcomes by the percentage of votes received by political parties at the 1946 election for the constitutional assembly, and in all subsequent 10 national elections for the Chamber of Deputies until 1987 included.

We consider four political groups. First the radical left, measured by the votes given to the Communist Party. We call this variable *Communist*. Since in 1948 the communists and the socialists formed a single electoral list, we also consider the votes received by these two parties together, and we call it *Communist and Socialist*. The second group is the Christian Democratic Party, which we call *Catholic*. The third group, which we call *Right Wing*, consists of the post-fascist party (MSI) and smaller parties that supported the monarchy. Finally, we also collected data for the small Republican party (*Republican*). The source of the electoral data is the Italian Ministry of Interior.

We also collected data on the last free elections held before the advent of fascism, namely in 1919, 1921, and 1924.[1] The Communist Party was very small in the 1921 and 1924 elections (and did not exist in 1919), so we lump together the socialist and communist votes in the pre-fascist period to gain precision. The right-wing vote cannot be separately measured in 1921, since fascists were running together with the more traditional and moderate liberals in that election. Hence, for the pre-fascist period we only collect the *Catholic*, *Communist and Socialist* and *Republican* variables.

Since there are several missing observations in pre-war data, in our baseline analysis we fill the missing observations in each election exploiting the remaining two elections plus additional observables. Our baseline sample consists of the about 5,700 municipalities for which we have both postwar and prewar political outcomes. We verify the robustness of the results to the pattern of missing observations.

3.2.2 War-related variables

To explore the mechanisms that could affect political outcomes, we collected several variables related to the Nazi occupation and the civil war. First, using Baldissara et al. (2000), we coded the presence of partisan brigades in the municipal area (meaning that their area of operation overlaps with the municipal area). We distinguish between left-wing brigades and other partisan brigades (lumping together catholic groups, liberals, and others), but the results are robust to a finer disaggregation between different partisan groups.

Second, from ANPI (National Association of Italian Partisans) we collected a list of 3,117 partisans with a short biography. This database is only a sample, but it was built to represent the political diversity of the resistance movement and includes almost all of the national and local leaders of the movement. From this source we create a dummy variable for whether at least a partisan in our sample was born in the municipality, and a dummy variable for whether at least a partisan born in the municipality was linked to a left-wing party in the postwar period. These variables capture the strength of local opposition to the fascist regime, rather than the presence of brigades in the area.

The third set of variables codes episodes of violence by the fascists or by the Germans. We define a dummy variable for municipalities that had at least one episode of violence, and we also distinguish between episodes where the majority of victims were civilians or partisans. The source is the "Atlas of Nazi and fascist massacres" (ANPI-INSMLI 2016), a database constructed by more than 90 researchers under the supervision of a joint historical commission established by the Italian and German governments in 2009.

Our fourth set of variables codes the location of two German divisions that were particularly violent and committed a very large number of criminal episodes against civilians: the 16th SS-Panzer-Grenadier-Division "Reichsfuhrer-SS" and the "Hermann Goering" division (Gentile (2015)). Their exceptional violence can be seen in Appendix Figure 3.C.1 in Appendix 3.C. Based on the German archives consulted by Gentile (2015), we have records on the precise location of these troops throughout the Italian civil war. We construct a dummy variable that equals 1 for municipalities within 15 Km from the location of either one of these divisions.

Fifth, we collected data on deportations to Germany. During WWII, about 40,000 Italians were deported

[1]Mussolini was appointed Prime Minister in 1922. Although formally free and regular, the 1924 election was held in a climate of violence and intimidation.

to Germany (about 7,500 were Jewish). Thanks to Mantelli and Tranfaglia (2013), we have data on the number of political deportations by municipality of capture (about 6,500 individuals) and by municipality of birth (around 14,000 individuals). We do not know the date of capture, however. Even though there are more missing observations, we rely on the municipality of capture, because internal migration would introduce larger measurement error in the birth data.

Finally, we coded the duration of the German occupation (measured in fraction or multiple of years) in each province, from the detailed maps in Baldissara et al. (2000). We were able to reconstruct the duration of the German occupation at the municipal level only near the Gothic and the Gustav lines, where the battlefront was more clearly defined. Throughout the rest of Italy, data on the duration of the German occupation are at the province level only.

3.2.3 Other city characteristics

From the Census (ISTAT) we collected data on total resident population, population density, and literacy rates for the years 1911, 1921, and then 1951, 1961, 1971, 1981, and 1991. As an indicator of economic development, from the 1951 Census we collected data on the number of industrial plants per capita in each municipality. We also collected data on elevation at the city hall, and on maximum and minimum elevation in the municipality. Finally, to include appropriate fixed effects, we reconstructed provincial borders at different dates. As a default, we use provinces as defined in 1921, but results are robust to province fixed effects defined on the basis of the boundaries at later dates. Thanks to Fontana et al. (2019) we also got data on the number of industrial plants and workers in 1927 (*Censimento Industriale 1927*), the number of agricultural firms and workers, the number of livestock, and surface devoted to agricultural production in 1929 (*Catasto Agrario 1929*). Appendix Table 3.C.1 reports summary statistics of these variables in the entire sample; Appendix Table 3.C.2 provides the same summary statistics for municipalities within a 50 Km radius around the Gothic line.

3.3 Empirical Strategy

3.3.1 Prior hypotheses

Did the German occupation leave a mark on the postwar Italian political system? In particular, did it affect the support enjoyed by extremist political parties? A priori, there are three main reasons to expect a lasting impact, the first two operating directly on citizens' attitudes, and the third one operating on political organizations.

First, in the areas under German occupation, the civil war between the fascists and their opponents was both longer and harsher. This in turn could lead to more entrenched and radicalized positions on both sides, reinforcing political identities and shaping attitudes in favor of both the communists and the extreme right-wing parties at the expense of the moderate parties. The Italian Communist Party tried indeed to capitalize on this identity channel in the aftermath of WWII, by pitching itself as the true guardian of the legacy of the resistance movement.

Second, the German occupation was actively opposed by the Italian resistance movement. To suppress it, Nazis often resorted to extreme forms of violence, not only against resistance fighters but also against civilians. This violence could leave a mark on political attitudes. A priori, the effect could go either way. On the one hand, Nazi violence (actual or just threatened) could lead to more antagonistic attitudes against the enemy. This would favor the communists, who were more involved in the resistance movement and stood up more forcefully against the Nazis. On the other hand, civilians could blame the partisan brigades (and hence mainly the communists), who were responsible for the German retaliation against civilians. Moreover, the extractive nature of the Nazi occupation, especially when contrasted to the Allies' behavior, could affect political attitudes directly.

Third, the German occupation could affect political organizations. Right-wing parties loyal to Mussolini were obviously more free to self-organize in the areas under German occupation. But the presence of active partisan brigades could also matter, since the postwar party system grew out of the resistance movement, and partisan brigades could be exploited to build grassroots organizations, as stressed by Costalli and Ruggeri (2015). Through this channel, a longer German occupation should thus give an advantage to the Communist Party (since its partisan brigades were more active and better organized), as well as to the right-wing parties linked to fascism.

3.3.2 Econometric framework

Our estimation strategy exploits geographic heterogeneity in the duration and nature of the Nazi occupation. We start by looking at the OLS correlations in all of Italy:

$$Y_i^{post} = \alpha_0 DUR_i + \alpha_1 V_i + \alpha_3 BIRTH_i + \alpha_2 PB_i + x_i' \beta + \gamma_p + \varepsilon_i, \tag{3.1}$$

where Y_i^{post} is a (post-treatment) electoral outcome for municipality i; DUR_i is the duration of the Nazi occupation (measured in years); V_i measures the occurrence of violence; $BIRTH_i$ measures opposition to the fascist regime, proxied by whether at least a partisan was born in the municipality; PB_i measures the presence of partisan brigades; x_i is a vector of covariates including illiterate share and population density in 1921 and 1951, electoral outcomes in 1919, 1921, and 1924, altitude, longitude, latitude, and a constant; γ_p are province or region fixed effects (as defined in 1921); ε_i is the random error term, capturing all omitted factors. The parameter α_0 captures the association between the treatment of interest and electoral outcomes.

Despite the interest of these country-wide correlations, some of the omitted factors in ε_i might be correlated with both the treatment and political outcomes. This is why, in order to identify the causal effect of the Nazi occupation, we implement a geographic RDD and compare postwar political outcomes in municipalities just above and just below the Gothic line. This line was conceived as the last defense for the German retreat. Its position was not only the outcome of a German decision, but also of random factors. As shown in Appendix Figure 3.C.2, there were three demarcation lines. The line labeled "Allies" is where the Allies stopped between August and mid-September 1944. The line labeled "Fall 1944", that runs through the mountain range, is the original line of defense set up by the Germans. But between late August and mid-September 1944 the Allies succeeded in breaching this line, and between November 1944 and April 1945 the battle front moved further North, to the Northern-most line depicted in Appendix Figure 3.C.2. This line too was finally breached in April 1945. Our RDD is on the Northern-most line "Nov. 1944–Apr. 1945," which was held for the longest period.

The final position of the Northern-most line was largely due to random events, which forced the Allies to stop their offensive between late October 1944 and the Spring of 1945. In August 1944, the Allies withdrew several divisions from the Italian front to launch a new offensive in Southern France. This decision was highly controversial: It was supported by the Americans, who wanted to create a distraction for the Germans from the ongoing battles in the rest of France, but it was opposed by the British, who instead leaned toward a stronger offensive in Italy. The American point of view prevailed, and this weakened the efforts of the Allies in Italy at a critical point in time Churchill (1959). A second important random event was the weather, which deteriorated harshly in late October. These are the words used by Churchill to describe those critical moments in October 1944: "The weather was appalling. Heavy rains had swollen the numberless rivers and irrigation channels [....]. Off the roads movement was often impossible. It was with the greatest difficulty that the troops toiled forward. [...] Not until the spring were the armies rewarded with the victory they had so well earned, and so nearly won, in the autumn" Churchill (see 1959, p.839).

To avoid the risk of confounding the effect of the Gothic line with that of pre-existing administrative boundaries, we control for province fixed effects (as defined in 1921). This implies that we draw inference by comparing municipalities within the same province that are North vs South of the Gothic line. Our identifying assumption is that, after controlling for distance from the line (and for province fixed effects), being just North or just South of the Gothic line is a random event uncorrelated with other unobservable determinants of political outcomes. This assumption can be indirectly tested and cannot be rejected for a number of pre-treatment observables. Any difference in political outcomes between municipalities North vs South of the Gothic Line can thus be attributed to the difference in the duration of the Nazi occupation. The treatment for being North of the line is a longer exposure to the Nazi occupation and to a more intense civil war for about six more months.[2]

Formally, we define d_i as the distance (in Km) from the Gothic line, with negative (positive) values identifying towns South (North) of the line, and estimate the following model in the interval $d_i \in [-\Delta, +\Delta]$:

$$Y_i^{post} = \sum_{k=0}^{p}(\delta_k d_i^k) + T_i \sum_{k=0}^{p}(\alpha_k d_i^k) + x_i'\beta + \eta_i, \tag{3.2}$$

where Y_i^{post} is any post-treatment outcome; T_i is a dummy identifying whether municipality i is North or South of the Gothic line; x_i is a vector of (time-invariant and pre-treatment) covariates including province fixed effects; p captures the order of the (spline) polynomial control function; η_i is the error term. The bandwidth Δ is either a (multiple) discretionary threshold or an optimal bandwidth as in Calonico et al. (2016). The parameter α_0 identifies the treatment effect of interest.[3] To avoid comparing municipalities close to the line but located far from each other along the East-West dimension, we perform a series of robustness checks by including latitude and longitude or fixed effects for 25 Km intervals of the Gothic line in the vector x_i Dell (2010) and, in our preferred specification, by using matching methods to compare nearest geographic neighbors just above and just below the Gothic line Keele and Titiunik (2014).

RDD allows us to estimate the causal effect of the Nazi occupation on postwar elections, but does not uniquely identify a particular mechanism. To discriminate between alternative hypotheses, we need additional (and stricter) assumptions. First, note that if we replace the outcome variable in equation (3.2) with a set of pre-treatment variables Y_i^{pre}, we can run balance tests that should normally deliver zero effects in order for the RDD to be valid. If we instead replace the outcome variable with "contextual" factors that happen to be potentially present in the context of Nazi occupation, we can test for demand-side vs supply-side potential mechanisms. Assume, for example, that we find a significant discontinuity in contextual factors that are likely to affect voters' behavior (the demand side), but not party organizations (the supply side). In order to interpret this as evidence of a demand-side mechanism, we also need to assume that there are no unobserved variables that impact on the demand side and that happen to have a discontinuity at the Gothic line. In our data, the variables V_i (occurrence of violence) and PB_i (presence of brigades) are natural

[2] In principle, similar estimates could be done around the Gustav line South of Rome, where the Germans also stood for several months. A number of reasons discouraged us from doing so, however. First, the battle for the Gustav line occurred much earlier in time, when the resistance movement was not yet organized. The civil war did not reach those areas, and the civilian population did not suffer as much damage and casualties as in Central Italy. This also reflected German orders, which became much more intolerant and aggressive against civilians only at a later stage Gentile (see 2015). Furthermore, prewar voting outcomes are missing for a large number of municipalities around the Gustav line.

[3] The estimated coefficient $\hat{\alpha}_0$ from equation (2) is not directly comparable with $\hat{\alpha}_0$ from equation (1), because they are measured in different metrics and because the former is a local effect. Indeed, α_0 in (2) is the causal effect of six more months of Nazi occupation in a period associated with intense violence (experienced or threatened). For the sake of comparison between the OLS and RDD coefficients, one should keep in mind that, if we use DUR_i as the outcome variable of the RDD estimations defined in equation (3.2), we find point estimates in the range between 0.524 and 0.550 (depending on the estimation method; all statistically different from zero at the 1% significance level), corresponding to half a year as expected.

candidates for demand-side and supply-side contextual factors, respectively[4]

3.4 OLS Baseline Estimates

In this section we estimate OLS regressions where the dependent variable is the vote share of the Communist Party in 1946 and subsequent elections (except 1948, when the communists did not run alone). The main independent variables of interest are the duration of Nazi occupation, different indicators of Nazi violence, the presence of partisan brigades, and whether one or more partisans were born in the municipality. We always control for latitude, longitude, altitude (maximum and at the city hall), illiteracy share in 1921 and 1951, population density in 1921 and 1951, vote shares of communists and socialists and of catholics in the 1919, 1921, and 1924 elections, as well as province or region fixed effects.

The baseline OLS estimates with *Communist 1946* as dependent variable are displayed in Table 3.6.1. We report both robust standard errors (second row) and standard errors corrected for spatial correlation (third row) as in Conley (1996). In columns (1)-(3), we do not include any control. In column (4) we introduce the controls listed above, in column (5) we add region fixed effects, and in column (6) we include province fixed effects.

In column (1) the postwar vote share of the Communist party is positively associated with the duration of Nazi occupation (in years) and with the occurrence of violence during the war (measured by having at least one episode of violence and being within 15 Km from violent Nazi divisions).[5]

In column (2) we include dummy variables for municipalities that were the birthplace of a partisan (either any partisan or a left-wing partisan). These variables capture the strength of local opposition to the fascist regime. As expected they are both positively correlated with the postwar communist vote share.

In column (3) we add two indicators for the presence of partisan brigades (left-wing or of any other brigade). The remaining estimated coefficients are unaffected. The presence of partisan brigades is negatively correlated with the postwar Communist vote share, but this result is not very robust to the inclusion of control variables and region (or province) fixed effects.

In column (4) we include the set of control variables mentioned above. As expected all the magnitudes are affected but the estimated coefficients remain highly significant, with the exception of the coefficient on the presence of left wing brigades. Inclusion of region fixed effects (5) and province fixed effects (6) further reduces the magnitudes of estimated coefficients, but once again the coefficients of interest remain significant except for that on the presence of partisan brigades.

According to the estimate in column (6), our preferred specification, half a year of additional Nazi occupation is associated with an increase in the communist vote share of about 1.7 percentage points (i.e., about 11.3% of the average vote share in the whole sample of 5,559 municipalities with no missing values). The occurrence of at least one episode of violence is associated with an increase in the communist vote share of 0.5 percentage point (i.e., about 3.3%).[6] Being close to the two violent Nazi divisions is associated with an increase in the communist vote share of 1.8 percentage points (i.e., about 11.9%). The same is true if the municipality was a birthplace of a partisan while being the birthplace of a left-wing partisan is associated with an increase in the communist vote share of 2.8 percentage points (i.e., about 18.5%). The association between the communist vote share and the presence of left-wing partisan brigades is not statistically differ-

[4]The presence of partisan brigades could also directly affects attitudes, however, since it determines who the locals associate with the resistence.

[5]All results are similar if the dummy is redefined to capture municipalities within 10 Km (available upon request).

[6]Violence episodes where the majority of victims were civilians also have a positive point estimate; the same holds if we consider violence episodes where the majority of victims were partisan (results available upon request).

ent from zero, while the presence of other brigades is negatively associated with the communist vote share, although this negative association is not particularly robust.

Anecdotal evidence suggests that sometimes local residents blamed the partisans for the German retaliation. If so, episodes of Nazi violence that occur where brigades are more actives should shift fewer votes toward the communists. As shown in column (7), this is what we find: episodes of Nazi violence are positively correlated with communist votes only in municipalities that do not intersect with the area of operation of any brigade. This finding is reassuring also because it reduces identification concerns about omitted variables possibly correlated with both the propensity to resist German troops and the likelihood to vote for the communists.

When considering spatially corrected standard errors, years of occupation lose significance. This is not surprising, given that here (unlike in the RDD estimates around the Gothic line) years of occupation are mainly measured at the province level. The statistical significance of the correlation between communist votes and episodes of violence, which are always measured at the municipality level, survives to the spatial correction procedure when considering regional fixed effects (column 5) but not when including province fixed effects (column 6). This may reflect spatial correlation in the occurrence of Nazi violence.

Results in Appendix Table 3.C.3, column 1, also show that the relationship between the communist vote and days of occupation is non-linear. As the duration of the occupation increases, the positive relationship with the postwar electoral results is stronger. A municipality with 700 (or more) days of occupation has, on average, 11.4% higher vote share for the communists in 1946 than one with roughly 150 days. Moreover, all results are qualitatively similar or stronger if we restrict the sample to regions where the occupation lasted more than one year for at least one municipality – see column 2 in Appendix Table 3.C.3.

The correlations between political outcomes and both the duration of Nazi occupation and the occurrence of violence are highly persistent. We estimated column (6) in Table 3.6.1 for all elections between 1946 and 1987.[7] Appendix Figure 3.C.3 depicts the estimated coefficients and (robust) confidence intervals for the duration of Nazi occupation, and for the dummy variables capturing the proximity to violent Nazi divisions, at least one episode of violence, being the birthplace of a partisan and of a left-wing partisan, and the presence of left-wing partisan brigades. Communist votes are positively associated with proximity to a violent German troop and to being the birthplace of a partisan until the late 1980s. The effects of a longer duration of the Nazi occupation, of being the birthplace of a left-wing partisan, and of at least one episode of violence also last more than one legislature, with a significance of 10% or lower until the late 1950s or early 1960s.

Overall, these results suggest that demand side, rather than supply side factors, explain the consensus toward the communists in the immediate aftermath of the civil war. The Communist Party gained votes in municipalities where the Nazi occupation lasted longer and was more violent, and where citizens were more willing to embrace the cause of radical opposition to the fascist regime, as captured by the dummy variable for being the birthplace of a partisan or a left wing partisan. On the other hand, the actual presence of partisan brigades connected with the Communist Party is not correlated with the communist vote share.

These estimates cannot be taken as entirely causal, however. Some (though not all) of the German violence was in retaliation against previous attacks by partisan troops, or induced by local hostility, so that there could be some omitted variables. Although Holland (2008) and Gentile (2015) stress that the location of élite troops was generally driven by military or logistical concerns (the war against the Allies, or the need to rest and train new conscripts), we cannot rule out that they were sent in areas with stauncher Italian opposition. We now turn to a causal test of these findings by means of geographic RDD.

[7] The only difference is that we now control for the Census data closest in time to the election used as outcome instead of 1951.

3.5 RDD Causal Effects

This section compares outcomes in municipalities just above and just below the Gothic line. Throughout we report five sets of RDD estimates. In the first four regressions, the control function in the running variable (distance from the line) is expressed as a first and second degree spline polynomial, and the sample is restricted to municipalities within 50 Km and 100 Km from the line. Following Gelman and Imbens (2014), we do not report polynomial specifications of higher degree. The fifth specification is a local linear regression with optimal bandwidth, estimated as in Calonico et al. (2016). As noted above, throughout we include province fixed effects, but results are very similar without them.[8]

3.5.1 Balance tests

We start by reporting balance tests for pre-treatment observables (Y_i^{pre}). Results are shown in Appendix Table 3.C.4. Very few estimated coefficients are statistically different from zero, and none of them for more than two out of five estimation methods; therefore, no consistent pattern emerges. Note that almost all of these variables have highly significant estimated coefficients in the OLS regressions estimated in Table 3.6.1 above (coefficients not reported), suggesting that they are relevant correlates of political outcomes.

Appendix Table 3.C.5 considers prewar political variables. Vote shares in 1919 and 1924 are balanced. The 1921 election outcomes seem more unbalanced, with the socialist and communist parties having more votes above the line. This unbalance is not particularly robust, however. It concerns only the 1921 election, and it is absent if we estimate the treatment effect by nearest-neighborhood matching (with replacement). Specifically, to do so, we restrict the analysis to only the two provinces (Bologna and Ravenna) with a sufficiently large number of municipalities on both sides of the line, and to the municipalities for which the Communist and Socialist 1921 vote share satisfies the common support assumption. We then match municipalities above vs below the line based on latitude and longitude (forcing the match within the province). This estimator thus compares political outcomes in a municipality above the line with the closest municipality below the line in the same province, giving more weight to comparisons of closer municipalities. As discussed by Keele and Titiunik (2014), this avoids the pitfall of giving more weight to comparisons of municipalities that have a similar distance from the line, but are very far apart from each other in a spatial (or other) dimension. As shown in Appendix Table 3.C.6, the 1921 election outcomes now appear balanced. There is a small imbalance in the 1919 election outcomes, which however tends to vanish within 25 Km of the line (where identification is more reliable). Finally, this lack of robustness is also confirmed visually by the placebo tests discussed below (see Appendix Figures 3.C.7 and 3.C.8).

Even if the balance tests on prewar elections do not point to the presence of structural unbalance around the Gothic line, the volatility of some of these tests could be due to a small sample issue. To cope with this potential problem, in what follows we always report RDD results on the postwar vote shares conditional on prewar vote share.

A possible concern is that the slight unbalance in the prewar elections could have grown larger during the fascist period. Unfortunately, we do not observe political attitudes in the intervening years. Nevertheless, we can use data on the birth place of partisans as a proxy for strong anti-fascist attitudes of the population. As described above, partisans were disproportionately recruited from the left of the political spectrum and their birth place contributes to explain the postwar communist vote share. The bottom two lines of Table 3.6.4, discussed in Section 3.5.4, show that the birth place of partisans (any partisan as well as communist

[8] In the 100 km neighborhood of the Gothic line there are 742 municipalities (in our sample), belonging to 5 regions and 25 provinces. Several of these provinces lie entirely North or South of the Gothic line, however. The Gothic line cuts through four provinces (Bologna, Firenze, Lucca, and Ravenna) that belong to two different regions (Tuscany and Emilia-Romagna) and that include 172 municipalities.

partisans) is balanced around the Gothic line. In Table 3.6.4, we also test whether the presence of partisan brigades, and in particular of communist brigades, is balanced above and below the line. The partisan brigades were a grassroots movement, and a significantly higher propensity to side with the communists during the fascist period should show up in a more diffused presence of partisan brigades. As discussed below, we do not find any unbalance along this dimension either.[9]

3.5.2 Election outcomes and persistence

We start by illustrating graphically the difference between communist vs catholic votes in 1946 around the Gothic line. In Appendix Figure 3.C.9 we plot the difference between the communist and catholic vote shares in 1946. Darker shades correspond to a larger communist vs catholic vote (black indicates a missing observation). Overall, the figure suggests that a longer German occupation is associated with left-wing radicalism, compared to what happens below the line.

The formal RDD tests reported in Table 3.6.2 confirm this visual impression. Electoral outcomes refer to the 1946 election for the constitutional assembly and to the 1948 national election. In 1946 the Communist Party ran alone, while in 1948 it merged with the Socialist Party. For the sake of comparison, we also report the sum of socialist and communist votes in 1946. As noted above, we always include province fixed effects and the vote shares of communists and socialists, and of the catholics, in 1919, 1921 and 1924. Estimates not conditional on postwar vote shares are qualitatively similar but larger in absolute value (see Appendix Table 3.C.7). Dropping province fixed effects has negligible effects on the estimates. This conditioning method is used in all the analysis reported below, unless indicated othersiwe.[10]

The results are very stark. For all estimation methods and for all indicators, the average vote share of the Communist Party (or of communists and socialists together) is significantly larger above the Gothic line. The size of the RDD coefficient is also large, generally 7-10 percentage points, depending on the estimation method and the outcome measure. Within 50 Km of the Gothic line, the Communist Party obtained on average about 36.7% of the votes, thus the effect of being above the line corresponds to around 20% of the average vote share. Taking into account that being just North vs just South of the line corresponds to an additional half year of occupation, if the effect were linear in time, one more year of Nazi occupation would increase the vote share of the extreme left by 40%. This is approximately four times as much as the effect estimated in the OLS regressions over all of Italy reported in the previous section.[11]

The larger communist vote is mainly at the expense of the moderate catholic and republican parties. According to the conditional estimates, the vote shares of both parties are systematically lower above the Gothic line, by about 1-4 percentage points for the Christian Democrats and by 2-3 percentage points for the Republican Party (this corresponds to about 7% and 40% of the average vote shares obtained by the Christian Democrats and Republican parties respectively within 50 Km of the Gothic line). The vote share for the extreme right is balanced around the Gothic line in both 1946 and 1948, with the only exception of unconditional estimates in 1948. Note that the gain in the communist vote is generally estimated to be larger than the catholic plus Republican loss, implying that other parties (the Socialist Party or other centrist parties) lost votes to the communists North of the line. Moreover, the composition of the moderate vote North of the line shifted toward the extreme right, since the catholic and Republican parties lost votes while the extreme right did not. Thus, overall the longer Nazi occupation and civil war induced a shift to the

[9] This can also be seen by visual inspection in Appendix Figure 3.C.10.

[10] In some of the robustness analysis reported in the next subsection, the local linear regressions did not converge when we included all the covariates on the RHS. When this is the case, we first estimate the residuals of the dependent variable on all the covariates and then we estimate the local linear regression on the estimated residuals.

[11] Also note that in the conditional regressions in Table 3.6.2 and in some of the robustness checks discussed below, the effect of being above the line is stronger on the communist votes alone than on the communist and socialist votes combined, suggesting that the effect is mainly a shift to the extreme left.

extreme left in the immediate postwar elections, and increased the polarization of the electorate.[12]

Appendix Figures 3.C.4 and 3.C.5 illustrate graphically the main polynomial regressions reported in Table 3.6.2, using a second order polynomial to fit the data. Each dot in the figures represents the average vote share in municipalities within 10 Km intervals North/South of the Gothic line. A statistically significant discontinuity is clearly visible, and it is particularly strong for the communist vote (unconditional or conditional on prewar elections).

Were these political effects a short lived reaction to the events associated with the war, or did they persist over time? The answer is that they lasted for several decades, until the end of the First Republic in the early 1990s. Figure 3.6.1 illustrates the pattern of RDD coefficients and confidence intervals for all elections between 1946 and 1987, estimated by local linear regressions conditioning on prewar election outcomes and provinces fixed effects (the last column in Panel B of Table 3.6.2). The left-wing parties retained a gain above the Gothic line, that shrinks from about 9 toward 5 percentage points in the late 1980s and remains statistically different from zero. The catholic party bears a loss of votes of 4-5 percentage points, also declining slightly in absolute value and statistically significant throughout the period. Similarly, the Republican party suffers a persistent loss of vote from 3% in 1946 to 1% in 1987. Interestingly, the extreme right-wing parties also lose votes above the line, but only from the 1950s onwards, and this effect too is quite persistent. Overall, the political effects of being exposed to a longer Nazi occupation North of the Gothic line are very large and persistent.

3.5.3 Robustness checks

In this subsection we discuss the robustness of the above causal inference. We first estimate the coefficients of interest with the same nearest-neighborhood matching estimator based on Euclidean distance (with replacement) discussed above with reference to the prewar election outcomes. The dependent variable is the residual of postwar outcomes on prewar vote shares. As discussed above, the sample is restricted to the two provinces where we have enough observations on both sides of the Gothic line, and the match is forced within the same province; we also restrict it to the municipalities for which the 1921 Communist and Socialist vote share satisfies the common support assumption.[13] In Table 3.6.3 we report two sets of matching estimates: One where we match based on latitude and longitude only, the other based on latitude, longitude, and prewar electoral outcomes. The estimated coefficients of the Communist (or Communist and Socialist) vote shares remain positive and statistically significant in most samples and specifications, although the point estimates are smaller in absolute value (about 5 percentage points of extra Communist vote shares for municipalities North of the line). The effects on the Catholic vote shares are weaker, whereas those on the Republican Party remain negative and generally statistically different from zero, and of a similar magnitude as in Table 3.6.2 (about 2 percentage points). Unconditional results are presented in Appendix Table 3.C.8

As apparent from Appendix Figure 3.C.9, voting outcomes exhibit some patterns in the East-West direction. We thus want to be sure that the RDD estimates only reflect the impact of being North vs South of the line, without being contaminated by other geographic patterns in the data. For this purpose, we perform a number of robustness checks. First, we estimate the same regressions with a first and second degree spline polynomial in distance that also includes as regressors a first and second degree polynomial in latitude and longitude, as well as the interaction of latitude and longitude and the same interaction squared. The local linear regressions are estimated on the residuals of an OLS regression on the relevant independent variables. Thus, the unconditional estimates are run on the residuals of a regression of the vote shares on the latitude and longitude terms; and the conditional estimates are run on the residuals of a regression that,

[12] We also estimated these same RDD regressions with voters' turnout as dependent variable, but we found no significant discontinuity (results available upon request).

[13] Imposing the common support assumption on all prewar vote shares, rather than just the 1921 vote share, would restrict the sample to only 30 municipalities.

besides latitude and longitude, also includes the prewar election outcomes. Province fixed effects are always included amongst the regressors. All results remain very similar, as shown in Appendix Table 3.C.9.

Second, we split the Gothic line in 25 Km intervals and we test our hypothesis (again with spline polynomials and local linear regressions) including fixed effects for each interval (here we omit the province fixed effects). In the spline polynomial specifications, the interval fixed effects are included amongst the regressors; the local linear regressions are estimated on the residuals of an OLS regression on the relevant independent variables.[14] This is equivalent to comparing municipalities above and below the line within each of these 25 Km intervals. Appendix Table 3.C.10 displays the results. All estimates are robust in terms of significance and magnitude.[15]

Appendix Figure 3.C.6 reports placebo tests for the main variables of interest to test whether our results might be attributed to random chance rather than a true causal effect. We shifted the location of the Gothic line North or South of its true position by 10 Km at a time, up to a distance of plus or minus 100 Km, and by 50 Km at a time, up to a distance of 250 Km. Estimation is by local linear regression as in the last column of Table 3.6.2. The results indicate a clear discontinuity in the estimated coefficient at the true location of the Gothic line, but not at the fake discontinuities. The catholic vote also displays a clear discontinuity. We also estimated the same placebo tests on prewar electoral outcomes. Here no clear pattern is evident, and the true location of the Gothic line generally does not stand out relative to the other position – see Appendix Figures 3.C.7 and 3.C.8. This again corroborates the conclusion that no structural unbalance of pre-treatment political attitudes is evident.

Next, we assess the robustness of the results to the method of dealing with missing observations. Appendix Table 3.C.11 reports the RDD estimates on the full sample that also includes municipalities for which all prewar elections data are missing (due to the missing variables, here we cannot control for pre-war vote shares). The communist vote share in 1946 remains significantly higher above the line (by about 9 percentage points), and most other estimated coefficients remain very similar to those in Table 3.6.2, although slightly smaller in absolute value. Appendix Table 3.C.12 restricts the sample in the opposite direction, namely we only include municipalities for which we have data on all three prewar elections (thus avoiding any imputation). Here the RDD estimates reveal even stronger effects than in the default sample, for both communist and catholic vote shares, with the exception of the Communist and socialists combined in 1946. The Republican Party vote share instead is not statistically different on both sides of the line, possibly because of the small number of observations.

Overall, these robustness checks confirm that the positive effect on the communist vote share is very robust, while the inference that the increase in the communist vote is only at the expense of the catholic vote (rather than also at the expense of the socialists or of other moderate parties) is more sensitive to the sample and to the estimation method.

3.5.4 Contextual factors

How could the prolonged German occupation and associated civil war have such important political effects? As already mentioned, there are several potential channels, some operating on the supply side, others on the demand side of politics. To shed light on this issue, we repeat our RDD estimates with alternative contextual factors as dependent variables, as well as by exploring possible sources of heterogeneity in the strength of the treatment effect.

[14]Namely, the unconditional estimates are run on the residuals of a regression of the vote shares on these 25 Km interval FE; and the conditional estimates are run on the residuals of a regression that, besides the 25 Km interval FE, also includes the prewar election outcomes.

[15]As a further check, we included fixed effects for provinces or for the electoral districts in the RDD regressions (there are 6 electoral districts within 50 Km of the Gothic line, and the line cuts through 3 of them). The results are similar and available upon request.

Partisan brigades. As discussed above, partisan brigades were disproportionately associated with the Communist party. Costalli and Ruggeri (2015) argue that this gave the Communist party an advantage, because it could exploit the grassroots network created by the brigades to build more effective local party organizations. The Nazi occupation could have enhanced this advantage, because partisan brigades remained operative for longer North of the line.

We have already seen in Section 3.4 that the OLS regressions do not support this argument, since the presence of brigades is not correlated with election outcomes in the full sample of Italian municipalities. A similar set of negative results holds when comparing outcomes above and below the Gothic line. Table 3.6.4 considers different indicators of partisan activity around the Gothic line. In Panel A, the outcome variables are the presence of partisan brigades (left wing or other).[16] In panel B, the outcome variables refer to partisans born in the municipality—these variables thus measure the strength of local opposition to the fascist regime, rather than the presence of brigades in the area. All of these outcomes are balanced around the Gothic line, with the exception of the presence of non-left wing brigades, which seems higher South of the line. These results are also apparent from Appendix Figure 3.C.10 and 3.C.11.

Next, we test whether the treatment effect of a longer Nazi occupation on vote shares is stronger where partisan brigades are active. This hypothesis is rejected by the data in Table 3.6.5: the estimated coefficient of the interaction between the presence of a left-wing brigade and being North of the line is negative or insignificant when the outcome variable is the communist vote share, while it is positive and significant when the outcome is the vote share of the right-wing or republican parties.[17] Thus, being in an area of operation of partisan activities dampened the effect of a longer Nazi occupation on the vote shares of extreme-left parties. This finding is inconsistent with the idea that a longer Nazi occupation favored the Communist party because it could exploit the partisan brigades to build grassroots local organizations.

Violence Finally, we ask whether Nazi violence was higher North of the line. The recorded episodes only capture some of the violence actually born by civilians. In particular, forced labor, evacuations of villages, and deportations are not included in the classification of episodes of violence. These other forms of violence were probably more diffuse North of the line, where the occupation lasted longer. Even where the violence did not actually occur, the threat of being hurt and the stress of the foreign occupation lasted longer North of the line, and this too could be reflected in political attitudes.

To capture at least some of these other forms of violence, in Panel A of Table 3.6.6 the outcome refers to the number of deported individuals arrested in the municipality. The estimated coefficient is almost always positive and is statistically significant in the last two columns, suggesting that there were more deportations North of the line.

Table 3.6.6 reports also RDD estimates for the occurrence of at least one episode of German or fascist violence in the municipality, disaggregated by whether they occurred before or after the end of October 1944, that is, the month when the Allies stopped South of the Gothic line. We report the episodes by whether a majority of the victims were partisans or civilians in Appendix Table 3.C.14.

Episodes of violence dated after October 1944 are significantly more widespread above the line, as expected, but episodes dated October 1944 or earlier occur more frequently below the line (the Germans also committed several atrocities during their retreat in the Summer of 1944). As a result, the overall occurrence of at least one episode is roughly balanced around the Gothic line. However, late-in-the-conflict violence was both more indiscriminate and more politically connotated, since it was associated with a more ruthless phase of the war and with the birth of the Italian fascist action squads.

[16] We obtain the same results (available upon request) using the closest distance to partisan brigades (left-wing or other).

[17] Appendix Table reports unconditional estimates.

3.5.5 Survey data

To assess whether the legacy of the Nazi occupation is still detectable, in November-December 2015 we conducted a survey of residents near the Gothic line. We interviewed 2,525 individuals, with at least 20 years of residence in their current municipality and above 40 years of age. The survey was conducted in 242 municipalities within 50 Km from the Gothic line (137 above and 105 below the line). All municipalities had a population of less than 25,000 inhabitants in 2011, and at least 7 individuals were interviewed in each municipality. The telephone interview lasted on average about 10 minutes, and contained about 30 questions, including questions about current socio-economic status (see the Appendix Tables 3.C.15 and 3.C.16 for more details).

Appendix Table 3.C.17 reports balance tests around the Gothic for a number of socio-demographic variables and for political preferences. All variables are balanced, except perhaps a slight unbalance in age and gender, which anyway is not robust across estimation methods. There is also no evidence that today respondents North of the line are more likely to vote left, compared to those South of the line. This difference between our survey and the historical voting outcomes is likely to reflect the evolution of the Italian political system in the Second Republic (the Communist Party no longer exists, and its current re-incarnation, the Democratic Party, is a moderate party).

A more protracted and intense civil war should leave a stronger mark in the memory of citizens and on local traditions. Our survey thus included a number of questions to explore whether this is so. Specifically we asked: "Do you remember or were you told whether a member of your family was a victim of violence during WWII or took part in the civil war as a partisan or as a supporter of Mussolini?" We also asked whether the municipality ever organized events to commemorate the resistance movement, whether the respondent participated in such events, and how congruent were the respondent's political preferences when young with that of his/her father (congruence is defined as casting a vote similar to that of the father in the respondent's first election). Appendix Table 3.C.18, Panel A displays the RDD estimates of these variables around the Gothic line. As expected, the memory of the civil war is stronger North of the line. Except for having a family member who was victim of violence, all estimated coefficients are positive, and several of them are statistically different from zero. Congruence of political preferences between father and child is also stronger North of the line, suggesting stronger political traditions within the family.

In the same spirit, we attempted to elicit anti-German sentiments by asking questions on wedding preferences by nationality, and questions on the Euro. Appendix Table 3.C.18, Panel B presents the RDD estimates, after recoding all the variables so that a positive coefficient indicates anti-German sentiment North of the line. All estimates have the expected positive sign, except for wedding preferences of French vs German. Only a few of them are statistically significant, however, suggesting only weak evidence of more anti-German sentiments.

Finally, we explore the correlations between individual political positions and the memory of the civil war, in the whole sample of respondents around the Gothic line. The results are shown in Appendix Table 3.C.19. In columns (1) and (2), the dependent variable is a dummy variable that equals one if the individual political position is left or center-left, and estimation is by Probit. In columns (3) and (4) estimation is by ordered Probit, and the dependent variable equals 2 if the political position is left, 1 if center-left, and 0 otherwise. Throughout we control for gender, age, years of education, and dummy variables for home ownership, college education, having children, vital record, being North of the Gothic line and 1921 Province fixed effects. As expected, individuals with a family member who took part in the civil war, or who suffered from WWII violence, or living in a municipality that commemorated the resistance are more likely be on the left, irrespective of the specification. A left-wing position is also more likely if political attitudes when young were congruent with their father's position. Altogether these results suggest that a left-wing position is indeed more likely for individuals who retain a stronger memory of the civil war, and indirectly support the idea that a stronger exposure to the civil war left a persistent mark on political attitudes in favor of the Communist Party.

3.6 Conclusion

The civil war and the Nazi occupation of Italy occurred at a critical historical juncture, just before the birth of a new democracy and the establishment of a new party system. For the first time in a generation Italian citizens were choosing political affiliations and forming political identities. We exploit the geographic heterogeneity in the duration and occurrence of the Nazi occupation and of the civil war, to study how these traumatic events shaped the newly born political system.

Our main finding is that, where the foreign occupation and the civil war lasted longer and were more intense, the radical left emerged as a much stronger political force. This effect was not just a temporary reaction to war traumas, but persisted until the late 1980s, leaving a legacy of left-wing political extremism in the Italian political system.

What accounts for this large impact? And why is it so persistent? We discussed two alternative explanations. They both revolve around the fact that the Communist Party was more active in the resistance movement. The first explanation stresses individual political attitudes. In reaction to a longer and more intense exposure to the violent Nazi occupation, voters identified with the radical political forces that stood up more forcefully against the enemy, and that in the end won the civil war. The second explanation emphasizes party organizations: the partisan brigades gave the communists an advantage in building a grassroots political organization in the areas where the resistance movement was active for longer. Although not conclusive, our evidence is more consistent with the first mechanism, operating through voters' attitudes and identities.

Overall, our results have several implications of general interest. First, civil war and widespread political violence reshape political identities in favor of the political groups that emerge as winners from the struggle. This goes to the benefit of more extremist political forces, which typically are more involved in violent conflict. Second, these effects are very long lasting, and persist even when the cleavages that gave rise to the civil war have disappeared. Third, these findings indirectly support an approach to voters' behavior that has a well established tradition in political science (e.g., Campbell et al., 1960 and Achen and Bartels, 2016), but is more at odds with conventional theories in political economics. Citizens vote for the parties with which they identify on cultural, moral, or social grounds. Political identification, in turn, is also shaped by intense and widely shared emotional experiences, and once formed it evolves slowly over time.

Tables

Table 3.6.1: OLS Estimates – Baseline

	Dependent variable: Communist 1946						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years of occupation	0.052	0.051	0.055	0.035	0.035	0.034	0.036
	(0.002)***	(0.002)***	(0.003)***	(0.009)***	(0.017)**	(0.018)*	(0.018)**
	(0.015)***	(0.014)***	(0.015)***	(0.042)	(0.036)	(0.025)	(0.025)
At least one episode of violence	0.039	0.024	0.028	0.015	0.011	0.006	0.012
	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.003)*	(0.004)***
	(0.014)***	(0.013)*	(0.012)**	(0.007)**	(0.004)**	(0.004)	(0.006)**
Within 15 km of violent Nazi	0.081	0.076	0.076	0.050	0.027	0.018	0.020
division	(0.006)***	(0.006)***	(0.006)***	(0.005)***	(0.005)***	(0.006)***	(0.006)***
	(0.032)**	(0.029)***	(0.030)**	(0.020)**	(0.012)**	(0.013)	(0.013)
Birthplace of a partisan		0.063	0.063	0.041	0.024	0.018	0.018
		(0.006)***	(0.006)***	(0.005)***	(0.004)***	(0.004)***	(0.004)***
		(0.014)***	(0.014)***	(0.008)***	(0.004)***	(0.004)***	(0.004)***
Birthplace of a left wing		0.059	0.058	0.042	0.032	0.028	0.027
partisan		(0.014)***	(0.014)***	(0.011)***	(0.010)***	(0.009)***	(0.009)***
		(0.014)***	(0.014)***	(0.010)***	(0.009)***	(0.009)***	(0.009)***
Presence of left wing partisan			-0.013	-0.001	-0.001	-0.001	0.004
brigades			(0.005)***	(0.004)	(0.003)	(0.003)	(0.004)
			(0.015)	(0.010)	(0.008)	(0.006)	(0.006)
Presence of other brigades			-0.058	-0.010	-0.012	-0.008	-0.003
than left wing			(0.006)***	(0.005)**	(0.004)***	(0.004)*	(0.005)
			(0.019)***	(0.011)	(0.008)	(0.007)	(0.007)
At least one violence episode *							-0.013
Presence of left wing brigades							(0.006)**
							(0.007)*
At least one violence episode *							-0.015
Presence of other brigades than left-wing							(0.008)*
							(0.011)
Presence of left wing brigades							-0.006
Within 15 km of violent Nazi division *							(0.010)
							(0.015)
Within 15 km of violent Nazi division *							0.004
Presence of other brigades than left-wing							-0.014
							(0.025)
Observations	5559	5559	5559	5559	5559	5559	5559
R-squared	0.123	0.161	0.174	0.486	0.584	0.640	0.640
Controls	No	No	No	Yes	Yes	Yes	Yes
Fixed Effect	No	No	No	No	Region	Province	Province

Note: Robust standard errors are displayed in parentheses in each second row; standard errors corrected for spatial correlation are displayed in parentheses in each third row. Significance level: ***<0.01, **<0.05, *<0.1. *Communist 1946*: Vote share of the Italian Communist Party (PCI) in the 1946 election. *Years of occupation*: years of occupation measured at province level (see Appendix for exceptions) *At least one violence episode*: Dummy equal to 1 if records report at least one episode of violence in the period considered. *Within 15 Km of violent Nazi divisions*: Dummy equal to 1 if the minimum distance of the municipality from one occupied by either RFSS or HG Division is less than 15 Km (using city hall as reference point). *Birthplace of a partisan*: Dummy equal to 1 if a partisan (or a left-wing partisan) is born in the municipality *Presence of partisan brigades*: Dummy equal to 1 if the area of the municipality intersects the area of operation of the partisan brigade (left wing or other). Other regressors include: Share of illiterate 1921 and 1951, population density 1921 and 1951, latitude, longitude, maximum altitude in the municipality, elevation city hall, vote shares of Communist-Socialist and Catholic in 1919, 1921, and 1924 and Province or Region Fixed Effects.

Table 3.6.2: RDD Causal Effects – Electoral Outcomes

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Estimates Conditional on Pre-war Elections					
Communist 1946	0.084	0.064	0.115	0.086	0.086
	(0.024)***	(0.024)***	(0.029)***	(0.025)***	(0.023)***
	275	742	275	742	327
Communist and Socialist 1946	0.079	0.049	0.092	0.078	0.072
	(0.022)***	(0.023)**	(0.028)***	(0.022)***	(0.021)***
	275	742	275	742	317
Communist and Socialist 1948	0.075	0.050	0.111	0.076	0.081
	(0.025)***	(0.024)**	(0.030)***	(0.024)***	(0.022)***
	275	742	275	742	291
Catholic 1946	-0.026	-0.003	-0.056	-0.027	-0.015
	(0.017)	(0.015)	(0.024)**	(0.017)	(0.014)
	275	742	275	742	648
Catholic 1948	-0.052	-0.034	-0.088	-0.049	-0.041
	(0.021)**	(0.020)*	(0.029)***	(0.021)**	(0.017)**
	275	742	275	742	412
Right Wing 1946	0.011	0.015	0.016	0.016	0.003
	(0.015)	(0.016)	(0.015)	(0.016)	(0.005)
	93	262	93	262	296
Right Wing 1948	-0.002	-0.001	-0.002	-0.003	-0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	224	599	224	599	316
Republican 1946	-0.033	-0.030	-0.021	-0.034	-0.028
	(0.015)**	(0.014)**	(0.019)	(0.016)**	(0.012)**
	275	742	275	742	543
Republican 1948	-0.027	-0.027	-0.013	-0.030	-0.024
	(0.012)**	(0.012)**	(0.015)	(0.013)**	(0.009)**
	275	742	275	742	598

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy; *Republican* corresponds to Republican Party. Estimates are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*.

Table 3.6.3: RDD Robustness – Nearest Neighbor Matching

	Nearest Neighbor Matching					
	Latitude and Longitude			Lat. Long., Pre War Elections		
	25 Km	50 Km	100 Km	25 Km	50 Km	100 Km
Estimates Conditional on Pre-war Elections						
Communist 1946	0.053	0.057	0.057	0.044	0.049	0.049
	(0.027)*	(0.027)**	(0.027)**	(0.030)	(0.028)*	(0.028)*
	40	46	46	40	46	46
Communist and Socialist 1946	0.022	0.026	0.026	0.041	0.046	0.046
	(0.023)	(0.024)	(0.024)	(0.022)*	(0.021)**	(0.021)**
	40	46	46	40	46	46
Communist and Socialist 1948	0.041	0.038	0.038	0.049	0.047	0.047
	(0.023)*	(0.023)*	(0.023)*	(0.022)**	(0.020)**	(0.020)**
	40	46	46	40	46	46
Catholic 1946	0.005	0.006	0.006	-0.012	-0.012	-0.012
	(0.018)	(0.017)	(0.017)	(0.020)	(0.018)	(0.018)
	40	46	46	40	46	46
Catholic 1948	-0.023	-0.021	-0.021	-0.040	-0.038	-0.038
	(0.022)	(0.021)	(0.021)	(0.022)*	(0.021)*	(0.021)*
	40	46	46	40	46	46
Right Wing 1948	0.001	0	0	0	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	40	46	46	40	46	46
Republican 1946	-0.026	-0.027	-0.027	-0.023	-0.023	-0.023
	(0.009)***	(0.009)***	(0.009)***	(0.013)*	(0.012)*	(0.012)*
	40	46	46	40	46	46
Republican 1948	-0.019	-0.021	-0.021	-0.017	-0.017	-0.017
	(0.008)**	(0.009)**	(0.009)**	(0.013)	(0.011)	(0.011)
	40	46	46	40	46	46

Note: Coefficients presented display the difference among mean above the line minus mean below the line for the municipalities within Bologna and Ravenna provinces (forcing the match within province) that are in the common support with respect *Communist and Socialist* 1921 vote share. Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Nearest-neighborhood matching based on latitude and longitude (left) or on latitude, longitude, and prewar election outcomes (right). Metric used: Euclidean distance with replacement. The dependent variable is the residual of the vote share on prewar election outcomes. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy; *Republican* corresponds to Republican Party. Estimates in Panel B are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*.

Table 3.6.4: RDD Contextual Factors – Presence of Partisan Brigades

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A: Presence of partisan brigades					
Presence of partisan brigades	-0.218	-0.128	-0.212	-0.213	-0.205
	(0.101)**	(0.092)	(0.148)	(0.103)**	(0.094)**
	275	742	275	742	283
Presence of left wing partisan brigades	0.013	0.041	-0.059	0.014	-0.026
	(0.120)	(0.115)	(0.155)	(0.120)	(0.098)
	275	742	275	742	379
Presence of other partisan brigades	-0.231	-0.168	-0.153	-0.227	-0.159
	(0.078)***	(0.076)**	(0.068)**	(0.077)***	(0.055)***
	275	742	275	742	531
Panel B: Municipality birthplace of a partisan					
Birthplace of a partisan	0.131	0.142	0.116	0.004	0.032
	(0.134)	(0.119)	(0.179)	(0.138)	(0.086)
	275	742	275	742	827
Birthplace of a left wing partisan	0.083	0.113	0.045	0.040	0.043
	(0.085)	(0.071)	(0.112)	(0.086)	(0.049)
	275	742	275	742	1233
Number of partisans, total	1.853	1.705	1.769	1.906	1.443
	(1.663)	(1.416)	(1.485)	(1.585)	(1.087)
	275	742	275	742	528
Number of left wing partisans	0.255	0.210	0.111	0.208	0.104
	(0.238)	(0.201)	(0.207)	(0.222)	(0.126)
	275	742	275	742	1089

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Presence of partisan brigades*: Dummy equal to 1 if the area of the municipality intersects the area of operation of the partisan brigade (left wing or other). *Birthplace of a partisan*: Dummy equal to 1 if a partisan (or a left-wing partisan) is born in the municipality. See Appendix 3.B for a description of left wing vs other partisan brigades, and for data sources.

Table 3.6.5: RDD Causal Effects by Presence of Partisan Brigades

	Polynomial Regression							
	First order				Second order			
	50 Km		100 Km		50 Km		100 Km	
	up	up*left brig.	up	up*left brig.	up	up*left brig.	up	up*left brig.
Estimates Conditional on Pre-war Elections								
Communist 1946	0.069	0.021	0.062	0.005	0.104	0.014	0.090	-0.004
	(0.031)**	(0.029)	(0.027)**	(0.018)	(0.034)***	(0.028)	(0.029)***	(0.019)
	275	275	742	742	275	275	742	742
Communist and Socialist 1946	0.107	-0.041	0.066	-0.025	0.128	-0.047	0.106	-0.038
	(0.028)***	(0.025)	(0.025)***	(0.017)	(0.032)***	(0.025)*	(0.026)***	(0.017)**
	275	275	742	742	275	275	742	742
Communist and Socialist 1948	0.080	-0.007	0.049	0.002	0.123	-0.015	0.083	-0.009
	(0.032)**	(0.030)	(0.027)*	(0.019)	(0.036)***	(0.029)	(0.029)***	(0.020)
	275	275	742	742	275	275	742	742
Catholic 1946	-0.023	-0.004	-0.004	0.001	-0.059	0.003	-0.033	0.008
	(0.022)	(0.021)	(0.017)	(0.014)	(0.029)**	(0.021)	(0.021)	(0.014)
	275	275	742	742	275	275	742	742
Catholic 1948	-0.041	-0.015	-0.035	0	-0.084	-0.005	-0.054	0.006
	(0.029)	(0.025)	(0.023)	(0.016)	(0.035)**	(0.024)	(0.026)**	(0.016)
	275	275	742	742	275	275	742	742
Right parties 1946	0.005	0.006	0.012	0.004	0.009	0.006	0.012	0.004
	(0.015)	(0.002)**	(0.016)	(0.002)**	(0.015)	(0.003)**	(0.016)	(0.002)**
	93	93	262	262	93	93	262	262
Right parties 1948	-0.004	0.002	-0.002	0.002	-0.003	0.002	-0.004	0.002
	(0.002)*	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)*	(0.001)*
	224	224	599	599	224	224	599	599
Republican 1946	-0.064	0.045	-0.042	0.020	-0.055	0.043	-0.052	0.024
	(0.021)***	(0.016)***	(0.017)**	(0.008)**	(0.023)**	(0.016)***	(0.019)***	(0.009)***
	275	275	742	742	275	275	742	742
Republican 1948	-0.050	0.034	-0.034	0.012	-0.038	0.031	-0.040	0.014
	(0.019)***	(0.014)**	(0.014)**	(0.007)*	(0.019)*	(0.013)**	(0.016)**	(0.007)**
	275	275	742	742	275	275	742	742

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line (column up) and the interaction between a dummy for the presence of a left-wing brigade and being North of the line (column up*left brig). Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948. Estimates are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*.

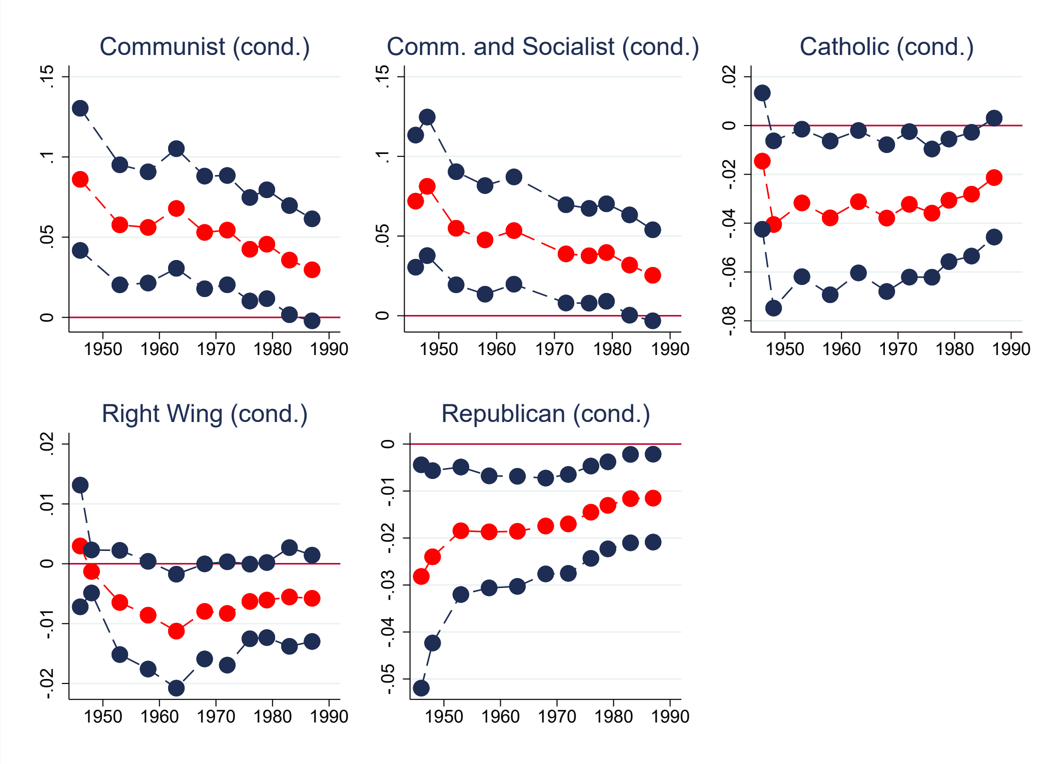
Table 3.6.6: RDD Contextual Factors – Episodes of Violence

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A. Number of deported people arrested in the municipality					
Entire Period	2.055	2.366	-0.049	2.780	2.020
	(1.285)	(1.480)	(2.608)	(1.369)**	(0.924)**
	275	742	275	742	326
Panel B. At least one violence episode					
Nov. 1944-Aug. 1945	0.151	0.131	0.135	0.141	0.075
	(0.132)	(0.116)	(0.174)	(0.133)	(0.072)
	275	742	275	742	1113
Jan. 1943-Oct. 1944	-0.311	-0.228	-0.312	-0.313	-0.208
	(0.104)***	(0.083)***	(0.163)*	(0.108)***	(0.083)**
	275	742	275	742	477
Entire Period (Jan. 1943-Aug. 1945)	-0.211	-0.152	-0.169	-0.212	-0.145
	(0.077)***	(0.065)**	(0.119)	(0.081)***	(0.079)*
	275	742	275	742	383

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *At least one violence episode*: Dummy equal to 1 if records report at least one episode of violence. January 1943–August 1945 is the entire period for which we have episodes recorded. January 1943–October 1944 (November 1944–August 1945) is the period before (after) the battlefront moved to the RDD Gothic line.

Figures

Figure 3.6.1: Long-Term Persistence – RDD



Note: Coefficients and 95% confidence intervals, estimated by local linear regressions as in the last column of Table 3.6.2, for all national elections from 1946 to 1987 and controlling for prewar electoral results. Data for the Communist Party are missing in 1948 as it ran with the Socialist Party; data for the Socialist Party are missing in 1948 as it ran with the Social Democratic Party; for the right-wing parties we restrict our attention to 1953 onwards to have consistent time comparisons, as by doing so we focus on the pro-fascism MSI and not on pro-monarchy parties.

3.A Online Appendix – Historical Background

This section summarizes the main events that led to the birth of the post-WWII Italian political system. Since we compare the elections in the immediate postwar period to the latest free elections before the fascist dictatorship, we start with a brief account of the Italian political system before the advent of fascism. We then turn to the WWII period—discussing the nature of the foreign occupation and of the civil war (i.e., our treatment)—and finally to the postwar Italian political system.[18]

3.A.1 Prewar period

At the end of World War I, Italy was a constitutional monarchy and the government was supported by a parliamentary majority of liberal-moderate representatives elected in 1913. Socialist and catholic movements were emerging, however. These new parties appealed to Italian voters who had only recently been enfranchised.

Before the consolidation of Mussolini's dictatorship, three free elections were held in 1919, 1921, and 1924 under universal male suffrage. Average turnout was around 60%. In 1919 and 1921, the electoral system was proportional, but voters could cast a preference vote for candidates running in different lists (the so called "panachage" system). In 1924, the electoral system entailed a large majority premium that gave two thirds of the seats to the party gaining a relative majority in a single national district, and assigned the remaining seats to the other parties according to a proportional rule. Thus, none of these electoral rules was identical to the pure proportional system with preference votes created after WWII, although all of them had important elements of proportionality.

In the 1919 election, the Italian political system was essentially split between three groups: A liberal-moderate coalition representing the political elites that had ruled Italy in the previous decades, and two emerging and antagonistic political groups, the catholics and the socialists. These new parties were on different positions on many issues, and were unable to form viable political alliances between them. In 1919 the liberal coalition retained a relative majority but, despite a large absenteeism rate, it lost many votes and seats to the socialist and catholic parties. This outcome led to a short period of instability, which resulted in a new election in 1921. The main novelties of the 1921 election were the gains obtained by the fascist candidates, who ran in the same lists as the traditional liberal bloc, and the fact that the Communist Party entered the ballot for the first time.[19] The votes and seats obtained by the catholics and socialists were roughly unchanged (or slightly lower) compared to 1919.

After a period of political violence and instability, in 1922 Mussolini was asked by the King to form a government. He received a vote of confidence by a parliamentary majority that included the catholic party, while the socialists (and the small communist group) voted against him. Mussolini soon changed the electoral law to a proportional system with a large majority premium for the party with a relative majority (see above). In 1924, a new election was held, and the fascist party obtained two thirds of the votes. Although formally free and regular, this election was held in a climate of violence and intimidation. Within a few years Mussolini further consolidated his power into a dictatorship.

Elections in 1919, 1921, and 1924 are not easily comparable between each other, but each of them displays within-municipality variation that conveys information on the underlying political preferences of the (local) population. General elections were also held in 1929 and 1934. Following a parliamentary reform enacted

[18] A more detailed historical account of these periods and episodes is provided in Romanelli (1995), Leoni (2001), Baldissara et al. (2000), Collotti et al. (2000), Collotti et al. (2006), Gentile (2015), Pavone (1991), and Matta (1996).

[19] The Italian Communist Party was founded on January 21, 1919 in Livorno as a split from the socialist movement. This was clearly a split from the extreme left as the reference model of the new party was the Bolshevik Revolution, and it was motivated by the claim "we want to do as in Russia."

in 1928, these elections took the form of a referendum with only the Fascist party running and with a voting system that did not guarantee the secrecy of the vote.[20] Moreover, to our knowledge, no data are available at the municipal level. We thus ignore these last two elections.

3.A.2 War period

The Gothic line

We can date the beginning of the Italian "civil war" (Pavone, 1991) in July 1943, when the Allies landed in Sicily. Since then and until May 1945, Italy was ravaged by war. On one side were the Germans, supported by the forces that remained loyal to Mussolini. On the opposite side were the Allies, supported by the Italian resistance movement (operating in the areas occupied by the Germans). Throughout this period, the overall estimated casualties were about 360,000, of which about 155,000 Italians. The Italian victims of the Nazi occupation and of the civil war were 70,000–80,000. Of these, at least 10,000 were civilians killed by Nazis or fascists, about 30,000 were resistance fighters, and about as many were fascists (see Gentile, 2015, pp. 4–5). In addition, about 40,000 civilians were deported to Germany (of which 7,500 were Jews), 90% of these died (see Rochat, 2005, p. 443).

The battlefront moved overtime, but it remained stuck for several months near a defensive line prepared by the Germans in Central Italy, the so called "Gothic line." Appendix Figure 3.C.12 illustrates the areas under German occupation, by number of days, as well as the Gothic line. Northern-Central Italy remained under German occupation for over two years, while the South for two to five months. As can be seen from Appendix Figure 3.C.12, the Germans were able to stop the Allies for several months between Rome and Naples (along the so called "Gustav Line," which was held by the Germans between December 1943 and May 1944). From there, the battlefront moved rapidly toward Northern-Central Italy, in the area between Florence and Bologna, where the Germans had prepared a strong and continuous line of defense. Preparation for the Gothic line had began well in advance, while the Germans were still trying to defend the area South of Rome. This allowed the Germans to prepare an effective defense system, which stopped the Allies between the Summer of 1944 and the Spring of 1945. The Gothic line was conceived as the last defense for German retreat. The barrier extended from the Western coast between *La Spezia* and *Massa* to the Eastern coast between *Pesaro* and *Rimini*. Basically, the line consisted of defensive positions and bunkers, hundreds of thousands of mines and booby traps, and a continuous anti-tank ditch almost six miles long; "Allied aerial reconnaissance photographs showed a dense network of machine-gun posts, gun positions and ditches." (Holland, 2008, p. 301).[21]

As can be seen from Appendix Figure 3.C.12, during the Summer of 1944 the battlefront remained stuck in an area about 50 Km South of the Gothic line. The continuous line in Appendix Figure 3.C.12 is the Gothic, which was held by the Germans between November 1944 and April 1945. The line was finally overcome by the Allies in April 1945, and in May the Germans surrendered control of Italy. The battles around the Gothic line brought much destruction to the area, with heavy casualties amongst Germans (around 48,000), Allies (32,000), and Italian fascists, partisans and civilians (altogether 30,000–40,000), see Montemaggi (1980). As discussed below, the Allies were extremely close to overcome the Gothic line before the Winter of 1944, but a combination of hard weather and divergences between the US and UK—with the former

[20] Voters could vote either "Yes" or "No" to approve the list of deputies appointed by the Grand Council of Fascism. Voters were provided with two equally sized sheets, white outside, inside bearing the words "Do you approve the list of members appointed by the Grand National Council of Fascism?" The electoral sheet with the "Yes" was also accompanied by the Italian flag and a fascist symbol, the one with the "No" had no symbol. Inside the voting booth there was a first ballot box where the voter left the discarded sheet and then delivered to the scrutineers the chosen sheet, so that they would ensure that it was "carefully sealed." Turnout was around 90% and approval of the fascist list over 98%.

[21] It is estimated that over 50,000 Italian forced workers were involved in building the Gothic line (Ronchetti, 2009).

prioritizing the invasion of France and the latter paying more attention to the Mediterranean—froze the battlefront at the Gothic line for six months.

Appendix Figure 3.C.2 zooms in the area around the Gothic line, illustrating the size and elevation of each municipality and how the battlefront moved during the Summer of 1944. There are three demarcation lines. The line labeled "Allies" is where the Allies stopped between August and mid-September 1944. The line labeled "Fall 1944" is the original line set up by the Germans. Between late August and mid-September 1944 the Allies succeeded in breaching this line (so called operation "Olive"). The line labeled "Nov. 1944–Apr. 1945" is where the Germans managed to contain the US-British offensive. From the end of October onwards, the Allies and the Germans were fighting along this line. It was finally breached in April 1945. Our RDD is on the Northern-most line "Nov. 1944–Apr. 1945," which was held for the longest period.

For the sake of our empirical analysis, it is important to note that the position of the last line of defense was not only the outcome of a German decision. It was also largely due to random events, which forced the Allies to stop their offensive between late October 1944 and the Spring of 1945. In August 1944, the Allies withdrew several divisions from the Italian front to launch a new offensive in Southern France. This decision was highly controversial: It was supported by the Americans, who wanted to create a distraction for the Germans from the ongoing battles in the rest of France, but it was opposed by the British, who instead leaned toward a stronger offensive in Italy. The American point of view prevailed, and this weakened the efforts of the Allies in Italy at a critical point in time (see Churchill, 1959). A second important random event was the weather, which deteriorated harshly in late October. These are the words used by Churchill to describe those critical moments in October 1944: "The weather was appalling. Heavy rains had swollen the numberless rivers and irrigation channels [....]. Off the roads movement was often impossible. It was with the greatest difficulty that the troops toiled forward. [...] Not until the spring were the armies rewarded with the victory they had so well earned, and so nearly won, in the autumn" (see Churchill, 1959, p.839).

Foreign occupation and resistance movement

In the North, Mussolini tried to revamp the fascist regime by claiming statehood for the areas under German occupation (with the exclusion of two territories directly annexed to the German Reich, close to the Alps and to the Northern Adriatic coastland) and by setting the new capital of his *Repubblica Sociale Italiana* (RSI) in the small town of *Salò*. But this experiment resulted in little more than a Nazi-backed puppet state, dependent entirely upon Germany and with no autonomous domestic or foreign policy of any sort. The Nazi occupation of Northern Italy is unanimously deemed as violent and extractive by the historical literature. As Rudolf Rahn, the German diplomat who was the plenipotentiary to the RSI, put it: "Everything in occupied Italy must be exploited by us for our war effort" (see Holland, 2008, p. 111). This meant coerced labor and deportations, handing over of all gold reserves, shutting down of factories to ship equipment to Germany, full control of the remaining factories for military purposes, and food reserves (if any) packed off to Germany.

In Allied-held Italy, all areas close to the battlefront were directly run by the Allied Military Government (AMG) and then, as the front advanced up toward the North, they were passed back to the authority of the Italian government, formally appointed by the King. At first, under prime minister Pietro Badoglio, the political legitimacy of the government was weak, since the monarchy was implicated with the fascist regime. But then the political parties outlawed by the fascist regime and active in the resistance movement (see below) gradually took responsibility and joined the governments lead by Ivanoe Bonomi from June 1944 until the end of WWII.

Although the autonomy of the government was severely limited by the Allied Control Commission, self-determination was much stronger South of the line and, most importantly, free speech was moving Italy closer to democracy. In particular, the Bonomi government started having greater responsibility after

September 1944, when Churchill and Roosevelt made a joined declaration shaping the future path toward Italy's self-determination and economic recovery. The sharp divide between the political (and psychological) situation North vs South of the Gothic line is best described by Italian lieutenant Eugenio Corti (see Holland, 2008, p. 251, italics ours): "I wondered if the British and Americans realized that *behind their lines* one could feel a respect for men. It felt like this whenever one saw notices where occupation troops threatened fines and at most jail sentences that *on the other side* were invariably punished with death. We would no longer hear talk of executions, and this fear—which makes man nothing more than a beast—would no longer hang over us."

Throughout the civil war period, the resistance movement grew rapidly, from a few thousands of fighters in the Fall of 1943 to several tens of thousands one year later. In addition, it is estimated that around 20,000 civilians were directly connected to the resistance movement, even if only few of them nested into political coordination (see Bocca, 2012, p. 265). Although the movement was spontaneous and did not have strong party affiliations, the leaders of the various groups were active members of political parties that the fascist regime had disbanded. Three main political affiliations can be identified: The left-wing groups, linked with the communist and socialist parties; the catholic groups, linked with the Christian Democratic party; and other centrist groups, linked with liberals that had opposed Mussolini. In addition there were several other small groups with no explicit political affiliation.[22] The left-wing brigades, and to a smaller extent the catholic, were by far the largest and more active organizations. The political parties active in the resistance movement joined forces in the "National Liberation Committee," which gave crucial support to the Bonomi governments.

In the North, the civil-war nature of the conflict was reinforced by the decision of Mussolini to give birth to the "black brigades," paramilitary groups directly run by the Fascist Party, who also attracted tens of thousands of volunteers, although poorly trained and equipped.

According to historical accounts, the effects of German occupation on the civilian population were not evenly distributed in time and space. Gentile (2015), in particular, stresses two stylized facts. First, combat troops near the front line were more ruthless and prone to hurt civilians than other troops in charge of logistics and administration. This reflected both the selection and composition of such troops, as well as the additional stress and danger that they faced. Second, following hierarchical orders, the German attitudes and tactics changed over time, and became particularly aggressive toward partisans and civilians alike from the Summer of 1944 onwards, when the danger posed by the resistance movement became more apparent. On June 17, 1944 Field Marshal Albert Kesselring, the German commander in chief in the Mediterranean, issued an order promising indemnity to soldiers who should exceed "normal restraint" in the choice of repression methods.[23]

Our (local) source of exogenous variation—the Gothic line—captures a treatment made up of both (i) the extractive Nazi occupation that characterized the last period of WWII and (ii) the civil war between the fascist and partisan brigades. The compound nature of this treatment reinforces its occurrence, as both elements operate along the same spectrum of political alignment. The control group includes municipalities occupied by the Allies, where free speech was allowed and self-determination by Italian authorities gradually developed.

[22] In our data set referring to the area around the Gothic line, we count 115 communist brigades (*Garibaldi*), 44 other left wing brigades (*Matteotti* and *Giustizia e Libertà*), and 59 non-marxist brigades (*Fiamme Verdi* and others).

[23] Nazi authorities also tried to make this clear to the Italian population. In the Summer of 1944, German planes dropped leaflets over Central Italy with the warning: "Whoever knows the place where a band of rebels is in hiding and does not immediately inform the German Army, will be shot. Whoever gives food or shelter to a band or to individual rebels, will be shot. Every house in which rebels are found or have stayed, will be blown up" (see Holland, 2008, p. 145).

3.A.3 Postwar period

The resistance movement and the political parties to which it was linked played a key role in the immediate aftermath of the war. Several leaders of the movement became prominent political figures and were elected in the postwar Parliament for several legislatures. The civil war contributed to shape the political identity of these parties and gave them a visibility and popularity that they had not enjoyed before, also due to the repression imposed by the fascist regime.

The first key decision of the new political leadership was to hold an election for a constitutional assembly. The election was held in 1946, simultaneously with a referendum on whether to abandon the monarchy. Monarchy lost and Italy became a Republic. With this election, suffrage became universal, thus women had the right to vote for the first time. The electoral rule for the constitutional assembly and for all subsequent elections until 1992 was proportional. All the main parties presented lists of candidates at the constitutional assembly, and the party system did not change significantly afterward. Hence, the election for the constitutional assembly is comparable to subsequent political elections. The first regular election was held in 1948. The only difference in party labels is that in 1948 the communist and socialist parties ran together under the label of "Popular Front," whereas they had run separately in the 1946 constitutional election. In 1953 and in subsequent elections they split again. Monarchist parties progressively disappeared from the political scene; the last election in which they ran was in 1968. On the extreme right, a party close to the fascists, *Movimento Sociale Italiano* (MSI), was founded on December 26, 1946 and appeared on the ballot in the 1948 election, but consolidated its vote share (around 5–7%) only from the 1953 election onwards.

The political system that emerged in the late 1940s reflected the legacy of the civil war in several respects. First, as already noted, most political leaders had played an important role in the resistance movement, at least in the period 1943–45. Second, the party system was highly polarized. On the left the largest party were the communists (the biggest communist party in Western countries), which at the time had strong ideological and financial links with the Soviet Union, while the extreme right remained loyal to the fascist regime.[24] The Italian Communist Party always maintained strong links with the Soviet regime; for instance, it supported the Soviet invasion of Hungary in 1956, most of its leaders received training in Moscow, and financial aids from the Soviet Union reached the Italian communists as late as in the early 1980s (Cervetti, 1999). Also on economic policy, the Communist Party maintained an extremist stance until the early 1980s, for instance opposing the Bill of Workers' Rights in 1970 (as it would have tempered and delayed the fall of capitalism) and the entry of Italy in the European monetary system in 1979. Third, and partly as a result of such ideological polarization, one of the main goals of the Constitutional assembly was to create a very inclusive and consensual political system, to minimize the risk of violent conflict. This resulted in a strictly proportional system, perfect bicameralism, and several checks and balances that diluted executive powers.

The main features of the Italian postwar political system remained roughly unchanged until the early 1990s, when several things changed. First, with the collapse of the Soviet Union, the Italian Communist Party made a credible and pronounced shift toward social democracy. Second, the Christian Democrats and the Socialist Party collapsed under the weight of corruption scandals, leaving room for new moderate forces led by Silvio Berlusconi. Third, the electoral rule was changed to a mixed-member system. Our analysis ends just on the edge of this transition.

[24] Until the early 1990s, the two biggest parties were the Christian Democrats, the ruling party over all of this period, with average vote shares of 35–40%, and the Communist Party, whose vote share grew from 15–20% right after the war to more than 30% in 1976. The vote share of the Socialist Party oscillated around 10–15%.

3.B Online Appendix – Data Sources and Description

The unit of observation is the municipality. We excluded the small region of Aosta Valley from our sample, because it always had a different electoral system. Moreover, its political scene has always been dominated by local parties. Geographic analysis used the Geographical Information Software (GIS) on the Italian 2001 administrative division map for what concerns municipalities structure (Source: ISTAT). In the main analysis we include 1921 Province or Region Fixed effects (Source: Elesh.it)

3.B.1 Political outcomes

Prewar political variables: We collected data on political outcomes before the war, for the elections held in 1919, 1921, and 1924. Here the source is Corbetta and Piretti (2009), who carried out a serious and meticulous work of reconstruction for that period. The Communist Party was very small in the 1921 and 1924 elections (and it did not exist in 1919), so we lump together the socialist and communist vote in the pre-fascist period using Leoni (2001) as reference. The right-wing vote cannot be separately measured in 1921, since fascists were running together with the more traditional and moderate liberals in that election. Hence for the pre-fascist period we only collect the *Catholic* and *Communist and Socialist* variables. Since there are several missing observations, in our baseline analysis we fill the missing observations in each election exploiting the remaining two elections plus additional observables. Thus, to fill the missing observations in, say, vote shares for catholics in 1924 we impute predicted values of an OLS regression of the available vote shares on non-missing vote shares for catholics in 1919 and/or 1921 plus the following observables: Population density in 1921, illiterate share in 1921 and regional fixed effect. And similarly for 1919 and 1921 and when communists-socialist vote shares are missing. The parties in the *Catholic* definition are: In 1919 Partito Popolare Italiano; in 1921 Partito Cristiano del Lavoro; Partito Popolare Dissidente; Partito Popolare Italiano and Popolari Dissidenti; in 1924 Partito Popolare Italiano. The parties in the *Communist and Socialist* definition are: In 1919 Blocco Socialista Riformista-Repubblicano e dei Combattenti; Partito Radicale-Socialista-Repubblicano; Partito Sindacalista; Partito Socialista Indipendente; Partito Socialista Indipendente; Partito Socialista Italiano; Partito Socialista Riformista; Partito Socialista Ufficiale; Partito del Lavoro; Sindacato dell'Impiego; Socialisti Autonomi and Unione Socialista Italiana; in 1921 Partito Socialista Autonomo; Partito Socialista Indipendente; Partito Socialista Riformista; Partito Socialista Ufficiale; Partito Comunista and Partito Comunista d'Italia; in 1924 Partito Socialista Massimalista; Partito Socialista Ufficiale; Partito Socialista Unitario; Partito Comunista and Partito Comunista d'Italia.

Postwar political variables: We measure political outcomes by the percentage of votes received by political parties at the 1946 election of the constitutional assembly, and in all subsequent 10 political elections for the Chamber of Deputies until 1987 (namely 1948, 1953, 1958, 1963, 1968, 1972, 1976, 1979, 1983 and 1987). Source: Italian Ministry of Interior. We consider three political groups. First the radical left, measured by the votes given to the Communist Party (Partito Comunista Italiano). We call this variable *Communist*. Since in 1946 the communist and the socialists (Partito Socialista Italiano) formed a single electoral list, the Popular Front, we also consider the votes received by these two parties together, and we call it *Communist and Socialist*. The second group is the Christian Democrats (Democrazia Cristiana), that we call *Catholic*. The third group, called *Right wing*, consists of the Movimento Sociale Italiano (a party close to the fascists) and of smaller parties that supported the monarchy (namely: In 1946 Blocco Nazionale della Libertà, in 1948 and 1953 Partito Nazionale Monarchico, in 1958 Partito Nazionale Monarchico and Partito Monarchico Popolare, in 1963 and 1968 Partito Democratico Italiano di Unità Monarchica). Since we are interested in how the German occupation shifted political preferences from a moderate to an extreme left vote, we also compute the difference between the vote to communist and the vote to catholic parties. This variable is called *Communist minus Catholic*. In some analysis we also use the variable *Communist and Socialist minus Catholic*.

3.B.2 War-related variables

Episodes of violence: We collected data on the number of episodes in each municipality, the date, and the number and kind of victims. The full data set includes: The number of violent episodes in each municipality (this is the variable used in Appendix Figure 3.C.1); date and municipality; total number of victims by status (civilian, partisan, soldier). Although the meticulous work done by the authors of "Atlas of Nazi and fascist massacres", since combining multiple sources entails the risk of double counting, and since counting the number of victims entails likely measurement error, our preferred measure is a dummy variable, that equals 1 if in the municipality (and interval of time where applicable) there was at least one episode of violence. We also consider dummy variables for whether the majority of victims were partisans or civilians.

Our source is the "Atlas of Nazi and fascist massacres" (ANPI-INSMLI, 2016).[25] This database lists all the massacres and the individual murders of civilians and resistance fighters killed in Italy during Second World War (mainly after September 8, 1943) both by German soldiers and soldiers of the Italian Social Republic outside of the armed fights.[26] These range from the first murders in the South to the withdrawal massacres committed in the days after the Liberation. The historical inquiry was conducted locally by more than ninety researchers under the supervision of a joint historical commission established by Italian and German governments in 2009. The commission used the results of previous studies of the same kind made in Apulia, Campania, Tuscany, Emilia Romagna, and Piedmont and used three main national common sources: (i) The database of violent crimes perpetrated against civilians during the German occupation of Italy, established by the Joint Historical Italian-German Commission and based on police reports stored in the Archives of the Historical Office of Army General Staff and the Historical Archives of the Carabinieri of Rome. (ii) The General Repository of war crime reports collected from 1945 by the Army Prosecutors office in Rome; this report was illegally dismissed in 1960 and was later recovered by the Parliamentary Commission of Inquiry while investigating on the reasons for the concealment of some files about Nazi-fascist crimes (14th Parliamentary term). (iii) The rulings and files of the judiciary proceedings debated in the Military courts during the last trial season (from 1994 until now).

This source was not immediately available to us, however. In a previous version we had started from a composite dataset that mainly relied on record of charges pressed to "Carabinieri" (Italian Police, CSIT (2012)), for violence episodes and massacres against Italian citizens and Allied personnel committed by Nazi-fascists forces in the period 1943-1945. We then integrated this source with the following additional sources: Collotti et al. (2000) and Collotti et al. (2006) and Gentile (2015).[27] This last source is particularly rich and detailed, since besides the Italian sources, it also incorporates episodes of violence reported in the German War Archives. Since CSIT (2012) (and partially also the other sources) reports single murders, we had assumed that two murders happening in the same municipality at no more than three days of distance were part of the same episodes. In order to avoid bias due to the same event counted twice we manually eliminated double episodes reported by CSIT (2012) or any other sources with meticulous checks on possible discrepancies on the location, the date or the number of victims involved in each episode.

Once we got access to the "Atlas of Nazi and fascist massacres" we recognized that this new source was more uniform and coherent than our first composite dataset, and thus in the current draft we only rely on the new source, the Atlas. Nevertheless, to assess robustness to possible measurement error, we merged the two data sets (our old composite dataset and the new data from the Atlas), trying to avoid double counting. The results reported in the paper are very similar to those obtained in the replications with this merged data set.

[25] Data downloaded in April 2016.

[26] The data span from February 1943 to May 1945, but only 21 out of 5,594 events are dated before September 1943.

[27] We also consider Matta (1996) for robustness checks, however since he reported only partial information for each episode we excluded it from the main analysis

Line of conflict: Based on Baldissara et al. (2000, figure 23), we have reconstructed the evolution of the battlefront around two main lines of conflicts, geo-referencing the corresponding maps: The Gustav line and the Gothic line. In both cases, a few months of adjustments before the final settlement of the battlefront have been necessary. Appendix Figure 3.C.2 illustrates the evolution of the battlefront around the Gothic line. There are three demarcation lines. (i) The line labelled "Allies" is where the Allies stopped between August 1944 and mid-September 1944. (ii) The line labelled "Fall 1944"is the original Gothic line set up by the Germans. Between late August and mid-September 1944 the Allies succeeded in breaching this line (the so called operation "Olive"). (iii) The line labelled "Nov. 1944–Apr. 1945"is where the Germans managed to contain the US-British offensive. From the end of October onwards, the Allies and the Germans were fighting along this line. It was finally breached in April 1945. Our RDD analysis is on the Northern-most line "Nov. 1944–Apr. 1945", which was held by the Germans for the longest period.

Years of occupation: Fraction (or multiples) of years of occupation by German troops. Data refer to provinces (all the municipalities in the same province have the same number of years of occupation), except for the provinces cut by a line of conflict (both for Gothic and Gustav line), where provincial data have been corrected as follows:

- For the municipalities above the line of conflict belonging to a province below the line, we assign the years of occupation of the closest province above the line.
- For the municipalities below the line of conflict belonging to a province above the line, we assign the years of occupation of the closest province below the line.

Definition of occupation: Physical presence of Nazi troops on the Italian territory, for military control or for defense against the Allies (for what concerns events after the Armistice of Cassibile). The starting date is the planning and constitution of the first Nazi troops of the Operation Achse (9 May 1943), after the end of the campaign of Tunisi. The aim of this operation was to react to the possible desertion of the Italian ally. Sources: Mainly Baldissara et al. (2000). Minor adjustments have been made using province specific references.

Partisan Brigades: We geo-referenced the maps of Baldissara et al. (2000) (figures 8, 12, 15, 16, 17, 18, 19) that report the area of activity of partisan brigades during World War II. We created a dummy variable for the presence of partisan brigades equal to one if the municipality partly overlaps with the area in which a partisan brigade was active during the conflict (*Presence of partisan brigades (Intersect)*) or whether the area of the municipality is contained entirely in the operation area of a brigade (*Presence of partisan brigades (Within)*). We also consider the minimum distance of each municipality city hall from the area of activity of each brigade. The brigades considered are the following:

- Left wing brigades: Brigade Garibaldi (Italian Communist Party), brigade Matteotti (Italian Socialist Party) and brigade Giustizia e Libertà (Partito d'Azione).
- Other brigades: Brigade Fiamme Verdi (Christian Democrats) and residual autonomous brigades.

List of partisans: From ANPI (National Association of Italian Partisans) we collected a list of 3,117 partisans with a short biography. We recover information on their birthplace and whether they were linked to a wing left party.

16th SS-Panzer-Grenadier-Division "Reichsfuhrer-SS" and "Hermann Goering" divisions location: We coded the location of these two specific German divisions, particularly violent and responsible for a very large number of criminal episodes against civilians. We have records of the precise location of these troops throughout the Italian civil war. From this we construct a dummy variable that takes value 1 for the municipalities that are within 15 Km or 10 Km from where either of these divisions have been located (measured as distance between city halls). We restrict attention to those two specific divisions, discarding all the other SS or Luftwaffe divisions, since in the reconstruction made by our main source (Gentile (2015)) those are the troops responsible for the majority and most dramatic episodes (e.g., Sant'Anna di Stazzema, Marzabotto).

Deported: Number of political deportations to Germany by municipality of capture. Source: Mantelli and Tranfaglia (2013). We have data on only 6,500 individuals, out of about 40,000 deported.

3.B.3 Other city characteristics

Geographic variables: We collected data on city hall elevation, and on maximum and minimum elevation in the municipality. Source: National Institute of Statistics (ISTAT). We also created a grid of 25 Km width covering all the Italian territory.

Industrial plants per capita: We collected data on the number of industrial plants per capita in each municipality from the 1951 Census. Source: ISTAT. Thanks to Fontana et al. (2019), we got access to the number of industrial plants and workers in 1927, we divided both measures by population in 1921. Source: *Censimento Industriale 1927*, ISTAT.

Agricultural variables: Thanks to Fontana et al. (2019), we got access to the number of agricultural firms and workers in 1929, we divided both measures by population in 1921. We also got the number of livestock (again per capita in 1921) and the percentage of surface devoted to agricultural production. Source: *Catasto Agrario 1929*, ISTAT.

Population and illiterate share: We collected data on total resident population, population density and literacy rates (1911, 1921, and then 1951, 1961, 1971, 1981 and 1991). Census were easily available only from 1971 onwards. For all the other Censuses we manually digitalized the data. Source: ISTAT.

3.B.4 Structure of Italian municipalities

The administrative structure in Italy changed over the years. In 1948 there were 7,392 municipalities, in 2001 the number had increased to 8,100. In order to build a time consistent panel, we took 2001 as the reference year. For all the years different from 2001 we performed the following adjustments:

- Change the names: Some municipalities changed their names, the main reason was to avoid confusion; names must be mapped in order to have a complete series for each municipality. One example is Madesimo in province of Sondrio that before 1983 was called Isolato.
- Consider aggregations *(i)*: Some municipalities merged into a single entity. For instance, at date t we observe municipalities A and B, but at date $t' > t$, we observe municipality C corresponding to the merger of A and B. In 2001 we only observe municipality C. Then only municipality C is included in the sample. For date t when C did not exist yet, we impute to C the data of $A + B$.
- Consider partial aggregations *(ii)*: It may be that some municipalities absorb a municipality that no longer exists. For instance at date t we observe A, B and X, but at date $t' > t$, we observe municipality A and B while territory of X has been split (not necessarily equally) between A and B. In 2001 we only observe municipality A and B. Then only municipalities A and B are included in the sample. For date t when also X existed, we impute data of X to both A and B; that is, at date t, we impute $A = A + X$ and $B = B + X$
- Consider disaggregations *(i)*: Some municipalities split their territory in two or more municipalities.

This situation is quite common in Italy, since Fascism tried to reduce the administrative centres, while the number of municipalities increased in the postwar period. For instance, suppose that at date t we observe only municipality C, but at date $t' > t$, we observe municipalities A and B corresponding to the separation (not necessarily equally) of C. In 2001 we observe A and B, but not C. Then we include in the sample both A and B. For date t, when A and B did not exist yet, we impute to both of them the data of C; that is, at t, we impute $A = C$, $B = C$.

- Consider partial disaggregations *(ii)*: We also track the case where C still exists in 2001 but at $t' > t$ parts of C where dismembered to give birth to A and B, with C still existing today. In this case, for all date prior to t we impute $A = C$ and $B = C$.

We neglect changes in the boundaries that do not determine the end of a municipality or the birth of a new one, since they do not alter municipalities structures and since our variables mainly refer to shares. All these adjustments used records in ISTAT and Italian Agency of Revenue, tracking changes in the period of interest. The only exception are municipalities born from municipalities that still exist: In these cases we had to manually check each split. These adjustments were made for all data at the municipality level (Census and electoral data, but also episodes of violence). When a municipality has data imputed as described above, we retain only the shares (e.g., illiterate share) and we discard absolute values (e.g., total number of illiterates).

Reference year for Provinces and Regions is 1921. We use GIS files (source Elesh.it) to assign each 2001 municipality to historical administrative units. As a robustness we also considered 1931, 1945 and 2001 administrative boundaries and the results are similar.

3.C Online Appendix – Additional Tables and Figures

In this section, we report additional tables and figures, which contain descriptive statistics and robustness checks, and are also discussed in the paper.

Table 3.C.1: Summary Statistics

Variable	Obs	Mean	Sd	Min	Max
Communist 1946 (%)	5,559	0.151	0.142	0	0.768
Socialist and Communist 1946 (%)	5,559	0.375	0.213	0.002	0.914
Catholic 1946 (%)	5,559	0.421	0.169	0.005	0.950
Right parties 1946 (%)	3,227	0.027	0.061	0.000	0.788
Republican 1946 (%)	5,217	0.031	0.065	0.000	0.683
Socialist and Communist 1948 (%)	5,384	0.267	0.191	0.000	0.809
Catholic 1948 (%)	5,384	0.540	0.172	0.021	0.974
Right parties 1948 (%)	5,199	0.033	0.062	0.000	0.732
Republican 1948 (%)	5,384	0.015	0.039	0.000	0.510
Socialist and Communist 1919 (%)	5,698	0.305	0.255	0	1
Catholic 1919 (%)	5,698	0.270	0.213	0	1
Socialist and Communist 1921 (%)	5,698	0.270	0.216	0	1
Catholic 1921 (%)	5,698	0.277	0.208	0	1
Socialist and Communist 1924 (%)	5,698	0.150	0.143	0	1
Catholic 1924 (%)	5,698	0.142	0.156	0	1
Years of occupation	5,698	1.514	0.663	0.173	1.984
Presence of partisan brigades	5,698	0.360	0.480	0	1
Presence of left wing partisan brigades	5,698	0.269	0.444	0	1
Presence of partisan brigades other than left wing	5,698	0.091	0.288	0	1
Birthplace of a partisan	5,698	0.158	0.365	0	1
Birthplace of a left wing partisan	5,698	0.029	0.167	0	1
At least one episode of violence (Jan. 1943-Aug. 1945)	5,698	0.286	0.452	0	1
At least one episode of violence (Nov. 1944-Aug. 1945)	5,698	0.122	0.328	0	1
At least one episode of violence (Jan. 1943-Oct. 1944)	5,698	0.214	0.410	0	1
Number of deported people arrested in the municipality	5,698	0.990	12.472	0	560
Municipality within 15 Km of violent Nazi divisions	5,698	0.183	0.387	0	1
Maximum elevation of the municipality	5,698	789.4	796.1	2.0	4,554
Elevation of the city hall	5,698	316.9	290.3	0	2035
Total population 1921	5,490	4,796	21,951	84	775,203
Total population 1951	5,433	7,052	37,862	74	1,651,753
Population density 1921 (ab./Kmq)	5,698	177.5	445.0	1.236	22,977
Population density 1951 (ab./Kmq)	5,698	247.2	537.2	3.530	21,647
Share of illiterates 1921	5,698	0.236	0.201	0	0.857
Share of illiterates 1951	5,698	0.090	0.086	0	0.457
Plants 1927/population 1921	5,441	0.043	0.020	0	0.336
Industrial workers 1927/population 1921	5,441	0.123	0.132	0	2.2
Plants 1951/population 1951	5,401	0.035	0.065	0	4.7
Agricultural workers 1929/population 1921	2,477	0.368	0.197	0	2.3
Number livestock 1929/population 1921	4,881	1.163	1.524	0	17.2
Agricultural firms 1929/population 1921	2,477	0.165	0.089	0	1
Agricultural area/total area 1929	4,948	0.923	0.082	0.013	1

Note: See Section 3.2 for variables' description, and Appendix 3.B for their sources and construction.

Table 3.C.2: Summary Statistics Within 50 Km of the Gothic Line

Variable	Obs	Mean	Sd	Min	Max
Communist 1946 (%)	275	0.367	0.135	0	0.699
Socialist and Communist 1946 (%)	275	0.635	0.151	0.150	0.911
Catholic 1946 (%)	275	0.259	0.121	0.064	0.667
Right parties 1946 (%)	93	0.011	0.007	0.002	0.050
Republican 1946 (%)	275	0.047	0.073	0.001	0.373
Socialist and Communist 1948 (%)	275	0.513	0.150	0.078	0.809
Catholic 1948 (%)	275	0.358	0.133	0.096	0.764
Right parties 1948 (%)	224	0.012	0.009	0.002	0.050
Republican 1948 (%)	275	0.035	0.060	0.001	0.335
Socialist and Communist 1919 (%)	275	0.523	0.226	0	1
Catholic 1919 (%)	275	0.230	0.149	0	1
Socialist and Communist 1921 (%)	275	0.382	0.195	0	1
Catholic 1921 (%)	275	0.245	0.194	0	1
Socialist and Communist 1924 (%)	275	0.137	0.109	0	1
Catholic 1924 (%)	275	0.083	0.092	0	1
Years of occupation	275	1.696	0.294	1.189	1.967
Presence of partisan brigades	275	0.473	0.500	0	1
Presence of left wing partisan brigades	275	0.400	0.491	0	1
Presence of partisan brigades other than left wing	275	0.073	0.260	0	1
Birthplace of a partisan	275	0.531	0.500	0	1
Birthplace of a left wing partisan	275	0.113	0.317	0	1
At least one episode of violence (Jan. 1943-Aug. 1945)	275	0.749	0.434	0	1
At least one episode of violence (Nov. 1944-Aug. 1945)	275	0.295	0.457	0	1
At least one episode of violence (Jan. 1943-Oct. 1944)	275	0.651	0.478	0	1
Number of deported people arrested in the municipality	275	2.531	14.343	0	180
Municipality within 15 Km of violent Nazi divisions	275	0.640	0.481	0	1
Maximum elevation of the municipality	275	619.1	602.2	2.0	2,165
Elevation of the city hall	275	203.0	270.2	0	1,388
Total population 1921	254	10,451	18,704	1,417	202,185
Total population 1951	266	13,924	28,177	823	340,526
Population density 1921 (ab./Kmq)	275	199.1	281.1	26.809	2,767
Population density 1951 (ab./Kmq)	275	243.9	372.4	26.328	4,221
Share of illiterates 1921	275	0.263	0.107	0	0.609
Share of illiterates 1951	275	0.098	0.040	0	0.236
Plants 1927/population 1921	254	0.045	0.016	0	0.147
Industrial workers 1927/population 1921	254	0.117	0.074	0	0.4
Plants 1951/population 1951	266	0.035	0.010	0	0.1
Agricultural workers 1929/population 1921	234	0.359	0.147	0	0.8
Number livestock 1929/population 1921	251	0.830	0.453	0	2.6
Agricultural firms 1929/population 1921	234	0.121	0.054	0	0
Agricultural area/total area 1929	261	0.923	0.072	0.245	1

Note: See Section 3.2 for variables' description, and Appendix 3.B for their sources and construction.

Table 3.C.3: OLS Estimates – Interactive Effects

	Dependent variable: Communist 1946	
	(1)	(2)
Years of occupation		0.046
		(0.018)**
		(0.028)
At least one violence episode	0.011	0.005
	(0.003)***	(0.003)
	(0.004)**	(0.004)
Within 15 km of violent Nazi division	0.026	0.021
	(0.005)***	(0.006)***
	(0.012)**	(0.014)
Birthplace of a partisan	0.024	0.016
	(0.004)***	(0.004)***
	(0.004)***	(0.004)***
Birthplace of a left wing partisan	0.031	0.034
	(0.010)***	(0.010)***
	(0.009)***	(0.009)***
Presence of left wing partisan brigades	-0.002	0
	(0.003)	(0.003)
	(0.007)	(0.006)
Presence of other brigades than left wing	-0.011	-0.007
	(0.004)***	(0.004)*
	(0.008)	(0.007)
Occupation ended between 05/11/1943 and 30/07/1944	0.031	
	(0.010)***	
	(0.017)*	
Occupation ended between 30/07/1944 and 08/04/1945	0.072	
	(0.018)***	
	(0.041)*	
Occupation ended after 08/04/1945	0.114	
	(0.023)***	
	(0.052)**	
Number of observations	5559	4639
R-squared	0.586	0.655
Controls	Yes	Yes
Fixed effect	Region	Province
Sample	Complete	Above Gustav

Note: Robust standard errors are displayed in parentheses in each second row; standard errors corrected for spatial correlation are displayed in parentheses in each third row. Significance level: ***<0.01, **<0.05, *<0.1. *Communist 1946*: Vote share of the Italian Communist Party (PCI) in the 1946 election. *Years of occupation*: years of occupation measured at province level (see Appendix for exceptions) *At least one violence episode*: Dummy equal to 1 if records report at least one episode of violence in the period considered. *Within 15 Km of violent Nazi divisions*: Dummy equal to 1 if the minimum distance of the municipality from one occupied by either RFSS or HG Division is less than 15 Km (using city hall as reference point). *Birthplace of a partisan*: Dummy equal to 1 if a partisan (or a left-wing partisan) is born in the municipality *Presence of partisan brigades*: Dummy equal to 1 if the area of the municipality intersects the area of operation of the partisan brigade (left wing or other). Other regressors include: Share of illiterate 1921 and 1951, population density 1921 and 1951, latitude, longitude, maximum altitude in the municipality, elevation city hall, vote shares of Communist-Socialist and Catholic in 1919, 1921, and 1924 and Province or Region Fixed Effects. Above Gustav sample: Abruzzi e Molise, Campania, Emilia-Romagna, Lazio, Liguria, Lombardia, Marche, Piemonte, Toscana, Umbria, Veneto, Venezia Giulia, Venezia Tridentina

Table 3.C.4: RDD Balance Tests – City Characteristics

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Share of illiterate 1921	-0.020	-0.023	0.046	-0.022	-0.001
	(0.024)	(0.021)	(0.036)	(0.025)	(0.014)
	275	742	275	742	1516
Share of illiterate 1951	-0.013	-0.008	0.008	-0.015	0.002
	(0.009)	(0.008)	(0.012)	(0.009)*	(0.006)
	275	742	275	742	894
Total population 1921	-1229	120	462	-801	-1608
	(4805)	(4079)	(3792)	(4441)	(2915)
	254	702	254	702	799
Total population 1951	-775	1195	478	2403	1213
	(7216)	(6193)	(5084)	(7074)	(4962)
	266	729	266	729	519
Population density 1921	88.065	23.388	-15.156	106	41.175
	(38.461)**	(29.826)	(53.134)	(42.435)**	(28.822)
	275	742	275	742	237
Population density 1951	113	27.461	-66.667	147	47.163
	(57.167)**	(46.154)	(71.475)	(62.763)**	(41.973)
	275	742	275	742	261
Female population 1921	-578	79.868	303	-385	-844
	(2450)	(2079)	(1901)	(2261)	(1499)
	254	702	254	702	790
Female population 1951	-334	695	278	1345	807
	(3806)	(3273)	(2620)	(3735)	(2646)
	266	729	266	729	494
Plants 1927/population 1921	0.004	0.003	0.002	0.002	0
	(0.004)	(0.004)	(0.006)	(0.004)	(0.003)
	254	700	254	700	526
Industrial workers 1927/population 1921	-0.004	-0.006	-0.032	-0.015	-0.015
	(0.021)	(0.020)	(0.023)	(0.020)	(0.015)
	254	700	254	700	642
Plants 1951/population 1951	0.004	0.003	0	0.003	0.001
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
	266	724	266	724	399
Agricultural workers 1929/population 1921	0.023	0.034	0.019	0.026	0.030
	(0.036)	(0.033)	(0.045)	(0.037)	(0.025)
	234	511	234	511	461
Number livestock 1929/population 1921	-0.121	-0.028	-0.045	-0.199	-0.127
	(0.091)	(0.077)	(0.129)	(0.101)**	(0.075)*
	251	676	251	676	347
Agricultural firms 1929/population 1921	-0.025	-0.032	-0.010	-0.013	-0.012
	(0.010)**	(0.008)***	(0.013)	(0.010)	(0.010)
	234	511	234	511	298
Agricultural area/total area 1929	-0.031	-0.016	-0.009	-0.030	-0.019
	(0.014)**	(0.007)**	(0.014)	(0.013)**	(0.011)*
	261	702	261	702	677
Maximum elevation	-245	-303	-130	-164	-168
	(129)*	(136)**	(165)	(131)	(126)
	275	742	275	742	380
Elevation of the city hall	-68.073	-82.597	-50.668	-1.622	-12.887
	(75.463)	(84.858)	(84.316)	(74.705)	(61.454)
	275	742	275	742	454

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. See Appendix 3.B for data sources and description.

Table 3.C.5: RDD Balance Tests – Prewar Political Variables

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Socialist 1919	0.101	0.161	0.025	0.048	0.043
	(0.039)***	(0.035)***	(0.052)	(0.040)	(0.037)
	275	742	275	742	527
Catholic 1919	-0.021	-0.076	0.012	0.042	0.023
	(0.033)	(0.028)***	(0.048)	(0.035)	(0.031)
	275	742	275	742	517
Republican 1919	-0.039	-0.054	0.007	-0.047	-0.007
	(0.026)	(0.024)**	(0.030)	(0.026)*	(0.024)
	248	683	248	683	225
Communist and Socialist 1921	0.090	0.122	0.030	0.104	0.061
	(0.039)**	(0.036)***	(0.046)	(0.040)***	(0.032)*
	275	742	275	742	509
Catholic 1921	-0.084	-0.102	-0.037	-0.040	-0.020
	(0.041)**	(0.041)**	(0.044)	(0.041)	(0.028)
	275	742	275	742	854
Republican 1921	-0.015	-0.030	0.028	-0.024	0.011
	(0.021)	(0.020)	(0.025)	(0.022)	(0.019)
	254	702	254	702	234
Communist and Socialist 1924	-0.003	0	0.016	0.020	0.001
	(0.022)	(0.017)	(0.027)	(0.022)	(0.017)
	275	742	275	742	695
Catholic 1924	0.002	-0.005	-0.020	0.026	0.013
	(0.018)	(0.015)	(0.026)	(0.019)	(0.013)
	275	742	275	742	567
Republican 1924	-0.008	-0.012	0.014	-0.011	0.006
	(0.012)	(0.010)	(0.016)	(0.012)	(0.013)
	258	704	258	704	238

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. Parties in the pre-fascist period have been lumped using as reference Leoni (2001). See Appendix 3.B for more details on these aggregations.

Table 3.C.6: RDD Nearest Neighbor Matching – Prewar Political Variables

	Nearest neighbor matching		
	Latitude and longitude		
	25 Km	50 Km	100 Km
Socialist and Communist 1919	0.054	0.066	0.066
	(0.033)	(0.035)*	(0.035)*
	40	46	46
Socialist and Communist 1921	0.010	0.015	0.015
	(0.027)	(0.027)	(0.027)
	40	46	46
Socialist and Communist 1924	0.013	0.007	0.007
	(0.026)	(0.026)	(0.026)
	40	46	46
Catholic 1919	0.003	-0.009	-0.009
	(0.039)	(0.038)	(0.038)
	40	46	46
Catholic 1921	-0.011	-0.021	-0.021
	(0.034)	(0.032)	(0.032)
	40	46	46
Catholic 1924	0.010	0.006	0.006
	(0.025)	(0.023)	(0.023)
	40	46	46
Republican 1919	-0.036	-0.041	-0.041
	(0.020)*	(0.020)**	(0.020)**
	40	46	46
Republican 1921	-0.005	-0.013	-0.013
	(0.016)	(0.017)	(0.017)
	40	46	46
Republican 1924	-0.007	-0.009	-0.009
	(0.011)	(0.011)	(0.011)
	40	46	46

Note: Coefficients presented display the difference among mean above the line minus mean below the line for the municipalities within Bologna and Ravenna provinces (forcing the match within province) that are in the common support with respect *Communist and Socialist* 1921 vote share. Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Nearest-neighborhood matching based on latitude and longitude. Parties in the pre-fascist period have been lumped using as reference Leoni (2001). See Appendix 3.B for more details on these aggregations. Metric used: Euclidean distance with replacement.

Table 3.C.7: RDD Causal Effects – Electoral Outcomes (Unconditional)

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Unconditional Estimates					
Communist 1946	0.113	0.120	0.120	0.102	0.100
	(0.029)***	(0.029)***	(0.039)***	(0.030)***	(0.029)***
	275	742	275	742	360
Communist and Socialist 1946	0.111	0.116	0.100	0.097	0.093
	(0.028)***	(0.028)***	(0.038)***	(0.029)***	(0.030)***
	275	742	275	742	342
Communist and Socialist 1948	0.113	0.124	0.118	0.097	0.101
	(0.032)***	(0.032)***	(0.042)***	(0.033)***	(0.032)***
	275	742	275	742	330
Catholic 1946	-0.049	-0.059	-0.060	-0.037	-0.024
	(0.021)**	(0.017)***	(0.032)*	(0.022)*	(0.019)
	275	742	275	742	663
Catholic 1948	-0.079	-0.098	-0.091	-0.062	-0.055
	(0.027)***	(0.026)***	(0.038)**	(0.028)**	(0.025)**
	275	742	275	742	446
Right Wing 1946	0.016	0.016	0.019	0.016	0.005
	(0.016)	(0.016)	(0.016)	(0.017)	(0.005)
	93	262	93	262	315
Right Wing 1948	-0.003	-0.003	-0.002	-0.004	-0.004
	(0.002)*	(0.002)	(0.003)	(0.002)*	(0.002)*
	224	599	224	599	314
Republican 1946	-0.038	-0.034	-0.025	-0.038	-0.029
	(0.017)**	(0.015)**	(0.020)	(0.017)**	(0.012)**
	275	742	275	742	542
Republican 1948	-0.033	-0.030	-0.017	-0.033	-0.024
	(0.015)**	(0.013)**	(0.016)	(0.014)**	(0.009)**
	275	742	275	742	596

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy; *Republican* corresponds to Republican Party.

Table 3.C.8: RDD Robustness – Nearest Neighbor Matching (Unconditional)

	Nearest Neighbor Matching					
	Latitude and Longitude			Lat. Long., Pre War Elections		
	25 Km	50 Km	100 Km	25 Km	50 Km	100 Km
Unconditional Estimates						
Communist 1946	0.065	0.074	0.074	0.057	0.065	0.065
	(0.029)**	(0.027)***	(0.027)***	(0.030)*	(0.028)**	(0.028)**
	40	46	46	40	46	46
Communist and Socialist 1946	0.047	0.056	0.056	0.064	0.072	0.072
	(0.026)*	(0.024)**	(0.024)**	(0.025)**	(0.023)***	(0.023)***
	40	46	46	40	46	46
Communist and Socialist 1948	0.059	0.063	0.063	0.066	0.067	0.067
	(0.027)**	(0.025)**	(0.025)**	(0.025)***	(0.023)***	(0.023)***
	40	46	46	40	46	46
Catholic 1946	-0.004	-0.009	-0.009	-0.022	-0.027	-0.027
	(0.019)	(0.018)	(0.018)	(0.021)	(0.019)	(0.019)
	40	46	46	40	46	46
Catholic 1948	-0.035	-0.040	-0.040	-0.053	-0.056	-0.056
	(0.023)	(0.023)*	(0.023)*	(0.025)**	(0.023)**	(0.023)**
	40	46	46	40	46	46
Right Wing 1948	-0.003	-0.003	-0.003	-0.003	-0.004	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)*	(0.002)**	(0.002)**
	40	46	46	40	46	46
Republican 1946	-0.027	-0.029	-0.029	-0.024	-0.025	-0.025
	(0.009)***	(0.009)***	(0.009)***	(0.014)*	(0.012)**	(0.012)**
	40	46	46	40	46	46
Republican 1948	-0.019	-0.021	-0.021	-0.017	-0.018	-0.018
	(0.008)**	(0.008)**	(0.008)**	(0.013)	(0.012)	(0.012)
	40	46	46	40	46	46

Note: Coefficients presented display the difference among mean above the line minus mean below the line for the municipalities within Bologna and Ravenna provinces (forcing the match within province) that are in the common support with respect *Communist and Socialist* 1921 vote share. Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Nearest-neighborhood matching based on latitude and longitude (left) or on latitude, longitude, and prewar election outcomes (right). Metric used: Euclidean distance with replacement. In the conditional estimates, the dependent variable is the residual of the vote share on prewar election outcomes. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy; *Republican* corresponds to Republican Party.

Table 3.C.9: RDD Robustness – Controlling for Latitude and Longitude

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A: Unconditional Estimates					
Communist 1946	0.089	0.079	0.102	0.091	0.100
	(0.026)***	(0.026)***	(0.037)***	(0.028)***	(0.030)***
	275	742	275	742	332
Communist and Socialist 1946	0.088	0.076	0.064	0.087	0.093
	(0.025)***	(0.024)***	(0.036)*	(0.027)***	(0.031)***
	275	742	275	742	316
Communist and Socialist 1948	0.083	0.075	0.089	0.086	0.103
	(0.029)***	(0.028)***	(0.040)**	(0.031)***	(0.034)***
	275	742	275	742	299
Catholic 1946	-0.023	-0.026	-0.039	-0.029	-0.026
	(0.018)	(0.016)*	(0.028)	(0.020)	(0.021)
	275	742	275	742	495
Catholic 1948	-0.044	-0.051	-0.066	-0.052	-0.067
	(0.022)**	(0.020)**	(0.033)**	(0.024)**	(0.028)**
	275	742	275	742	310
Right parties 1946	0.017	0.014	0.019	0.016	0.004
	(0.015)	(0.017)	(0.016)	(0.017)	(0.003)
	93	262	93	262	857
Right parties 1948	-0.003	-0.002	-0.002	-0.004	-0.004
	(0.002)*	(0.002)	(0.003)	(0.002)**	(0.002)**
	224	599	224	599	517
Republican 1946	-0.045	-0.035	-0.013	-0.036	-0.029
	(0.016)***	(0.016)**	(0.018)	(0.016)**	(0.012)**
	275	742	275	742	561
Republican 1948	-0.038	-0.032	-0.009	-0.032	-0.024
	(0.014)***	(0.014)**	(0.015)	(0.014)**	(0.010)**
	275	742	275	742	591
Panel B: Estimates Conditional on Pre-war Elections					
Communist 1946	0.079	0.051	0.109	0.076	0.088
	(0.023)***	(0.023)**	(0.030)***	(0.024)***	(0.023)***
	275	742	275	742	307
Communist and Socialist 1946	0.074	0.039	0.068	0.067	0.073
	(0.020)***	(0.019)**	(0.028)**	(0.021)***	(0.021)***
	275	742	275	742	289
Communist and Socialist 1948	0.067	0.036	0.096	0.066	0.082
	(0.023)***	(0.022)*	(0.032)***	(0.024)***	(0.022)***
	275	742	275	742	254
Catholic 1946	-0.019	0.003	-0.045	-0.021	-0.015
	(0.016)	(0.014)	(0.023)*	(0.017)	(0.016)
	275	742	275	742	453
Catholic 1948	-0.039	-0.020	-0.074	-0.041	-0.050
	(0.019)**	(0.017)	(0.028)***	(0.020)**	(0.019)***
	275	742	275	742	289
Right parties 1946	0.012	0.013	0.013	0.016	0.001
	(0.014)	(0.016)	(0.014)	(0.017)	(0.003)
	93	262	93	262	926
Right parties 1948	-0.002	-0.002	-0.002	-0.003	-0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
	224	599	224	599	687
Republican 1946	-0.038	-0.032	-0.009	-0.030	-0.028
	(0.014)***	(0.015)**	(0.016)	(0.015)**	(0.012)**
	275	742	275	742	563
Republican 1948	-0.031	-0.029	-0.005	-0.028	-0.024
	(0.011)***	(0.013)**	(0.013)	(0.013)**	(0.009)**
	275	742	275	742	593

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Local RDD uses residuals from OLS regressions on geographical variables (Panel A) and also on prewar electoral results (Panel B) as dependent variables. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy. Estimates in Panel B are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*. Other regressors include: Latitude, longitude, latitude squared, longitude squared, latitude*longitude, latitude*longitude squared and Province 21 Fixed Effects.

Table 3.C.10: RDD Robustness – 25 Km-Width FE

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A: Unconditional Estimates					
Communist 1946	0.131	0.181	0.111	0.124	0.142
	(0.028)***	(0.024)***	(0.040)***	(0.038)***	(0.028)***
	275	742	275	742	670
Communist and Socialist 1946	0.132	0.234	0.056	0.129	0.108
	(0.028)***	(0.026)***	(0.038)	(0.039)***	(0.045)**
	275	742	275	742	290
Communist and Socialist 1948	0.125	0.214	0.090	0.121	0.126
	(0.031)***	(0.027)***	(0.043)**	(0.041)***	(0.048)***
	275	742	275	742	310
Catholic 1946	-0.051	-0.138	-0.034	-0.060	-0.077
	(0.023)**	(0.022)***	(0.031)	(0.033)*	(0.027)***
	275	742	275	742	608
Catholic 1948	-0.083	-0.182	-0.077	-0.087	-0.094
	(0.026)***	(0.025)***	(0.035)**	(0.037)**	(0.041)**
	275	742	275	742	314
Right parties 1946	0.006	0.003	0.009	0.008	-0.010
	(0.011)	(0.009)	(0.011)	(0.011)	(0.003)***
	93	262	93	262	642
Right parties 1948	-0.007	-0.006	-0.002	-0.007	-0.008
	(0.002)***	(0.002)***	(0.003)	(0.003)**	(0.002)***
	224	599	224	599	572
Republican 1946	-0.037	-0.059	-0.007	-0.032	-0.018
	(0.014)***	(0.011)***	(0.019)	(0.017)*	(0.017)
	275	742	275	742	289
Republican 1948	-0.030	-0.047	-0.002	-0.026	-0.011
	(0.012)**	(0.009)***	(0.016)	(0.014)*	(0.015)
	275	742	275	742	279
Panel B: Estimates Conditional on Pre-war Elections					
Communist 1946	0.109	0.095	0.104	0.091	0.091
	(0.025)***	(0.021)***	(0.032)***	(0.030)***	(0.023)***
	275	742	275	742	298
Communist and Socialist 1946	0.102	0.122	0.056	0.091	0.064
	(0.025)***	(0.021)***	(0.031)*	(0.032)***	(0.025)**
	275	742	275	742	291
Communist and Socialist 1948	0.095	0.100	0.089	0.082	0.081
	(0.027)***	(0.022)***	(0.034)**	(0.031)***	(0.029)***
	275	742	275	742	344
Catholic 1946	-0.036	-0.042	-0.029	-0.034	-0.033
	(0.020)*	(0.017)**	(0.026)	(0.026)	(0.018)*
	275	742	275	742	663
Catholic 1948	-0.067	-0.080	-0.071	-0.059	-0.058
	(0.023)***	(0.019)***	(0.030)**	(0.028)**	(0.028)**
	275	742	275	742	311
Right parties 1946	0.004	0.003	0.007	0.008	0.009
	(0.010)	(0.010)	(0.010)	(0.011)	(0.006)
	93	262	93	262	46
Right parties 1948	-0.005	-0.004	-0.002	-0.005	-0.005
	(0.003)*	(0.002)*	(0.003)	(0.003)*	(0.002)***
	224	599	224	599	451
Republican 1946	-0.029	-0.055	-0.013	-0.027	-0.023
	(0.013)**	(0.010)***	(0.018)	(0.016)*	(0.013)*
	275	742	275	742	471
Republican 1948	-0.022	-0.044	-0.006	-0.022	-0.018
	(0.011)**	(0.009)***	(0.015)	(0.013)*	(0.012)
	275	742	275	742	412

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Local RDD uses residuals from OLS regressions on the 25 Km interval FE (Panel A) and also on prewar electoral results (Panel B) as dependent variables. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy. Estimates in Panel B are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*.

Table 3.C.11: RDD Robustness – Full Sample Analysis

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Unconditional Estimates					
Communist 1946	0.113	0.118	0.125	0.111	0.090
	(0.024)***	(0.026)***	(0.032)***	(0.026)***	(0.023)***
	329	829	329	829	464
Communist and Socialist 1946	0.118	0.132	0.101	0.116	0.076
	(0.025)***	(0.025)***	(0.036)***	(0.026)***	(0.023)***
	329	829	329	829	700
Communist and Socialist 1948	0.121	0.140	0.124	0.117	0.087
	(0.027)***	(0.028)***	(0.038)***	(0.029)***	(0.026)***
	329	829	329	829	542
Catholic 1946	-0.048	-0.072	-0.045	-0.046	-0.029
	(0.019)**	(0.016)***	(0.031)	(0.021)**	(0.018)
	329	829	329	829	904
Catholic 1948	-0.087	-0.113	-0.097	-0.079	-0.049
	(0.024)***	(0.023)***	(0.035)***	(0.025)***	(0.022)**
	329	829	329	829	675
Right parties 1946	-0.002	-0.003	0.003	-0.001	0.004
	(0.009)	(0.010)	(0.009)	(0.009)	(0.005)
	146	346	146	346	254
Right parties 1948	-0.003	-0.004	-0.001	-0.003	-0.003
	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)
	278	684	278	684	1552
Republican 1946	-0.039	-0.030	-0.024	-0.040	-0.028
	(0.015)**	(0.014)**	(0.019)	(0.016)***	(0.012)**
	329	829	329	829	670
Republican 1948	-0.031	-0.026	-0.014	-0.033	-0.023
	(0.013)**	(0.012)**	(0.013)	(0.013)***	(0.009)***
	329	829	329	829	703

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line on the entire sample of Italian municipalities (*i.e.* without dropping municipalities with missing prewar political variables). Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy.

Table 3.C.12: RDD Robustness – Non-Missing Prewar Elections

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A: Unconditional Estimates					
Communist 1946	0.149	0.162	0.143	0.147	0.148
	(0.052)***	(0.050)***	(0.075)*	(0.055)***	(0.057)***
	142	438	142	438	159
Communist and Socialist 1946	0.108	0.129	0.060	0.089	0.100
	(0.053)**	(0.049)***	(0.074)	(0.054)*	(0.051)**
	142	438	142	438	198
Communist and Socialist 1948	0.133	0.155	0.108	0.123	0.129
	(0.057)**	(0.053)***	(0.078)	(0.059)**	(0.060)**
	142	438	142	438	169
Catholic 1946	-0.068	-0.089	-0.055	-0.053	-0.065
	(0.030)**	(0.024)***	(0.052)	(0.034)	(0.039)*
	142	438	142	438	153
Catholic 1948	-0.104	-0.130	-0.093	-0.093	-0.099
	(0.039)***	(0.037)***	(0.060)	(0.043)**	(0.050)**
	142	438	142	438	137
Right parties 1946	0.039	0.037	0.039	0.042	0.001
	(0.003)***	(0.002)***	(0.004)***	(0.003)***	(0.006)
	39	118	39	118	451
Right parties 1948	-0.005	-0.003	-0.003	-0.005	-0.005
	(0.003)*	(0.003)	(0.003)	(0.003)**	(0.003)**
	112	333	112	333	152
Republican 1946	-0.014	-0.014	0.008	-0.012	-0.011
	(0.025)	(0.021)	(0.033)	(0.024)	(0.021)
	142	438	142	438	186
Republican 1948	-0.014	-0.014	0.009	-0.012	-0.009
	(0.019)	(0.016)	(0.028)	(0.019)	(0.018)
	142	438	142	438	174
Panel B: Estimates Conditional on Pre-war Elections					
Communist 1946	0.130	0.097	0.158	0.113	0.119
	(0.039)***	(0.042)**	(0.045)***	(0.042)***	(0.042)***
	142	438	142	438	130
Communist and Socialist 1946	0.074	0.047	0.075	0.048	0.051
	(0.045)	(0.041)	(0.047)	(0.041)	(0.032)
	142	438	142	438	187
Communist and Socialist 1948	0.097	0.065	0.125	0.077	0.083
	(0.049)**	(0.041)	(0.050)**	(0.041)*	(0.038)**
	142	438	142	438	129
Catholic 1946	-0.051	-0.019	-0.073	-0.026	-0.030
	(0.029)*	(0.025)	(0.033)**	(0.027)	(0.023)
	142	438	142	438	147
Catholic 1948	-0.084	-0.051	-0.111	-0.061	-0.063
	(0.037)**	(0.032)	(0.036)***	(0.032)*	(0.028)**
	142	438	142	438	112
Right parties 1946	0.037	0.033	0.039	0.041	0.005
	(0.003)***	(0.003)***	(0.005)***	(0.004)***	(0.008)
	39	118	39	118	236
Right parties 1948	-0.004	-0.002	-0.003	-0.005	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)*	(0.003)
	112	333	112	333	134
Republican 1946	-0.002	-0.009	0.009	-0.005	-0.009
	(0.023)	(0.021)	(0.030)	(0.023)	(0.021)
	142	438	142	438	179
Republican 1948	-0.003	-0.011	0.010	-0.007	-0.009
	(0.018)	(0.015)	(0.025)	(0.018)	(0.017)
	142	438	142	438	175

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line on the sample of Italian municipalities with all the three political variables prewar (1919, 1921, 1924) not missing. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948, and for comparison we compute also *Communist and Socialist* in 1946 as Italian Communist Party (PCI) + Italian Socialist Party (PSI); *Catholic* corresponds to the Christian Democrats (DC); *Right Wing* corresponds to Movimento Sociale Italiano (MSI) plus smaller parties supporting monarchy. Estimates in Panel B are conditional on the 1919, 1921, and 1924 vote shares of *Catholic* and *Communist and Socialist*.

Table 3.C.13: RDD Causal Effects by Presence of Partisan Brigades (Unconditional)

	Polynomial Regression							
	First order				Second order			
	50 Km		100 Km		50 Km		100 Km	
	up	up*left brig.	up	up*left brig.	up	up*left brig.	up	up*left brig.
Unconditional Estimates								
Communist 1946	0.108	0.008	0.132	-0.017	0.118	0.002	0.121	-0.024
	(0.037)***	(0.034)	(0.031)***	(0.021)	(0.045)***	(0.033)	(0.033)***	(0.021)
	275	275	742	742	275	275	742	742
Communist and Socialist 1946	0.148	-0.052	0.146	-0.046	0.142	-0.058	0.138	-0.057
	(0.035)***	(0.031)*	(0.030)***	(0.020)**	(0.043)***	(0.030)*	(0.031)***	(0.020)***
	275	275	742	742	275	275	742	742
Communist and Socialist 1948	0.128	-0.022	0.141	-0.024	0.138	-0.028	0.121	-0.032
	(0.041)***	(0.037)	(0.035)***	(0.023)	(0.048)***	(0.036)	(0.036)***	(0.023)
	275	275	742	742	275	275	742	742
Catholic 1946	-0.053	0.006	-0.073	0.021	-0.068	0.012	-0.055	0.025
	(0.027)*	(0.025)	(0.021)***	(0.017)	(0.036)*	(0.024)	(0.025)**	(0.017)
	275	275	742	742	275	275	742	742
Catholic 1948	-0.076	-0.004	-0.113	0.022	-0.093	0.005	-0.081	0.025
	(0.035)**	(0.030)	(0.029)***	(0.020)	(0.043)**	(0.029)	(0.031)***	(0.020)
	275	275	742	742	275	275	742	742
Right parties 1946	0.010	0.006	0.012	0.005	0.013	0.007	0.012	0.005
	(0.016)	(0.003)**	(0.016)	(0.002)***	(0.016)	(0.003)**	(0.016)	(0.002)***
	93	93	262	262	93	93	262	262
Right parties 1948	-0.005	0.003	-0.003	0.001	-0.004	0.003	-0.005	0.002
	(0.002)**	(0.002)	(0.002)*	(0.001)	(0.003)	(0.002)	(0.002)**	(0.001)*
	224	224	599	599	224	224	599	599
Republican 1946	-0.070	0.046	-0.046	0.021	-0.059	0.044	-0.056	0.025
	(0.024)***	(0.017)***	(0.018)***	(0.009)**	(0.025)**	(0.017)**	(0.020)***	(0.009)***
	275	275	742	742	275	275	742	742
Republican 1948	-0.056	0.034	-0.037	0.012	-0.041	0.031	-0.044	0.015
	(0.022)**	(0.015)**	(0.016)**	(0.007)*	(0.021)*	(0.015)**	(0.017)**	(0.007)**
	275	275	742	742	275	275	742	742

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line (column up) and the interaction between a dummy for the presence of a left-wing brigade and being North of the line (column up*left brig). Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *Communist* corresponds to the vote share of the Italian Communist Party (PCI); *Communist and Socialist* corresponds to the Popular Front (FP) in 1948.

Table 3.C.14: RDD Contextual Factors – Episodes of Violence

	Polynomial Regression				
	First order		Second order		Local RDD
	50 Km	100 Km	50 Km	100 Km	
Panel A. At least one violence episode against civilians					
Nov. 1944-Aug. 1945	0.163	0.113	0.099	0.150	0.075
	(0.125)	(0.108)	(0.167)	(0.127)	(0.067)
	275	742	275	742	961
Jan. 1943-Oct. 1944	-0.303	-0.286	-0.304	-0.288	-0.183
	(0.124)**	(0.103)***	(0.177)*	(0.125)**	(0.101)*
	275	742	275	742	478
Entire Period (Jan. 1943-Aug. 1945)	-0.175	-0.198	-0.239	-0.173	-0.109
	(0.107)	(0.092)**	(0.141)*	(0.109)	(0.097)
	275	742	275	742	408
Panel B. At least one violence episode against partisans					
Nov. 1944-Aug. 1945	0.082	0.083	0.076	0.052	0.053
	(0.100)	(0.083)	(0.127)	(0.099)	(0.052)
	275	742	275	742	1257
Jan. 1943-Oct. 1944	-0.016	0.036	-0.108	-0.038	-0.006
	(0.136)	(0.119)	(0.184)	(0.139)	(0.079)
	275	742	275	742	962
Entire Period (Jan. 1943-Aug. 1945)	0.013	0.116	-0.013	0.005	0.051
	(0.137)	(0.120)	(0.185)	(0.140)	(0.075)
	275	742	275	742	1206

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. *At least one violence episode against civilians*: Dummy equal to 1 if records report at least one episode of violence in which the majority of victims were civilians. *At least one violence episode against partisans*: Dummy equal to 1 if records report at least one episode of violence in which the majority of victims were partisans. January 1943–August 1945 is the entire period for which we have episodes recorded. January 1943–October 1944 (November 1944–August 1945) is the period before (after) the battlefront moved to the RDD Gothic line.

Table 3.C.15: Survey Data – Variables Description

Variable	Definition	Question	Answers
Male Years of age Years of residency	Dummy for male respondent Age of the respondent Duration of the residency in the municipality		
College level education	Dummy equal to 1 if highest educational attainment is at least college (answers 1 or 2)	What is the highest educational degree you obtained?	1 - PhD/Master – 2 - College – 3 - Senior High School – 4 - Junior High School – 5 - Primary School – 6 - Primary School not finished
Years of education Married, widow(er), separated or divorced One or more children House ownership	Number of years spent at school (inferred from the answer to the previous question) Dummy equal to 1 if the respondent is married, widow(er), separated or divorced Dummy equal to 1 if the respondent has at least one child Dummy for house ownership		
Left wing political preferences Center political preferences Right wing political preferences Independent political preferences	Dummy for left wing political preferences (answers 1 or 2) Dummy for center political preferences (answer 3) Dummy for right wing political preferences (answer 4 or 5) Dummy for independent political preferences (answer 6)	How would you define your political position with a single word?	1 - Left – 2 - Center-Left – 3 - Center – 4 - Center-Right – 5 - Right – 6 - Independent
Congruence with father's political preferences	Dummy for congruence with father's political preferences (answer 1)	How close were your vote to that of your father the first time you voted?	1 - Very close – 2 - Quite close – 3 - Not that close – 4 - Not close
One family member took part in the civil war One family member took part in the civil war as a partisan	Dummy for the presence of a family member who took part in the civil war (answer 1 or 2) Dummy for the presence of a family member who took part in the civil war as a partisan (answer 1)	Do you remember, or were you told whether any member of your family took part in the civil war in the period 1943-1945? If so, as a partisan or as a Mussolini's supporter?	1 - Yes, as a partisan – 2 - Yes, as Mussolini's supporter – 3 - No
One family member was victim of violence during WWII	Dummy for the presence of a family member who was victim of violence during WWII (answer 1)	Do you remember, or were you told whether a member of your family was a victim of violence or deprivations during WWII? If so, from whom?	1 - Yes (add description) – 2 - No
The municipality organized an event to commemorate the Resistance Participation to an event organized to commemorate the Resistance	Dummy for the organisation of commemorating events in the municipality (answers 1 or 2) Dummy for the participation to commemorating events in the municipality (answer 2)	Do you remember whether your municipality has ever organized an event to commemorate the Resistance and the Partisan war? If so, did you attend?	1 - Yes, but I did not attend – 2 - Yes, and I attended – 3 - No
Excessive German predominance	Dummy equal to 1 if the respondent agrees with excessive German predominance in Europe (answers 1 or 2)	How strongly do you agree with the statement "The Euro introduction has worsened the risk of an excessive German predominance in Europe"?	1 - Strongly Agree – 2 - Agree – 3 - Disagree – 4 - Strongly Disagree
The Euro was harmful for Italy	Dummy equal to 1 if the respondent believes that the introduction of Euro has been harmful to Italy (answers 1 or 2)	How strongly do you agree with the statement "The introduction of Euro in Italy has been positive for our country"?	1 - Strongly Agree – 2 - Agree – 3 - Disagree – 4 - Strongly Disagree
Wedding preference, Poland over Germany Wedding preference, UK over Germany Wedding preference, France over Germany Wedding preference, Germany ranked last	Dummy for Poland ranked over Germany Dummy for UK ranked over Germany Dummy for France ranked over Germany Dummy for Germany ranked last	I am going to present different nationalities. Would you tell, in order, for which ones of them you wouldn't be particularly happy in the event of the wedding of a relative with a person of that nationality.	1 - Poland – 2 - UK – 3 - Germany – 4 - France

Note: The first column indicates the name of the variable used in the analysis. The second includes a brief description and, when appropriate, columns 3 and 4 contain the relevant survey question with possible answers. The original questionnaire was administered in Italian, the content has been translated to the benefit of non-Italian speakers.

Table 3.C.16: Survey Data – Summary Statistics

Variable	Obs	Mean	Sd	Min	Max
Male	2,491	0.299	0.458	0	1
Years of age	2,467	66.136	11.245	41	95
Years of residency	2,443	52.449	17.613	20	95
College level education	2,119	0.088	0.283	0	1
Years of education	2,119	9.683	4.241	0	21
Married, widow(er), separated or divorced	2,112	0.911	0.286	0	1
One or more children	2,098	0.865	0.342	0	1
House ownership	2,029	0.934	0.248	0	1
Left wing political preferences	1,970	0.424	0.494	0	1
Center political preferences	1,970	0.072	0.258	0	1
Right wing political preferences	1,970	0.123	0.328	0	1
Independent political preferences	1,970	0.381	0.486	0	1
Congruence with father's political preferences	1,713	0.779	0.415	0	1
One family member took part in the civil war	2,270	0.320	0.467	0	1
One family member was victim of violence during WWII	2,252	0.226	0.419	0	1
One family member took part in the civil war as a partisan	2,252	0.191	0.393	0	1
The municipality organized an event to commemorate the Resistance	2,226	0.704	0.456	0	1
Participation to an event organized to commemorate the Resistance	2,226	0.330	0.470	0	1
Excessive German predominance	1,940	0.308	0.462	0	1
The Euro was harmful for Italy	2,279	0.259	0.438	0	1
Wedding preference, Poland over Germany	1,054	0.275	0.447	0	1
Wedding preference, UK over Germany	1,066	0.604	0.489	0	1
Wedding preference, France over Germany	1,064	0.647	0.478	0	1
Wedding preference, Germany ranked last	1,081	0.189	0.391	0	1

Note: See Appendix Table 3.C.15 for variables' description.

Table 3.C.17: Survey data – Balance Tests

	Polynomial Regression		
	First order	Second order	Local RDD
Panel A: Socio-Demographic Variables			
Male	0	0.009	0.087
	(0.045)	(0.057)	(0.067)
	2491	2491	641
Years of age	-0.101	0.091	-3.508
	(1.123)	(1.448)	(1.847)*
	2467	2467	563
College level education	0.045	0.037	0.036
	(0.031)	(0.042)	(0.052)
	2119	2119	632
Married, widow(er), separated or divorced	-0.026	-0.022	-0.064
	(0.032)	(0.043)	(0.047)
	2112	2112	694
One or more children	-0.046	-0.062	-0.063
	(0.038)	(0.049)	(0.049)
	2098	2098	900
House ownership	-0.015	-0.026	-0.040
	(0.029)	(0.040)	(0.051)
	2029	2029	643
Panel B: Political Preferences			
Left wing political preferences	0.044	-0.074	-0.021
	(0.054)	(0.069)	(0.064)
	1970	1970	1031
Center political preferences	-0.022	-0.007	-0.016
	(0.029)	(0.037)	(0.034)
	1970	1970	1123
Right wing political preferences	-0.008	0.007	0.008
	(0.033)	(0.043)	(0.041)
	1970	1970	1075
Independent political preferences	-0.014	0.075	0.030
	(0.053)	(0.069)	(0.064)
	1970	1970	1060

Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. See Appendix Table 3.C.15 for variables' description.

Table 3.C.18: Survey Data – Historical Memory, Civil War, and Germany

	Polynomial Regression		
	First order	Second order	Local RDD
Panel A: Historical memory and civil war			
Family member was victim of violence during WWII	-0.050	0.013	-0.031
	(0.049)	(0.062)	(0.064)
	2270	2270	758
Family member took part in the civil war	0.092	0.070	0.109
	(0.042)**	(0.053)	(0.054)**
	2252	2252	786
Family member took part in the civil war as a partisan	0.109	0.109	0.117
	(0.040)***	(0.052)**	(0.052)**
	2252	2252	883
The municipality organized an event to commemorate the Resistance	0.020	0.012	0.002
	(0.044)	(0.055)	(0.059)
	2226	2226	696
Participation to an event organized to commemorate the Resistance	0.064	0.013	0.030
	(0.048)	(0.063)	(0.061)
	2226	2226	1142
Panel B: Sentiment toward Germany			
Excessive German predominance	0.062	0.101	0.048
	(0.050)	(0.064)	(0.071)
	1940	1940	609
The Euro was harmful for Italy	0.037	0.120	0.102
	(0.045)	(0.058)**	(0.057)*
	2279	2279	1073
Wedding preference, Poland over Germany	0.047	0.175	0.172
	(0.065)	(0.087)**	(0.104)*
	1054	1054	238
Wedding preference, UK over Germany	0.065	0.093	0.001
	(0.075)	(0.100)	(0.120)
	1066	1066	325
Wedding preference, France over Germany	-0.087	-0.090	-0.103
	(0.072)	(0.097)	(0.118)
	1064	1064	270
Wedding preference, Germany ranked last	0.042	0.120	0.085
	(0.054)	(0.077)	(0.081)
	1081	1081	396

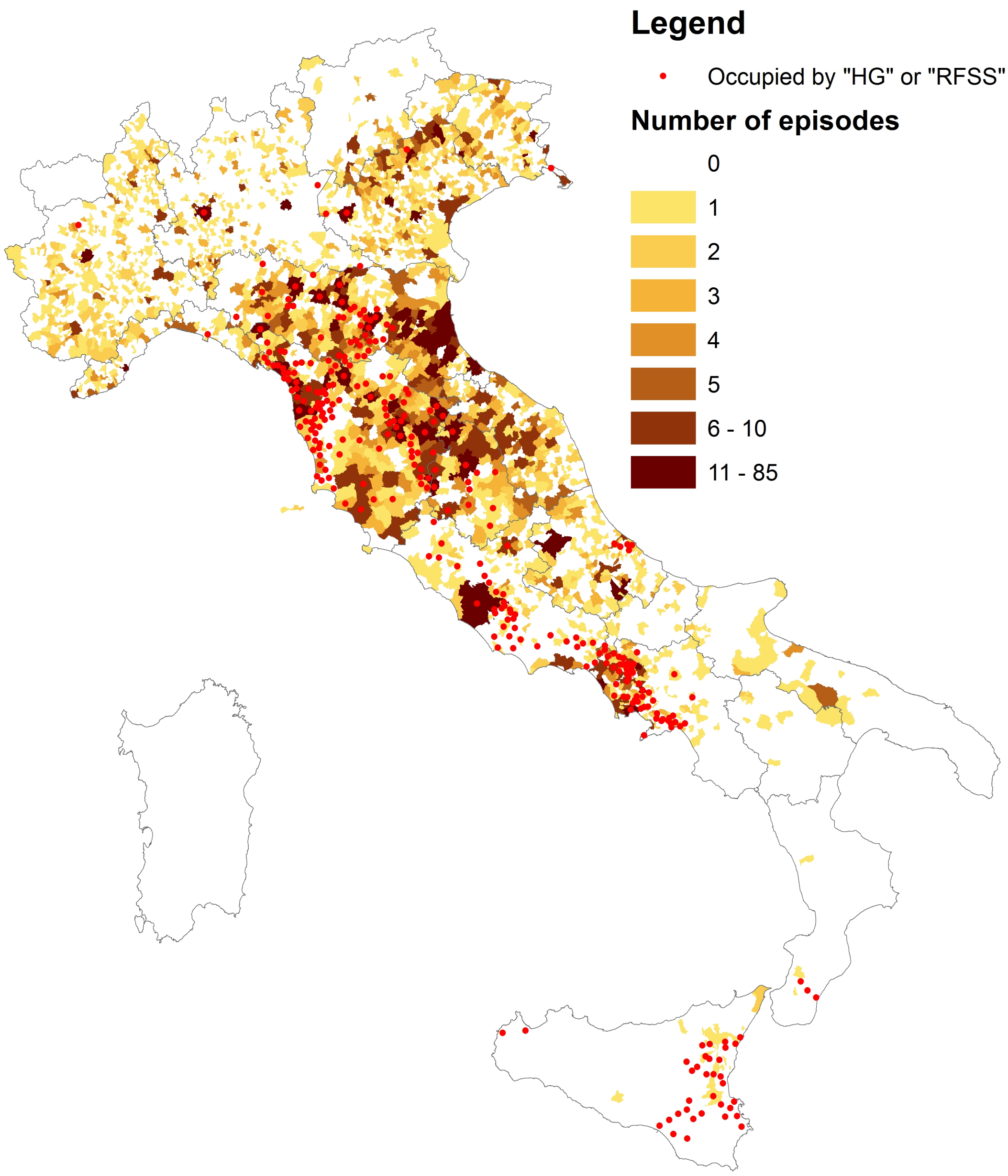
Note: RDD coefficients of being (just) above vs being (just) below the Gothic line. Robust standard errors are displayed in parentheses for polynomial regressions. Conventional standard errors are displayed in parentheses for local RDD. Significance level: ***<0.01, **<0.05, *<0.1. Number of observations reported in each third row. Regressions include Province Fixed Effects. See Appendix Table 3.C.15 for variables' description.

Table 3.C.19: Survey Data – Left-Wing Political Preferences

	left wing preferences (a)		left wing preferences (b)					
	(1)	(2)		(3)			(4)	
			Non-Left	Center-Left	Left	Non-Left	Center-Left	Left
Family member was victim of violence during WWII	0.146	0.162	-0.055	0.010	0.045	-0.064	0.012	0.052
	(0.071)**	(0.072)**	(0.026)**	(0.005)**	(0.021)**	(0.026)**	(0.005)**	(0.021)**
Family member took part in the civil war	0.168	0.199	-0.063	0.011	0.051	-0.073	0.013	0.059
	(0.079)**	(0.080)**	(0.029)**	(0.005)**	(0.023)**	(0.029)**	(0.006)**	(0.023)**
Congruence with father's political preferences	0.156	0.121	-0.074	0.013	0.061	-0.060	0.011	0.049
	(0.067)**	(0.069)*	(0.025)***	(0.005)***	(0.021)***	(0.025)**	(0.005)**	(0.021)**
The municipality organized an event	0.469	0.444	-0.170	0.030	0.139	-0.169	0.031	0.138
to commemorate the Resistance	(0.081)***	(0.083)***	(0.031)***	(0.007)***	(0.025)***	(0.032)***	(0.007)***	(0.026)***
Number of observations	1,481	1,481		1,481			1,481	
Wald	87.126	122.997		92.214			122.164	
Other covariates	NO	YES		NO			YES	

Note: Coefficients represent marginal effect at the mean value for Probit regressions in columns (1) and (2), for ordered Probit regressions in columns (3) and (4). Robust standard errors are displayed in parentheses. Significance level: ***<0.01, **<0.05, *<0.1. Dependent variables: (a) dummy variable equal to 1 if the individual declared Left or Center-Left political preferences; (b) Categorical variable equal to 2 if the individual declared Left political preferences, to 1 if Center-Left preferences, to 0 otherwise. Other covariates include: Age, sex, years of education, and dummies for house ownership, college education, children, vital record, and position with respect to the Gothic line. Regressions include Province Fixed Effects. See Appendix Table 3.C.15 for variables' description.

Figure 3.C.1: Violence Episodes and Municipalities Occupied by "HG-RFSS"



Note: Geographic distribution of violence episodes (by number/intensity) and of violent Nazi divisions (16th SS-Panzer-Grenadier-Division "Reichsfuhrer-SS" and "Hermann Goering"). See Appendix 3.B for historical sources.

Figure 3.C.2: Evolution of the Gothic Line

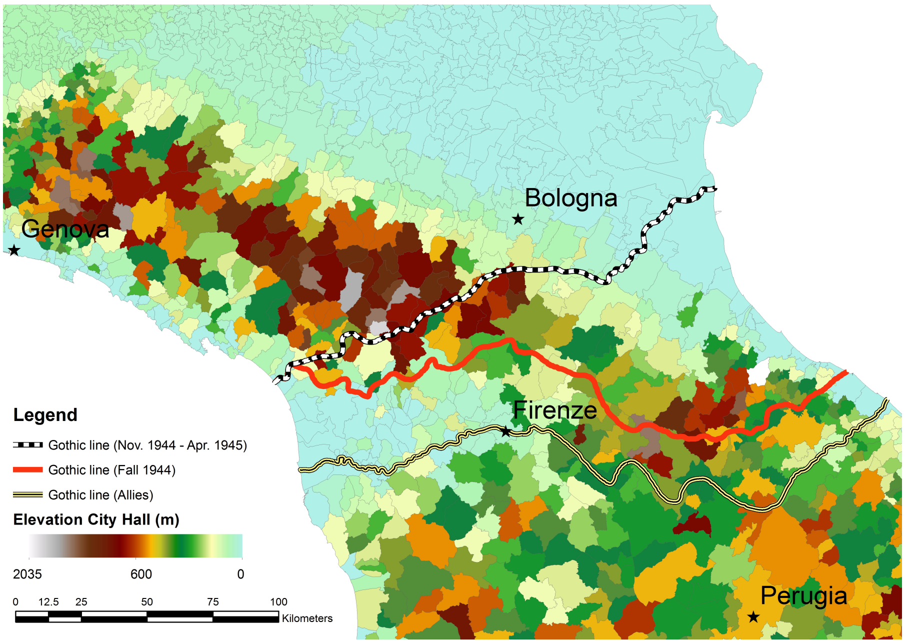
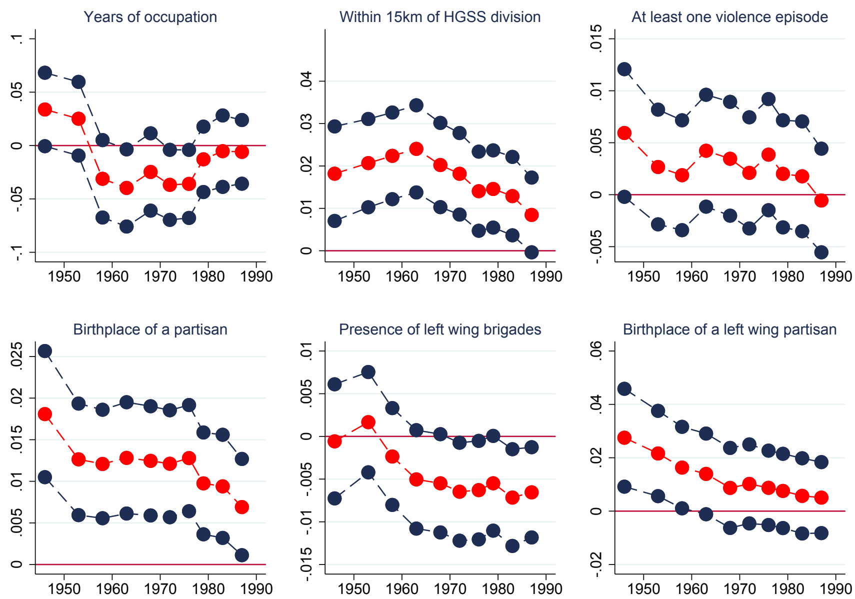
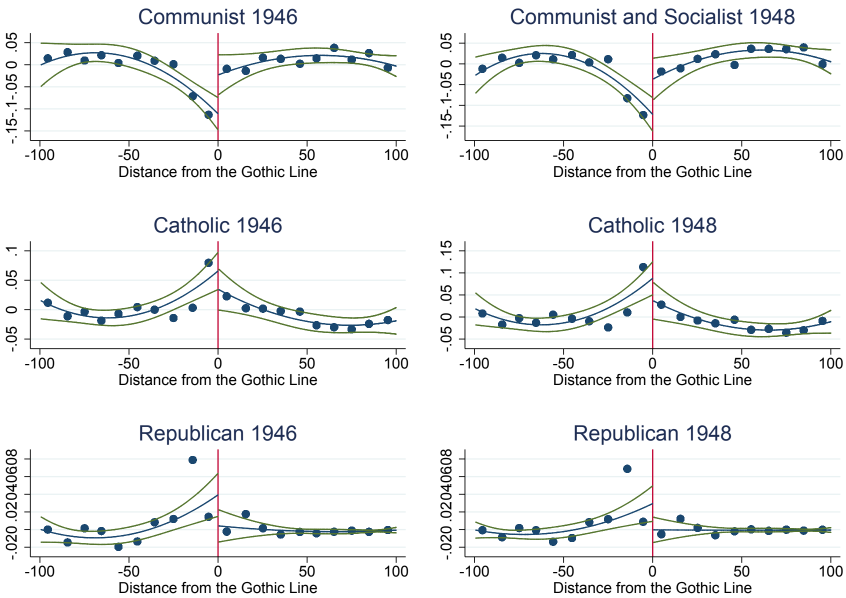


Figure 3.C.3: Long-Term Persistence – OLS



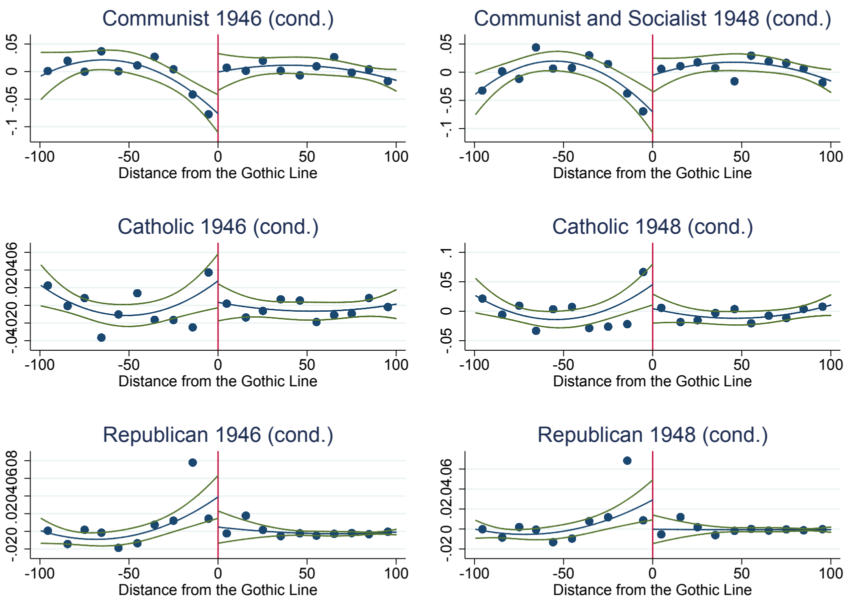
Note: Coefficients and 95% confidence intervals of the variable *Years of occupation*, the dummy *At least one violence episode*, the dummy *Within 15 Km of violent Nazi divisions*, the dummy *Birthplace of a partisan*, the dummy *Birthplace of a left wing partisan* and the dummy *Presence of left wing partisan brigades* estimated for all national elections from 1946 to 1987 in specifications as in column (6) of Table 3.6.1 with *Communist* vote share as dependent variable. The only difference is that we now control for the Census data closest in time to the election used as outcome instead of 1951. Data for the Communist Party are missing in 1948 as it ran with the Socialist Party.

Figure 3.C.4: RDD Discontinuities (Unconditional)



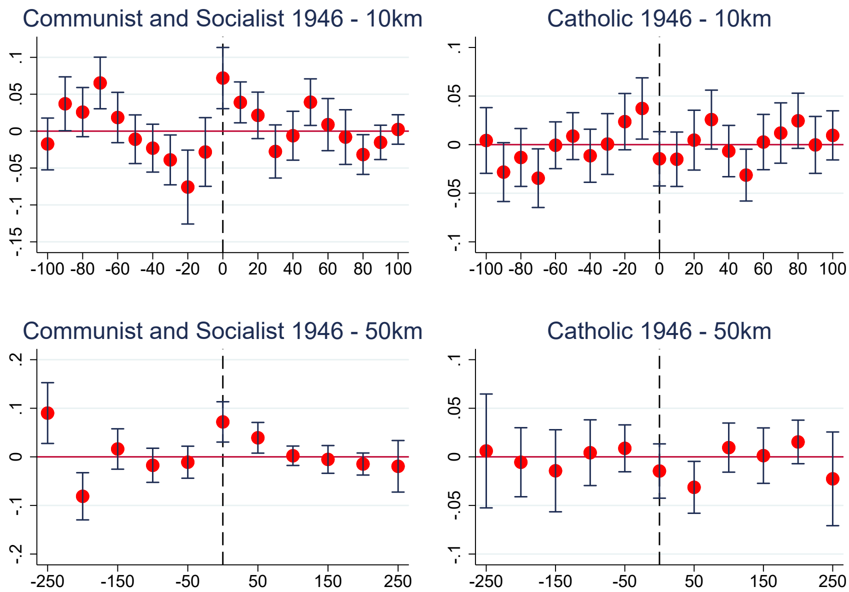
Note: (Unconditional) second order polynomial regressions at the 100 Km bandwidth shown in the fourth column of Table 3.6.2. Each dot corresponds to the average vote share for all municipalities within the corresponding 10 Km interval.

Figure 3.C.5: RDD Discontinuities (Conditional)



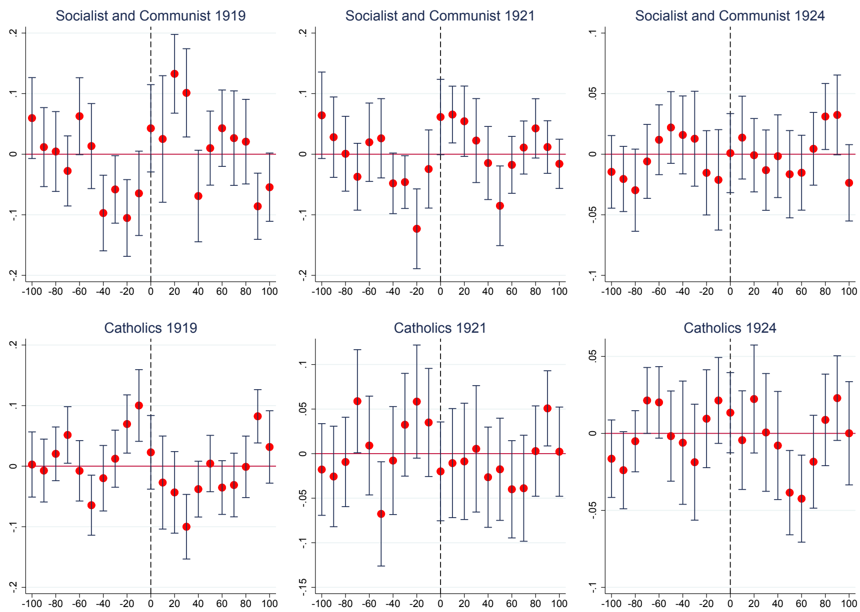
Note: (Conditional) second order polynomial regressions at the 100 Km bandwidth shown in the fourth column of Table 3.6.2. Each dot corresponds to the average vote share for all municipalities within the corresponding 10 Km interval.

Figure 3.C.6: Placebo Coefficients – Postwar Outcomes



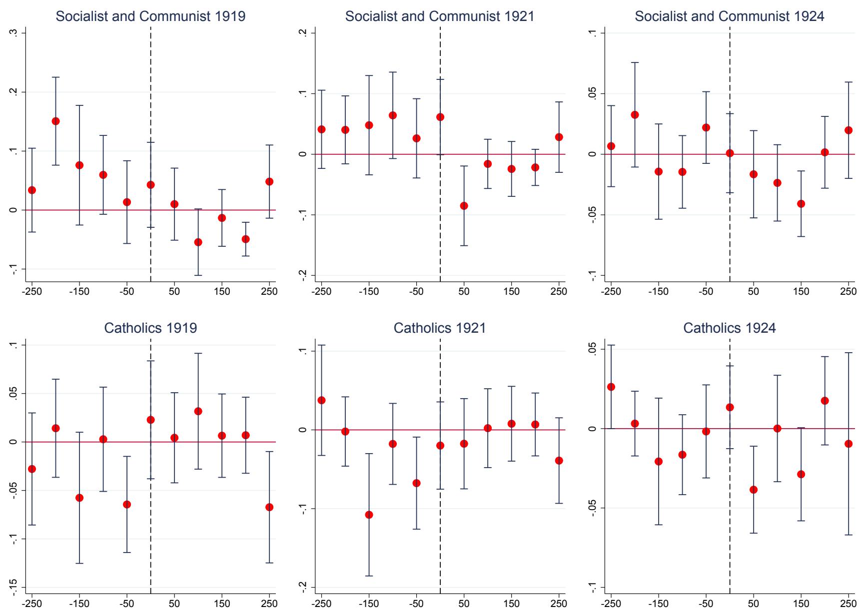
Note: Coefficients and 95% confidence intervals, estimated by local linear regression as in the last column of Table 3.6.2, including Province Fixed Effects, shifting the position of the Gothic line North or South of its true position by 10 Km at a time up to plus or minus 100 Km (first row), and by 50 Km at a time up to plus or minus 250 Km (second row).

Figure 3.C.7: Placebo Coefficients (10 Km) – Prewar Elections



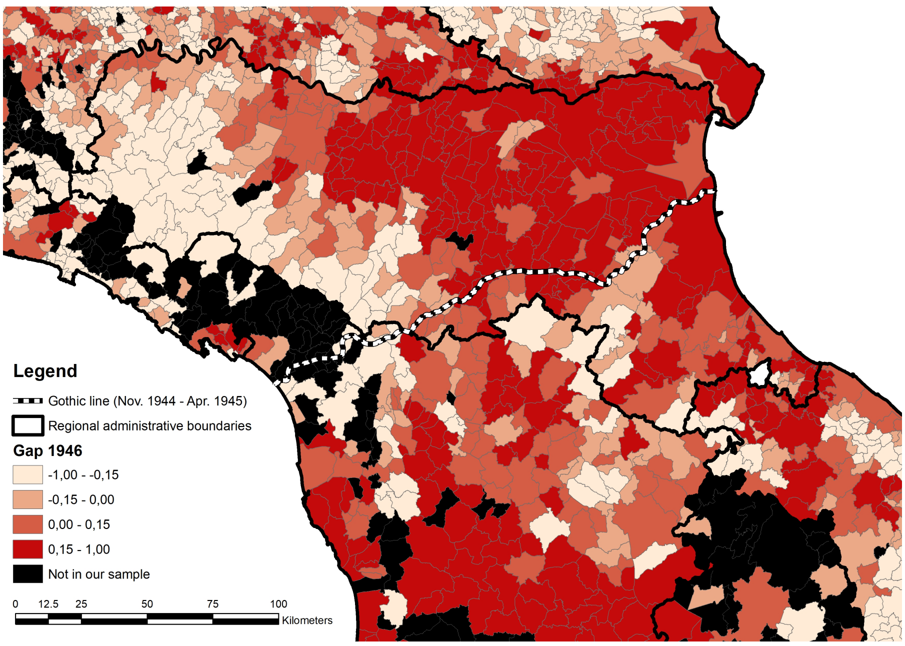
Note: Coefficients and 95% confidence intervals, estimated by local linear regression as in the last column of Table 3.C.5, including Province Fixed Effects, shifting the position of the Gothic line North or South of its true position by 10 Km at a time up to plus or minus 100 Km.

Figure 3.C.8: Placebo Coefficients (50 Km) – Prewar Elections



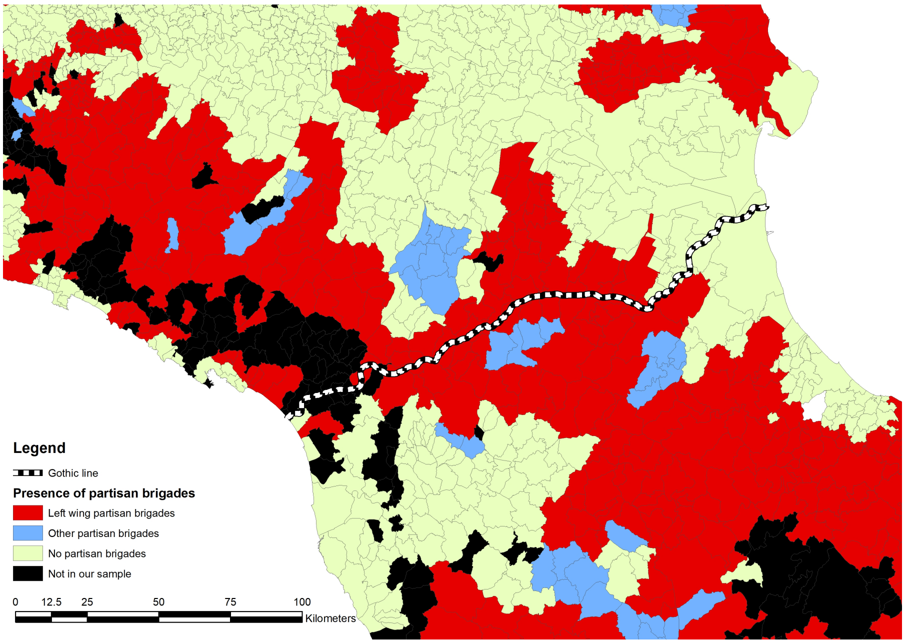
Note: Coefficients and 95% confidence intervals, estimated by local linear regression as in the last column of Table 3.C.5, including Province Fixed Effects, shifting the position of the Gothic line North or South of its true position by 50 Km at a time up to plus or minus 250 Km.

Figure 3.C.9: Communist minus Catholic in 1946



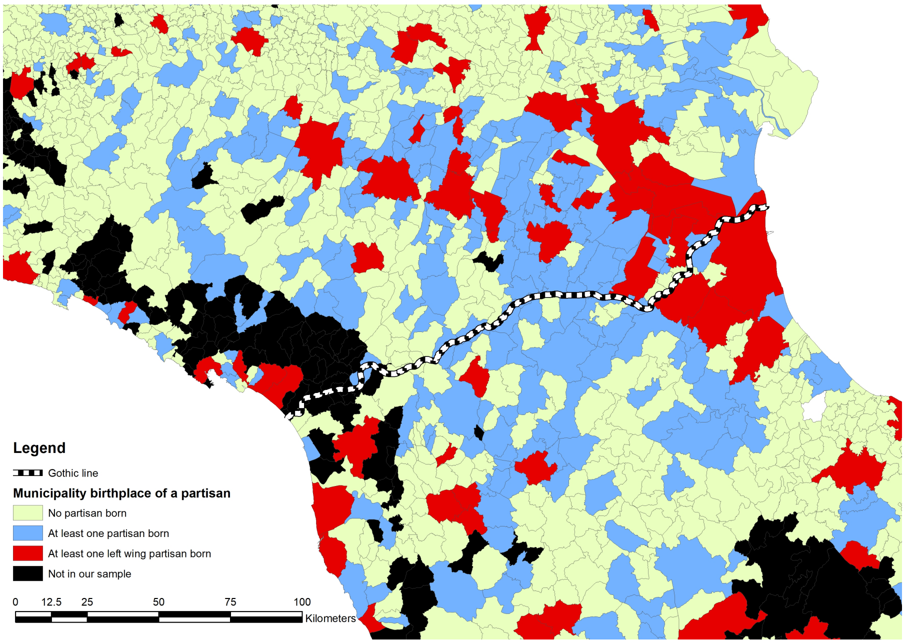
Note: Geographic distribution of the variable *Communist minus Catholic 1946*

Figure 3.C.10: Presence of Partisan Brigades



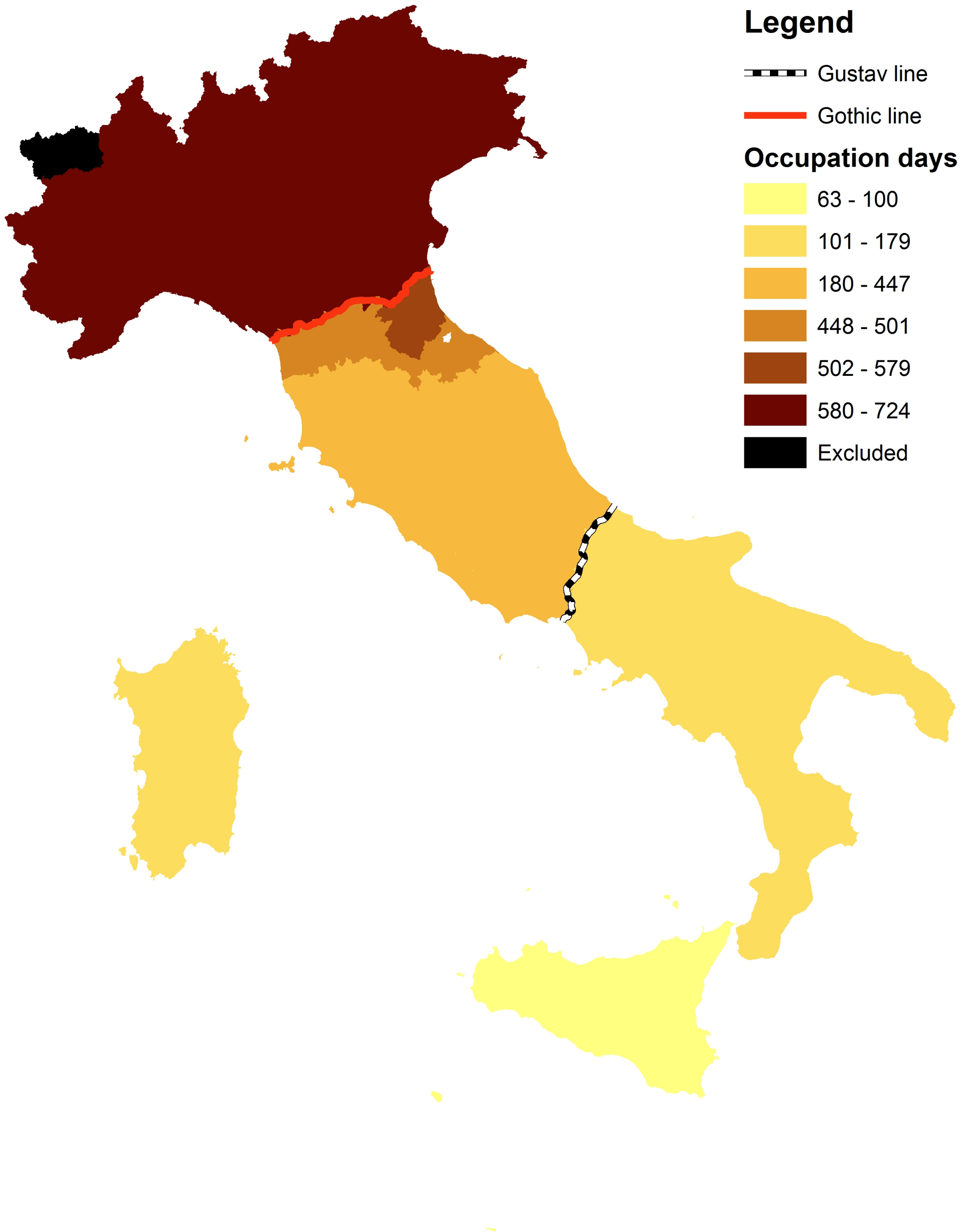
Note: Geographic distribution of left-wing and other partisan brigades. See Appendix 3.B for historical sources.

Figure 3.C.11: Municipality birthplace of a partisan



Note: Geographic distribution of birthplace of partisans. See Appendix 3.B for historical sources.

Figure 3.C.12: Italy under Nazi Occupation



Bibliography

Abramitzky, R. and L. Boustan (2017). Immigration in american economic history. *Journal of Economic Literature 55*(4), 1311–45.

Acemoglu, D., S. Johnson, and J. A. Robinson (2001). The colonial origins of comparative development: An empirical investigation. *American Economic Review 91*(5), 1369–1401.

Acharya, A., M. Blackwell, and M. Sen (2015). Explaining attittudes from behavior: A cognitive dissonance approach.

Achen, C. H. and L. M. Bartels (2016). *Democracy for Realists. Why Elections Do Not Produce Responsive Government*. Princeton University Press.

Aghion, P., C. Harris, P. Howitt, and J. Vickers (2001). Competition, imitation and growth with step-by-step innovation. *The Review of Economic Studies 68*(3), 467–92.

Airbnb (2018). Airbnb uk insights report. Technical report.

Alesina, A., R. Baqir, and W. Easterly (1999). Public goods and ethnic divisions. *The Quarterly Journal of Economics 114*(4), 1243–1284.

Alesina, A. and E. La Ferrara (2005). Ethnic diversity and economic performance. *Journal of Economic Literature 43*(3).

Alfaro, L., P. Conconi, H. Fadinger, and A. F. Newman (2016). Do prices determine vertical integration? *The Review of Economic Studies 83*(3), 855–888.

Almagro, M. and T. Domínguez-Iino (2020). Location sorting and endogenous amenities: Evidence from amsterdam. *Working Paper*.

Amiti, M. and J. Konings (2007). Trade liberalization, intermediate inputs, and productivity: Evidence from indonesia. *American Economic Review 97*(5), 1611–1638.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American Statistical Association 103*(484), 1481–1495.

ANPI-INSMLI (2016). Atlante delle stragi naziste e fasciste in italia. www.straginazifasciste.it, visited on April 28, 2016.

Autor, D., D. Dorn, G. Hanson, and K. Majlesi (2020). Importing political polarization? the electoral consequences of rising trade exposure. *American Economic Review 110*(10), 3139–83.

Autor, D., D. Dorn, G. H. Hanson, G. Pisano, and P. Shu (2020). Foreign competition and domestic innovation: Evidence from u.s. patents. *American Economic Review: Insights Forthcoming*.

Avdeenko, A. and T. Siedler (2016). Intergenerational correlations of extreme right-wing party preferences and attitudes toward immigration. *Scandinavian Journal of Economics 119*(3), 768–800.

Balcells, L. (2011). Continuation of politics by two means: Direct and indirect violence in civil war. *Journal of Conflict Resolution 55*(3), 397–422.

Baldissara, L. et al. (2000). *Atlante storico della Resistenza italiana*. B. Mondadori.

Barron, K., E. Kung, and D. Proserpio (2020). The effect of home-sharing on house prices and rents: Evidence from airbnb. *SSRN Working Paper*.

Bauer, M., C. Blattman, J. Chytilova, J. Henrich, E. Miguel, and T. Mitts (2016). Can war foster cooperation? *Journal of Economic Perspectives 30*(3), 249–274.

Bayer, P., F. Ferreira, and R. McMillan (2007). A unified framework for measuring preferences for schools and neighborhoods. *Journal of Political Economy 115*(4), 588–638.

Becker, S. O. and T. Fetzer (2016). Does migration cause extreme voting? *CAGE Working Paper 306*.

Bellows, J. and E. Miguel (2009). War and local collective action in sierra leone. *Journal of Public Economics 93*(11), 1144–1157.

Bernard, A. B., S. J. Redding, and P. K. Schott (2011). Multiproduct firms and trade liberalization. *The Quarterly journal of Economics 126*(3), 1271–1318.

Besley, T. and T. Persson (2011). *Pillars of prosperity: The political economics of development clusters*. Princeton University Press.

Blattman, C. (2009). From violence to voting: War and political participation in uganda. *American Political Science Review 103*(02), 231–247.

Block, M. K., F. C. Nold, and J. G. Sidak (1981). The deterrent effect of antitrust enforcement. *Journal of Political Economy 89*(3), 429–445.

Bocca, G. (2012). *Storia dell'Italia partigiana*. Feltrinelli Editore.

Borusyak, K., P. Hull, and X. Jaravel (2020). Quasi-experimental shift-share research designs. *Working Paper*.

Bradford, A. and A. Chilton (2018). Competition law around the world from 1889 to 2010: The competition law index. *Journal of Competition Law and Economics 14*, 393–432.

Bradford, A., A. S. Chilton, C. Megaw, and N. Sokol (2019). Competition law gone global: Introducing the comparative competition law and enforcement datasets. *Journal of Empirical Legal Studies 16*(2), 411–443.

Broz, J. L., J. Frieden, and S. Weymouth (2020). Populism in place: The economic geography of the globalization backlash. *International Organization*.

Calder-Wang, S. (2020). The distributional impact of the sharing economy on the housing market. *Working Paper*.

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2016). rdrobust: Software for regression discontinuity designs.

Campbell, A., P. Converse, W. Miller, and D. Stoke (1960). *The American Voter*. The University of Chicago Press.

Canetti, D. and M. Lindner (2015). Exposure to political violence and political behavior. *Psychology of Change: Life Contexts, Experiences, and Identities, ed. Katherine Reynolds and Nyla Branscombe. New York: Psychology Press*, 77–95.

Canetti-Nisim, D., E. Halperin, K. Sharvit, and S. E. Hobfoll (2009). A new stress-based model of political extremism personal exposure to terrorism, psychological distress, and exclusionist political attitudes. *Journal of Conflict Resolution 53*(3), 363–389.

Cervetti, G. (1999). *L'oro di Mosca. La verità sui finanziamenti sovietici al PCI raccontata dal diretto protagonista*. Dalai Editore.

Churchill, W. (1959). *Memoirs of the Second World War: An Abridgement of the Six Volumes of The Second World War*. Boston: Houghton Mifflin Co.

Colantone, I. and P. Stanig (2018). Global competition and brexit. *American Political Science Review 112*(2), 201–218.

Collotti, E., R. Sandri, and F. Sessi (2000). *Dizionario della resistenza I*. G. Einaudi.

Collotti, E., R. Sandri, and F. Sessi (2006). *Dizionario della resistenza II*. G. Einaudi.

Conley, T. G. (1996). *Econometric modelling of cross-sectional dependence*. Ph. D. thesis, Department of Economics, University of Chicago, Chicago, IL.

Conley, T. G. (1999). Gmm estimation with cross sectional dependence. *Journal of Econometrics 92*(1), 1–45.

Conley, T. G. (2010). *Microeconometrics*, Chapter Spatial Econometrics, pp. 303–313. Palgrave Macmillan UK.

Corbetta, P. and M. S. Piretti (2009). *Atlante storico-elettorale d'Italia: 1861–2008*. Zanichelli.

Costalli, S. and A. Ruggeri (2015). Forging political entrepreneurs: Civil war effects on post-conflict politics in italy. *Political Geography 44*, 40–49.

Covarrubias, M., G. Gutiérrez, and T. Philippon (2019). From good to bad concentration? u.s. industries over the past 30 years. *National Bureau of Economic Research* (w25983).

CSIT (2012). Banca dati delle violenze nazifasciste denunciate dai carabinieri (1944–1946). CSIT stands for: Commissione storica italo-tedesca.

Dahlberg, M., K. Edmark, and H. Lundqvist (2012). Ethnic diversity and preferences for redistribution. *Journal of Political Economy 120*(1), 41–76.

De Loecker, J. (2011). Product differentiation, multiproduct firms, and estimating the impact of trade liberalization on productivity. *Econometrica 79*(5), 1407–1451.

De Loecker, J., J. Eeckhout, and G. Unger (2020). The rise of market power and the macroeconomic implications. *The Quarterly Journal of Economics 135*(2), 561–644.

Dehdari, S. H. and K. Gehring (2018). The origins of common identity: Evidence from alsace-lorraine. *CesIFO Working Paper* (6556).

Dell, M. (2010). The persistent effects of peru's mining mita. *Econometrica 78*(6), 1863–1903.

Dell, M. and P. Querubin (forthcoming). Nation building through foreign intervention: Evidence from discontinuities in military strategies. *Quarterly Journal of Economics*.

Devarajan, S. and D. Rodrik (1989). Trade liberalization in developing countries: do imperfect competition and scale economies matter? *American Economic Review 79*(2), 283–287.

Digimap (2020). Points of interest [CSV geospatial data], scale 1:1250, tiles: GB, ordnance survey (GB), using: EDINA digimap ordnance survey service, downloaded: 2020.

Djankov, S., E. Glaeser, R. La Porta, F. Lopez-de Silanes, and A. Shleifer (2003). The new comparative economics. *Journal of Comparative Economics 31*(4), 595–619.

Draca, M., S. Machin, and R. Witt (2011). Panic on the streets of london: Police, crime, and the july 2005 terror attacks. *American Economic Review 101*(5), 2157–81.

Duso, T., M. Claus, M. Shaefer, and K. D. Tran (2020). Airbnb and rents: Evidence from berlin. *Working Paper*.

Dustmann, C., K. Vasiljeva, and A. Piil Damm (2019). Refugee migration and electoral outcomes. *The Review of Economic Studies 86*(5), 2035–2091.

Easterly, W. and R. Levine (1997). Africa's growth tragedy: Policies and ethnic divisions. *The Quarterly Journal of Economics 112*(4), 1203–1250.

Edelman, B. and M. Luca (2014). Digital discrimination: The case of airbnb.com. *Harvard Business School Working Paper 14*(054).

Edelman, B., M. Luca, and D. Svirsky (2017). Racial discrimination in the sharing economy: Evidence from a field experiment. *American Economic Journal: Applied Economics 9*(2), 1–22.

Eichengreen, B. (2018). *The Populist Temptation: Economic Grievance and Political Reaction in the Modern Era*.

Faccio, M. and L. Zingales (2017). Political determinants of competition in the mobile telecommunication industry. *National Bureau of Economic Research* (w23041).

Farronato, C. and A. Fradkin (2018). The welfare effects of peer entry in the accommodation market: The case of airbnb. *NBER Working Paper 24361*.

Ferwerda, J. and N. L. Miller (2014). Political devolution and resistance to foreign rule: A natural experiment. *American Political Science Review 108*(3), 642–660.

Fetzer, T. (2019). Did austerity cause brexit? *American Economic Review 109*(11), 2849–86.

Fontana, N., M. Manacorda, G. Russo, and M. Tabellini (2019). Emigration and long-run economic development: the effects of the italian mass migration. *Working paper*.

Fouka, V. and H.-J. Voth (2013). Reprisals remembered: German-greek conflict and car sales during the euro crisis.

Fradkin, A., E. Grewal, and D. Holtz (2020). Reciprocity in two-sided reputation systems: Evidence from an experiment on airbnb. *Working Paper*.

Garcia-López, M.-A., J. Jofre-Monseny, R. Martínez Mazza, and M. Segú (2019). Do short-term rental platforms affect housing markets? evidence from airbnb in barcelona. *IEB Working Paper 05*.

Ge, Y., C. R. Knittel, D. MacKenzie, and S. Zoepf (2020). Racial discrimination in transportation network companies. *Journal of Public Economics*.

Gelman, A. and G. Imbens (2014). Why high-order polynomials should not be used in regression discontinuity designs.

Gentile, C. (2015). *I crimini di guerra tedeschi in Italia (1943–1945)*. Einaudi.

Goldsmith-Pinkham, P., I. Sorkin, and H. Swift (2020). Bartik instruments: What, when, why, and how. *American Economic Review 110*(8), 2586–2624.

Goldstein, M., M. S. Khan, and L. H. Officer (1980). Prices of tradable and nontradable goods in the demand for total imports. *The Review of economics and Statistics*, 190–199.

Greene, W. (2018). *Econometric Analysis*. Pearson.

Grosfeld, I. and E. Zhuravskaya (forthcoming). Cultural vs. economic legacies of empires: Evidence from the partition of poland. *Journal of Comparative Economics*.

Grosjean, P. (2014). Conflict and social and political preferences: Evidence from World War ii and civil conflict in 35 European countries. *Comparative Economic Studies 56*(3), 424–451.

Grullon, G., Y. Larkin, and R. Michaely (2019). Are us industries becoming more concentrated? *Review of Finance 23*(4), 697–743.

Guiso, L., P. Sapienza, and L. Zingales (2016). Long-term persistence. *Journal of the European Economic Association 14*(6), 1401–1436.

Guriev, S. and E. Papaioannou (2020). The political economy of populism. *Journal of Economic Literature*.

Gutiérrez, G. and T. Philippon (2017). Declining competition and investment in the us. *National Bureau of Economic Research* (w23583).

Hall, P. (1992). *The Bootstrap and Edgeworth Expansion*. Springer.

Halla, M., A. F. Wagner, and J. Zweimüller (2017). Immigration and voting for the far right. *Journal of the European Economic Association 15*(6), 1341–1385.

Hirschman, A. O. (1970). *Exit, Voice, and Loyalty*. Harvard University Press.

Holland, J. (2008). *Italy's Sorrow. A Year of War, 1944–45*. Harper Press.

Hsiang, S. M. (2010). Temperatures and cyclones strongly associated with economic production in the caribbean and central america. *PNAS 107*(35), 15367–15372.

Hylton, K. N. and F. Deng (2007). Antitrust around the world: An empirical analysis of the scope of competition laws and their effects. *Antitrust Law Journal 74*(2), 271–341.

Iwanowsky, M. and A. Madestam (2017). Surviving the killing fields: The cultural and political heritage of the khmer rouge. *mimeo*.

Keele, L. J. and R. Titiunik (2014). Geographic boundaries as regression discontinuities. *Political Analysis 23*(1), 127–155.

Knack, S. and P. Keefer (1997). Does social capital have an economic payoff? a cross-country investigation. *The Quarterly Journal of Economics 112*(4), 1251–1288.

Kocher, M. A. and N. P. Monteiro (2015). What's in a line? natural experiments and the line of demarcation in wwii occupied france. Available at SSRN: ssrn.com/abstract=2555716.

Koster, H. R., J. van Ommeren, and N. Volkhausen (2019). Short-term rentals and the housing market: Quasi-experimental evidence from airbnb in los angeles. *Working Paper*.

Kovak, B. K. (2013). Regional effects of trade reform: What is the correct measure of liberalization? *American Economic Review 103*(5), 1960–76.

Krishna, P. and D. Mitra (1998). Trade liberalization, market discipline and productivity growth: new evidence from india. *Journal of development Economics 56*(2), 447–462.

La Porta, R., F. Lopez-de Silanes, A. Shleifer, and R. W. Vishny (1998). Law and finance. *Journal of political economy 106*(6), 1113–1155.

Larcom, S., F. Rauch, and T. Willems (2017). The benefits of forced experimentation: Striking evidence from the london underground network. *The Quarterly Journal of Economics 132*(4), 2019–2055.

Leoni, F. (2001). *Storia dei partiti politici italiani*. Guida editori.

Lupu, N. and L. Peisakhin (2017). The legacy of political violence across generations. *American Journal of Political Science 61*(4), 836–851.

Mantelli, B. and N. Tranfaglia (2013). *Il libro dei deportati*, Volume 3. Ugo Mursia Editore.

Margalit, Y. (2019). Economic insecurity and the causes of populism, reconsidered. *Journal of Economic Perspectives 33*(4), 152–70.

Matta, T. (1996). *Un percorso della memoria: guida ai luoghi della violenza nazista e fascista in Italia*. Mondadori Electa.

Mayhew, D. R. (2004). *Electoral Realignments: A Critique of an American Genre*. Yale University Press.

Mian, A. and A. Sufi (2014). What explains the 2007-2009 drop in employment? *Econometrica 82*(6), 2197–2223.

Miguel, E., S. M. Saiegh, and S. Satyanath (2011). Civil war exposure and violence. *Economics & Politics 23*(1), 59–73.

Montemaggi, A. (1980). *L'offensiva della linea gotica: Autunno 1944*. Giudicini e Rosa.

Mudde, C. and C. R. Kaltwasser (2017). *Populism*. Populism, Oxford University Press.

Nicholson, M. W. (2008). An antitrust law index for empirical analysis of international competition policy. *Journal of Competition Law and Economics 4*(4), 1009–1029.

Ochsner, C. and F. Roesel (2016). Migrating extremists. *CESifo Working Paper No. 5799*.

Orbis (2016). Orbis - bureau van dijk. *Brussels, Belgium*.

Pavcnik, N. (2012). Trade liberalization, exit, and productivity improvements: Evidence from chilean plants. *The Review of Economic Studies 69*(1), 245–276.

Pavone, C. (1991). *Una guerra civile. Saggio storico sulla moralità nella Resistenza*. Bollati Boringhieri, Torino.

Peeters, P., S. Gössling, J. Klijs, C. Milano, M. Novelli, C. Dijkmans, E. Eijgelaar, S. Hartman, R. Heslinga, Jasper Isaac, O. Mitas, S. Moretti, J. Nawijn, B. Papp, and A. Postma (2018). Research for tran committee - overtourism: impact and possible policy responses. *European Parliament, Policy Department for Structural and Cohesion Policies*.

Philippon, T. (2019). *The Great Reversal: How America Gave Up on Free Markets*. Harvard University Press.

Putnam, R. D. (1993). The prosperous community: Social capital and public life. *The American Prospect* (13), 1993.

Quattrone, G., D. Proserpio, D. Quercia, L. Capra, and M. Musolesi (2016). Who benefits from the "sharing" economy of airbnb? *WWW*.

Rochat, G. (2005). *Le guerre italiane 1935-1943: dall'impero d'Etiopia alla disfatta*. G. Einaudi.

Rodden, J. (2019). *Why Cities Lose: The Deep Roots of the Urban-Rural Political Divide*.

Rodrik, D., A. Subramanian, and F. Trebbi (2004). Institutions rule: the primacy of institutions over geography and integration in economic development. *Journal of economic growth 9*(2), 131–165.

Romanelli, R. (1995). *Storia dello Stato italiano dall'Unità a oggi*. Donzelli Editore, Roma.

Ronchetti, G. (2009). *La linea gotica: i luoghi dell'ultimo fronte di guerra in Italia*. Mattioli 1885.

Rondon, T. (2019). Unfairbnb! the effect of short-term letting on electoral behaviour. *Working Paper*.

Rozenas, A. and Y. M. Zhukov (2019). Mass repression and political loyalty: Evidence from stalin's terror by hunger. *American Political Science Review 113*, 569–583.

Schaefer, M. and K. D. Tran (2020). Airbnb, hotels, and localized competition. *Working Paper*.

Sheppard, S. and A. Udell (2016). Do airbnb properties affect house prices? Department of economics working papers, Department of Economics, Williams College.

Sims, D. P. (2007). Out of control: What can we learn from the end of massachusetts rent control? *Journal of Urban Economics 61*(1), 129–151.

Sundquist, J. L. (2011). *Dynamics of the Party System: Alignment and Realignment of Political Parties in the United States*. Brookings Institution Press.

Suárez Serrato, J. C. and P. Wingender (2016). Estimating local fiscal multipliers. *NBER Working Paper 22425*.

Syverson, C. (2019). Macroeconomics and market power: Context, implications, and open questions. *Journal of Economic Perspectives 33*(3), 24–43.

Tabellini, M. (2020). Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration. *Review of Economic Studies 87*(1), 454–486.

Tiebout, C. M. (1956). A pure theory of local expenditures. *The Journal of Political Economy 64*(5), 416–424.

Tzioumis, K. (2018). Demographic aspects of first names. *Sci Data 5*.

UNCTAD (2008). The effects of anti-competitive business practices on developing countries and their development prospects. *United Nations, New York and Geneva*.

UNWTO (2020). World tourism barometer. Technical Report 1, World Tourism Organization.

van Lohuizen, A. and B. Smith (2017). Projections of demand and supply for visitor accommodation in london to 2050. *GLA Economics Working Paper 88*.

Vickers, J. (1995). Concepts of competition. *Oxford Economic Papers, New Series 47*(1), 1–23.

Voigtländer, N. and H.-J. Voth (2012). Persecution perpetuated: The medieval origins of anti-semitic violence in Nazi Germany. *Quarterly Journal of Economics 127*(3), 1339–1392.

Walter, B. F. (2017). The extremists's advantage in civil wars. *International Security 42*, 7–39.

Westfall, P. H. and S. S. Young (1993). *Resampling-Based Multiple Testing: Examples and Methods for p-Value Adjustment*. Wiley.

WTTC (2019). City travel & tourism impact 2019. Technical report.

Xu, B. (2003). Trade liberalization, wage inequality, and endogenously determined nontraded goods. *Journal of International Economics 60*(2), 417–431.

Yasmeen, R. (2019). Top 100 city destinations 2019 edition. *Euromonitor International*.

Young, A. (2018). Channeling fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results. *The Quarterly Journal of Economics 134*(2), 557–598.

Zervas, G., D. Proserpio, and J. W. Byers (2017). The rise of the sharing economy: Estimating the impact of airbnb on the hotel industry. *Journal of Marketing Research 54*(5), 687–705.